



How does family matter? On the role of heterogeneous family compositions in determining children's outcomes using longitudinal administrative data

Dissertation presented to obtain the degree of Doctor in Economics

by

Silvia Palmaccio

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Doctoral Committee

Advisors:

Prof. dr. Kristof De Witte	KU Leuven
Prof. dr. Deni Mazrekaj	Utrecht University

Members:

Prof. dr. Erwin Ooghe	KU Leuven
Prof. dr. Mike Smet	KU Leuven
Prof. dr. Dieter Verhaest	KU Leuven
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Prof. dr. Trudie Schils	Maastricht University

Chair:

Prof. dr. Wim Moesen	KU Leuven

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General introduction

Families are the central pillar of children's development. They play a key role in the education and the formation of children, endowing them with skills and abilities that are important for their later-in-life outcomes (Heckman et al., 2006; Mogstad and Torsvik, 2021). In education, parental inputs such as time, economic resources, and parenting practices are shown to significantly affect children's academic attainment (e.g. Amato and Fowler, 2002; Bono et al., 2016; Boonk et al., 2018; Price, 2008). Siblings matter as well, as older siblings' educational choices and academic performance are shown to spill over on younger siblings' educational outcomes (e.g. Altmejd et al., 2021; Nicoletti and Rabe, 2019).

Besides education, family plays an important role also in determining other outcomes in adulthood, from the labour market to fertility. On the labour market, parents are shown to influence their children's occupations, either directly through their networks of acquaintances or indirectly by passing on their interests and preferences (Corak and Piraino, 2011; Kramarz and Skans, 2007, 2014). With respect to fertility behavior, the presence of living parents who can provide active childcare and economic support as grandparents has been shown to affect the probability that children enter into parenthood or have higher-order births (Aassve et al., 2012; Pessin et al., 2021; Rutigliano, 2020; Thomese and Liefbroer, 2013). Siblings influence fertility decisions as well, as the timing of first childbearing appears to be affected by siblings' timing of first childbearing (Lyngstad and Prskawetz, 2010). More generally, throughout life, family acts as a safety net, providing shelter in case of economic or health difficulties (Seltzer, 2019).

Traditionally, families are defined as consisting of a mother and a father, united in a stable marital union, who have children that are all biologically related to both. Although this family composition has not disappeared, diverging trends in marriage and divorce rates and the normalization of marriage and parental rights for same-sex couples have made it less obvious over time in developed countries. Indeed, a considerable number of children live in families that do not include both biological parents or where the parents are a same-sex couple. However, due to the difficulties associated with reconstructing family compositions longitudinally and the traditional reliance of earlier literature on crossectional data[1], there

[1]See the Section 3 titled "Research challenges and administrative datasets" in this general introduction.

is still limited evidence about how children who lived in similar 'nontraditional' family structures in childhood perform in their life.

This doctoral dissertation uses longitudinal administrative datasets to explore the role of heterogeneous family compositions on children's outcomes. Specifically, this doctoral dissertation focuses on four family dimensions. First, from an aggregated and more traditional perspective, it explores the role of family socioeconomic background in driving children's outcomes. Then, it digs deeper into family composition effects by examining the consequences on children of parental divorce, complex sibling structures, and same-sex parenting. The primary contribution of this doctoral dissertation is therefore **to provide novel evidence about the influence on children of heterogeneous family environments using large longitudinal administrative data**.

The remainder of this introduction is structured as follows. Section 1 provides a more detailed overview of how and under what aspects families have changed over time, highlighting policy responses to these changes. Section 2 outlines the specific objectives of this doctoral dissertation. Section 3 discusses the challenges associated with doing family research and describes the datasets used in this doctoral dissertation. Finally, Section 4 provides a detailed overview of the chapters, outlining research questions and predictions.

1 The expanding complexity of family compositions

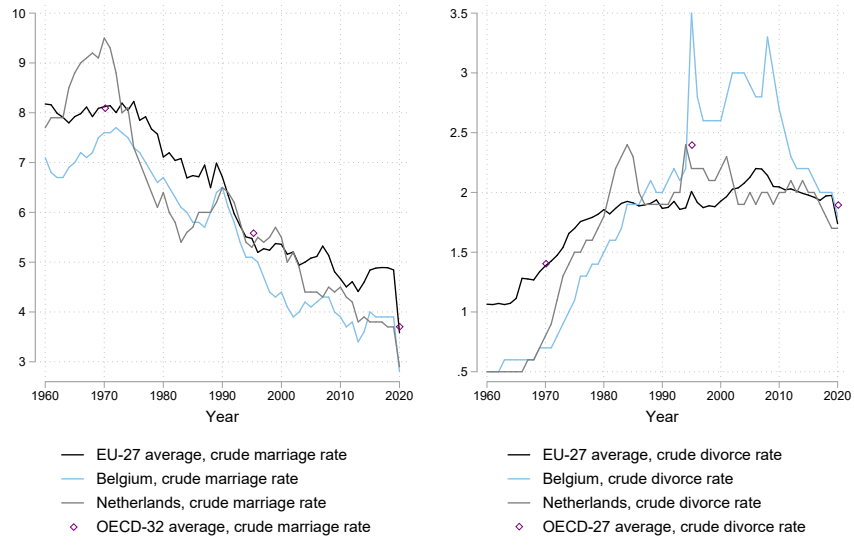
1.1 Trends in marriage, divorce, and same-sex unions

Given the importance of family, much attention has been paid to family characteristics as key factors influencing children's outcomes. Traditionally, scholars and policymakers have been concerned about how social inequalities can be exacerbated in families with low levels of parental education and income, as children are prevented from developing those skills and abilities they need to succeed in life (Heckman et al., 2006). In this regard, a notable initiative was in the United States the 'No Child Left Behind' act passed by the US Congress in 2001. This law increased the role of the federal government in ensuring that schools meet the educational goals set by the states, paying particular attention to ensuring that students from marginalized groups, including students from low-income families, improved their academic performance. In the European Union, the European Commission launched in 2021 the 'EU Strategy on the Rights of the Child', whose aim is to promote and protect children's rights by, among other things, "strengthening the socio-economic inclusion of children" as it is "essential to address the passing of poverty and disadvantage through generations." (European Commission, 2021, p. 5).

At the same time, in recent decades, several demographic, social, and legal trends have intertwined, making scholars and policymakers worry about how shifts in another of the family dimensions may negatively reflect on children's outcomes (Raley and Sweeney, 2020). That is, changes in family composition. On the one hand, marriage rates have sub-

stantially decreased, with more and more couples preferring other forms of commitment, such as cohabitation, to marital unions. On the other hand, partly as a result of divorce reforms that made it "easier to divorce", divorces have significantly increased (González and Viitanen, 2009). To put this into perspective, as shown in Figure 1, across OECD countries the crude marriage rate (the number of marriages per 1000 persons) decreased by more than 50 percent in relative terms from 8.1 in 1970 to 3.7 in 2020 (OECD, 2023a). Similarly, the crude divorce rate (the number of divorces per 1000 persons) rose by almost 40 percent from 1.4 in 1970 to 1.9 in 2020 (OECD, 2023a). The rise in divorce rates also resulted in an increase in second or subsequent marriages, as for almost one in five married persons, the marriage follows a prior divorce (OECD, 2023a).

Figure 1: Trends in crude marriage rates (left panel) and crude divorce rates (right panel) across OECD countries, EU countries, Belgium, and the Netherlands



Source: OECD (2023a) Family Database, re-elaboration of OECD data on marriage and divorce rate. *Notes:* Crude marriage rates indicate the number of marriages per 1000 persons; crude divorce rates indicate the number of divorces per 1000 persons. For France, crude marriage and divorce rates cover years 1995-2020; for Cyprus, crude marriage and divorce rates cover years 1964-2019; for Ireland, crude divorce rates cover years 1997-2017; for Italy, crude divorce rates cover years 1970-2020; for Spain, crude divorce rates cover years 1980-2020; for Malta, crude divorce rates cover years 2011-2020.

Changes in marriage and divorce trends induced a number of reactions from policymakers. In some cases, policies have been implemented to reinforce marriage and marital life (Brotherson and Duncan, 2004). For instance, some states in the US[2] introduced a

[2] Louisiana, Arkansas, and Arizona.

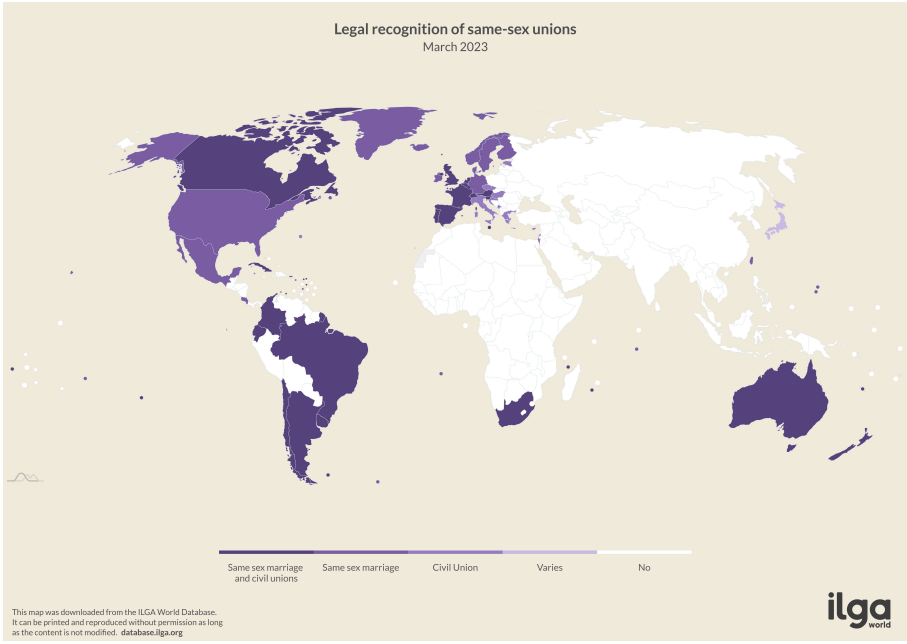
novel type of marriage (the *covenant marriage*), which entails more complex legal procedures in the event that spouses intend to divorce (Brotherson and Duncan, 2004). Other countries in Europe (e.g. Austria) introduced cash transfers to incentivize marriage (Frimmel et al., 2014). A consistent body of policies, however, has been directed at supporting single parents. As it will be discussed later in this introduction[3], single parents are among the most vulnerable subgroups in society as they often suffer from employment instability and work-life imbalance, which expose them to high risks of poverty and social exclusion (European Commission, 2019b; OECD, 2022a). In the European Union, although family law falls within the competence of the member states, several initiatives have been introduced in order to improve the economic condition of single parents (Nieuwenhuis, 2020). For instance, as part of the Europe 2020 strategy introduced in 2010 to address the challenges posed by the 2008 financial crisis, a specific flagship program (the *European Platform against Poverty*) was created. In this program, member states were asked to 'define and implement measures addressing the specific circumstances of groups particularly at risk', which included single parents (European Commission, 2010). Finally, a last set of policies has been directed at ensuring that children living in new family structures after parental separation enjoy the same rights as children from intact, married families. For example, during the Covid-19 pandemic, it was observed that children with nonbiological parents experienced difficulties moving between EU countries. This was because the parenthood relationship between children and their nonbiological parents was often not recognized in those member states different from the one in which the parental relationship was originally established. To overcome these difficulties, the European Commission adopted a proposal introducing the European Certificate of Parenthood, under which family relationships established in one member country should be recognized in all other EU countries (European Commission, 2022b).

In addition to changes in marriage and divorce rates, over the past years, there have also been changes in the sex composition of partners allowed to marry. The requirement that two partners need to be of opposite sex to formalize their union has indeed been overcome in several countries around the world. With a landmark law passed in 2001, the Netherlands was the first country to allow partners of the same sex to marry. Thereafter, the legislation on same-sex couples' rights quickly evolved, and numerous countries in the world currently recognize same-sex unions (see Figure 2). Among OECD countries, 24 out of the 38 member states recognize same-sex marriage, and among countries for which data are available, same-sex marriages accounted for 2.3 percent of total marriages in 2020 (OECD, 2023a). In addition or as an alternative to same-sex marriage, some countries recognize same-sex civil unions, which, depending on the country's institutional context, may offer the same rights and protections guaranteed by marriage or only part of them.

Although same-sex unions have become prevalent over the years, it should be noted

[3]See the subsection titled "Need for adequate policy frameworks".

Figure 2: Same-sex couples' union rights worldwide



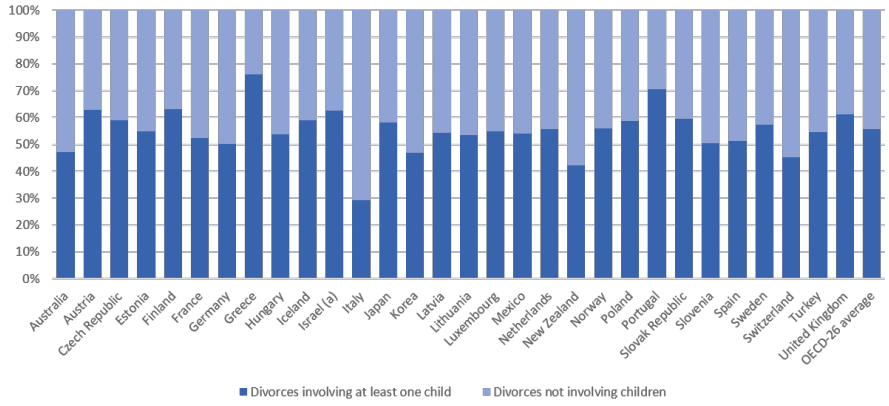
Source: ILGA World, retrieved at https://ilga.org/ilga-world-maps.

that their recognition is neither trivial nor ubiquitous. Most countries that grant same-sex couples' union rights are on the European or American continents, and even within continents, there are great differences between countries in legal recognition. In the European Union, same-sex union rights are generally granted in Northern and Western countries, whereas Eastern and post-socialist countries lag behind. Among the strongest opponents, Hungary has written in its new constitution that 'marriage is the union of one man and one woman' (Article L, Section 1), and similarly also Poland constitutionally defines marriage as 'a union of a man and a woman' (Article 18). Eastern countries lag behind also in the protection of LGBT rights, as discriminatory episodes have seen Poland introduce LGBT-free zones and Hungary bans any content discussing homosexuality aimed at under-18s (De Witte et al., 2022). These episodes prompted the European Commission to take legal action against Poland and Hungary, which resulted in a ruling by the European Court of Justice that violations of the fundamental rights of EU citizens can be a legit reason for cutting the funds member states receive from the EU (Court of Justice of the European Union, 2022).

1.2 Changes in children's living arrangements

All these changes are reflected in the widening of family compositions in which children may live. First, a growing proportion of children resides in families that do not include both biological parents. Data from the OECD (2023b) Family Database show that in most OECD countries more than half of the divorces that occurred in 2017 involved at least one child under the age of 18 (see Figure 3). Also, the share of parents that are separated has increased over the last forty years (OECD, 2023b). Thus, after parental separation, a considerable number of children reside in single-parent families or in stepfamilies, i.e. families in which one of the biological parents is in a marital or cohabiting relationship with a new partner. Digging deeper into the numbers, OECD (2023c) data show that, on average across EU countries, 15.8 percent of children aged 11–15 lived with a single parent in 2013–14 whereas 7.8 percent of children lived in a stepfamily. With respect to the evolution of complex family structures over time, it appears that in the past two decades the share of children living in a single-parent family has remained stable on average across EU countries (see Figure 4).[4]

Figure 3: Share of divorces that involve at least one dependent child across OECD countries

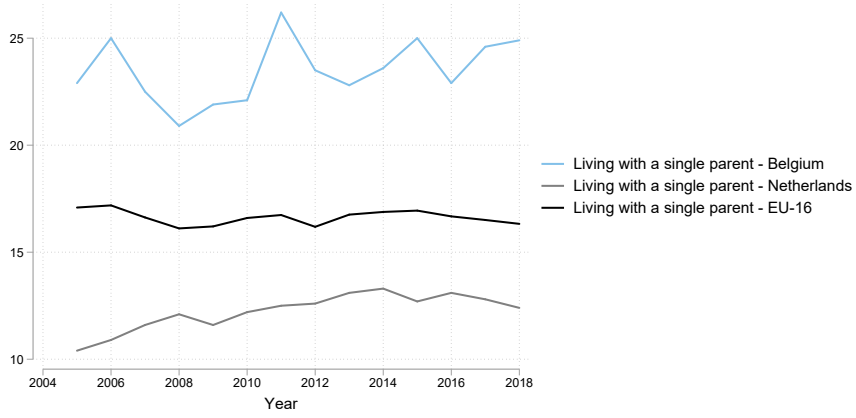


Source: OECD (2023b) Family Database, re-elaboration of OECD data on family dissolution and children (Table SF3.2.B. Distribution of divorce by number of children involved, 2017 or latest). *Notes:* Dependent children are 'children under the age of 18 who depend on either of the parties to a divorce at the time the petition for divorce is filed' (OECD, 2023b).

Second, the sibling composition within the family has also become different, as children are more than ever likely to live in families with i) fewer (full) siblings; ii) more 'nontraditional' siblings. With respect to the former, the increased participation of women in the labor market and the traditionally negative correlation between education and fertility have led many men and women to postpone the decision to start a family. As a result, the av-

[4]No data on stepfamilies is available to track their evolution over time.

Figure 4: Share of children living with a single parent across EU countries over the years



Source: OECD (2023d) Family Database, re-elaboration of OECD data on children in families (Chart SF1.2.B. Trends in the living arrangements of children, 2005 to 2018). *Notes:* EU-16 include Austria, Belgium, Czech Republic, Denmark, Estonia, Finland, Germany, Greece, Italy, Latvia, Lithuania, Netherlands, Poland, Slovenia, Spain, Sweden.

erage age at which men and women become parents has risen and many men and women remain childless or have fewer children than expected. For example, in the Netherlands, the average age at which women have their first child increased from 28.6 in 2000 to 30.2 in 2020 (OECD, 2023e). Moreover, the percentage of women that, at ages 45-49, remained childless with respect to the total number of women having the same age and born in the same cohort increased from 11.3 for women born in 1945 to 17.9 for women born in 1970 (OECD, 2023f). Although not true in every context[5], these changes occurred in several OECD countries, leading the total fertility rate[6] across OECD countries to fall by 44 percent from 2.84 in 1970 to 1.59 in 2020 (OECD, 2023g). As a consequence, household sizes have become smaller, with children having fewer full siblings (siblings with whom they genetically share both biological parents) than in the past (OECD, 2011; Kravdal and Rindfuss, 2008).

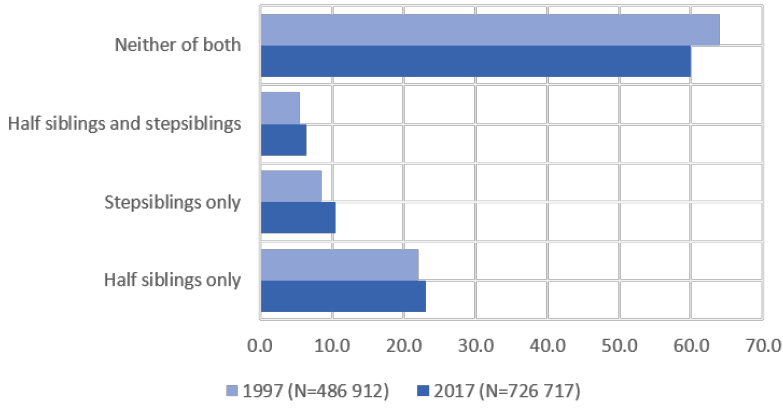
While the number of full siblings has over time decreased, there has however been an increase in the complexity of sibling structures within the household. After separation, if parents repartner, children may find themselves living with half-siblings (siblings with whom they genetically share only one parent), stepsiblings (siblings with whom they do not genetically share any parent but are legally related because the parents are in a romantic relationship), or a combination of half-siblings and stepsiblings. Aggregated data at

[5]For example, there is evidence that in Belgium women with a high level of education desire to have a similar or even higher number of children than women with a low level of education (Van Peer, 2019).

[6]The total number of children that a woman would have if she were to live through all her reproductive years.

the OECD or EU level is not available. However, US Census data show that, in the US, 17.0% of children under 18 were estimated to be living with at least one half-sibling in 2014 (Knop, 2020). Just five years before (in 2009), the corresponding percentage was 10.8% (Knop, 2020). Moreover, over the same years, it was estimated that the percentage of children under 18 living with at least one stepsibling increased from 1.7% in 2009 to 2.2% in 2014 (Knop, 2020). Also in the EU, a considerable number of children have nontraditional siblings. For example, data from Statistics Netherlands (2020) show that, among children whose parents do not live together anymore, 23.1 percent had half-siblings, 10.5 percent had stepsiblings, and 6.4% had both half-siblings and stepsiblings in 2017 (see Figure 5).

Figure 5: Sibling composition of children from non-intact families in the Netherlands

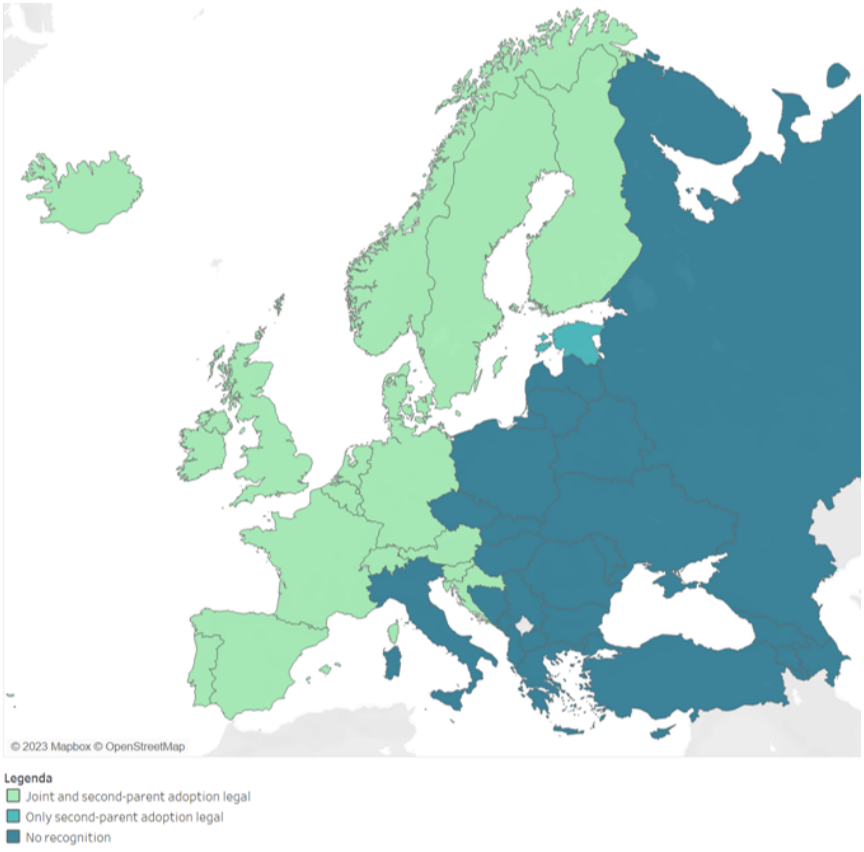


Source: Statistics Netherlands (2020). *Notes:* Children from non-intact families are children who do not live with both biological parents.

Finally, in parallel with the legalisation of same-sex marriages, same-sex couples have been recognized the right to become parents in an increasing number of countries and children may therefore be living in a family with two mothers or two fathers. There are three possible ways by which same-sex couples can form a family: i) by one partner's adoption of the other partner's child (*second-parent adoption)* ii) by the resort to reproductive technologies (e.g. IVF treatment) iii) by joint adoption. Currently, twenty-two countries in Europe allow joint adoption or second-parent adoption for same-sex couples (ILGA World, 2020) (see Figure 6). Although aggregate estimates at the OECD or EU level are not available, and even country-level estimates are difficult to obtain, there is evidence that same-sex families have become increasingly prevalent over the years, both as a result of the aforementioned legal reforms and more tolerant attitudes (Aldén et al., 2017). For instance, it was estimated that in the year after same-sex marriage was legalized in the Netherlands, about 4,320 same-sex couples were raising at least one child together (around one-tenth of

the total number of 48,000 same-sex couples) (Mazrekaj et al., 2023; Steenhof and Harmsen, 2003). Moreover, an average of two out of every thousand children in Sweden are estimated to have lesbian parents (Aldén et al., 2017). Taken together, the considerable number of children living in heterogeneous family compositions is reflected in the broadening of the needs and concerns of these families compared to the traditional father-and-mother family composition.

Figure 6: Same-sex couples' adoption rights in Europe



Source: ILGA World (2020) database, as of 2020. *Notes:* The data for Croatia, Slovenia, and Switzerland have been modified to account for these countries having legalized joint adoption for same-sex couples in 2022.

1.3 Need for adequate policy frameworks

Despite the prevalence of heterogeneous family compositions and the considerable response by policymakers to the aforementioned trends, legal protection of the rights of children raised in 'nontraditional' families is still inadequate. For instance, to date, same-sex couple's

parental rights are not recognized in 11 of the 27 states of the European Union[7], despite same-sex families being present in these countries as well mostly because same-sex partners can move and become parents in countries where same-sex couples' parenting rights are legal (Tryfonidou and Wintemute, 2021). Moreover, there are still legal distances between marriage and cohabitation, so that children born outside marriage and children in cohabiting stepfamilies benefit from less social and legal support than children born in two-parent biological married families (Miho and Thévenon, 2020; Stewart and Timothy, 2020).

This lack of normative protection is relevant for a number of reasons. First, eligibility for social assistance and benefit programs is determined on the basis of detailed information about household composition, size, and economic condition of all family members (Berger and Carlson, 2020). By defining family ties based on biology only, governments might exclude potential recipients that would benefit from social assistance. For instance, in the Netherlands, higher education grants for children from disadvantaged socioeconomic contexts are provided on the basis of the biological parents' income only, with no role played by the income of stepparents.[8] Second, and strictly related to the earlier point, legal uncertainties in family relationships can contribute to further reinforcing economic and social inequalities. There is evidence that it is parents with lower socioeconomic status who are at higher risk of union dissolution, with single parenthood being more common in low-SES families than in high-SES families (de Leeuw et al., 2023; McLanahan and Jacobsen, 2014; Miho and Thévenon, 2020). The negative consequences of parental divorce may therefore be especially detrimental for those working-class families who already experience hardships due to lower economic resources (Miho and Thévenon, 2020).

The practical consequences of the invisibility of these families become evident in the aftermath of a drastic global shock such as Covid-19. The social assistance measures introduced to help families in a country like Italy, where same-sex families are not recognized, were not accessible to same-sex parents (Monaco, 2022). Further, learning deficits due to Covid-19 were shown to be larger for children in single-parented families than children in two-parented families, even when differences in the family socioeconomic status were taken into account (de Leeuw et al., 2023).

Finally, a lot of stigma still surrounds heterogeneous family compositions. In a survey conducted by the European Commission which investigated citizens' attitudes and perceptions towards LGBTQ rights, 26% of respondents did not believe that same-sex couples should have the same marriage rights as different-sex couples (European Commission, 2019a). Large variations were found across countries, with Northern countries generally showing more tolerant and accepting attitudes towards LGBTQ rights than Southern and Eastern countries. For instance, only 8% of respondents disagreed with marriage rights

[7]These countries are Bulgaria, Cyprus, Czech Republic, Greece, Hungary, Italy, Latvia, Lithuania, Poland, Romania, and Slovakia.

[8]More details on the conditions for accessing the grant can be found on the following government website: https://duo.nl/particulier/supplementary-grant/.

for same-sex couples in the Netherlands, whereas 35% of respondents in Italy and 74% of respondents in Bulgaria opposed them. Similarly, stereotypes about stepfamilies are often negative and have their roots in literature and folklore (Valiquette-Tessier et al., 2016). These stereotypes portray stepparents as less caring and affectionate than biological parents, and stepfamilies as more chaotic and destructive than biological two-parent families (Valiquette-Tessier et al., 2016).

1.4 This dissertation

Given the earlier discussed importance of families and the necessity of an appropriate policy framework, there is a need to provide policymakers and judges with data that might help them reduce discretion in decision-making (Stewart and Timothy, 2020). The existing literature on the consequences of heterogeneous family compositions on children is still limited, however, due to methodological and data challenges related to reconstructing family compositions across time. The overall objective of this doctoral dissertation is thus to examine **the life consequences on children of childhood heterogeneous family compositions using longitudinal administrative data**.

Four family dimensions. For this purpose, we explore the role played by four different family dimensions, namely family socioeconomic background, parental divorce, complex sibling structures, and same-sex parenting. These dimensions are selected as representative of the ongoing social, demographic, and legal changes, which, as discussed earlier, have already become prevalent (e.g. socioeconomic inequality or parental divorce) or are about to become more so (e.g. same-sex parenting) in most developed countries. As outcomes, we examine a range of educational, labor market, and fertility outcomes, each chosen as appropriate with respect to the family dimension examined and the current state of the art of the literature.

Data and methods. The use of rich and longitudinal administrative datasets allows us to observe and reconstruct family compositions over time while accounting for an extensive range of individual and family background characteristics. Additionally, to account for bias possibly stemming from selection on unobservables, we employ a range of quantitative methodologies among which Difference-in-Differences, Generalized Synthetic Control, sibling fixed effects models, Oster bounds, and Coarsened Exact Matching. These methodologies have the objective of either directly modeling unobserved heterogeneities or of ensuring the validity of findings by addressing specific endogeneity concerns.

Interdisciplinary focus. While it is important to investigate the quantitative relationships between heterogeneous family compositions and children's outcomes, it is also important to provide an explanation of *why* deviations from traditional family compositions during childhood are expected to lead to different child outcomes. By combining an economic and sociological approach, this doctoral dissertation aims to advance the un-

derstanding of the social processes underlying the observed relationships. To this end, we develop comprehensive theoretical frameworks, within which special attention is paid to the proposition of new theoretical mechanisms that explain the relationships between heterogeneous family compositions and children's outcomes. As such, this doctoral dissertation makes an explicit link between economics and sociology as it combines the rigor of quantitative methods required by economic science with the advancement of theoretical knowledge required by sociological science.

2 Objectives

In assessing heterogeneous family composition effects, this doctoral dissertation explores first, from an aggregate and more traditional perspective, the role played by differences in family socioeconomic status in determining children's outcomes. Then, it investigates more in detail family composition effects by examining the influence on children of parental divorce, complex sibling structures, and same-sex parenting.

As a first step, this doctoral dissertation focuses on the role of family socioeconomic background in schooling decisions. The first specific objective is to examine whether, in families with different socioeconomic backgrounds, parents are concerned differently about their children's school choices. Earlier literature did not reach a general consensus on which social category (if any) is most responsive to schooling decisions. One strand showed how academic orientation of families is largely determined by their level of income (Hastings et al., 2005; Burgess et al., 2015; Farre et al., 2018). Conversely, other studies (Mizala and Urquiola, 2013; Koning and Van der Wiel, 2013; Edmark et al., 2014) found no strong evidence of a differentiated response according to social categories. We contribute to this literature by providing evidence of parents' schooling choices in response to the provision of information on school quality.

As next step, this doctoral dissertation opens the box of family composition effects and focuses on the role of parental divorce in influencing children's short-term and long-term outcomes. Earlier literature has consistently shown descriptive evidence of a negative association between parental divorce and several children's outcomes, such as education, health, and psychological outcomes (for a review, see McLanahan et al., 2013). This literature, however, has two main limitations. First, the majority of these outcomes were measured during childhood or adolescence, and the long-term consequences of parental divorce on adult outcomes remain unclear (McLanahan et al., 2013). Second, earlier literature traditionally examined family composition effects by defining family structures from a parent-child perspective (e.g. comparing single-parented families versus two-parented families), but has overlooked the additional impact that complex sibling structures formed after parental separation might exert on children.

As second specific objective, this doctoral dissertation focuses on the long-term con-

sequences of parental divorce by examining the adult fertility outcomes of children who experienced parental divorce in childhood. As discussed earlier, an important demographic trend that has characterized virtually all developed countries over the last decades is the decline of fertility below replacement level (Morgan and Taylor, 2006; Kravdal and Rindfuss, 2008). Combined with the increased aging of the population, this trend has put pressure on governments as it exacerbates social inequalities by hindering governments' capability of providing social welfare. Whether parental divorce significantly influences adult fertility choices is still unclear, however, as earlier literature traditionally examined the influence of parental divorce on children's first childbirth *timing* or on fertility *intentions* measured at a young age (e.g., Merz, 2012; Axinn and Thornton, 1996; Ermisch and Francesconi, 2001; Ermisch et al., 2004). This chapter contributes to both the literature on family disruption effects and the literature on fertility drivers by providing first evidence on population data of realized fertility choices of children from divorced families.

As third specific objective, this doctoral dissertation examines how complex sibling structures formed after parental separation impact children's educational outcomes. As discussed earlier, after separation many parents remarry or cohabit with other partners, leading children to experience complexity not only in terms of parental composition, but also in terms of sibling composition within the household. Earlier literature compared children with half-siblings to children without half-siblings but living in traditional family compositions (e.g. Sundström, 2013; Ginther and Pollak, 2004; Gennetian, 2005; Halpern-Meekin and Tach, 2008). Yet, it remains unclear whether having resident stepsiblings or younger half-siblings additionally affects the educational outcomes of children on top of the effects due to parental separation and repartnering. We add to earlier literature on parental divorce and on complex sibling structure effects by providing novel evidence of how the birth of a half-sibling and the presence of stepsiblings affect the school performance of children who experienced family reconstruction.

For the last objective, this doctoral thesis focuses on the long-term consequences of living in yet another family composition, namely a same-sex family. Existing evidence on the adult outcomes of children with same-sex parents is to date scarce because, similarly to what has been discussed for divorced families, earlier literature could almost exclusively observe children's outcomes during childhood or adolescence. Using administrative datasets, census data, or population-based surveys, children in same-sex families have most frequently been compared to children in different-sex families in terms of school performance or childhood health (Mazrekaj et al., 2023). As a fourth objective, this doctoral dissertation examines the early labor market performance of young adults who lived in a same-sex family in childhood. We specifically focus on young adults who do not enrol in higher education shortly after leaving high school. The transition from school to the labor market is particularly important to be examined given that, on the one hand, young workers are more exposed to labor market adversities (e.g. unemployment) than adult workers (Eurostat, 2022c). On

the other hand, unfavorable labor market conditions are prevalent among low-skills individuals and at-risk groups (e.g. sexual minorities) are at higher risk of facing discrimination (Oesch, 2010; Drydakis, 2009; Weichselbaumer, 2003) We contribute to earlier literature by providing the first evidence on population data of how children who lived in a same-sex family in childhood perform early on in the labor market.

3 Research challenges and administrative datasets

There exist several important challenges associated with investigating the influence of heterogeneous family compositions on children's outcomes. One first challenge lies in the observation itself of family composition. For instance, same-sex families are not directly observed in the vast majority of data sources. This is because surveys or large-scale data sources do not generally collect information about individuals' sexual orientation, and researchers need to presume whether individuals are gay or straight by looking at the sex of the partner with which one is in a formal relationship (Mazrekaj et al., 2023). More generally, the more the family structure is complex (e.g. in the case of cohabiting families or post-separation families), the more it is difficult to accurately reconstruct family composition. On the one hand, family boundaries are subject to ambiguity in how different family members view them (Brown and Manning, 2009). On the other hand, family compositions can change over time and it becomes challenging to reconstruct them longitudinally.

Traditionally, family composition effects have been examined using surveys. Surveys, however, are prone to a number of errors. One such error is the response error, which occurs when individuals do not report an event of interest or do not remember the correct date on which an event occurred. This type of error is crucial in family research (Kreyenfeld and Bastin, 2016)[9], as the reconstruction of family structure relies on individuals correctly remembering all their relationships. However, parents in post-separation family arrangements may be particularly prone to recall bias. There is evidence that short marriages or remarriages are more likely to be incorrectly reported or not reported at all (Mitchell, 2010). Moreover, cohabiting relationships are also often misreported, with the level of inaccuracy increasing the further back the relationship goes in time (Teitler et al., 2006). Exceptional additional challenges arise when having to observe same-sex families (Manning et al., 2014b). First, survey samples are generally not large enough to allow a statistically valid comparison between children in same-sex and different-sex families. Second, they are most frequently crossectional, making it difficult to distinguish whether children

[9]It should be noted that another error that might affect results in survey-based studies is the nonresponse error, arising when sampled individuals do not participate in the interview or do not answer an item on the questionnaire (Mitchell, 2010). This error transforms into bias whenever it is more frequently committed by certain groups (e.g. treatment group) compared to others (e.g. control group) Groves (2006). In a critical assessment of the reliability of divorce studies using surveys, Mitchell (2010) finds evidence of nonresponse errors being committed more frequently by divorced individuals than by nondivorced individuals, leading to an underestimation of divorce frequencies of about 10 percent (Mitchell, 2010).

were raised by same-sex parents from birth or happened to live in a same-sex family for just few years.

To overcome these and additional challenges, this doctoral dissertation employs rich administrative dataset from Flanders (the Dutch-speaking region of Belgium) and longitudinal population dataset from the Netherlands. These countries provide the ideal contexts to explore the role of heterogeneous socioeconomic backgrounds (Belgium) and family compositions (Netherlands). Belgium has experienced a persistent influx of permanent immigrants and inequalities in education that are higher than the average recorded in OECD countries (OECD, 2022b, 2012). This has reflected in increased student diversity in the classroom (OECD, 2017; Iterbeke, 2021). As such, Belgium provides an interesting context for examining differential parental responses by socioeconomic status and thus for addressing the first research objective. The dataset is collected by the Flemish Ministry of Education, and provides detailed information at student level about each student's school choice and socioeconomic status. With respect to the latter, the dataset allows us to observe a series of individual socioeconomic indicators (e.g. mother's education, at-home language, or whether the child is a recipient of an educational grant) which are used to assign additional funding to schools with particularly disadvantaged student bodies.

For the remaining three objectives of this doctoral dissertation, we use the rich population data from the Netherlands. The Netherlands is particularly suited to explore family composition effects. In terms of LGBTQ rights, it is among the most progressive countries in the world due to both the tolerant attitude of its citizens and its advanced legal protection (Mazrekaj et al., 2020). In particular, as same-sex partnership was legalized in 1998, the Netherlands offers a context where same-sex families have been present for more than twenty years. Moreover, with its moderate to high divorce rates, the Netherlands is a country that exemplifies the Western European context (Kalmijn, 2021). Having introduced unilateral divorce in 1971, it allows to follow individuals after divorce for a period of almost five decades.

The Dutch administrative data are a collection of municipal registers, assembled together by Statistics Netherlands (CBS). In the Netherlands, each municipality maintains a separate population register where it is kept track of all the residents living within its area of jurisdiction. In these registers, each resident is assigned a unique identification number which allows to retrieve a variety of data on the individual's demographics (e.g. sex, age, date of birth, educational attainment, income), marital and cohabiting status, and residential address. This identification number also allows to uniquely link residents to their parents and children. A key feature of this dataset is that children and parents can be followed longitudinally from children's birth until the last year for which data is available. This feature is crucial to address our research objectives as it allows to reconstruct all family and sibling structures in which children lived during childhood. Moreover, as information comes from objective sources (i.e. the registers), no misclassification of family structure arises due

to recall bias.

4 Outline of the doctoral dissertation

This doctoral dissertation consists of four chapters. **Chapter 1** evaluates whether families with different socioeconomic backgrounds care differently about children's school choices. **Chapter 2** and **3** focus on separated families. Specifically, whereas **Chapter 2** investigates parental divorce as a potential driver of children's fertility choices, **Chapter 3** examines the impact on educational outcomes of nontraditional siblings. Finally, Chapter 4 focuses on same-sex families, and explores the influence that same-sex parents may exert on children's labor market outcomes.

Chapter 1

In this chapter, we examine parental response to the publication of an indicator of school quality. In doing so, we explore whether in families with diverse socioeconomic backgrounds, parents respond differently to information provision about school quality. For this purpose, we focus on primary education and exploit an open government initiative in Flanders that consisted of the publication of school inspection reports on a governmental website. To assess causality, we employ a difference-in-differences design and compare schools' first-grade enrolment and socioeconomic composition before and after the publication of the inspection reports. Our research question reads as follows:

RQ1: *"Do families with heterogeneous socioeconomic backgrounds respond differently to information provision on school quality in primary education?"*

Several mechanisms might lead to a stronger response from parents with a higher socioeconomic background compared to parents with a lower socioeconomic background. First, high-SES parents may value schooling more than low-SES parents (*culture of poverty theory*, Lareau, 1987) which may motivate them to carefully select schools from the very beginning of their education. Second, being more selective, high-SES parents may be less likely to choose schools on the basis of home-school distance alone but more willing to travel long distances to attend the school of their choice (Van Goeverden and De Boer, 2013). Finally, high-SES parents may possess better skills in researching and interpreting online information (Rothbaum et al., 2008), which may extend to researching and interpreting school inspection reports. Based on these mechanisms, it is speculated that:

"The schools' socioeconomic composition is likely to change after the publication of school inspection reports in primary education, as high-SES parents will be more likely to respond than low-SES parents to the information shocks."

Chapter 2

This chapter compares children from divorced families with children from intact, married families on a range of marital and fertility outcomes (age at first child, age at first marriage, childlessness, number of children). We employ population data from the Netherlands and look at children born in 1970–1980, which we follow from birth until age 40. In the estimations, as baseline, we use a linear regression model estimated by Ordinary Least Squares (OLS), and test the validity of our results against different concerns as follows. First, to account for imbalances in family types, we pre-process our data using Coarsened Exact Matching. Second, to investigate the role of unobservables in driving our findings, we construct a bounding estimator following (Oster, 2019). Also, we exploit parental death as an exogenous source of parental absence and compare children from bereaved families with children from divorced families. The main research question reads as follows:

RQ2: *"Does parental divorce influence children's marital and fertility outcomes in adulthood?"*

With respect to fertility outcomes, it is unclear ex-ante whether experiencing parental divorce will lead to higher or lower fertility in adulthood. On the one hand, parental divorce is expected to lead to higher realized fertility. One first possible reason lies in that economic hardships following parental divorce might lead children from divorced families to start working early, have an early marriage, become parents soon, and thus have more children over time (*economic disruption hypothesis*, McLanahan and Bumpass, 1988). A second reason lies in that children from divorced families might be less likely to postpone parenthood to favor career and education (*human capital and fertility hypothesis*, based on Becker, 1960; Kravdal and Rindfuss, 2008). Finally, children from divorced families may engage in risky sexual behaviors more often (Zeratsion et al., 2014), leading to more frequent unwanted pregnancies (*risk-behavior hypothesis*). On the other hand, other theories predict rather lower fertility of children from divorced families compared to children from intact, married families. Parental divorce may instill in children pessimistic views about marriage and family, leading them to postpone the decision of starting a family with their partner or to want fewer children to protect them from the negative consequences of possible future family disruptions (*socialization hypothesis*, Diekmann and Engelhardt, 1999). Finally, parental divorce might lower children's barriers to divorce (Corak, 2001; Amato and Patterson, 2017), and lead, on the one hand, to more frequent future union breakdowns, thus preventing children from being in a stable union for long enough to become parents or have higher-order births (*union duration hypothesis*, own conceptualization). On the other hand, this intergenerational transmission of divorce may also lead children from divorced families to remarry and have children with other partners, thus increasing their total fertility (*multi-partner fertility hypothesis*, own conceptualization). Based on these mechanisms, we assert that:

"*It is theoretically ambiguous whether experiencing parental divorce in childhood leads to higher or lower realized fertility in adulthood.*"

Chapter 3

In this chapter, we explore whether complex sibling structures (namely having a younger residential half-sibling and/or having lived with a stepsibling) experienced after parental separation affect children's educational outcomes. For this purpose, we use a linear regression model estimated at the individual level which we complement with a sibling fixed effects model estimated at the sibling level to account for unobserved family characteristics. To estimate the latter, we exploit within-family and across-families variations in the complex sibling structures experienced by full siblings when taking the standardized test scores at the end of primary education. We use population data from the Netherlands, and focus on children born in 1995-2008, whom we observe from birth until the end of primary education. For part of the sample (cohorts born in 1995-2003), we also observe diploma attainment at the end of secondary education. Our research question reads as follows:

RQ3: *"Does the birth of a half-sibling or the presence of stepsiblings after parental separation affects children's educational outcomes?"*

Several mechanisms are expected to negatively affect the relationship between complex sibling structures and children's educational outcomes. First, focal children might receive fewer economic and time resources from their (step)parents once nontraditional siblings enter the household. This might be 1) because the presence of nontraditional siblings dilutes the (supposedly fixed) amount of resources originally destined for the focal children (*resource dilution theory*, based on Becker and Tomes, 1976; Cáceres-Delpiano, 2006; Downey, 2001); 2) because stepparents might more likely invest in their biological kinship rather than in stepchildren (*kin selection theory*, based on Mazrekaj et al., 2020). The second mechanism predicts that when nontraditional siblings enter the household, stepchildren may feel estranged from their home and develop a sense of non belonging which may negatively reflect on their school outcomes (*estrangement hypothesis*, based on Mostafa et al., 2018; Tach et al., 2010; Berger et al., 2012). On the basis of these mechanisms, we speculate the following:

"The birth of a half-sibling or the presence of stepsiblings in the household negatively affects children's educational outcomes in primary and secondary education."

Chapter 4

In this chapter, we explore the early labor market outcomes of young adults who lived in a same-sex family in childhood. Specifically, we compare young adults from same-sex families with young adults from different-sex families on several labor market outcomes

(annual wages, hourly wages, employment, fulltime employment, industry choices). Using the rich population dataset from the Netherlands, we focus on children born in 1995–1999 who did not enroll in higher education after high school and whom we follow from birth until age 21. The choice of this specific sample stems from the fact that, first, the data allowing us to reconstruct household structure begin in 1995. Second, we exclude children enrolled in higher education because they may either not work or they may be on the labor market with a short-term perspective. Finally, we chose to observe children at age 21 because that is the minimum age at which they can earn the adult minimum wage. As our main model specification, we estimate a linear regression model, but further test the sensitivity of our findings to imbalances in family types using Coarsened Exact Matching, and to selection on observables by constructing the bounding estimator developed by Oster (2019).

As discussed earlier, there are two possible ways according to which children may be living with same-sex parents. On the one hand, children may live with same-sex (step)parents after having first lived with their biological different-sex parents who broke up because of the coming out of one of the parents (*later-formed same-sex family*). On the other hand, children may be born in a same-sex family from two same-sex partners who became parents via reproductive technologies or adoption (*planned same-sex family*). Because young adults from later-formed same-sex families inevitably experienced the separation of their different-sex parents, the influence of same-sex parenting on their outcomes might be confounded with parental separation. Based on these considerations, our main research questions read as follows:

RQ4: *"How do children who lived in a same-sex family in childhood perform early on in the labor market? Are there heterogeneities in labor market performance between young adults from later-formed same-sex families and planned same-sex families?"*

It is not clear ex-ante how young adults from same-sex families will perform on the labor market. On the one hand, several theories predict a positive influence of same-sex parenting on children's labor market performance. For instance, young adults from same-sex families are likely to grow up in an environment where sex stereotypes are less pronounced, which may enhance especially young girls' selfconfidence and thus reflect on higher earnings (*sex identity theory*, own conceptualization based on Stacey and Biblarz, 2001; Bordalo et al., 2019; Santos-Pinto, 2012). Young adults from same-sex families might also perform better on the labor market because they generally perform well in school, partly because of their generally high socioeconomic status and partly for reasons that are still unexplained (*human capital/signalling theory*, based on Aldén et al., 2017; Mazrekaj et al., 2020; Kabátek and Perales, 2021; Spence, 1973; Becker, 1993), or because their family units are usually small which allows parents to invest more economic or time resources in them (*family formation theory*, based on Black et al., 2007; Mazrekaj et al., 2020; Downey, 2001; Black et al.,

2005). On the other hand, young adults from same-sex families might be subjected to discrimination on the labor market (*discrimination theory*, Stacey and Biblarz, 2001). This discrimination may either be stemming indirectly from their parents' weaker labor market networks, or directly from employers presuming children's sexual orientation based on parents' sexual orientation. Finally, young adults from same-sex families may be more likely to choose occupations in nontraditional industries (i.e. industries predominantly occupated by the opposite sex) as a result of the generally egalitarian partition of household and labor chores among same-sex parents (*sector-selection theory), own conceptualization based on Fulcher et al., 2008; Biblarz and Stacey, 2010*. Based on the aforementioned mechanisms, we state that:

"It is theoretically ambiguous how young adults from same-sex families will perform early on in the labor market."

Table 1: Overview of this doctoral dissertation

Introduction	General overview of the objectives of this doctoral dissertations			
	Family environment	**Research question**	**Data**	**Methods**
Chapter 1	Socioeconomic background	*Do families with heterogeneous socioeconomic backgrounds respond differently to information provision on school quality in primary education?*	Flemish administrative data Inspection data	Difference-in-Differences Generalized Synthetic Control
Chapter 2	Divorced families	*Does parental divorce influence children's marital and fertility outcomes in adulthood?*	Dutch registry data	OLS CEM Oster bounds Parental death comparison
Chapter 3	Complex sibling structures in stepfamilies	*Does the birth of a half-sibling or the presence of stepsiblings after parental separation affects children's educational outcomes?*	Dutch registry data	OLS Sibling fixed effects CEM
Chapter 4	Same-sex families	*How do children who lived in a same-sex family in childhood perform early on in the labor market? Are there heterogeneities in labor market performance between young adults from later-formed same-sex families and planned same-sex families?*	Dutch registry data	OLS CEM Oster bounds
Conclusion	Overview of findings, policy implications, and avenues for future research			

Chapter 1

Information shocks and parental response in education. A case study of an open government initiative.[1]

Abstract

School inspection reports provide transparent information in the market for schools. As an indicator of school quality, they allow parents to make well informed school choices for their children. This chapter analyses parental response to the publication of school inspection reports, by looking at enrolment and socioeconomic composition in first grade of primary schools. We employ a unique natural experiment in Belgium where schools are randomly selected for inspection, and school inspection reports are the first and only source of objective quality data publicly available. This results in exogenous information shocks. Our findings indicate that information about school quality strongly affects school choice. After the publication, the number of students enrolled in positively evaluated schools significantly increases, whereas negatively evaluated schools do not change their size. The overall effects are found to be driven by schools located in rural areas. In urban municipalities, where schools face capacity constraints, surges in demand cannot be accommodated. No differentiated response is observed by schools' socioeconomic composition.

[1]This chapter is based on: Palmaccio, S., Schiltz, F., & De Witte, K. (2022). Information shocks and parental response in education. A case study of an open government initiative. *Government Information Quarterly*, 39(3), 101702.

1 Introduction

Public institutions are increasingly expected to share information about their quality, from healthcare providers to municipalities and education institutions. This push for transparency of public sector performance is driven by the idea that accountability will improve management practices through benchmarking and increased competition. In the education market, increased transparency in indicators used to measure quality can help parents to choose a school (Koning and Van der Wiel, 2013). In a context of free school choice, schools compete for students and the funding assigned to them (MacLeod and Urquiola, 2015; Hoxby, 2003). Advocates of competition argue that it improves student achievement (Burgess, 2016) and raises school productivity (Hoxby, 2003). The most cautious point to possible negative externalities of school choice. For example, more school segregation if schools are allowed to select students (e.g., Hsieh and Urquiola, 2006), and more incentives for schools to select students if parents base their school choice on peer characteristics rather than academic effectiveness (Abdulkadiroğlu et al., 2020). In any case, these accountability mechanisms will only affect school performance if parents are informed about the relative quality of schools (Mizala and Urquiola, 2013) and if quality affects choice (Santos et al., 2017). In other words, clear and accessible information on the quality of schools is a necessary condition to reap the benefits of school choice and competition.

In this chapter, we start assessing the influence of heterogeneous family environments on children's outcomes at an aggregated and more traditional level. Specifically, this chapter investigates parents' response to information shocks using the online publication of school inspection reports in Flanders (the Dutch speaking region of Belgium) as case study. In doing so, this chapter also examines whether differences in family socioeconomic status lead high-SES parents to respond differently than low-SES parents to the provision of information on school quality. A number of studies has focused on the direct effects of information provision on school choice (Hastings and Weinstein, 2008; Mizala and Urquiola, 2013; Koning and Van der Wiel, 2013; Nunes et al., 2015). However, contrary to earlier studies, this chapter provides evidence on the impact of information in a context where parents have no access to any other indicator of school quality (e.g. test scores). Importantly, as the information is uniquely released on a government website, the particular setting of our study allows to empirically examine the effects of an open government initiative, namely the online publication of school inspection reports.

We label this unprecedented provision of information as "information shock". This terminology is often employed in the economics (e.g. Avdic and Karimi, 2018; Bursztyn, 2016; Jarociński and Karadi, 2020) and finance (e.g. Armstrong et al., 2021; Harakeh, 2020; Hung et al., 2015; Jiang and Zhu, 2017) literature to indicate an exogenous, unanticipated provision of information to individuals, that affects an outcome of interest.[2] Our iden-

[2] For instance, the term 'information shock' is used to indicate the disclosure of information on public spending

tification strategy exploits a unique natural experiment in Flanders (the Dutch speaking region of Belgium), where primary schools are randomly selected for school inspection. Apart from school inspection reports, no other objective information on school quality is accessible by parents due to the lack of central examinations. That is, apart from the reports, parents can only rely on subjective sources of information such as word of mouth or schools' self-assessed information such as a school's philosophy or accent placed in teaching and curriculum. As a result, the disclosure of information is the first and only source of objective school quality data parents can rely on.[3] By defining the publication of inspection reports a "shock", therefore, we intend to highlight the exogeneity of the event (schools are randomly selected for inspection), its unpredictability (schools do not know in advance whether they are going to be inspected), and the absence of other accountability tools available to parents before the reports are published. This allows us, in the estimations, to employ a difference-in-differences model using inspected schools as our treatment group and non-inspected schools as our control group. By comparing schools before and after the publication of inspection reports, we attempt to single out the causal effect of information shocks.

With this chapter, first, we contribute to the literature examining the empirical effects of open government (e.g. Aitamurto and Landemore, 2016; Aitamurto and Chen, 2017; de Kool and Bekkers, 2015; Grimmelikhuijsen et al., 2020; Gunawong, 2015; Lee et al., 2019; Maseh and Katuu, 2017; Mergel et al., 2018; Žuffová, 2020). Within two months after the inspection, inspection reports are released online on a government website. The lack of other objective information on school quality as well as the speed in the circulation of information, creates a setting that allows us to (indirectly) relate parental response to open government. Although a large body of literature has explored the theoretical effects of open government, evidence on its empirical effects is still limited, particularly with regard to the use and attitudes of citizens toward open government (Tai, 2021; Wirtz et al., 2019). In a review of the literature, Tai (2021) identifies only two studies investigating the empirical impacts of open government on individuals (Aitamurto and Landemore, 2016; de Kool and Bekkers, 2015). The first study focuses on the effects of a crowdsourced law reform in Finland (Aitamurto and Landemore, 2016). The second study, closer to ours, investigates the perceived impact of online inspection reports in the Netherlands (de Kool and Bekkers, 2015). The authors find that even though Dutch parents consult the inspection website, they consider the inspection data to be of little use. However, de Kool and Bekkers (2015) use a mixed methodology consisting for one part of semi-structured interviews, and for the other part of surveys (that may be prone to response and recall biases). We complement

in Bursztyn (2016); on the quality of public companies in Hung et al. (2015); Harakeh (2020); on monetary policy interventions in Jarociński and Karadi (2020); on oil prices in Armstrong et al. (2021); and on partners' matching quality in Avdic and Karimi (2018).

[3] The absence of any other source of school quality data allows us to consider school inspection reports as a proxy for school quality in the remainder of this chapter. However, and as we further discuss in Section 7, we acknowledge that school inspection reports are only imperfect measures of school quality.

their analysis by offering empirical evidence of parental response using a combination of objective administrative data and public inspection data.

This chapter also makes a specific contribution to the literature on information provision and school choice (Hastings and Weinstein, 2008; Koning and Van der Wiel, 2013; Nunes et al., 2015; Mizala and Urquiola, 2013), as we examine the important role market dynamics play in shaping the effects of performance information. Specifically, we investigate whether the effects of information shocks might be different according to the degree of urbanization of the municipality where the school is located. We may expect that schools located in city centers experience a higher level of competition as a result of the broader school choice set available to parents, while the opposite may be true for schools located in low dense areas. If urban schools are already at the maximum capacity of enrolments for the academic year when the reports are published, they may not be able to accommodate an immediate rise in demand following the information shock, and no effect would be found. To account for school market dynamics, first, we assess the effect of information shocks distinctively for rural and urban schools. Second, as a robustness test, we explicitly define school local markets as the sets of schools laying within a certain radius of kilometers from the evaluated school.

Our results provide evidence that parents do respond to information shocks. Primary schools face an increase in demand, measured by a rise in first grade students, after inspection reports are published. The effect grows larger over time and is driven by schools that received a positive evaluation, whereas schools with negative evaluations do not significantly lose students. No change in schools' SES composition emerges, suggesting that both high- and low-SES parents are likely to react to school quality data. Finally, a difference is detected according to the spatial distribution of schools in the local market. Specifically, information shocks are found to positively affect parental school choice in rural areas, while no effect is found in urban areas. We interpret these findings as evidence that parents consulted school inspection reports, thereby using the government website on which the reports were published. Our findings are robust to a battery of tests, including various fixed effects specifications, synthetic control estimates, different specifications of school local markets, and alternative definitions of primary schools depending on the grades offered in the same building.

2 Literature review

This section begins by illustrating earlier literature that has examined the impacts on society of government proactive release of information. Then, we review earlier research on information provision and school choice. Finally, we discuss the determinants of school choice and reflect on how these determinants relate to past research on information provision and school choice. By describing earlier literature, we illustrate the specific contributions of

this chapter.

2.1 Information provision and public response

Several studies have shown that the proactive release of information to the public by the government is beneficial to society. Information dissemination can be implemented primarily through disclosure regulations, i.e. through policies that require recipients to disclose certain types of information.

The objectives and the empirical effects of disclosure regulations are various. Disclosure programs ultimately aimed at improving collective health and safety have been shown to increase firms' compliance with health standards in the drinking water industry (Bennear and Olmstead, 2008), to discourage hazardous behaviors (related to lead paint) in the housing market (Bae, 2012), to reduce work-related accidents (Johnson, 2020), and to lower recidivism among sex offenders (Duwe and Donnay, 2008; Levenson and Cotter, 2005). Disclosure regulations requiring restaurants to disclose health inspection scores have been found to improve restaurants' future inspection scores and to reduce hospitalizations due to food poisoning (Jin and Leslie, 2003).[4] Finally, disclosure regulations directed at reducing the environmental footprint of production have been found to abate firms' pollutant emissions (Shi et al., 2021), to increase firms' green energy supply in the energy industry (Delmas et al., 2010), and to be negatively correlated with firms' toxic releases (Karkkainen, 2001; Koehler and Spengler, 2007).

Disclosure regulations can also be implemented by governments through the enforcement of labelling requirements.[5] Earlier literature has shown that these requirements are for the most part effective in influencing the behavior of the public. For instance, energy labels that assign an official certification to energy efficient buildings have been found to affect rental prices in the housing market (Eichholtz et al., 2010; Hyland et al., 2013). Nutrition labels that reveal product nutrients were found to influence consumers' dietary habits and to discourage the purchase of unhealthy products (Variyam, 2008; Balcombe et al., 2010). Finally, eco-labels that disclose product sustainability have been shown to affect consumer preferences and to redirect those preferences toward environmentally friendly products (see e.g. Teisl et al., 2002; Loureiro and Lotade, 2005; Bernard et al., 2015).

The release of government information by the government may also affect the level of trust society places in government and the level of public corruption. Regarding trust, some studies showed a positive impact of government transparency on public trust (e.g. Cook et al. 2010), while other studies found that this effect is not significant or is even negative (for a review, see Cucciniello et al., 2017). A possible explanation for these findings is provided by Grimmelikhuijsen et al. (2020), who distinguished between latent transparency

[4] Consistent results have been found when the health inspection scores are disclosed by the media in Almanza et al. (2002).

[5] Note that labelling requirements are not always mandatory. For instance, energy labels and ecolabels are voluntary programs.

(i.e. citizens' awareness that they can monitor governmental processes) and manifest transparency (i.e. the possibility to monitor governmental processes), finding that latent transparency is not reflected in increased trust in government. Regarding corruption, earlier research mostly found that the dissemination of government information reduces the level of public corruption (Cordis and Warren, 2014;Lindstedt and Naurin, 2010; conditional on media freedom in Žuffová, 2020) but increases the likelihood of uncovering corrupt actions (Cordis and Warren, 2014; Vadlamannati and Cooray, 2017). These two counterbalancing effects may explain other studies' seemingly counterintuitive finding that increased transparency is correlated with higher corruption or higher perception of corruption (Cordis and Warren, 2014).

With this chapter, we contribute to this vast but still growing literature examining the effects of government proactive release of information to the public. Our unique empirical research design allows us to show that public dissemination of accountability indicators is effective in influencing choices in the education market.

2.2 Information provision and school choice

A large body of literature has examined the effects of information provision in the education market. Some studies have focused on indirect outcomes of school quality information, such as changes in property prices (Figlio and Lucas, 2004; Machin, 2011), school fees (Andrabi et al., 2017) or student behavior (Holbein and Ladd, 2017; Avitabile and De Hoyos, 2018). Other studies have examined the impact of information disclosure on student test scores and school achievements (Hussain, 2015; Camargo et al., 2018).[6] An important strand of the literature, to which this chapter is directly related, focused on the explicit effects of school quality information on school choice looking at changes in schools' enrolment and composition (Hastings and Weinstein, 2008; Koning and Van der Wiel, 2013; Nunes et al., 2015; Mizala and Urquiola, 2013). These studies, however, could not observe parental response in a setting in which parents had no access to other objective information. Further, while the concept of increased transparency in performance indicators is not novel in the education literature, it is unexplored what the effects of this increased transparency might be if the information is conveyed using Information and Communication Technologies (ICTs). In fact, earlier literature on information provision and school choice could only focus on information disclosed to the public on paper sources.

Hastings and Weinstein (2008) show in two experiments that, when endowed with a simplified three-pages information sheet on school performance, low-income families are more likely to enroll their children in higher-scoring schools. While proving that a shift from complex to simplified information significantly affects parental school choice, their setting does not allow to focus on the effects of a shift from no information to complex

[6]Note that the absence of data on test scores prevents us from examining the effects of information shocks on student achievements in our chapter.

information (i.e. public data on school outcomes). Koning and Van der Wiel (2013) study how the disclosure of school rankings, released by a national newspaper, impact school choice in the Netherlands. Similarly, Nunes et al. (2015) compare schools before and after the publication of school rankings by a national newspaper in Portugal. The two studies find schools to be significantly affected by the disclosure of school rankings. However, in both settings students are subjects to national central examinations in their final year of education, with the resulting outcomes being publicly available. Consequently, parents can obtain some form of school quality information, aside from the content of school rankings. Also, in both studies, shortly after the rankings were released by the newspapers, other sources – national magazines and public authorities – started to share similar information in parallel (online and on paper), offering parents additional means of information. Finally, Mizala and Urquiola (2013) apply a sharp regression discontinuity design to study the effects on school choice of the publication (by a national newspaper and online) of award-winning schools in Chile, finding no parental response. Again, as an evaluating system already existed prior to the 'intervention', parents could have accessed school quality information before the publication of award winners.

Note that, in the literature on information provision and school choice, online information was only additional to the main source provided on paper. For instance, parents could consult an online school choice guide in Hastings and Weinstein (2008), could access the national exam scores on a public website in Nunes et al. (2015), could consult alternative online school rankings published by the Minister of Education in Koning and Van der Wiel (2013), or could check on a webpage the same list of award-winning schools published on the national newspaper in Mizala and Urquiola (2013). The presence of multiple sources of school quality information, however, did not allow these studies to investigate with a specific focus the effects of information provision using ICTs, nor it allowed to relate this information to open government.

2.3 The role of school choice determinants and market dynamics

Earlier literature examining parental preferences for school attributes suggest that parents prefer schools with high academic performance, similar socio-economic composition to their own, and close to their residence (Burgess et al., 2015; Hastings et al., 2009). Other studies looking at parental preferences for school effectiveness, defined as the capability of schools to generate a causal improvement in students' achievements, have found little evidence that parents reward this school attribute (Abdulkadiroğlu et al., 2020; Rothstein, 2006). Finally, another strand of literature indirectly examined preferences of parents for high-performing schools by looking at co-movements between house prices and measured school quality (Figlio and Lucas, 2004; Machin, 2011). These studies found housing valuations to be higher in areas with better performing schools, suggesting that parents are willing to move residence so that their children can attend good quality schools.

Some of these findings, however, are heterogeneous and might emerge in settings in which parents have unequal or incomplete access to information on school quality. For instance, both Hastings et al. (2009) and Burgess et al. (2015) show that parental preferences for academic performance is driven by high-SES parents. However, it may be that although concerned about the relative quality of schools, low-SES parents are not able to observe it but rather substitute it with other easy-to-determine school attributes. Similarly, parents may not value school effectiveness because they are unable to identify it and rather proxy it with other observable school characteristics such as school composition (Abdulkadiroğlu et al., 2020). It is interesting, then, to understand how the literature on information provision relates to the determinants of school choice.

In contrast with findings of Hastings et al. (2009) and Burgess et al. (2015), the literature on information provision and school choice appear to suggest that both high- and low-income parents equally value high-performing schools. Hastings and Weinstein (2008) show that low-SES parents are more likely to choose high-scoring schools when endowed with simplified information on school quality. Mizala and Urquiola (2013) and Koning and Van der Wiel (2013) do not find strong evidence of a differentiated response according to social categories. Further, in line with Hastings et al. (2009); Burgess et al. (2015), this literature appear to confirm that, even when endowed with information on school quality, parents still value proximity to school as a choice determinant. For instance, Hastings and Weinstein (2008) show that parents are more likely to respond to information provision if they live close to a high-performing school. Similarly, Koning and Van der Wiel (2013) reveal that the distance to a school is the most important determinant of school choice also when information on school quality is published through rankings.

Still unexplored in the literature on information provision and school choice is, however, the role that market dynamics play in shaping the effects of performance indicators. For instance, it may be that parents (and students) continue to value home-school distance because they are not able to enroll their children in the good quality schools as identified by the performance indicators. This might happen if schools are at their maximum capacity of enrollments. With population increasingly moving from rural to city centers (Nusche et al., 2015), urban schools can easily reach their capacity limits and may not be able to physically allocate a potential rise in demand following an inspection. Therefore, the effect of information provision might differ according to the level of competition schools face in the local school market. In particular, if market dynamics play a role in influencing the attended effects of accountability policies such as school inspection reports, we would expect a positive effect of information shocks in rural schools, while no effect in urban schools.

3 Setting

3.1 Education system in Flanders

Freedom of education is key in Flanders.[7] Students (and their parents) are free to select their preference school, and families are not forced to change neighborhood of residence to attend good-performing schools (i.e. there are no catchment areas).[8] To ensure that the choice of school is free for all children regardless of their socioeconomic background, a series of regulations are in place. First, schools are not allowed to select students. In city centers, where subscriptions exceed the capacity of schools, lottery systems offer students an equal opportunity to enroll in the school of their choice. Second, in an attempt to reduce school segregation, quotas are in place to guarantee a number of positions for low SES students. This number depends on the socioeconomic environment such that the socioeconomic composition of schools reflects the neighborhood – a parish inside the municipality – they are located in. In fact, quota are set for both low- and high-SES students. Different schools located in the same socioeconomic environment will have the same number of positions available for both groups. Therefore, schools with many low SES students will have relatively more positions available for high SES students, and vice versa.[9]

On the supply side, every person has the right to establish an educational institution, creating a broad heterogeneity in the Flemish educational landscape. Providers include local community governments, private providers (mostly catholic schools) and a centralized state school system, all operating within overlapping geographic regions. All schools, public and private, are funded by the Ministry of Education of Flanders. The equal entitlement to public funding ensures that schools do not to have to rely on other means (e.g. property taxes) for funding. This promotes school integration as it prevents school offer from depending on families' socioeconomic background or neighborhood's wealthiness.[10] Since schools are publicly funded and not allowed to select students, and no tuition fees can be charged to parents in compulsory education, the perceived quality of schools remains the sole means to compete with other schools and attract students.[11]

[7] In the sample, we also include the schools in the Brussels Region that have Dutch as instruction language, as these schools are funded and inspected in exactly the same way as the schools in the Flemish region.

[8] As a consequence, families in Belgium are not likely to move their residence to attend the preferred school for their children. This contrasts to other settings where changing the neighborhood in which you live can be an expedient to elude the school catchment area. Therefore, we will not focus on the (indirect) effects of school inspection reports on residence choices or house prices.

[9] Although in principle the double-quota system is at play in Belgium, it should be noted that in practice, SES quotas are often not respected. First, they come into play only when a school is affected by capacity constraints (Havermans et al., 2018). Second, when children enrol in school, there exist certain groups (e.g., students who have older siblings enrolled in the same school or children of school staff or children who live closer to school) who enrol regardless of a schools SES composition. Thus, it is possible to expect an effect of information shocks also on a school's socioeconomic composition.

[10] The absence of property taxes as a means of school funding entails that no perfect correlation can emerge between school quality and neighborhood quality.

[11] Before the disclosure of school inspection reports, parents would judge a school's quality based on their perception. This indirect information would come from open school days, school infrastructure, SES-characteristics of the student body and school reputation (among the others).

We identify two categories of education facilities: institutions offering both primary education and preschool, and institutions only offering primary education.[12] Despite compulsory education starting at age 6, more than 95% of children attend preschool from the age of 3. We hypothesize that when a child turns six years-old and needs to enroll in the first grade of primary education, parents are more likely to inquire about school quality data if the preschool the child is currently enrolled does not provide primary education. That is, we hypothesize larger responsiveness of parents when they are forced to look for a new institution of primary education. Therefore, to isolate parental response in a context where choice cannot be driven by continuity with preschool, in the remainder of the chapter we focus our attention on institutions that only provide primary education.[13]

3.2 School inspections

In the decentralized Flemish system, there are no central examinations[14], but school inspectorates monitor whether the content and the teaching activities are sufficient to attain the minimal goals and competences set by the government. Goals are centrally imposed and equal for all primary schools. In the absence of central examinations, inspections are the only source of accountability of schools towards the government and parents.

By law, schools are selected for inspection in a random manner. Every year, a different sample of schools is randomly sorted for inspection. Once a school is selected for inspection, it will not be exposed to a random treatment for the incoming 10 years following the inspection.[15] Announced with at least fourteen days' notice, inspections are carried out by members of the Inspectorate through school visits. During the visit, that lasts from three to four days, inspectors examine schools on multiple aspects. Specifically, every school is examined on its capacity to: satisfy the educational regulations; monitor its own quality; address independently its deficits. Although examined in all three aspects, schools are given an evaluation only with respect to their ability to satisfy the educational regulations set by the government. To this end, inspectors investigate schools' education quality and school infrastructure (i.e. compliance with a number of norms, among which safety and hygiene norms).

[12] Apart from the grades offered, no structural differences emerge between the two types of education facilities. The decision of having (not having) preschool is exogenously made by the school board while establishing the school, and does not affect school offer.

[13] In the sample, the majority (around 70 percent) of the preschool students enrolled in an institution offering both preschool and primary education attend the same institution when enrolling in primary education. Only around 30 percent of them switch to a new institution. In Section 6.3, we present a robustness check with the sample enlarged to comprehend also the schools that include preschool, and find no parental response. We further test the hypothesis of no difference in inspection coefficients among the two (original and enlarged) samples, and reject the null for all our significant findings. This confirms our hypothesis that there is a larger parental response when schools only offer primary education.

[14] Currently, all education providers have their own tests, which are used by the schools on a voluntary basis. However, these data are not available to the public.

[15] Since January 2018, a new approach (Inspectie 2.0) was launched. Among other things, the new approach has shortened the time lapse between one inspection and another, from ten to six years. However, as our data cover until academic year 2016/2017, we refer throughout the chapter to the original inspection framework.

To assess education quality, the inspectors examine a selection of two to three courses, different from school to school. Among the most recurrently evaluated courses we find Dutch language, introduction to social sciences, mathematics and music, whose frequency of assessment is around 20 percent each. Other courses, such as French language, physical education or ICT, are less frequently evaluated, and they account jointly for 20 percent of the assessments.[16] In the absence of standardized test scores, inspectors rely for the evaluation only on school processes (Penninckx et al., 2014). Hence, they attend lessons, examine school documentations, and engage in discussions about teaching and management quality with teachers, principals and a sample of students and parents. Each course is compulsorily always evaluated through an assessment of the educational offer, the equipment, the evaluation practices and the learning guidance[17], and can receive either a favourable or an unfavourable evaluation.

After the visit, the inspectors redact an audit report where detailed information regarding the inspection visit are disclosed (we show in Appendix 7 selected and translated parts of an inspection report example). Importantly, every school receives one evaluation on education quality and another one on school infrastructure. Each evaluation can be either positive ('favourable'), 'restricted favourable' or 'unfavourable'.[18] For educational matters, a school receives a positive evaluation if it passes all evaluated courses. If small shortcomings are found with respect to some courses, schools can still receive a positive evaluation contingent on an exemption by the inspectorate for specific classes. Conversely, in case of significant shortcomings on one or more courses, the evaluation given to schools will be restricted favourable or unfavourable. In the former case, schools will face a follow-up inspection three years later to determine whether proper adjustments were put in place.[19] The latter category is assigned to schools that fail to achieve attainment targets and development goals set by the central government. While in theory these schools will be closed, in practice, schools are offered a second chance to develop a strategy to overcome their structural deficiencies, while being monitored on their progresses by an external agency.

As of 2012 inspection reports are published on the inspectorates website, where all inspected schools are listed and their relative inspection reports (including possible follow-up reports) are published.[20] The inspection reports are available only on the inspectorates web-

[16] We group these courses as 'other' in the remainder of this chapter. More detailed information on subjects' assessments can be found in Table 1.A3 of Appendix A.

[17] Note that, in addition to these mandatory aspects, the school inspectorate might decide to examine further aspects. However, these further aspects are supplementary to the mandatory ones, that are by contrast always included.

[18] As almost all schools in our sample score positively with respect to school infrastructure, we focus the analysis on evaluations related to education quality, which offer us enough variance to go by.

[19] Note that the inspectors do not have the legal right to advise schools on how to address their weaknesses. However, schools can rely on the analysis appearing in the inspection report to understand how to improve their strengths and overcome their shortfalls.

[20] Reports are accessible at the following government website: https://data-onderwijs.vlaanderen.be/onderwijsaanbod/bao/lijst. The government website is constantly updated with the most recent reports. To better visualize the structure of an inspection report, we show in Appendix B a translated example of the table of contents of an inspection report, a subject assessment, and the final evaluation given to a school. The original inspection report can be consulted at this link: https://app.akov.be.

site, as schools are not allowed to market these reports to attract students. Prior to 2012, no record of inspection reports was publicly available and schools had no to little incentives to use the inspection reports. Therefore, parents are not expected to be aware of (and react to) prior inspections. In particular, if a school received a positive evaluation prior to 2012, parents would not be aware of it as the evaluation would only be communicated internally to the school. If a school received a negative evaluation prior to 2012, parents would likely be even less informed. The school principal is by law forbidden to spread information from school inspections. Moreover, poor performing schools would hardly attract parents even if they implemented improvements in school practices. Without the knowledge that these schools are urged by the Ministry to tackle their deficiencies, parents would likely rely for schooling decisions on past school characteristics, such as the school's prior (presumably poor) reputation. Therefore, the absence of public reports prior to 2012 entails that schools can be compared before and after the information vacuum was filled by inspection reports.

Our motivation to focus on primary schools in subsequent analyses is threefold. First, inspection reports of primary schools are published within two months following an inspection. This short time period between school inspection and the publication of the report allows us to specify of a clear cut-off year in our empirical analysis. Second, primary schools have a high degree of homogeneity. They offer the same education, forming children on the same set of subjects, irrespective of the location and the provider of education.[21] Third, in primary education, the socioeconomic composition of schools cannot be affected by a school's (financial) policy. Unlike in secondary education, where anecdotal evidence points to strategic decisions of schools to organize expensive school trips in order to influence student composition, the total yearly expenditure that can be charged to parents in primary education is limited to a fixed ceiling (i.e., 85 euro per school year). This ceiling is set by the central government to guarantee effective free school choice and it prevents schools from influencing the student (socioeconomic) composition.

4 Dataset

4.1 Data and variables

Our dataset combines administrative data (2001–2016) with publicly available inspection data (2012–2016).[22] Administrative data provides us with information, at student level, about individual school choice and socioeconomic status. Inspection reports indicate, at school level, the overall and subject-specific evaluations for all inspected institutions, covering a range of five academic years. It should be reminded that all schools in our main sample only offer primary education. Over the period examined, 199 schools were exam-

[21] Note that schools using different methods (e.g. Steiner, Freinet) can still differentiate their offering, while teaching the same content.

[22] Every year refers to the beginning of the academic year (e.g., 2001 refers to academic year 2001/2002).

ined, i.e. the 38.94 percent of total schools in our sample. Further analysing the outcome of the inspection (see Table 1.1), 60.3% of the institutions received a positive ('favourable') evaluation, whereas 39.7% received a negative ('restricted favourable') score.[23]

Table 1.1: Summary statistics of inspected schools

Year	Inspected primary schools	Positive(%)	Negative(%)
2012	57	68.42	31.58
2013	50	60.00	40.00
2014	37	54.05	45.95
2015	26	50.00	50.00
2016	29	62.07	37.93
Total	199	60.30	39.70

Notes: In the period examined, 199 primary schools were inspected. The majority of schools (60.3%) reported a positive ('favourable') evaluation. The remaining 39.7% can be further disentangled in 'restricted favourable' (39.2%) and 'unfavourable' (0.5%). We group the latter two categories into the same category (negative). The total number of schools in the sample (both inspected and non inspected) is 511. Note that all schools in the sample (both inspected and non inspected) only offer primary education.

Our analysis focuses on two main outcome variables: school size and socioeconomic (SES) composition.[24] School size is measured as the total number of students enrolled in first grade of primary education. SES composition is the average socioeconomic composition of the student body characterizing the first grade, and is included to study heterogeneity in parental response, possibly resulting from families' diverse socioeconomic status.[25] It ranges from 0 (high socioeconomic status) to 1 (low socioeconomic status), and is constructed first at student level, as a weighted average of five equal opportunities indicators. These indicators are used in Flanders to assign extra funds to schools with a certain percentage of disadvantaged students. Specifically, they take into account whether: the student belongs to the travelling population; the mother of the student does not have a degree of secondary education; the student lives (permanently or temporarily) outside his family; the student does not speak the native language Dutch at home; the student receives one or more school grants (De Witte et al., 2019, pp 164). To each indicator is assigned a weight, and the weighted sum is computed for every student to obtain a measure of his socioeconomic status.[26] Then, at school level, the SES composition of a school's student body is obtained

[23] Since only 1 school received a negative ('unfavourable') score, it was dropped out of the sample as not representative of its category. Our subsequent analysis holds irrespective of including this specific school. Moreover, since schools receiving a 'restricted favourable' evaluation still experience it as 'unfavourable' (Penninckx et al., 2014), throughout this chapter we refer to a 'restricted favourable' evaluation as negative.

[24] We express our dependent variables in levels. However, to ensure that our effects are not driven by the absolute size, we have also estimated the model in logarithms. The results are virtually unchanged.

[25] Note that, although schools in Flanders do not select students, high or low SES students might sort into different schools according their preferences.

[26] Specifically, the weight coefficient expressed in point is 0.8 points if the student belongs to the travelling population or lives outside his family; 0.6 points if the mother of the student does not have a degree of secondary education;

by averaging out the socioeconomic status of all students attending the first grade of that school.

To further investigate market dynamics related to information shocks, we consider the geographic localization of schools and divide the sample according to whether schools are located in rural or urban municipalities. By distinguishing between rural and urban municipalities, we implicitly define different markets for schools according to the degree of urbanization of the municipality where the school is located. Specifically, the local market for urban schools is expected to be characterized by a high level of school competition due to the low transportation costs experienced by parents. By contrast, the local market for rural schools is presumably characterized by a low level of school competition as a consequence of the high transportation costs incurred by parents.[27] To distinguish between rural and urban schools, we consider an indicator of population density and set the threshold to 750 inhabitants per km^2 as the dividing value. This threshold value was chosen because it corresponds to the lowest population density recorded among the 13 "central cities" (*centrumsteden* in Dutch) in Flanders, i.e., the most populous cities as defined by the Flemish government.[28] Municipalities below such threshold are labeled *rural*, whereas municipalities above it *urban*. Table 1.A2 in Appendix A provides summary statistics for several levels of population density. The threshold of 750 inhabitants per km^2 allows us to have balanced sub-samples, with around 54% of observations in the rural municipalities, and 46% in the urban municipalities. The Kernel densities (Figure 1.A1 in Appendix A) reveal a substantial difference in rural and urban areas, both in school size and in the SES composition. Urban schools tend to have larger first grade school classes, and more dispersion in the socioeconomic composition. Conversely, rural schools are slightly less numerous and present a smaller share of low-SES students.

4.2 Random assessment of education inspectorates

To assure internal validity, schools need to be randomly selected for the inspection such that inspected and non-inspected schools have equal expectations prior to the inspection. In the absence of a random selection, endogeneity issues due to selection bias and unobserved heterogeneity might threaten the validity of results.[29]

Despite by law schools are selected for inspection in a random manner, we assess in

0.4 points if the student receives one or more school grants; 0.2 points if the student does not speak Dutch at home. Points can be accumulated up to a maximum of 1.2 points per student. To facilitate interpretation for the purposes of this study, however, these points have been re-expressed by having 1 as the upper limit. More information on the weighting scheme can be found in De Witte et al. (2019) and De Witte et al. (2018).

[27] In Section 6.3, as an additional robustness test we define the school local markets more explicitly on the basis of school distances. Our results are in line with the findings from the main analysis.

[28] To validate the results of this study, future work might consider using the urbanization indicator given by defining the 13 central cities instead of the population density used in this chapter.

[29] For example, endogeneity issues might arise if the school inspectorate specifically targets schools located in certain neighborhoods, schools with high (low) student-teacher ratio, or schools with high (low) percentages of disadvantaged students, among the others.

three ways the random selection of schools. First, we show in Table 1.2, both for inspected and control schools, means and standard errors of our outcome variables on their values prior to 2012 (i.e. before the start of inspections). Most importantly, we test for the differences in the two groups by regressing each variable on a treatment indicator, and we report the relative *p*-values in the third column.[30] We find that the mean values of both school size and SES composition do not significantly differ between inspected and non-inspected schools. In the bottom part of the table, we compare the indicators used to build the aggregated SES indicator, and again we mostly detect similarity between inspected and non-inspected schools.[31] As second approach, we compare the Kernel distributions of inspected and control schools on school size, share of non-Dutch speaking students, share of low educated mothers, and more generally, average SES composition, all on their 2011 values. We plot the result in Figure 1.1. Again, we generally observe a close resemblance among densities, especially with respect to the indicators used to build the aggregated SES indicator. A formal KolmogorovSmirnov test for the difference in distributions (Table 1.A1 in Appendix A), confirms the absence of significant differences in the underlying densities.

Table 1.2: Descriptive statistics of inspected and control schools

Variables	Uninspected	Inspected	*p*-value
School size	35.952 (19.87)	33.653 (17.71)	0.174
SES composition	0.195 (0.16)	0.174 (0.15)	0.115
Other background characteristics (%)			
Non-Dutch speakers	0.122 (0.18)	0.129 (0.21)	0.653
Low-educated mothers	0.227 (0.19)	0.198 (0.17)	0.061
Part of the travelling population	0.004 (0.02)	0.006 (0.06)	0.661
Receivers of school grant	0.253 (0.17)	0.222 (0.16)	0.019
Lives outside family household	0.003 (0.01)	0.003 (0.01)	0.127

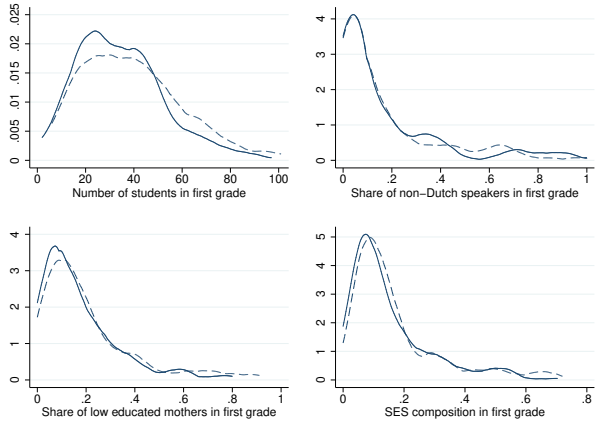
Notes: Reported are mean values and standard errors in parenthesis, all prior to 2012 (i.e. the starting of inspections), for uninspected and inspected schools. Variables include outcome variables (school size and SES composition) and the five equal opportunities indicators used to build SES composition. School size is measured as the number of students in first year of primary education. SES composition is the average SES composition of the student body in the first year of primary education and is measured as a weighted average of five equality indicators: if the student belongs to the travelling population, if the mother does not have a degree of secondary education, if the student lives (permanently or temporarily) outside his family, if the student does not speak the native language Dutch at home, if the student receives one or more school grants. To test for a significant difference in the two groups, we regress each variable on an inspection indicator (equal to 1 if the school has been inspected), and we report the *p*-values in the last column. Standard errors are clustered at school level.

Finally, a direct consequence of random assessment is that both inspected and control schools should follow the same trend before the inspection. Hence, we follow Autor (2003)

[30] We cluster standard errors at school level to account for autocorrelation of error terms within the same schools over multiple time periods.

[31] The only indicator to be statistically different at 5% level is "receivers of school grants". However, this indicator receives the lowest relative weight in the construction of SES composition.

Figure 1.1: Kernel densities for school size and SES composition in rural and urban municipalities



Notes: Comparison of Kernel distribution densities for school size, share of non-Dutch speakers, share of low educated mothers, and average SES composition for inspected (solid line) and non-inspected (dashed line) schools, on their values prior to 2012 (i.e. starting of inspections). Average SES composition is obtained by a weighted average of five equality indicators, and ranges from 0 (high socioeconomic status) to 1 (low socioeconomic status).

in testing the Parallel Trend Assumption (PTA), by interacting a dummy variable for inspected schools with a relative time variable. We expect the coefficients of the time periods preceding the year of inspection t_0 to be close to zero and not significant. As a result, the difference in trends between inspected and uninspected schools does not statistically diverge from zero, and the PTA holds. Table 1.A4 in Appendix A shows the results for school size and SES composition. We find that the estimates for both dependent variables are in line with our expectations, as no coefficient before t_0 is significant, and they are all close to zero.[32]

Hence, all analyses support our argument that the selection process of schools for inspection is (both by law and in practice) random.[33]

[32] The PTA test is run including school and year fixed effects, as well as school time trend.

[33] Note that negatively evaluated schools will face, after three years, a follow-up inspection that is not random (anymore). Therefore, relative to these schools, parents will be able to consult the follow-up inspection report as well as the original report. As our data cover academic years 2012-2016, this concerns only 38 schools in our sample, namely the 20 percent of the total inspected schools. We test the robustness of our findings to the second report that a fraction of schools receives by estimating our models on a redefined sample. In this sample, we follow inspected schools up to a maximum of two years following the inspection. In this fashion, we ensure that no follow-up inspection report could be published (yet). The results are virtually unchanged with respect to the findings of our main analysis.

5 Empirical Strategy

To assess the impact that information shocks, driven by the publication of inspection reports, exert on school enrolment and composition, we use the following model specification:

$$Y_{it} = \beta_0 + \beta_1 D_{it} + \gamma_i + \delta_t + \theta_i t + \epsilon_{it} \tag{1.1}$$

where Y_{it} indicates the outcome variable: school size (i.e., number of first grade students) or SES composition, in school i at time t; D_{it} is the inspection indicator equal to 1 if school i was inspected in period t or before, and 0 otherwise[34]; δ_t denotes time fixed effects, γ_i school fixed effects, and $\theta_i t$ school specific time trends (with θ_i denoting the coefficient on the school specific time trend). School fixed effects allow us to control for a series of unobserved characteristics that differ among schools but are invariant over time, such as school district attractiveness, school location, school reputation, principal's management ability. We further include year dummies and school specific time trends to control for schools growing trajectories. Next, as schools are repeatedly observed over time, we would expect unobserved variations in school size and SES composition to be correlated across time within units, being determined by serially correlated error terms. Moreover, we do not expect inspections to affect our outcome variables homogeneously in our sample. Hence, we account for serial autocorrelation and allow for heteroscedasticity in the error terms, by clustering standard errors at the school level.[35]

The interpretation of β_1 as a causal parameter relies on the assumption that after conditioning on γ_i, δ_t and $\gamma_i \delta_t$, the post-inspection difference between inspected and uninspected schools can be completely attributed to the effect of information shocks, more formally, $E(Y_{0it}|\gamma_i, \delta_t, \gamma_i\delta_t, D_{it}) = E(Y_{0it}|\gamma_i, \delta_t, \gamma_i\delta_t)$.[36] The random selection of schools for inspection and the PTA's satisfaction, as illustrated in Section 4, imply the latter assumption to hold.

Although schools are randomly drawn for inspection, one could argue that it is unrealistic to assume that positive and negative outcomes are allocated randomly. Good (and bad) performing schools might differ from their control group on a series of observed and unobserved characteristics. Specifically, good schools might be located in areas with better

[34] To assess the impact of a positive evaluation, we set D_{it} equal to 1 if school i was positively evaluated in period t or before, and equal to 0 otherwise. Vice versa to assess the impact of a negative evaluation. Note that, to estimate the effects of positive and negative school reports, the evaluations given to schools by the inspectorate should reflect schools' long-term characteristics and should not be sensitive to short-term circumstances. As school assessments are made on the basis of school processes and not, for example, on the basis of test scores – more sensitive to short-term circumstances, we expect the evaluations to mirror these (long-term) characteristics. Moreover, even if in some cases school evaluations would reflect short-term circumstances (e.g. higher absence or conflicts at work in case of a negative evaluation), we might expect this to happen at random, both in case of a positive and a negative evaluation.

[35] A test by Wooldridge (2002) rejects the null of no first-order correlation, further endorsing the choice of clustered standard errors.

[36] Given that we have staggered treatments, we test whether our results are robust to estimating heterogeneous treatment effects models following De Chaisemartin and dHaultfoeuille (2020). The results so obtained (not reported) are consistent with those shown in Table 1.3.

economic conditions, less low-educated parents or larger share of Dutch-speaking households. As a result, these schools might have higher probabilities of receiving a positive evaluation than their control group. Similarly, bad performing schools can have higher likelihood of receiving a negative evaluation as a consequence of enduring structural deficiencies. If present, unobserved long-lasting (but time-varying) determinants might invalidate the causal estimates identified by Equation (1).

To account for observed and unobserved heterogeneity and further check the robustness of our results, we follow Xu (2017) in applying the *generalized synthetic control (GSC)* approach. The GSC approach is an extension of the synthetic control method (SCM) first introduced by Abadie et al. (2010), and it combines the intuition of the SCM with the estimation of an iterative fixed effect (IFE) model. The GSC estimator is well suited for our analysis for three reasons. First, similarly to other synthetic control estimators, the GSC estimator allows to limit endogeneity issues possibly arising from the omission of unobserved covariates. As mentioned earlier, an issue of assessing the effects of information shocks for positively and negatively evaluated schools relies in that these schools might systematically differ from their control group on a series of observed and unobserved characteristics. In this respect, the GSC estimator allows us to compare positively (negatively) evaluated schools to a 'synthetic' control group of schools that is expressly designed to resemble as close as possible the inspected schools on their pre-inspection outcome variables. The idea is that a combination of control schools will constitute a better comparison group for the inspected schools than the original control schools. In fact, this new synthetic control group will reflect the (observed and unobserved) long-term characteristics of positively (negatively) evaluated schools. Put differently, since the synthetic control group replicates the trajectories of positively (negatively) inspected schools over multiple years prior to the inspection, it will also model the underlying unobserved heterogeneities. As second advantage, the synthetic control is the ideal methodology to be employed in a scenario with few treated and control units observed over multiple years. Particularly, the GSC is well suited for our analysis since it allows to have multiple treated units in multiple time periods. Finally, a last advantage of the GSC estimator is that it allows to derive inference by computing standard errors and confidence intervals through parametric bootstrapping.

We specify the functional form as follows:[37]

$$Y_{it} = \alpha_{it} D_{it} + \gamma_i + \delta_t + \lambda_i' f_t + \epsilon_{it} \tag{1.2}$$

where Y_{it} is the outcome for school i at time t, γ_i and δ_t are school and time fixed effects, α_{it} is the effect of positive (negative) information shocks, D_{it} is the positive (negative) inspection indicator equal to 1 if school i received a positive (negative) evaluation in period t or before, and 0 otherwise[38], and $\lambda_i' f_t$ is the factor component of the model that allows to capture

[37] The functional form is assumed to be the same for inspected and non-inspected schools.

[38] Similarly to the DiD analysis, to assess the overall effect of information shocks D_{it} is set to 0 if school i was

several forms of unobserved heterogeneity on top of the fixed effects already specified, with f_t being a vector of r time-varying unobserved coefficients (*latent factors)*, and λ_i a vector of r school-specific intercepts (*factor loadings)*.[39] According to the value of D_{it}, we can distinguish the outcome of an inspected school i at time t, $Y_{it}(1) = \alpha_{it} + \lambda_i' f_t + \gamma_i + \delta_t + \epsilon_{it}$, from the outcome that would have been observed in the same school i, had it not been subject to the inspection: $Y_{it}(0) = \lambda_i' f_t + \gamma_i + \delta_t + \epsilon_{it}$. The difference defines the causal effect of the publication of an inspection report, i.e. our information shock:

$$\alpha_{it} = Y_{it}(1) - Y_{it}(0) \tag{1.3}$$

for every inspected school i at time t, in all post-inspection periods, i.e. $t > t_0$. We are then interested in the estimate of the parameter α_{it} to find the effect of the treatment. Since $Y_{it}(1)$ is observed, we are left with the estimation of $Y_{it}(0)$. Hence, we need to build a synthetic control group that mimics as good as possible the evolution of our outcome variable, with respect to inspected schools in each pre-inspection year. To do so, the GSC method requires to construct $\hat{Y}_{it}(0)$ in three steps, in which the factor loadings of every unobserved common factor are estimated to minimize the mean square error of the predicted inspected schools in the pre-inspection periods. In other words, we estimate weights that minimize the distance between the observed and predicted outcomes in the pre-inspection periods. Then, $\hat{Y}_{it}(0)$ is computed accordingly, and a prediction exercise is done to obtain estimates for the post-inspection periods. Finally, such estimates are averaged over all inspected schools in every post-inspection period, and noise is smoothed out. The average treatment effect on the treated (ATT) by period is thus defined as follows:

$$\hat{ATT}_t = \frac{1}{N_{tr}} \sum_i \left[Y_{it}(1) - \hat{Y}_{it}(0) \right] = \frac{1}{N_{tr}} \sum_i \hat{\alpha}_{it} \tag{1.4}$$

for $t > t_0$ and i part of the treatment group. The construction of the counterfactual allows us to overcome the issue of non-parallel trend between positively or negatively evaluated schools and uninspected schools. As previously mentioned, another advantage of the GSC method is that it allows to obtain uncertainty estimates of standard errors and confidence intervals, through parametric bootstrapping. The purpose is to build B series of simulated outcome values for inspected and uninspected schools.[40] To do so, residuals are resampled from different distributions according to whether the school belongs to the control or treatment group. For every bootstrapped series of simulated outcomes, the ATT is com-

inspected in period t or before, and 0 otherwise.

[39] School and time fixed effects are already a special combination of f_t and λ_i. They can be obtained by setting $f_{1t} = 1, \lambda_{i2} = 1$, with $\lambda_{i1} = \gamma_i$ and $f_{2t} = \delta_t$, so that $f_{1t}\lambda_{i1} + f_{2t}\lambda_{i2} = \gamma_i + \delta_t$. Other forms of unobserved heterogeneity include school-specific linear time trends, quadratic time trends or autoregressive components. A cross-validation algorithm is implemented to select the appropriate number of factors to be estimated in addition to the school and time fixed effects.

[40] In both cases such series are constructed without the treatment effect, i.e. they do not include the inspection effect α_{it}

puted. Finally, the variance of the treatment effect can be computed over all bootstrapped simulated ATTs.

6 Results

6.1 Overall results

Table 1.3 presents the estimated effect of information shocks on school size (column 1) and SES composition (column 3). Our findings indicate a positive and significant effect on school size. The estimates suggest that an inspected school experiences, on average, an increase in first grade enrolments by around 1.8 students. The estimated effect corresponds to an increase in school size of about 5 percent. However, such higher enrolment rate does not translate into a significantly different socioeconomic composition of the school.

The availability and propagation over the years of the information on school quality can have a dual effect. On the one hand, it can contribute to build up a school's reputation, as the passing of time increases the chance to reach a larger audience. On the other hand, it may fade away when too many years have passed since its disclosure, as deemed to be no longer accurate. With available data between 2012 an 2016, we can follow schools up to four years after the information shock. We show the results in columns (2) and (4) of Table 1.3, with periods ranging from the year of inspection (t_0) to four years after the inspection (t_{+4}). The results confirm the pattern observed in the overall specification, with class size strongly responding and SES composition never being statistically different from zero. More specifically, our results point out a durable reputation effect over time, with coefficients raising from 1.7 additional students in t_0 (corresponding to a percentage increase in school size of 5%) to around 2.7 additional students in t_{+3} (corresponding to a percentage increase in school size of 8%). The significance of the effect disappears in t_{+4}. As we control for school specific time trends, we are able to attribute the increase in the number of students primarily to the disclosure of inspection reports. We also detect rising standard errors, but we attribute this higher uncertainty to the reduced number of observations available when more years have passed since the inspection. Having evidence that schools face an average increase in demand following the publication of school reports, we interpret the absence of significance of the socioeconomic composition, both overall and overtime, as a signal that high-SES parents and low-SES parents are equally like to respond to information shocks.[41]

As every school is inspected on several courses, information shocks might affect schools

[41] It should be noted that the impact of school inspection reports may vary depending on the year in which the reports were published. In other words, school inspection reports published in 2016 may have different effects than school inspection reports published in 2012. This is because, first, over time more parents may be more aware of the existence of the reports. Second, the composition of the parents reached may be different, as, for example, over time low-SES parents may be more likely to be aware of the existence of the reports. Exploring this mechanism further may be an avenue worth exploring by future research.

Table 1.3: Effect of information shocks on school size and SES composition

	School size		SES composition	
	(1)	(2)	(3)	(4)
D: Inspection	1.839**		0.000885	
	(0.627)		(0.00467)	
$D \times t_0$		1.714**		0.000346
		(0.626)		(0.00505)
$D \times t_{+1}$		1.969*		0.00259
		(0.792)		(0.00553)
$D \times t_{+2}$		2.250*		0.00298
		(0.972)		(0.00856)
$D \times t_{+3}$		2.708*		0.00147
		(1.182)		(0.0127)
$D \times t_{+4}$		2.888		0.0158
		(1.719)		(0.0164)
Constant	35.63***	35.60***	0.176***	0.176***
	(0.0636)	(0.0776)	(0.000473)	(0.000659)
School FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
School × time trends	Yes	Yes	Yes	Yes
N	6423	6423	6423	6423
R^2	0.914	0.914	0.894	0.894

Notes: The table shows the impact of information shocks on school size and SES composition, overall and according to how many years elapsed since the inspection. School size is measured as the number of students in first year of primary education. SES composition is the average SES composition of the student body in the first year of primary education and is measured as a weighted average of five equality indicators: if the student belongs to the travelling population, if the mother does not have a degree of secondary education, if the student lives (permanently or temporarily) outside his family, if the student does not speak the native language Dutch at home, if the student receives one or more school grants. Robust standard errors, clustered at school level, in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$)

differently according to which subjects are being examined. To investigate the heterogeneous effect for different courses, Table 1.A5 in Appendix A shows the interaction of the inspection dummy D with the courses covered during the inspection. Overall, no significant effects emerge from the interaction terms, both on school size and SES composition. In column (1), significance at 1 percent level is detected only for the category 'other'. However, this category consists of several minor courses - such as French language, physical education, technology or ICT - that are evaluated at a consistent lower frequency, preventing us from deriving causal inference individually. This suggests that when reacting to information shocks, parents do not express preferences on the subjects examined. On the contrary, they most likely use the new information at their disposal to locate inspected schools, irrespective of which courses have been evaluated.

Next, we examine whether the geographic localization of the schools helps us better

understand the effect of information shocks. The DiD estimation reported in Table 1.4 suggests heterogeneity in the results relating to whether the school is located in a lowly or highly densely populated area, corresponding to municipalities with population density below and above 750 inhabitants per km^2.[42] We refer to the former as *rural* and to the latter as *urban*. The results highlight how the baseline outcomes of Table 1.3 are mostly driven by schools located in rural areas. In column (1), the overall effect on school size for rural schools is significant at 1% level and higher than what previously reported. The same is true in t_0, t_{+1} and t_{+2}. The effect becomes not significant from t_{+3} onward, signalling less persistence of the reputation effect. Conversely, our results point out no response to information shocks for urban schools, with the overall effect being close to one and not statistically different from zero.[43] Such difference may seem counter-intuitive, as students in city centers should be in the position to easily change schools, unlike peers living in outer municipalities who potentially need more travelling time. However, it can be explained by the enrolment capacities of urban schools in Flanders. There is evidence that schools located in highly populated municipalities are likely to have reached the maximum capacity of students being able to enroll.[44] Therefore, in the first years after the publication of the inspection reports, even if urban schools are faced with higher demand, they are not able to accommodate it as being already at their maximum. No effects are found again with respect to the socioeconomic composition.

Finally, to further explore the difference between schools located in rural and urban municipalities, we interact the inspection dummy D with population density, which we arrange in five categories. Results are presented in Table 1.A7 in Appendix A. Again, we find evidence of the distinct response in rural and urban areas. Indeed, the impact on schools in regions with a population density larger than 750 inhabitants per km^2 is never significant, whereas there is evidence that below 750 inhabitants per km^2 information shocks affect school enrolment. We do not observe any difference in SES composition.

[42] The value of 750 inhabitants per km^2 as a partitioning limit for the level of population density is confirmed by several robustness checks, each with a different threshold for rural and urban areas. Results are reported and discussed in Table 1.A6 in Appendix A.

[43] When comparing the coefficients of rural and urban schools by means of a test of equality of coefficients, we find that the estimates in urban schools do not statistically differ from the ones of rural schools (p-value=0.361). Since the estimated coefficients in urban schools are positive and above the unit, this comes as no surprise as it likely that the estimates in rural areas differ more significantly from zero than from the positive (albeit not significant) estimates in urban areas.

[44] In their OECD Review of School Resources, Nusche et al. (2015) show how urban schools in Flanders are struggling to face an increased demand for places, whereas rural schools are experiencing lower enrolments. This unequal shifting of demand from rural to urban schools is presented as one of the challenges the Flemish educational system would have to address in the following years. Supplementary evidence from academic year 2014/2015 points to around 2700 students not being able to enroll in community urban schools due to the shortage of school places (De Standaard, 2014). Finally, a technical report on school capacities proves most schools located in urban municipalities to experience capacity constraints (Groenez and Surkyn, 2018).

Table 1.4: Effect of information shocks on school size and SES composition in rural and urban municipalities

	Rural				Urban			
	School size		SES composition		School size		SES composition	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D: Inspection	2.416**		−0.00222		1.277		0.00269	
	(0.876)		(0.00537)		(0.886)		(0.00796)	
$D \times t_0$		2.372*		−0.00336		1.009		0.00291
		(0.921)		(0.00597)		(0.830)		(0.00832)
$D \times t_{+1}$		2.440*		−0.00114		1.484		0.00521
		(1.038)		(0.00612)		(1.190)		(0.00950)
$D \times t_{+2}$		2.762*		0.00222		1.977		0.00244
		(1.294)		(0.00964)		(1.440)		(0.0142)
$D \times t_{+3}$		2.459		0.00789		2.999		−0.00463
		(1.414)		(0.0127)		(1.827)		(0.0211)
$D \times t_{+4}$		3.215		0.00809		2.727		0.0206
		(2.266)		(0.0174)		(2.543)		(0.0272)
Constant	32.10***	32.09***	0.133***	0.133***	39.87***	39.81***	0.228***	0.228***
	(0.0800)	(0.0887)	(0.000490)	(0.000601)	(0.100)	(0.133)	(0.000903)	(0.00131)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School × time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3503	3503	3503	3503	2920	2920	2920	2920
R^2	0.902	0.902	0.784	0.784	0.918	0.919	0.918	0.918

Notes: The table shows the estimated effect of information shocks on school size and SES composition, for schools located in rural and urban municipalities. Rural (urban) municipalities are defined as having less (more) than 750 inhabitants per km^2. Estimates show the effect over all years and disentangled according to how many years elapsed since the inspection. School size is measured as the number of students in first grade and SES composition as the average socioeconomic composition of the student body in first grade. Robust standard errors, clustered at school level, in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$)

6.2 The effect of positive and negative evaluations

After schools are inspected, they receive either a positive or negative evaluation. Above, we presented the *overall* response of parents to information shocks. However, it is likely that positively evaluated schools followed, and will follow, a different growth path in terms of school size and composition than negatively evaluated schools. We disentangle such effects and report the results in Table 1.5. From Table 1.5 two results emerge: first, a clear distinction between the effect of a positive and a negative evaluation on school size; second, the repeated absence of impact on the socioeconomic composition.

As for the former, we detect a strong positive response of school size following a favorable evaluation, and no response when the school receives an unfavourable evaluation. Parents, when having to choose a school for their children, are attracted by positively evaluated schools that inevitably see the average number of students enrolled in the first grade increase. The overall effect amounts on average to 2.3 additional registrations, strongly significant at 1% level (and corresponding to a percentage increase in school size of 7%). When further disentangling the time effects, reported in column (2) of Table 1.5, we observe a positive and long-lasting reaction of school size until t_{+3}. Moreover, as the coefficient increases in magnitude, we find evidence that schools consolidate their reputation through the years.

Table 1.5: Effect of positive and negative evaluations on school size and SES composition

	Positive				Negative			
	School size		SES composition		School size		SES composition	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D: Inspection	2.255**		−0.00374		1.393		0.00795	
	(0.771)		(0.00555)		(1.021)		(0.00759)	
D×t_0		1.939*		−0.00418		1.353		0.00670
		(0.778)		(0.00592)		(0.998)		(0.00850)
D×t_{+1}		2.602**		−0.00298		1.146		0.0108
		(0.969)		(0.00632)		(1.241)		(0.00848)
D×t_{+2}		2.546*		−0.00119		2.063		0.00852
		(1.104)		(0.00948)		(1.606)		(0.0146)
D×t_{+3}		3.595**		0.000646		1.512		−0.00103
		(1.309)		(0.0142)		(1.930)		(0.0219)
D×t_{+4}		3.504		0.0201		1.842		−0.00182
		(2.079)		(0.0196)		(2.299)		(0.0226)
Constant	36.03***	36.00***	0.174***	0.174***	36.33***	36.32***	0.187***	0.187***
	(0.0605)	(0.0705)	(0.000436)	(0.000606)	(0.0559)	(0.0641)	(0.000416)	(0.000507)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School × time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	5170	5170	5170	5170	4475	4475	4475	4475
R^2	0.915	0.915	0.886	0.886	0.918	0.918	0.904	0.904

Notes: The table reports the estimated effect of positive and negative evaluations on school size and SES composition. Estimates show the effect over all years and disentangled according to how many years elapsed since the inspection. School size is measured as the number of students in first grade and SES composition as the average socioeconomic composition of the student body in first grade. Robust standard errors, clustered at school level, in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$)

The peak occurs in time t_{+3}, with about 3.6 additional students enrolling, corresponding to an increase of 10% in the first grade of primary schools. On the other hand, no significant effect is detected, neither overall nor over time, when the outcome of the inspection is unfavourable. However, the coefficients (and standard errors) related to a negative evaluation are positive and increasing through the time periods, and are not statistically different from the coefficients of a positive evaluation. A test of equality of coefficients fails to reject the null of equality of coefficients with p-value = 0.500. Hence, schools with an unfavorable evaluation are not able to attract new students, as one can legitimately expect, but they also do not attract less students in the first grade after the publication of the reports. This is likely due to two reasons. First, before the information shocks, parents might already be able to identify schools with poor performances. Their (perceived) judgement is later confirmed by the negative assessments, and no effect is detected. Second, knowing that by law negatively evaluated schools are urged to tackle the deficiencies disclosed by the reports, parents might become more optimistic about the change in school quality of these schools in the near future. Specifically, they might interpret the negative evaluation as a signal of commitment to improve school practices, and would react by not punishing the schools after having received a negative evaluation.

Turning to the socioeconomic composition, disentangling positive from negative scores does not shed any light on the social group driving the results. We interpret it again as

evidence that both high and low-SES parents are equally like to respond to information shocks.

Our results are in line with Koning and Van der Wiel (2013), whose findings indicate a rise (fall) in enrolment after a positive (negative) score obtained by the school in the ranking. In particular, the authors find that when all school tracks are considered, schools with a positive score face an increase in enrolments of 2 additional students, while schools with the most positive score see entries increase of 7 additional students, corresponding to a percentage increase in school size of 3% and 9% respectively. Koning and Van der Wiel (2013) also find no differentiated response according to students' socioeconomic characteristics[45]. The consistency of findings is accentuated by the country Koning and Van der Wiel (2013) choose for their analysis, namely the Netherlands, whose condition of public free school choice makes it similar to our setting.

6.3 Robustness checks

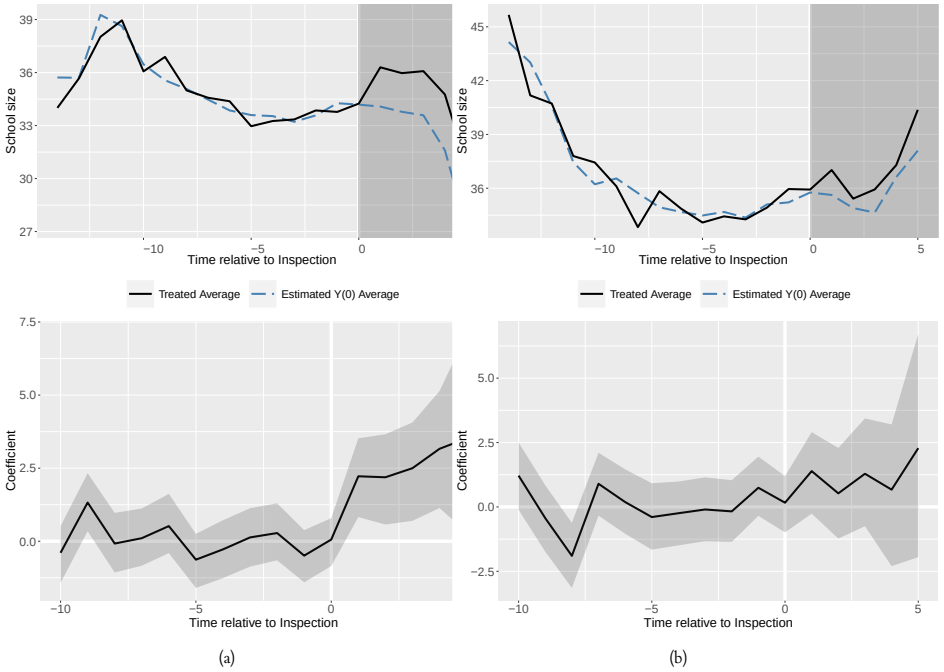
To test the robustness of our findings, first, we re-propose the main findings of this chapter with the sample enlarged to include all schools providing primary education in Flanders. Hence, we now add institutions that jointly provide primary education and preschool. As hypothesized, the results, presented in Table 1.A8, suggest no significant effect both on school size and on SES composition. We argued before that parents exert school choice primarily for continuity with preschool, as they are already familiar with the institution's environment and their children can attend the same school of their fellow peers. Conversely, when parents are forced to choose the institution as the automated choice is not available, they are more apt to make an informed choice and are influenced by the inspection reports. As a matter of fact, even when we further analyse the effects of a positive and negative evaluation, we do not find significance in any of the dependent variables.[46]

Second, as discussed in Section 5, although schools are randomly drawn for inspection, positive and negative evaluations are hardly random. Specifically, good performing schools (and bad performing schools) are likely to differ from the control group on a series of observed and unobserved characteristics. Therefore, as second robustness test, we re-estimate the findings by the use of a generalized synthetic control approach.

[45] Out of 40 interaction coefficients for students' characteristics, Koning and Van der Wiel (2013) find only two of them to be significantly different from 0.

[46] It might be argued that, in case a school offers both preschool and primary education, parents might have consulted the inspection reports before enrolling the child in primary school. If this was the case, however, we would expect an increasing effect over time the more years have passed since the inspection. Further analyses (not reported in the text) reveal no effect of inspection reports over time for schools offering both preschool and primary schooling. Therefore, we rule out this as an important mechanism underlying school choice.

Figure 1.2: Generalized synthetic control estimates of the effect of positive evaluation (a) and negative evaluation (b) on school size.



Notes: Top graphs report the average number of students enrolled in first grade for inspected schools (solid line) and for estimated synthetic control group (dashed blue line). Bottom graphs report estimated ATTs by period, with plotted 95% level confidence intervals.

Figure 1.2 plots the synthetic control estimates of the effects of a positive (panel a) and negative (panel b) evaluation on school size. For every panel, two graphs are displayed. On the top, we observe the average number of first grade students both for inspected schools (black line) and synthetic control group (dashed blue line); on the bottom, the difference among the averages, i.e. the causal effect of the inspection, is explicitly plotted in every time period. The horizontal axis indicates the time relative to the year of inspection, with 0 intended as the last pretreatment period; 1 as the year of inspection and so on, until 5 as four years after inspection.

Findings obtained with the generalized synthetic control approach are in line with the ones obtained from DiD analysis. A positive evaluation results in a large and persistent response in first grade enrolments, with the overall effect strongly significant at 0.1% level and close in size to the DiD estimate. Plotted confidence intervals at 95% level confirm that a favorable evaluation consolidates a school's reputation over time, with estimates consistently rising in size. Increasing standard errors signal again more uncertainty around the estimates

due to the fewer number of observations available several years after the inspection. Turning to the negative evaluations in panel (b), we find that analogously to Table 1.5, an unfavorable evaluation does not change the first grade enrolment. Although the graph shows a mild difference between the treated average and the synthetic counterfactual, such divergence is never statistically significant. Finally, when analysing the effect on the SES composition (shown in Figure 1.A2 of Appendix A), we do not find any effect of positive or negative evaluations.

The exact coefficients of the aforementioned generalized synthetic control (GCC) estimation are reported in Table 1.A9. Moreover, we employ the GSC method to test the robustness of our baseline, and of the rural and urban settings (Table 1.A10). Both Tables 1.A9 and 1.A10 additionally report estimates for DiD regressions with different levels of fixed effects: School FE; School FE and school time trend; School FE, school time trend and year FE. These robustness tests provide some further insights. First, GSC estimates are mostly consistent with the distinction, previously highlighted, existing between rural and urban municipalities. With the threshold being set again to 750 people per km^2, we have confirmation that information shocks strongly affect rural areas. As opposed to Table 1.4, a milder response emerges for highly populated municipalities. However, from the time composition, we observe how such response is determined by time periods t_{+3} and t_{+4}. We deduce that, similarly to what obtained within the DiD framework, urban schools face capacity constraints in the years immediately following the publication of an inspection report. Those limits might be later overcome to accommodate a rise in demand. Second, Tables 1.A9 and 1.A10 show primary schools to be significantly affected in most of regressions, confirming strong parental response also under different levels of fixed effects (FE). However, we sense that these patterns might be overestimating the actual impact of information shocks. Indeed, when looking at regressions under school FE, and especially under school FE and school time trend, there is a tendency for coefficients to be more significant than what obtained in our main specification. We explain such difference as follows. Controlling for only school FE excludes from the analysis both yearly cohorts and schools individual growing trajectories. A school located in areas with positive growing rates may see the level of its students rise not only by means of the information shocks, but also as a result of the mere inflow of persons. More inhabitants implicates more children living in the neighborhood, hence more school subscriptions.[47] Furthermore, even when school trends are included, without year FE coefficients will still be upward biased as we do not account for changes that can affect schools simultaneously, for instance as a result of a governmental policy. Hence, both school individual trend and year cohorts are needed to provide correct inference. Moreover, and as discussed above, our selected specification is additionally supported by the synthetic control analysis.

[47] A similar reasoning applies to the socioeconomic composition, as such inflow may alter students' economic distribution for reasons other than the inspection reports.

As third robustness test, we further enquire the effects of positive and negative evaluations, however separately for rural and urban municipalities. Findings, reported in Table 1.A11, are consistent with results from our main specification. In rural areas, a positive evaluation has a positive impact on schools size, whereas no effect is found with respect to a negative evaluation. In urban areas, neither a positive nor a negative evaluation are affecting any of the dependent variables.

Finally, as last robustness check, we further enquire whether the spatial distribution of schools may affect the results obtained so far. When parents face non-zero transportation costs, the demand for a school might depend not only on the (absolute) quality of that school, but also on how it relates to the quality of other schools in the surrounding neighborhoods. So far, we have indirectly accounted for the spatial distribution of schools through the heterogeneous analysis of rural and urban areas. By distinguishing the effect of information shocks separately for rural and urban schools, we have implicitly defined different local markets for schools according to the level of urbanization of the municipality where they are located. The use of the GSC approach (combined with the rural and urban distinction) has helped us to better define the local markets by modelling the unobserved heterogeneities, and has provided us similar results as in the main analysis.

In this section, we explicitly define school local markets and account for school competition within the market, by extending our main model of Equation (1) as follows. First, we estimate the discrete indicators for positive and negative evaluations in the same regression.[48] By doing so, we estimate the effect of positive and negative evaluations with respect to the control schools that receive no inspection. Second, we add four additional control variables. This set of additional covariates allows to account for the number of competitors in the local market, the relative magnitude of each school within the local market and the relative quality of every school with respect to the other competitors. Specifically, as first variable we include the total number of schools laying in the school local market. As second variable, we control for each school's market share within the local market. To obtain a school's market share, we divide the number of students enrolled in the first grade of that school by the total number of students enrolled in the first grade of all schools laying within the local market. Lastly, we include two discrete indicators for the fraction of positively and negatively evaluated schools in the school local market.

Next, to determine a school's local market, first, we use schools' geographic coordinates to obtain the exact location of every school in Flanders. Second, we define a school's local market as the set of schools that are located within a certain radius of kilometers from the evaluated school.[49] As primary school children in Belgium attend schools located within an average distance of 2 kilometers from home, with standard deviation of approximately 2.5 kilometers (Boussauw et al., 2014), we consider four possible thresholds for the radius: 1,

[48] The positive and negative indicators are defined following the same notation as in Equation (1).

[49] Note that we define as competitors both schools that offer only primary education and schools that offer primary and pre-primary education jointly.

2.5, 5 and 10 kilometers. The smaller the radius, the more the school local market captures school competition in dense populated municipalities. Conversely, the higher the radius, the more the local school market accounts for school competition also in more dispersed areas. We show our results in Table 1.6 below.

Table 1.6: Effect of positive and negative evaluations on school size and SES composition for different levels of school local markets

	School size				SES composition			
	Radius: 1 km (1)	Radius: 2.5 km (2)	Radius: 5 km (3)	Radius: 10 km (4)	Radius: 1 km (5)	Radius: 2.5 km (6)	Radius: 5 km (7)	Radius: 10 km (8)
Positive evaluation	1.515*	1.392*	1.892**	2.166**	−0.00254	−0.00324	−0.00593	−0.00423
	(0.631)	(0.697)	(0.667)	(0.724)	(0.0057)	(0.00608)	(0.00674)	(0.00648)
Negative evaluation	0.863	1.131	1.408	1.816	0.00871	0.00839	0.00316	0.00494
	(0.819)	(0.903)	(0.833)	(0.998)	(0.00741)	(0.00767)	(0.00868)	(0.00801)
Number of competitor schools[a]	2.576***	−0.268	−0.260*	−0.0957	−0.000418	−0.000511	−0.000165	0.000238
	(0.616)	(0.249)	(0.119)	(0.0657)	(0.00494)	(0.00152)	(0.000987)	(0.000471)
Fraction of positively evaluated schools[a]	0.00327	0.012	−0.0119	0.00959	−0.0000426	0.0000195	0.0000152	−0.000379
	(0.0109)	(0.00993)	(0.0158)	(0.0245)	(0.000077)	(0.000101)	(0.00014)	(0.000247)
Fraction of negatively evaluated schools[a]	−0.00298	−0.0132	−0.000649	−0.0635	−0.000049	−0.0000506	0.000264	0.000826
	(0.0136)	(0.0142)	(0.0198)	(−0.0328)	(0.000144)	(0.000113)	(0.000232)	(0.000439)
Market share[a]	0.671***	0.416***	0.663*	0.646	−0.000162	−0.000309	−0.000585**	−0.000144
	(0.0455)	(0.0983)	(0.311)	(0.586)	(0.00018)	(0.000206)	(0.000221)	(0.000275)
Constant	12.54***	22.39***	31.80***	37.63***	0.188***	0.192***	0.189***	0.160***
	(3.153)	(4.72)	(6.394)	(6.758)	(0.0172)	(0.0153)	(0.024)	(0.0327)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School × time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	6423	6423	6423	6423	6423	6423	6423	6423
R^2	0.937	0.928	0.927	0.918	0.895	0.895	0.895	0.895

The table shows the impact of positive and negative evaluations on school size and SES composition, for different levels of the school local market. A school's local market is defined as the set of schools laying within a certain radius of kilometers from the school. We consider four possible radius: 1, 2.5, 5 and 10 kilometers. School size is measured as the number of students in first year of primary education. SES composition is the average SES composition of the student body in the first year of primary education.

[a] Number of competitor schools, fraction of positively evaluated schools, fraction of negatively evaluated schools and market share are adaptively computed according to the radius of the local market considered. Fraction of positively evaluated schools ranges from 0 (no school in the local market receives a positive evaluation) to 100 (all schools in the local market receive a positive evaluation); fraction of negatively evaluated schools ranges from 0 (no school in the local market receives a negative evaluation) to 100 (all schools in the local market receive a negative evaluation); market share ranges from 1 to 100 (the reference school is the only school in the local market).

Notes: Robust standard errors, clustered at school level, in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In Table 1.6, we report in columns (1) to (4) the estimated model for school size and in columns (5) to (8) the estimated model for the socioeconomic (SES) composition. Moreover, for every dependent variable we display the school local market defined as the set of schools within a radius of 1, 2.5, 5 and 10 kilometers respectively. Overall, from Table 1.6 three results emerge: first, a significant effect of positive evaluations on school size and no effect of negative evaluations; second, an increasing effect of positive evaluations according to the radius of the school local market; third, no effect on the socioeconomic composition. As for the former, our results show that when accounting for local competition, schools that received a positive evaluation see a significant increase of the number of students enrolled. Conversely, schools that received a negative evaluation are not significantly affected. This

result holds regardless of the radius of the local market. That is, both when considering competition in the immediate vicinity of the school and when the competition is enlarged to include more distant schools, we obtain that inspection reports significantly affect the number of student enrolled in positively evaluated schools. Secondly, our findings suggest an increasing effect of positive evaluations, both in terms of size and statistical significance, as the school local market is defined more extensively. In particular, the effect of a positive evaluation rises from 1.4 additional students in column (2) to 2.2 additional students in column (4). Our findings are in line with the heterogeneous analysis of rural and urban schools. When the school local market is defined in a small radius, we are mostly controlling for competition in dense populated areas. As we have seen, schools in urban areas are not able to accommodate abrupt rises in enrollments, hence the effect of positive evaluations is expected to be smaller and less significant. By contrast, when the school local market is defined to encompass more distant schools, we are defining a school local market also for schools located in more dispersed areas. Since the competition is not anymore only in the immediate vicinity of the school, the effect is expected to be higher. Finally, no effects are found on the socioeconomic composition, as in all our main specifications. Therefore, from all robustness results it becomes evident that not selection bias is driving the effect, but parental responses.

7 Discussion and conclusion

This chapter has examined parental response to exogenous information shocks, defined as the publication of school inspection reports on a government website in a context of initial, absolute information vacuum. Focusing on the Flemish region of Belgium, we have shown that the selection process of school inspections is random and the resulting information is the only source of objective school quality data available to parents due to the absence of central examinations. As a result, enrolments in the first grade and the SES composition can be compared before and after the information shock using a difference-in-differences framework, with uninspected schools as counterfactual.

Our findings indicate that parents do respond to information shocks, as the publication of school inspection reports strongly affects primary schools. The effect is driven by positively evaluated schools, where the number of first grade students significantly rises by 2.3 students after the publication of reports (an increase of around 7 percent). The effect is persistent over time, reaching after three years from the inspection an impact of almost 4 additional students. Conversely, schools with an unfavorable evaluation do not significantly lose students. It is possible that these results arise because, first, parents were able to identify poor-performing schools even before the inspection reports were published. Thus, they are not surprised by the negative evaluation and no effect is detected. Second, it is possible that parents have become optimistic about the future quality of poorly performing schools,

since by law these schools are required to address their deficiencies. We also observe that our results are driven by schools located in rural areas, while no significance is found for urban schools. We interpret these findings as evidence of the role that market dynamics play in shaping parental response to the publication of performance indicators. In urban municipalities, where schools face capacity constraints, it is not possible to accommodate abrupt rises in demand, and no impact is found. Finally, we never detect significance in the socioeconomic composition.

The effect sizes revealed in this study are in line with earlier literature on information provision and school choice. In particular, our findings are consistent with those of Koning and Van der Wiel (2013) who show, relative to all school tracks, an increase in school size between 3 and 9 percent for schools with at least a positive score in the ranking. While not extremely large, we believe that our results are suggestive of a sizeable effect given the type of intervention (the information is "only" available on the inspectorate's website). As the average school in our sample has 35 students enrolled in the first year of primary education (which approximately corresponds to two classes of 18 students each), this means that a good-performing school sees entries increase by 1 student per class after the reports are published. Our results are also consistent with Koning and Van der Wiel (2013) (and with Mizala and Urquiola, 2013) in that the effect does not appear to be differenciated according to socioeconomic groups.

From a policy perspective, this chapter suggests that introducing more transparency is beneficial for two reasons. First, it helps reducing information asymmetries between parents and schools by providing the former with (a proxy of) school quality information that was not available before and that is largely employed. In particular, this chapter indirectly speaks to the important role that open government can have in conveying more transparency in the education market. In contrast with de Kool and Bekkers (2015), we find strong evidence of parental response to information provision which suggests that parents largely consulted the government website on which inspection reports were published. Second, introducing more transparency is beneficial as it makes schools accountable of their performances by signaling both contemporaneous school practices and future actions to improve school practices.

Regarding the external validity, our findings are relevant for all countries that – similar to Belgium before inspection reports – so far have no information on school quality accessible by parents.[50] The similarity in effects between this chapter and the study of Koning and Van der Wiel (2013), further suggests that parents adhere importance to a value judgement released by authorities (newspaper in the Netherlands, inspectorate in Belgium) on school quality. Therefore, although the inspection data object of this chapter are not open data in its strictest definition (for instance, they are not machine-readable), we might expect similar parental interest in consulting performance indicators even if the information

[50] For instance, Italy, where parents have no access to school quality indicators as the test scores from the INVALSI standardized testing are not available for public consultation.

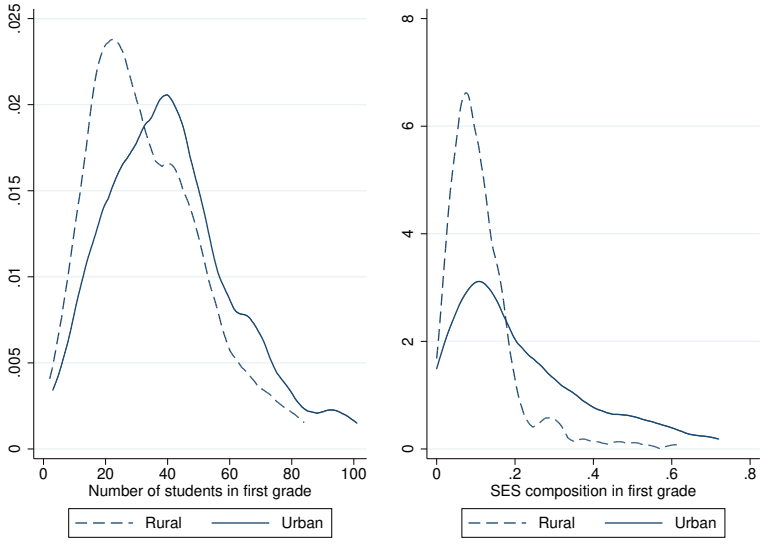
was released in a different way, e.g., through open data platforms. Given the complexity of open data (for an overview, see Zuiderwijk and Janssen, 2014; Ansari et al., 2021), however, it would then be essential to have a thorough reflection about how to convey such data while exploiting its full potential.

Three conclusive remarks must be addressed. First, this study employs a quasi-experimental design, which per se does not produce causal evidence. However, given the context, data and methodologies applied, we believe that our estimates are robust and could be interpreted as causal effects. Second, our results also reveal that parents do not make use of school inspection reports when the institution where their children are enrolled for primary school also offers preschool. This suggests that parents are more likely to attribute importance to school reports when forced by contingency to evaluate alternatives, and has important policy implications as it might hinder some of the attended effects of accountability policies such as school inspection reports. Third, school inspection reports might be only imperfect measures of school quality as they share some of the downsides of other low-stake accountability policies. When students are evaluated on their performance, teachers may have the incentive to act strategically and 'teach to the test' or 'teach to the inspection'. For instance, to score higher, teachers may train students on test-specific skills and give little space to less quantifiable, yet important, soft skills (Jacob, 2005). Most worrisome, teachers may help students during their performance (Jacob and Levitt, 2003) or make sure the lowest achieving pupils are not present when the evaluation is carried out (Figlio and Getzler, 2006). Additionally, when conducted at low frequencies, inspection reports may induce positively evaluated schools to 'rest on their laurels' (de Wolf and Janssens, 2007).

All things considered, like earlier studies using other measures of school quality (e.g. Nunes et al., 2015), we do not aim to question the appropriateness of school inspection reports as evaluation tools. Rather, we stress the complicated relationship between performance indicators and actual school quality. The particular setting of our study allows us to consider school inspection reports as a proxy for quality, as no other objective information on schools' performance is available. Given the discussed possible downsides of school inspection reports as a low-stake accountability policy, however, we emphasize that increased transparency should go hand in hand with high internal validity of the underlying documents. To this end, inspection visits (and reports) should be designed to prevent strategic teacher behavior, and be conducted at reasonable frequencies. With so much at stake, it is incumbent upon the government to ensure that inspection methods are top-notch and that assessments are as accurate as possible. This is particularly important since, as we show in this chapter, introducing more transparency has real-life implications on citizens' choices and behavior.

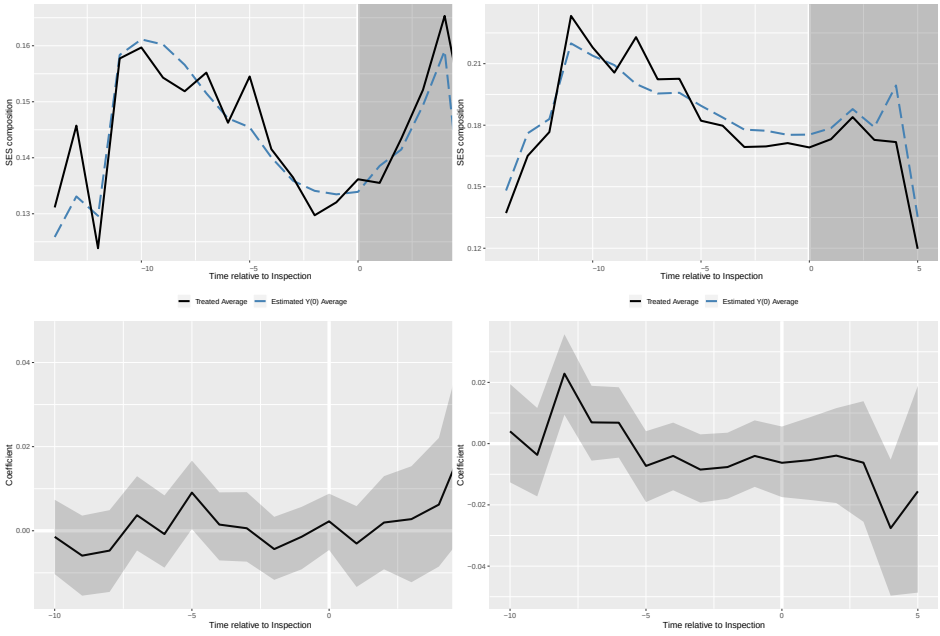
Appendix A: Figures and Tables

Figure 1.A1: Comparison of Kernel densities for school size (left panel) and SES composition (right panel) in rural and urban municipalities.



Notes: SES composition is measured by a weighted average of five equality indicators.

Figure 1.A2: Generalized synthetic control estimates of the effect of positive evaluation (a) and negative evaluation (b) on SES composition.



Notes: Top graphs report the average SES composition of the student body in first grade for inspected schools (solid line) and for estimated synthetic control group (dashed blue line). Bottom graphs report estimated ATTs by period, with plotted 95% level confidence intervals.

Table 1.A1: Comparing inspected and uninspected schools prior to inspection

	Combined K-S	*p*-value
School size	0.1235	0.101
Non-Dutch speakers (%)	0.0442	0.991
Low-educated mothers (%)	0.0988	0.295
SES composition	0.0902	0.403

Notes: For each variable, the two-sample Kolmogorov-Smirnov test for equality of distribution functions has been applied to compare inspected and uninspected schools, on their 2011 values (i.e. prior to the starting of inspections). SES composition is measured as a weighted average of five equality indicators. None of the school characteristics indicate a difference between the two sample distributions.

Table 1.A2: Summary statistics for several categories of population density

	Observations		Schools		Inspection (%)	
Population density	Freq.	Percent	Freq.	Percent	Positive	Negative
0–250	634	9.87	57	11.15	66.67	33.33
250–500	1943	30.25	153	29.94	67.24	32.76
500–750	862	13.42	66	12.92	57.14	42.86
750–1500	1347	20.97	103	20.16	60.87	39.13
>1500	1637	25.49	132	25.83	51.02	48.98
Total	6423	100.00	511	100.00	60.30	39.70

Notes: The table shows absolute and relative frequencies for increasing levels of population density. The total number of schools observed in the sample is 511.

Table 1.A3: Summary statistics of subjects' evaluations

	Subjects				
Evaluation	Social sciences	Dutch	Math	Music	Other
Pass[a]	64	85	81	44	65
Pass (%)[b]	68.09	85.86	90.00	50.00	69.89
Not pass[a]	30	14	9	44	28
Not pass (%)[b]	31.91	14.14	10.00	50.00	30.11
Total					
Absolute values[c]	94	99	90	88	93
Absolute/Tot(%)[d]	20.3	21.3	19.4	19.0	20.0

Notes: [a] Number of schools that passed (did not pass) the evaluation of each subjects.
[b] Percentage of schools that passed (did not pass) the evaluation of each subjects, measured with respect to the total number of schools evaluated in each subject.
[c] Total number of schools evaluated in each subject.
[d] Frequency of evaluation of each subject.
'Other' includes courses evaluated at a lower frequency, not enough to be individual categories (i.e. physical education, ICT, French language, science and technology, and social learning).

Table 1.A4: Estimates of leads and lags of the inspection effect on school size and SES composition

	(1) School size	(2) SES composition
$t-2$	-0.145	-0.000283
	(0.675)	(0.00555)
$t-1$	0.0130	-0.000666
	(0.745)	(0.00638)
School Inspection in t	1.673	-0.0000504
	(0.928)	(0.00782)
$t+1$	1.924	0.00213
	(1.162)	(0.00848)
$t+2$	2.199	0.00246
	(1.378)	(0.0117)
$t+3$	2.654	0.000892
	(1.574)	(0.0158)
$t+4$	2.826	0.0152
	(2.033)	(0.0197)
Constant	35.61***	0.176***
	(0.151)	(0.00128)
School FE	Yes	Yes
Year FE	Yes	Yes
School × time trends	Yes	Yes
N	6423	6423
R^2	0.914	0.894

Notes: Following Autor (2003), the above results are obtained by interacting an indicator of whether a school is subject to an inspection, with a relative time variable. School size is measured as the number of students in first grade and SES composition as the average socioeconomic composition of the student body in first grade. The estimates confirm that anticipatory effects are not significantly different from zero before treatment. Robust standard errors, clustered at school level, in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$)

Table 1.A5: Heterogeneous analysis of the courses examined during inspections and their effects on school size and SES composition

	(1) School size	(2) SES composition
D× Introduction to social sciences	1.002	0.0217
	(1.236)	(0.0137)
D× Dutch	-1.774	0.0103
	(1.207)	(0.0139)
D× Mathematics	-0.141	-0.00607
	(1.240)	(0.0139)
D× Music	1.942	0.000133
	(1.321)	(0.0135)
D× Other	2.823**	-0.0143
	(1.027)	(0.0131)
Constant	35.65***	0.202***
	(0.0631)	(0.000761)
School FE	Yes	Yes
Year FE	Yes	Yes
School × time trends	Yes	Yes
N	6423	6423
R^2	0.914	0.832

Notes: The table shows the effect of each course covered during inspections on school size and SES composition. This effect is estimated by interacting the inspection dummy D with the subjects examined. School size is measured as the number of students in first grade and SES composition as the average socioeconomic composition of the student body in first grade. Robust standard errors, clustered at school level, in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$)

Table 1.A6: Effect of information shocks on school size and SES composition with several thresholds defined for rural and urban municipalities

PANEL A: rural	<250		<500		<750		<1000		<1500	
	(1) Size	(2) SES	(3) Size	(4) SES	(5) Size	(6) SES	(7) Size	(8) SES	(9) Size	(10) SES
Inspection	−1.734	−0.00640	1.583	−0.0112	2.416**	−0.00222	2.272**	−0.00448	2.234**	−0.00145
	(1.795)	(0.0103)	(1.075)	(0.00575)	(0.876)	(0.00537)	(0.763)	(0.00490)	(0.702)	(0.00474)
Constant	25.55***	0.129***	30.27***	0.126***	32.10***	0.133***	33.31***	0.143***	33.70***	0.145***
	(0.173)	(0.000991)	(0.0943)	(0.000504)	(0.0800)	(0.000490)	(0.0722)	(0.000463)	(0.0705)	(0.000477)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School × time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	634	634	2577	2577	3503	3503	4315	4315	4786	4786
R^2	0.893	0.654	0.896	0.745	0.902	0.784	0.905	0.832	0.903	0.827
PANEL B: urban	>250		>500		>750		>1000		>1500	
	(1) Size	(2) SES	(3) Size	(4) SES	(5) Size	(6) SES	(7) Size	(8) SES	(9) Size	(10) SES
Inspection	2.190**	0.00174	2.099**	0.00708	1.277	0.00269	1.069	0.00983	0.650	0.00817
	(0.664)	(0.00505)	(0.755)	(0.00659)	(0.886)	(0.00796)	(1.106)	(0.00985)	(1.380)	(0.0122)
Constant	36.74***	0.182***	39.22***	0.210***	39.87***	0.228***	40.39***	0.244***	41.28***	0.269***
	(0.0677)	(0.000515)	(0.0835)	(0.000728)	(0.100)	(0.000903)	(0.128)	(0.00114)	(0.143)	(0.00126)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School × time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	5789	5789	3846	3846	2920	2920	2108	2108	1637	1637
R^2	0.912	0.900	0.916	0.914	0.918	0.918	0.922	0.919	0.929	0.928

Notes: The table shows the estimated effect of information shocks on school size and SES composition, for different thresholds defining rural and urban municipalities. Panel A shows estimation outcomes for rural municipalities, defined as having less than 250, 500, 750, 1000 and 1500 citizens per km^2 respectively (reading the table from left to right entails that the sample is intrinsically less rural). Panel B shows estimation outcomes for urban municipalities, defined as having more than 250, 500, 750, 1000 and 1500 inhabitants per km^2 (reading the table from left to right entails that the sample is intrinsically more urban). Estimates from Panel B reveal that information shocks do not significantly affect school size as of column 5, thus suggesting no response of parents in highly dense municipalities. Estimates from Panel A reveal that information shocks significantly affect school size as of column (5), where the sample is enlarged to include low to middle rural areas. As we do not consider realistic to define as rural, zones populated up to 1000 and 1500 people per km^2, the limit value of 750 is robust to our checks. No significance is detected with respect to SES composition. Robust standard errors, clustered at school level, in parenthesis.

Table 1.A7: Effect of information shocks on school size and SES composition for different categories of population density

	(1) School size	(2) SES composition
D × *density:*		
<250	0.160	−0.0157
	(1.512)	(0.0101)
250–500	3.193**	−0.0113
	(1.130)	(0.00630)
500–750	2.867*	0.0171
	(1.216)	(0.0107)
750–1500	1.683	0.00252
	(1.145)	(0.0101)
>1500	0.393	0.00930
	(1.328)	(0.00967)
Constant	35.64***	0.176***
	(0.0626)	(0.000471)
School FE	Yes	Yes
Year FE	Yes	Yes
School × time trends	Yes	Yes
N	6423	6423
R^2	0.914	0.895

Notes: The table shows the estimated effect of information shocks on school size and SES composition, for schools located in municipalities with different levels of population densities. The effect is estimated by interacting the inspection dummy *D* with various categories of population densities. School size is measured as the number of students in first grade and SES composition as the average socioeconomic composition of the student body in first grade. Robust standard errors, clustered at school level, in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$)

Table 1.A8: Effect of information shocks on school size and SES composition for the expanded sample

	Baseline		Positive		Negative	
	(1) School size	(2) SES composition	(3) School size	(4) SES composition	(5) School size	(6) SES composition
Inspection	0.115	−0.00339	0.276	−0.00359	−0.0549	−0.00485
	(0.204)	(0.00212)	(0.256)	(0.00272)	(0.312)	(0.00308)
Constant	27.87***	0.193***	27.73***	0.189***	27.78***	0.193***
	(0.0222)	(0.000231)	(0.0210)	(0.000224)	(0.0197)	(0.000194)
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
School × time trends	Yes	Yes	Yes	Yes	Yes	Yes
N	40666	40666	32245	32245	28287	28287
R^2	0.900	0.876	0.902	0.866	0.902	0.876

Notes: The table shows the estimated effect of information shocks on school size and SES composition, when the sample is extended to include schools that jointly offer primary education and preschool. Outcomes in columns (1) and (2) refer to the overall effect; outcomes in columns (3) to (6) refer to the effects of a positive and a negative evaluation. School size is measured as the number of students in first grade and SES composition as the average socioeconomic composition of the student body in first grade. Robust standard errors, clustered at school level, in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$)

Table 1.A9: Effect of positive and negative evaluations on school size and SES composition, with gradually added fixed effects and generalized synthetic control estimates

	Positive								Negative							
	School FE		School FE School× time		School FE School× time Year FE		Generalized Synthetic Control		School FE		School FE School× time		School FE School× time Year FE		Generalized Synthetic Control	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Outcome: *School size*																
D: School Inspection	2.247***		3.567***		2.255**		2.527***		1.705*		2.953**		1.393		1.105	
	(0.550)		(0.697)		(0.771)		(0.612)		(0.692)		(0.999)		(1.021)		(0.718)	
D×t_0		2.029**		2.902***		1.939*		2.222***		1.674		2.579**		1.353		1.390
		(0.656)		(0.737)		(0.778)		(0.694)		(0.882)		(0.989)		(0.998)		(0.811)
D×t_{+1}		2.612***		3.874***		2.602**		2.189**		1.361		2.790*		1.146		0.527
		(0.741)		(0.868)		(0.969)		(0.779)		(0.882)		(1.191)		(1.241)		(0.931)
D×t_{+2}		2.369**		4.048***		2.546*		2.499*		2.157*		4.055**		2.063		1.288
		(0.797)		(0.958)		(1.104)		(0.850)		(1.078)		(1.504)		(1.606)		(1.093)
D×t_{+3}		2.556**		5.115***		3.595**		3.162**		1.768		3.516*		1.512		0.671
		(0.919)		(1.079)		(1.309)		(1.017)		(1.192)		(1.763)		(1.930)		(1.372)
D×t_{+4}		1.023		4.894**		3.504		3.518*		1.784		4.027*		1.842		2.281
		(0.921)		(1.778)		(2.079)		(1.730)		(1.223)		(1.969)		(2.299)		(2.270)
Constant	36.03***	36.03***	35.93***	35.90***	36.03***	36.00***			36.31***	36.31***	36.24***	36.23***	36.33***	36.32***		
	(0.0432)	(0.0431)	(0.0548)	(0.0587)	(0.0605)	(0.0705)			(0.0379)	(0.0379)	(0.0547)	(0.0592)	(0.0559)	(0.0641)		
N	5170	5170	5170	5170	5170	5170	5130	5130	4475	4475	4475	4475	4475	4475	4436	4436
R^2	0.876	0.876	0.913	0.913	0.915	0.915			0.882	0.882	0.917	0.917	0.918	0.918		
Outcome: *SES composition*																
D: School Inspection	−0.00622		0.0166**		−0.00374		0.0029		−0.0216*		0.0277***		0.00795		−0.0091	
	(0.00499)		(0.00508)		(0.00555)		(0.00425)		(0.00843)		(0.00713)		(0.00759)		(0.00577)	
D×t_0		−0.0113		0.00903		−0.00418		−0.0030		−0.0232**		0.0199*		0.00670		−0.0054
		(0.00573)		(0.00559)		(0.00592)		(0.00496)		(0.00870)		(0.00804)		(0.00850)		(0.00692)
D×t_{+1}		−0.0104		0.0143**		−0.00298		0.0020		−0.0218***		0.0298***		0.0108		−0.0039
		(0.00593)		(0.00538)		(0.00632)		(0.00565)		(0.00607)		(0.00741)		(0.00848)		(0.00811)
D×t_{+2}		−0.00409		0.0258**		−0.00119		0.0028		−0.0192		0.0383**		0.00852		−0.0062
		(0.00725)		(0.00781)		(0.00948)		(0.00680)		(0.0146)		(0.0144)		(0.0146)		(0.00989)
D×t_{+3}		−0.00600		0.0392**		0.000646		0.0062		−0.0269		0.0420*		−0.00103		−0.0276*
		(0.00917)		(0.0121)		(0.0142)		(0.00767)		(0.0240)		(0.0209)		(0.0219)		(0.01134)
D×t_{+4}		0.0223*		0.0790***		0.0201		0.0220		−0.00947		0.0626**		−0.00182		−0.0156
		(0.0108)		(0.0166)		(0.0196)		(0.01239)		(0.0176)		(0.0202)		(0.0226)		(0.01737)
Constant	0.174***	0.174***	0.172***	0.172***	0.174***	0.174***			0.189***	0.189***	0.186***	0.186***	0.187***	0.187***		
	(0.000392)	(0.000399)	(0.000399)	(0.000496)	(0.000436)	(0.000606)			(0.000461)	(0.000470)	(0.000391)	(0.000460)	(0.000416)	(0.000507)		
N	5170	5170	5170	5170	5170	5170	5130	5130	4475	4475	4475	4475	4475	4475	4436	4436
R^2	0.830	0.830	0.882	0.883	0.886	0.886			0.853	0.853	0.901	0.901	0.904	0.904		

Notes: The table shows the estimated effect of positive and negative evaluations on school size (top panel) and SES composition (bottom panel), for different levels of fixed effects and for synthetic control estimation. Estimates show the effect over all years and disentangled according to how many years elapsed since the inspection. School size is measured as the number of students in first grade and SES composition as the average socioeconomic composition of the student body in first grade. Robust standard errors, clustered at school level, in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.A10: Effect of information shocks on school size and SES composition in the baseline specification and in rural and urban municipalities, with gradually added fixed effects and generalized synthetic control estimates

	Baseline								Rural								Urban							
	School FE		School FE Schoolx time		School FE Schoolx time Year FE		Generalized Synthetic Control		School FE		School FE Schoolxtime		School FE Schoolx time Year FE		Generalized Synthetic Control		School FE		School FE Schoolx time		School FE Schoolx time Year FE		Generalized Synthetic Control	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Outcome: *School size*																								
D: School																								
Inspection	2.042***		3.332***		1.839**		1.854***		2.382***		4.015***		2.416**		1.970**		1.712*		2.608**		1.277		2.402*	
	(0.431)		(0.577)		(0.627)		(0.486)		(0.522)		(0.737)		(0.876)		(0.639)		(0.680)		(0.886)		(0.886)		(0.832)	
Dxt_0		1.893***		2.778***		1.714**		1.851*		2.308**		3.641***		2.372*		2.276**		1.444		1.841*		1.009		1.565
		(0.528)		(0.592)		(0.626)		(0.529)		(0.715)		(0.849)		(0.921)		(0.722)		(0.779)		(0.812)		(0.830)		(0.815)
Dxt_{+1}		2.121***		3.454***		1.969*		1.390*		2.486***		4.092***		2.440*		1.819*		1.745*		2.770*		1.484		1.085
		(0.570)		(0.705)		(0.792)		(0.589)		(0.735)		(0.899)		(1.038)		(0.868)		(0.875)		(1.087)		(1.190)		(0.944)
Dxt_{+2}		2.289***		4.049***		2.250*		1.871*		2.575**		4.681***		2.762*		2.157*		2.014*		3.382**		1.977		2.250
		(0.642)		(0.822)		(0.972)		(0.699)		(0.815)		(1.053)		(1.294)		(1.007)		(0.985)		(1.257)		(1.440)		(1.096)
Dxt_{+3}		2.282**		4.551***		2.708*		2.104*		2.241*		4.716***		2.459		1.400		2.295*		4.236**		2.999		3.720*
		(0.730)		(0.939)		(1.182)		(0.884)		(1.062)		(1.044)		(1.414)		(1.281)		(1.001)		(1.472)		(1.827)		(1.260)
Dxt_{+4}		1.247		4.573***		2.888		2.826*		2.010*		5.076**		3.215		1.603		0.574		3.988*		2.727		6.615**
		(0.744)		(1.361)		(1.719)		(1.393)		(0.796)		(1.784)		(2.266)		(1.964)		(1.186)		(2.006)		(2.543)		(2.160)
Constant	35.61***	35.61***	35.48***	35.45***	35.63***	35.60***			32.11***	32.11***	31.96***	31.94***	32.10***	32.09***			39.82***	39.82***	39.72***	39.68***	39.87***	39.81***		
	(0.0437)	(0.0437)	(0.0584)	(0.0629)	(0.0636)	(0.0776)			(0.0477)	(0.0473)	(0.0673)	(0.0668)	(0.0800)	(0.0887)			(0.0771)	(0.0773)	(0.100)	(0.113)	(0.100)	(0.133)		
N	6423	6423	6423	6423	6423	6423	6329	6329	3503	3503	3503	3503	3503	3503	3466	3466	2920	2920	2920	2920	2920	2920	2863	2863
R^2	0.878	0.878	0.912	0.912	0.914	0.914			0.864	0.864	0.900	0.900	0.902	0.902			0.881	0.881	0.917	0.917	0.918	0.919		
Outcome: *SES composition*																								
D: School																								
Inspection	−0.0121**		0.0208***		0.000885		0.0024		−0.0186***		0.0173***		−0.00222		−0.0018		−0.00569		0.0245***		0.00269		0.0006	
	(0.00448)		(0.00418)		(0.00467)		(0.00352)		(0.00489)		(0.00482)		(0.00537)		(0.00482)		(0.00740)		(0.00693)		(0.00796)		(0.00543)	
Dxt_0		−0.0159**		0.0132**		0.000346		−0.0012		−0.0233***		0.00916		−0.00336		−0.0027		−0.00791		0.0176*		0.00291		−0.0009
		(0.00489)		(0.00464)		(0.00505)		(0.00389)		(0.00580)		(0.00568)		(0.00597)		(0.00558)		(0.00797)		(0.00745)		(0.00832)		(0.00569)
Dxt_{+1}		−0.0149***		0.0204***		0.00259		0.0044		−0.0222***		0.0160**		−0.00114		−0.0028		−0.00747		0.0250***		0.00521		0.0050
		(0.00433)		(0.00442)		(0.00553)		(0.00437)		(0.00556)		(0.00510)		(0.00612)		(0.00671)		(0.00656)		(0.00726)		(0.00950)		(0.00703)
Dxt_{+2}		−0.00975		0.0305***		0.00298		0.0038		−0.0114		0.0313***		0.00222		0.0025		−0.00807		0.0300*		0.00244		0.0003
		(0.00711)		(0.00729)		(0.00856)		(0.00561)		(0.00752)		(0.00849)		(0.00964)		(0.00816)		(0.0119)		(0.0117)		(0.0142)		(0.00809)
Dxt_{+3}		−0.0133		0.0405***		0.00147		−0.0009		−0.0177		0.0453***		0.00789		0.0005		−0.0100		0.0379*		−0.00463		−0.0118
		(0.0103)		(0.0107)		(0.0127)		(0.00675)		(0.00908)		(0.0110)		(0.0127)		(0.01032)		(0.0164)		(0.0168)		(0.0211)		(0.00939)
Dxt_{+4}		0.0118		0.0743***		0.0158		0.0126		−0.00277		0.0656***		0.00809		−0.0104		0.0244*		0.0823***		0.0206		0.0199
		(0.00941)		(0.0131)		(0.0164)		(0.01053)		(0.0138)		(0.0144)		(0.0174)		(0.01609)		(0.0123)		(0.0207)		(0.0272)		(0.01431)
Constant	0.178***	0.178***	0.174***	0.174***	0.176***	0.176***			0.135***	0.135***	0.132***	0.131***	0.133***	0.133***			0.229***	0.229***	0.226***	0.225***	0.228***	0.228***		
	(0.000454)	(0.000463)	(0.000424)	(0.000515)	(0.000473)	(0.000659)			(0.000447)	(0.000454)	(0.000441)	(0.000498)	(0.000490)	(0.000601)			(0.000839)	(0.000848)	(0.000785)	(0.000965)	(0.000903)	(0.00131)		
N	6423	6423	6423	6423	6423	6423	6329	6329	3503	3503	3503	3503	3503	3503	3466	3466	2920	2920	2920	2920	2920	2920	2863	2863
R^2	0.841	0.841	0.891	0.892	0.894	0.894			0.699	0.699	0.776	0.778	0.784	0.784			0.867	0.867	0.915	0.916	0.918	0.918		

Notes: The table shows the estimated effect of information shocks on school size (top panel) and SES composition (bottom panel), for different levels of fixed effects and for synthetic control estimation. Reported is the effect over all years and disentangled according to how many years elapsed since the inspection. Estimates are for the full sample (baseline), and for rural and urban subsamples. Rural (urban) municipalities are defined as having less (more) than 750 inhabitants per km^2. School size is measured as the number of students in first grade and SES composition as the average socioeconomic composition of the student body in first grade. Robust standard errors, clustered at school level, in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.A11: Effect of positive and negative evaluations on school size and SES composition in rural and urban municipalities

PANEL A: rural	Positive				Negative			
	School size		SES composition		School size		SES composition	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D: Inspection	2.590*		-0.00396		2.209		0.00279	
	(1.059)		(0.00561)		(1.332)		(0.0106)	
D×t_0		2.234*		-0.00490		2.653		0.000116
		(1.130)		(0.00657)		(1.402)		(0.0119)
D×t_{+1}		3.321**		-0.00163		1.000		0.000289
		(1.266)		(0.00696)		(1.531)		(0.0103)
D×t_{+2}		2.543		-0.00296		3.420		0.0129
		(1.389)		(0.0107)		(2.382)		(0.0169)
D×t_{+3}		3.107*		0.00120		0.973		0.0258
		(1.494)		(0.0153)		(2.719)		(0.0155)
D×t_{+4}		4.113		0.0196		1.147		-0.0345
		(2.744)		(0.0188)		(2.494)		(0.0250)
Constant	32.54***	32.52***	0.134***	0.133***	31.68***	31.68***	0.141***	0.141***
	(0.0793)	(0.0846)	(0.000420)	(0.000551)	(0.0561)	(0.0592)	(0.000449)	(0.000455)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School × time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2898	2898	2898	2898	2443	2443	2443	2443
R^2	0.903	0.903	0.785	0.785	0.905	0.905	0.791	0.791

PANEL B: urban	Positive				Negative			
	School size		SES composition		School size		SES composition	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D: Inspection	1.912		-0.00454		0.875		0.00914	
	(1.113)		(0.0103)		(1.485)		(0.0114)	
D×t_0		1.592		-0.00408		0.386		0.00957
		(1.026)		(0.0105)		(1.384)		(0.0123)
D×t_{+1}		1.756		-0.00587		1.362		0.0167
		(1.463)		(0.0111)		(1.895)		(0.0133)
D×t_{+2}		2.712		-0.000676		1.423		0.00165
		(1.739)		(0.0163)		(2.166)		(0.0222)
D×t_{+3}		4.169		-0.00129		1.776		-0.0144
		(2.160)		(0.0241)		(2.696)		(0.0325)
D×t_{+4}		2.902		0.0193		2.361		0.0160
		(3.137)		(0.0351)		(3.327)		(0.0321)
Constant	40.48***	40.44***	0.225***	0.225***	41.91***	41.89***	0.243***	0.243***
	(0.0926)	(0.117)	(0.000857)	(0.00120)	(0.104)	(0.123)	(0.000793)	(0.00104)
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School × time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2272	2272	2272	2272	2032	2032	2032	2032
R^2	0.920	0.920	0.911	0.911	0.921	0.922	0.929	0.929

Notes: The table shows the estimated effect of positive and negative evaluations on school size and SES composition in rural (PANEL A) and urban (PANEL B) municipalities. Rural (urban) municipalities are defined as having less (more) than 750 inhabitants per km^2. Estimates show the effect over all years and disentangled according to how many years elapsed since the inspection. School size is measured as the number of students in first grade and SES composition as the average socioeconomic composition of the student body in first grade. Robust standard errors, clustered at school level, in parenthesis.

Appendix B: Translated example of inspection report

B.1 Example of table of contents of an inspection report

PREFACE

1. **SUMMARY**
2. **SCREENING REPORT**
   (a) Subjects of the inspection visit
   (b) Process indicators or process variables of the inspection visit
3. **DOES THE SCHOOL RESPECT THE EDUCATIONAL REGULATIONS?**
   (a) Does the school respect the criteria for approval?
      i. Does the school respect the educational objectives?
         A. preschool education: Dutch
         B. preschool education: Social sciences
         C. preschool education: Dutch and social sciences
         D. Primary education: Dutch
         E. Primary education: Social sciences
         F. Primary education: ICT
      ii. Does the school respect the 'habitability, safety and hygiene' criteria for approval?
      iii. Does the school respect the other criteria for approval?
   (b) Does the school respect the other regulations?
4. **DOES THE SCHOOL MONITOR ITS OWN QUALITY?**
   (a) Learning guidance
   (b) Educational organization
5. **GENERAL POLICY OF THE SCHOOL**
6. **STRENGTHS AND WEAKNESSES OF THE SCHOOL**
   (a) What is the school doing well?
   (b) What can the school improve?
   (c) What should the school improve?

7. EVALUATIONS RELATED TO THE CRITERIA FOR APPROVAL

8. FINDINGS RELATED TO OTHER REGULATIONS

B.2 Example of a subject assessment

3.1.1.4 Primary education: Dutch Satisfactory

The teachers achieve the educational objectives for the subject 'Dutch' at a satisfactory level. The choice to work explicitly in a more result-oriented way has a positive effect on the performance of the students. The teachers plan a wide range of education activities, with specific attention to the active involvement of the students. The relatively close adherence to the educational regulations supports cohesion and development.

- **Educational offer** (*Planning; reference framework; Balanced and complete; Cohesion; broad harmonic formation; active learning*)

  Based on the analysis of the output data, the school team took some measures to work in a more results-oriented way. To this end, the school team is collaborating with the Brussels External Guidance Service and with their own pedagogical and guidance services. This approach is implemented in a few classes, where the performance of the pupils is clearly improved.

  The teachers closely follow the educational regulations to plan the language offer and attain the educational objectives. In this way they ensure a sufficiently targeted, gradual and balanced educational offer.

  To meet the educational needs of the pupils, the teachers focus on the reading and speaking learning areas. In addition to the conventional activities, they plan extra reading activities, reading assignments, and extra speaking activities. In this way, they enrich the vocabulary of the students and improve their reading and speaking abilities. Not all teachers are equally engaged in these activities.

  The spelling classes are usually offered according to the educational regulations, but in some classes students receive extra lessons. This leads sometimes to an oversupply of the educational offer. The writing classes are offered according to the educational regulations and sometimes integrate social sciences topics or current affairs topics.

  Teachers rarely use heuristics to give classes. The educational guidelines that the kindergarten department aims to develop do not receive a strong follow-up in the primary school department.

- **Educational organization** (*Class management; Teaching time)*

  Class management is in some classes aimed at teaching the students how to to work together cooperatively. In these classes, students are given the opportunity to study and learn in a differentiated way. This creates a stimulating and rich language environment. The teachers' educational approach is supportive and challenging.

The teachers plan sufficient time to teach, in accordance with the educational needs of the children. They also often integrate, albeit less intentionally, a variety of linguistic terms pertaining to other subjects.

- **Use of the material: Equipment** *(Educational resources)*

  The school has only the necessary learning materials to give classes. The budget that the teachers receive to purchase materials does not allow them to invest in additional language stimulating tools. The school team made agreements to coordinate the images used in support of the Dutch classes. In some classes, the students do not have a good view of the board.

  Some classes are too small and overcrowded. In these classes, teachers cannot efficiently find space to form groups and create speaking activities.

- **Guidance: Partnerships** *(Collaborations with partners)*

  The school is part of a local network whose partners strengthen each other. The many school initiatives have a positive effect on the students' language skills. They take part into cultural activities and are given the opportunity to participate in all kinds of creative-language activities.

- **Evaluation: Evaluation practices.** *(Balanced and representative. Focused on redirecting. Student monitoring system)*

  In first grade, teachers make systematic use of the opportunity to digitally track the progress of the students. This leads to targeted support and, if necessary, to the adjustment of the educational offer.

  In other grades, teachers make use of the 'method testing' *(methodetoetsen)*, sometimes supplemented with their own tests. During these years, there is a less targeted support and adjustment of the educational offer. Only a few teachers carry on with a targeted differentiated approach.

  In addition, standardized tests for speaking and technical reading are in place. It is surprising that a significant group of students is not progressing in the development of reading skills. However, this does not lead to targeted support and increased focus on technical reading.

- **Guidance: Learning Guidance.** (Perception. Care)

  Despite the relatively good evaluation practices, the student monitoring system does not provide a clear picture of the language development and the learning path of the students. After discussing with the student, the school team rarely formulates concrete and targeted actions to support the student's language development in the longer term. With respect to weak-abilities students, it is difficult to determine what type of guidance is provided and what the result of any remediation is.

- **Quality control**

  The school team is working on an output oriented policy for Dutch. It analyzes the evolution of the students' results and links them with clear expectations. A positive development is noticeable.

B.3 Example of final evaluation given to a school

7 EVALUATIONS RELATED TO THE CRITERIA FOR APPROVAL

In implementation of the decree on the quality of education of 8 May 2009, the evaluation is:

FAVORABLE

- for respecting the 'educational objectives' criteria for approval for preschool education.
- for respecting the 'habitability, safety and hygiene' criteria for approval.
- for respecting the other criteria for approval.

LIMITED FAVORABLE

- for not respecting the 'educational objectives' criteria for approval for primary education
  - failure to respect the educational objectives for ICT, social sciences.

To monitor these shortages, the Education Inspectorate will carry out another check as of 16-05-2017.

Chapter 2

Fertility Outcomes of Adult Children with Divorced Parents: Evidence from Population Data.[1]

Abstract

Most developed countries have experienced two major demographic trends in recent decades: declining fertility rates and increasing divorce rates. Yet, it is empirically understudied whether the exposure to family disruption in childhood influences individuals fertility choices in adulthood. Using administrative data from the Netherlands that spans three generations, we provide first evidence on population data of completed fertility and childlessness of children whose parents divorced. Our results suggest that adult children from divorced families have lower completed fertility and remain more frequently childless than adult children from intact, married families. Our findings support the hypothesis that the lower completed fertility arises because adult children from divorced families are themselves more likely to separate from their partner and are, therefore, not in a stable relationship long enough to start a family or expand their existing one. Our results are robust to treatment effect bounds, to using parental death as a source of parental loss, and to Coarsened Exact Matching.

[1]This chapter is based on: Palmaccio, S., Mazrekaj, D., & De Witte, K. (2023). Fertility Outcomes of Children with Divorced Parents: Evidence from Population Data.
The data for this study are protected by a confidentiality agreement and we are precluded from sharing the data with others. Interested readers can consult the online supplement for information on how to obtain access to the data.
For their helpful comments and suggestions, we would like to thank participants of the reading groups at Oxford University and Utrecht University, and participants of the conferences of the American Sociological Association, the European Society for Population Economics, the Association for Applied Econometrics, the Turkish Economic Association, the Leuven Economics of Education Research, the European Association of Labour Economists, as well as seminar and workshop participants at KU Leuven.

1 Introduction

In recent decades, the fertility rate in almost all developed countries has fallen below the replacement level (the rate necessary to keep the population steady) (OECD, 2011). The share of childless couples has increased, and many individuals are without children voluntarily (Abma and Martinez, 2006). A recent survey conducted in the United States by the Pew Research Center showed that roughly half of the respondents without children intended to remain so by choice (Brown, 2021). Although it can be debated whether low fertility rates are beneficial or detrimental to society as a whole, the decline in fertility has posed practical challenges for most developed countries, as the combination of increased life expectancy and declining birth rates has put the sustainability of the welfare state at risk (Sleebos, 2003). Historically, earlier literature has investigated the decline in fertility mostly in relation to mens and (especially) womens increasing levels of education and labor market participation (e.g. Baudin et al., 2015; Kravdal and Rindfuss, 2008).

Nonetheless, a second demographic trend has characterized developed countries over the past decades. Divorce rates have notably risen causing an increasing proportion of adults to have experienced the dissolution of their parents' marriage. In the United States, the share of children living with only one parent was almost three times higher in 2021 (26.3 percent) than in 1960 (9.10 percent) (Current Population Survey, 2021). Moreover, almost one in two marriages is expected to end in divorce (Kennedy and Ruggles, 2014; Raley and Sweeney, 2020), and about half of children born in the early '80s were expected to experience family disruption by age 18 (Furstenberg Jr et al., 1983). Despite the rapid change in family structure, the relationship between family disruption in childhood and fertility choices in adulthood remains to date empirically understudied.

In this chapter, we begin exploring more in detail the influence on children of heterogeneous family compositions by investigating whether parental divorce during childhood influences children's fertility outcomes in adulthood. To the best of our knowledge, this is the first study to compare adult children from divorced families to adult children from intact, married families on a range of fertility and marital outcomes using population data. Earlier research explicitly focusing on parental divorce and fertility either only examined children's likelihood to become parents in their early twenties (e.g., Ermisch and Francesconi, 2001; Ermisch et al., 2004), finding that children from divorced families are more likely to become parents at a young age than their counterparts from intact, married families. Alternatively, Rijken and Liefbroer (2009) examined the relationship between parental divorce and number of children in adulthood – finding no significant association – but it relied on survey-gathered data. Surveys, however, are prone to a number of errors, among which non-response errors have been shown to lead scholars to underestimate divorce frequencies leading to incorrect inference (Mitchell, 2010).

We contribute to the literature by analyzing a range of fertility and marital outcomes

in adulthood (age at first marriage, age at first child, childlessness, and total number of children) using administrative longitudinal data from the Netherlands. We look at the entire population of children born between 1970 and 1980, which we follow from birth until 2020. Our administrative dataset provides two important advantages over previous studies. First, our data allow us to observe individuals until they are old enough to assume that most of them are past their reproductive years. By reconstructing lifetime fertility, we avoid biases due to censored outcomes or socio-economic composition effects and observe men's and women's fertility patterns over time. Second, administrative data prevent non-response error or self-reporting bias that might arise in survey-gathered divorce data (Mitchell, 2010).

The second contribution of our this chapter is theoretical. Previous research has shown that adult children from divorced families are themselves more likely to divorce from their spouse or separate from their partner than adult children who lived in a stable, intact family (Corak, 2001; Amato and Hohmann-Marriott, 2007). Because of their higher union instability, we hypothesize that adult children from divorced families will not be in a stable relationship for long enough to start a family or expand their existing one with higher-order births. We label this mechanism as the "union duration hypothesis", and expect a negative relationship between parental divorce in childhood and fertility outcomes in adulthood mediated by adult children's differential relationship duration.

It is important to already reflect that divorce is likely correlated with a range of unobserved characteristics which, if not accounted for, may lead to overestimating the influence of parental divorce on adult children's outcomes. To this end, first, we follow Oster (2019) and construct a bounding estimator for the effect of divorce on fertility and marital outcomes. Second, we employ a quasi-experimental design in which we compare the outcomes of adult children who experienced parental loss by divorce to the outcomes of adult children who experienced parental loss by death. Albeit intrinsically different, parental divorce and parental death have in common that children experience a form of parental absence during childhood. Importantly, parental death is likely less correlated with unobservable factors, such as parental conflict, which could arise in the context of parental divorce. Thus, under the assumption that parental death is a less endogenous event than parental divorce, the outcomes of bereaved children offer a more unbiased benchmark against which to compare the outcomes of adult children from divorced families (Corak, 2001). Finally, we pre-process our data using Coarsened Exact Matching.

Our results show that parental divorce significantly influences offsprings fertility and marital outcomes in adulthood. Adult children from divorced families are found to have lower number of children than adult children from intact, married families. They are also more likely to remain childless. Conditional upon becoming parents, adult children from divorced families have their first child at a younger age, although this negative association is mostly found in the sample of women and is likely driven by adult children from divorced families having lower socioeconomic status than their comparison group from in-

tact, married families. Interestingly, we find no clear pattern by child age at the time of divorce regarding the lower number of children at completed fertility. We also find that, over time, the negative association between parental divorce and family size has somewhat declined. A possible explanation for the lower fertility of adult children from divorced families relative to adult children from intact, married families is provided by the union duration hypothesis. Being at greater risk of union dissolution themselves, adult children from divorced families might see their relationship end before they have their first child or subsequent children. Our mediation analysis confirms that union duration plays a significant role in mediating the relationship between parental divorce and fertility outcomes. Concerning marital outcomes, our findings show that adult children from divorced families postpone marriage relative to adult children from intact, married families, although these associations are small and mostly not meaningful.

2 Background

The prevailing view in the literature on divorce is that children from divorced families tend to fare worse than children who lived in intact, two-parented families during childhood (see Amato, 2000; McLanahan et al., 2013 for reviews). Earlier studies have shown that parental divorce is associated with children's lower psychological well-being, impaired cognitive and non-cognitive abilities, lower likelihood of obtaining tertiary education, and reduced socioeconomic well-being in adulthood. As the factors that lead parents to divorce may also be the same factors that negatively affect children's outcomes rather than parental divorce per se, however, a debated issue is whether parental divorce is causally related to children's outcomes, or whether these mostly negative associations are spurious (Amato and Anthony, 2014; Lang and Zagorsky, 2001).

In light of this, a growing body of literature has employed empirical strategies to address the issue of causality (for more details, see McLanahan et al., 2013). Although several studies have shown a possible role of selection in determining children of divorce's outcomes (e.g. Aughinbaugh et al., 2005; Björklund and Sundström, 2006; Björklund et al., 2007; Lang and Zagorsky, 2001; Steele et al., 2009), most studies have confirmed that family disruption negatively affects children's psychological well-being (Amato, 2003; Biblarz and Gottainer, 2000; Cherlin et al., 1998; Magnuson and Berger, 2009), educational attainment (Biblarz and Gottainer, 2000; D'Onofrio et al., 2007; Grätz, 2015; Ermisch and Francesconi, 2001; Sun, 2001; Sun and Li, 2002), and, to a lesser extent, adult outcomes such as employment (Corak, 2001; Ermisch and Francesconi, 2001).

Underinvestigated so far, however, are the fertility outcomes of adult children from divorced families. Some earlier studies have indirectly examined the association between parental divorce in childhood and fertility outcomes in adulthood by including parental divorce as a control variable when more broadly exploring the determinants of fertility

(e.g. Melkersson and Rooth, 2000; Studer et al., 2018). These studies, however, did not formulate explicit hypotheses on the direction of this association, nor did they discuss the magnitude and the sign of the estimated coefficients. Other studies have examined the correlation between parental divorce and fertility intentions, finding a negative association between parental divorce and the willingness to have large families (Merz, 2012; Axinn and Thornton, 1996). A limitation of these latter articles, however, is that they only examined respondents' fertility intentions but could not observe realized choices.

Among earlier studies that more explicitly looked at the influence of parental divorce on fertility and marital outcomes, most previous research compared adult children from divorced families to adult children from intact, married families on the likelihood of early childbearing and divorce. Within the studies that reasoned about causality, Corak (2001) compared adult children from divorced families to adult children from bereaved and intact families on their marital behavior, finding that children with divorced parents are more likely to divorce themselves. Ermisch and Francesconi (2001); Ermisch et al. (2004) looked at the likelihood that children become parents at age 21 or less, showing that parental absence is associated with early childbearing even when unobserved family characteristics are accounted for in sibling fixed effects models. Nonetheless, both Ermisch and Francesconi (2001); Ermisch et al. (2004) measured entry into parenthood as the likelihood that children become parents at the start of the reproductive age, and did not observe other fertility and marital outcomes such as completed fertility, childlessness, and age at first marriage.

To the best of our knowledge, only Rijken and Liefbroer (2009) examined the relationship between parental divorce in childhood and other fertility outcomes (i.e. age at first child and number of children) in adulthood. Using survey data from the Kinship Panel Study, Rijken and Liefbroer (2009) found that adult children who experienced parental divorce in childhood are not significantly different in first childbearing timing and number of children than adult children who did not experience parental divorce. Although an important contribution, this study suffers from two main limitations. First, Rijken and Liefbroer (2009) did not employ any estimation method to assess the extent to which the estimated coefficients may suffer from selection bias. Second, Rijken and Liefbroer (2009) used survey-gathered data to investigate the relationship between parental divorce and fertility outcomes. However, as illustrated by Mitchell (2010), survey data are may be subject to a variety of errors (e.g. recalling, misreporting, or nonresponse errors) among which nonreponse errors are shown to lead to the underestimation of divorce frequencies and to potentially incorrect inference.

We extend earlier literature by explicitly observing the relationship between parental divorce and a range of marital and fertility outcomes (i.e. age at first child, childlessness, number of children, age at first marriage) using population data. Measuring completed fertility in relation to parental divorce is particularly challenging for at least two reasons. First, the researcher needs to have access to sufficiently rich data to define the family struc-

ture of both the parents and the children once they are grown up. Most earlier studies obtained this type of information retrospectively using surveys which, as discussed earlier, are prone to a range of errors that may lead to biased estimation of the key relationship of interest. The second challenge lies in observing completed fertility. To reconstruct fertility over lifetime, the data should cover enough years to follow children until they most likely have completed their reproductive life. Our administrative data allow us to overcome both challenges by observing individuals born in 1970–1980 up until 2020 and by reconstructing family structure over time with detailed longitudinal data from population registers.

3 Theoretical Mechanisms

3.1 Main hypotheses

Several mechanisms can influence fertility and marital outcomes of adult children from divorced families and we do not expect every mechanism to apply to all outcomes (see Table 2.1 below for an overview of the main hypotheses).

Table 2.1: Overview of the theoretical mechanisms

Theoretical mechanism	Age at first marriage (1)	Childlessness (2)	Number of children (3)	Age at first child (4)
Economic disruption hypothesis	–	–	+	–
Human capital and fertility hypothesis	–	–	+	–
Risk behavior hypothesis	–	–	+	–
Socialization hypothesis	+	+	–	+
Union duration hypothesis	"n/a"	+	–	"n/a"
Multi-partner fertility hypothesis	"n/a"	–	+	"n/a"

Notes. This table shows an overview of the relationship between having experienced parental divorce in childhood (before age 18) and the outcome variables, as predicted by the theoretical mechanism discussed in Section 3. Note that we do not expect every mechanism to affect all outcome variables, and we denote unaffected outcome variables with "n/a".

As parental divorce often causes the disruption of family resources, the *economic deprivation hypothesis* predicts that, by forcing children from divorced families to take on adult roles prematurely, parental divorce leads to early first births and early first marriages (McLanahan and Bumpass, 1988), which, in turn, may be reflected in higher completed fertility of adult children from divorced families relative to adult children from intact families. More specifically, this theory emphasizes that children from divorced families often start working early and are more likely to drop out of school or discontinue their education after secondary school graduation in order to help their family financially. The early onset of adult life, in turn, may result in adult children from divorced families marrying and forming families at a younger age than adult children from intact, married families. Further, as higher age at first child is negatively associated with the probability of higher-order births (Morgan and Rindfuss, 1999; Tomkinson, 2019), a consequence of the economic deprivation theory is that adult children from divorced families might have, on average, higher completed fertility than adult children from intact, married families. This theory may also

hold true in the Netherlands, where although the welfare system in the 1970s (when the adult children in our sample were born) was particularly extensive and women could rely on income transfers for sustainment (Manting and Bouman, 2006; Poortman, 2000), most children resided with their mothers after divorce (Kalmijn, 2016), and divorced women are likely to experience a drop in income that persists even after income transfers are accounted for (Poortman, 2000).

Related to the economic deprivation hypothesis is the *human capital and fertility hypothesis*. Traditionally, earlier literature has shown that highly-educated men and women delay first births and postpone marriage compared to lower-educated men and women (Kravdal and Rindfuss, 2008; Bystrov, 2014; Manning et al., 2014a). Moreover, standard economic theory also indicates that, among women, educational attainment is negatively correlated with the number of children at completed fertility and positively correlated with childlessness (Becker, 1960; Becker et al., 1990). Among men, this relationship has been less frequently studied and there is evidence it has even reversed over time (Kravdal and Rindfuss, 2008). Although these features appear to no longer be true in every context, descriptive statistics confirm these features to be true in the Netherlands, where highly-educated women postpone parenthood and are more likely to remain childless than lower-educated women (CBS, 2012). Moreover, conditional on becoming mothers, highly educated Dutch women also have lower fertility than their lower-educated counterparts. By contrast, for men, the negative association between education and fertility is true only with respect to the timing of first parenthood, whereby lower-educated men have their first child earlier than higher-educated men.

As most studies on divorce conclude that parental divorce negatively affects children's educational attainment (see, e.g., Biblarz and Gottainer, 2000; D'Onofrio et al., 2007; Grätz, 2015; Ermisch and Francesconi, 2001; Sun, 2001; Sun and Li, 2002), the expected undereducation of men and women with divorced parents relative to men and women with married parents may be reflected in a lower proportion of the former postponing first births and marriages and having fewer children than the latter. As a result, adult children from divorced families might become parents and marry at a younger age than adult children from intact married families, have larger families, and remain childless less often than adult children from intact families. Further, because the negative association between education and fertility is presumably stronger for women than for men, the mechanism underlying the human capital and fertility hypothesis is expected to play a stronger role for women than for men from divorced families.

In sum, both the economic deprivation hypothesis and the hypothesis of human capital and fertility predict family formation at an early age and a preference over large families by adult children from divorced families relative to adult children from intact families. Such behavior is expected to be influenced either by the disruption of household income (economic deprivation hypothesis), or by the lower educational attainment obtained by

adult children from divorced families (human capital and fertility hypothesis). A possible consequence of these hypotheses is that while children experiencing parental divorce in childhood may be significantly affected by a drop in household income or a change in educational outcomes, children experiencing parental divorce in early adulthood (e.g. age 25 or older) are less likely susceptible to these events, for instance because they have already completed their education cycle. Therefore, a testable implication is that, while adult children who experienced parental divorce in childhood and adolescence (i.e. at age 17 or less) are affected by divorce in the directions predicted by these mechanisms, adult children who experienced parental divorce in early adulthood (e.g. at age 25 or more) are not significantly affected by divorce in their fertility and marital outcomes.[2]

A final mechanism predicting higher fertility of adult children from divorced versus intact, married families is the *risk-behavior hypothesis*. Earlier research has shown that parental divorce increases the likelihood that children will adopt risky behavior such as smoking, drinking, or substance use (Zeratsion et al., 2014). In turn, a risky lifestyle may lead children from divorced families to have unplanned pregnancies more often, especially in early adulthood. Because unplanned fertility increases the number of children beyond the expected parity (Morgan and Taylor, 2006), a consequence of the risk-behavior hypothesis is that adult children from divorced families remain less frequently childless and have more children than adult children from intact, married families. The risk behavior hypothesis also predicts that adult children from divorced families become parents (and marry) at a younger age than their comparison group from intact, married families.

Parental divorce may not only affect adult children's socioeconomic status or risk behavior, but it may be a scarring event that influences adult children's attitudes and beliefs about marriage and family. The *socialization hypothesis* (Diekmann and Engelhardt, 1999) predicts that adult children with divorced parents absorb from their parents pessimistic views about marriage and family. These feelings may lead them to permanently substitute marriage with other forms of commitment such as cohabitation, or to postpone marriage until they find a partner they are confident to marry. In both cases, adult children from divorced families may postpone decisions about family until they feel their partner is appropriate to embark on starting a family. Since individuals are generally not aware of when they become infertile (Morgan and Taylor, 2006), having a stable relationship later on might be reflected in unforeseen difficulties in conceiving a child. This may be especially true for women, whose fertility decline is typically more rapid than that of men (Morgan and Taylor, 2006). A consequence of the socialization hypothesis might therefore be that men and (especially) women from divorced families are more likely to become parents at a later age, remain more often childless, and have more frequently lower-order births. Pes-

[2] It should be noted that the satisfaction of these hypotheses is necessary but not sufficient to prove the role of the economic deprivation theory and the standard theory of human capital and fertility.

simistic views on marriage may also more directly be reflected on intended parity, that is, on preferences about family size (Axinn and Thornton, 1996). Both men and women from divorced families may want to have smaller families, for example, to invest in their offspring and compensate for the effects of possible future family breakdowns.

Parental divorce may also more directly affect adult children's attitudes and beliefs about divorce or separation, thereby influencing their subsequent fertility outcomes. As discussed earlier, individuals who experienced parental divorce in childhood are themselves more likely to divorce from their spouse or separate from their cohabiting partner (Corak, 2001; Amato and Patterson, 2017), and this association holds true also in the Netherlands (Dronkers, 1997). Although a large number of studies have documented the intergenerational transmission of divorce, it is theoretically unclear and empirically unexplored how the transmission of divorce from one generation to another is translated into children's fertility outcomes in adulthood. On the one hand, by separating from their partners more often than their counterparts from intact families, adult children from divorced families might remain childless more frequently and have a lower number of children than their counterparts from intact families. We call this the 'union duration hypothesis' and expect a negative relationship between parental divorce and adult fertility. Note that, unlike the socialization theory which predicts negative fertility outcomes because of pessimistic beliefs about family, the union duration hypothesis predicts a decrease in completed fertility because adult children from divorced families are not in a stable relationship for long enough relative to adult children from intact families. On the other hand, as divorce often leads to remarriage and having children with multiple partners, with recent research showing evidence of intergenerational transmission of multi-partner fertility (Lappegård and Thomson, 2018), parental divorce may result in children from divorced families having a lower likelihood of remaining childless and a higher total number of children compared to their counterparts from intact, married families. We call this the 'multi-partner fertility' hypothesis and expect a positive relationship between parental divorce in childhood and fertility outcomes in adulthood.

Because we expect the union duration and the multi-partner fertility hypotheses to be mutually exclusive, we focus on the union duration hypothesis and test it on the sample of children who have been married at least once. In particular, we examine the role of marriage duration as a mediator of the negative association between parental divorce and fertility outcomes.[3] It should be noted that because women are generally more likely than men to initiate a divorce (Kalmijn and Poortman, 2006), the experience of parental divorce may more easily lower the barriers to divorce for women than for men. Previous literature has indeed shown a stronger intergenerational transmission of divorce among women

[3]This theory does not explain the relationship between parental divorce and age at first birth and age at first marriage. Moreover, because age at first marriage is an outcome variable, marriage duration cannot be a mediator of the relationship between parental divorce and age at first marriage.

than among men (Lyngstad and Engelhardt, 2009; Feng et al., 1999; Kalmijn and Poortman, 2006). Therefore, women from divorced families may remain in a stable relationship for an even shorter period of time than men from divorced families, thus remaining more frequently childless and having fewer children than men from divorced families. Put differently, the union duration hypothesis may play a stronger role for women than for men from divorced families.

3.2 Moderating hypotheses

While the aforementioned theories predict an overall direction for the relationship between parental divorce and fertility and marital outcomes, several theories suggest that the consequences of parental divorce may be different according to the subgroups involved. The *divorce-adjustment stress* hypothesis (Amato, 2000) posits that children from divorced families experience emotional distress at different times during the divorce depending on their age at the time of the divorce. As a result, they may react differently to their parents' divorce. Older children at the time of divorce might view their parents' divorce favourably because they likely have been exposed to several years of a non-functioning marriage between their parents. By contrast, younger children at the time of divorce may view their parents' divorce unfavourably because they may not have foreseen the break-up of their family. A testable consequence of the divorcement-adjustment stress hypothesis is that the influence of parental divorce on fertility and marital outcomes is larger in magnitude for children experiencing parental divorce during preschool or primary school than for children experiencing parental divorce during secondary school.

The relationship between parental divorce and fertility outcomes may also differ depending on the degree of parental involvement in children's lives after the divorce (*parental distance hypothesis*). Earlier research has shown that children who experience parental divorce in childhood generally have weaker emotional bonds with their parents and a deteriorated relationship with especially the noncustodial parent (Aquilino, 2005; Zill et al., 1993; Amato and Cheadle, 2005). Nonetheless, divorced parents may be involved to different extents in children's lives after the divorce. Previous research has shown that greater time or financial involvement of the noncustodial parent in children's life is positively associated with children's well-being and educational outcomes (Amato and Gilbreth, 1999; Viry, 2014; King and Sobolewski, 2006). Having a good relationship with the noncustodial parent might also be relevant for adult fertility decisions, as (grand)parents have been shown to play a role in the decision to have the first or subsequent children (Aassve et al., 2012; Thomese and Liefbroer, 2013; Pessin et al., 2021; Rutigliano, 2020). It is therefore possible that the degree of involvement of the noncustodial parent after the divorce might help mitigate some of the positive or negative effects of parental divorce on children's fertility outcomes.

Using geographical proximity between parents after the divorce as a proxy for parental involvement, a testable hypothesis is that the influence of parental divorce on fertility and marital outcomes is smaller in magnitude for children whose parents lived closer to each other after the divorce (e.g. within a radius of 0–5 km) than for children whose parents lived farther apart from each other after the divorce (e.g. outside a radius of more than 75 km). Parents living within a 5 km radius of each other are assumed to be within a walking or cycling distance that allows for frequent interaction with children, whereas parents living outside a 75 km radius are assumed to be at a greater distance that makes frequent contact with children more difficult.[4]

Finally, the influence of parental divorce on adult children's fertility and marital outcomes may be changing over time and may be different for adult children born in early birth cohorts compared to adult children born in later birth cohort (*trend hypothesis*, Gähler and Garriga, 2013). It is not straightforward, ex ante, whether the direction of this association should be expected to be positive or negative over time. On the one hand, two mechanisms suggest that the influence of parental divorce over time should be decreasing in the Netherlands. First, women's labor market participation has substantially risen over the years (Nientker and Alessie, 2019), potentially leading divorced mothers to have more economic resources with which to mitigate the negative consequences posited by the economic disruption hypothesis and the human capital and fertility hypothesis. Second, the motivations behind divorce have shifted over the years from severe motivations (such as violence or infidelity) to behavioral motivations (such as relational incompatibility) (De Graaf and Kalmijn, 2006), and parents are becoming more conscious of the potential difficulties caused to their children by divorce (Gähler and Garriga, 2013). Therefore, divorced parents might be attempting more frequently to be involved in children's lives after the divorce, which, as discussed earlier, is mostly beneficial for children's outcomes.

However, other mechanisms suggest that the influence of parental divorce on fertility and marital outcomes might be increasing over time. The normalisation of divorce has lead an increasing proportion of low-conflict marriages to dissolve. Earlier studies have found that the dissolution of low-conflict marriages is more harmful to children than the dissolution of high-conflict marriages (Amato, 2001; Gähler and Garriga, 2013). Thus, if the breakdown of low-conflict marriages prevails over time, it is possible that the influence of parental divorce on fertility and marital outcomes will increase in magnitude. To test the trend hypothesis, we compare the marital and fertility outcomes of adult children from divorced families born in later birth cohorts (e.g. 1977–1980) to the marital and fertility outcomes of adult children from divorced families born in earlier birth cohorts (e.g. 1970–1973). If any of these mechanisms are true, we would expect a significant difference in the

[4] It should be noted that although noncustodial parents living far away from children might do so for economic reasons (e.g. job opportunities), this may lead to greater difficulties in reconciling demanding work conditions with family life after divorce, thus impairing the quality of their relationship with their children.

influence of parental divorce on the outcomes of children born in the most distant birth cohorts.

4 Data

4.1 Sample construction

We use administrative data from the Netherlands that cover the entire Dutch population listed in the population registers from 1994 until 2020. The population registers are individually held by each municipality in the Netherlands, and keep track of the inhabitants living within the territory of competence of each municipality. In the registers, each Dutch resident is assigned a unique identifier number and is required to register any child birth within three days from the birth. These identifier numbers uniquely link parents to adult children, and allow to follow both parents and adult children throughout the years and retrieve a range of information on their demographics, marital status, and fertility outcomes. This means that, on the parents' side (i.e. the first generation), we observe whether the parents of the child were married.[5] As we follow marriages until 2020, we also observe whether the parents of the child are divorced, and if so, in what year the divorce occurred. Using the same data on marriages, on the adult children's side (i.e. the second generation), we observe whether adult children married when they grew up, and if so, in what year the marriage occurred. Further, we have information on whether adult children (both those who married and those who did not marry) became parents or remained childless, and we observe the total number of offspring born (i.e. the third generation) as well as the birth year of every child.

To study the fertility and marital outcomes of adult children from divorced families, we restrict the sample as follows. First, we select cohorts of adult children born between 1970 and 1980 (a total of 2,009,229 adult children), so that we observe the fertility outcomes of men and women aged at least 40 years old. Second, as our panel ends in 2020, we look at fertility and marital outcomes of men and women aged 40 years old between 2010 and 2020. However, our results are robust to approximating completed fertility at age 45 and estimating our model on the birth cohorts 1970–1975 that are aged 45 between 2015 and 2020 (see Table 2.A7 in the online supplement).[6] Third, we focus on adult children whose parents were married and exclude adult children whose parents were cohabiting.[7] This restriction is necessary since our data provide information on cohabitation agreements only

[5] Note that in the Netherlands it is possible for two partners to enter a 'registered partnership' as an alternative to the marriage. Because registered partnerships were introduced in 1998, however, the parents whose divorce we examine throughout the chapter could only be married.

[6] Nonetheless, it might be possible that some men might not have reached the end of their reproductive life even at age 45. However, we expect this to concern a minority of men in our sample and to not alter the main conclusions of this chapter.

[7] Note that we also excluded children born to single mothers, as it would not be possible to assess the influence of parental separation since the parents of these children were never together.

as of 1995, and thus we do not observe whether parents cohabit before that year. As unmarried cohabitation was still uncommon in the 1970s (Latten and Mulder, 2013), however, the majority of families in our sample were married at the adult child's birth.[8] Fourth, we exclude observations with missing covariates. The final sample consists of 1,814,820 adult children born in the Netherlands between 1970 and 1980.

4.2 Variable construction

Outcome variables. We measure fertility and marital outcomes by examining several variables of interest: age at first marriage, childlessness, age at first child, and number of children. We construct 'age at first marriage' by including a continuous variable indicating individuals' age at their first marriage or at their first registered partnership.[9] Given that in the Netherlands registered partnership is almost a perfect substitute for marriage (Trandafir, 2014), for the studied adult children, we do not distinguish between registered partnerships and marriages. As a second outcome variable, we examine the extensive margin of fertility by including an indicator for whether individuals had children or remained childless by age 40 ('childlessness'). We code this indicator as 1 if individuals did not have children by age 40, and 0 if they had at least one child. Next, within the group of mothers and fathers, we include a discrete variable for the age at which individuals became parents ('age at first child'). Finally, we look at completed fertility by including a discrete variable for the total number of children born to each individual in our sample by age 40 ('number of children'). This number can include zero, meaning that an individual remained childless.

Control variables. As measures of adult children's background socioeconomic status, we include two variables.[10] First, we include a discrete variable for maternal age at birth. Early childbearing is in most cases an indicator of low socioeconomic status (while the contrary holds for late childbearing), and it may be associated with a higher risk of marital dissolution if it drives parents into early marriage – a strong predictor of marital instability (Amato, 1996). Second, as proxy for parental income, we include indicators for children's neighborhood of residence during childhood (at age 11).[11] The socioeconomic status is shown

[8] It should be noted that we do not observe separation between parents before divorce. It is therefore possible that in our sample the married parents in the control group separated, which would bias our estimates toward zero. However, because our data cover parental divorces up to age 40, we can expect that separated parents either divorced during their children's lifetimes or got back together. Eliminating in further analysis parents who may have been separated but not yet divorced at the time of construction of our key independent variable (i.e., parents who divorced after the children were 18 years old), leaves the results shown in Table 2.2 virtually unchanged. This suggests a limited role of bias stemming from the inability to observe separation before divorce.

[9] This means that individuals only in cohabitation agreements are not considered in the estimations using 'age at first marriage' as outcome variable. However, in additional analyses, we also examine the relationship between parental divorce and first cohabitation timing (see Table 2.A5).

[10] Note that we do not include a control variable for parents' highest level of education because of the large number of missing observations of this variable. In fact, we observe maternal education level for only 19.5 percent of the sample, while we observe paternal education level for 17.4 percent of the sample.

[11] Note that, to minimize the number of observations we drop due to missing information on the child's neighborhood, for a small percentage of observations in our sample for which we do not observe this variable at age 11 (21.2 percent), we impute it using the first neighborhood of residence that is not missing when the child is between the

to negatively influence the likelihood of divorce (South, 2001). Moreover, it accounts for (grand)parents' propensity to provide financial and time support to adult children when they become parents, which in turn positively affects adult children's (and especially adult daughters') decision to enter into parenthood (Pessin et al., 2021).

As fertility outcomes may depend on family and cultural heritage (Fernández and Fogli, 2006, 2009), we also control for family background characteristics. First, we account for preferences over family size that might result from the number of siblings adult children grew up with by including a discrete variable for adult children's number of full siblings.[12][13] Second, we account for adult children's ethnic background by looking at the country of birth of their parents. We construct a categorical variable, consisting of separate dummies, coded 0 if both parents are born in the Netherlands; 1 if one parent is born in the Netherlands, the other outside the Netherlands, and 2 if both parents are born outside the Netherlands.

Finally, we account for the declining trend in fertility rates and the increasing trend in marriage and childbearing postponement by including indicators for adult children's year of birth.

Measures to test the theoretical mechanisms. To test the union duration hypothesis, we construct a continuous variable accounting for the number of years adult children have been married in adulthood. For the subsample of children who were married at least once, we construct marriage duration as the total number of years adult children have been married or in a registered partnership by age 40.[14]

To test the parental distance hypothesis, we construct a moderator variable for parental involvement using the geographical distance between parents after the divorce.[15] Earlier research has shown that noncustodial parents living closer to children after the divorce are more involved in their children's lives by having more frequent contacts and providing larger amounts of paid child support (Cheadle et al., 2010; Viry, 2014). A higher degree of parental involvement, in turn, may reinforce adult children's relational bonds with the

ages of 11 and 17. Our results are not sensitive to this choice. First, we find consistent results when we use only those observations for which the child's neighborhood of residence is available at age 11. Second, we find consistent results even when we do not include neighborhood indicators at all (see Table 2.A11 in the appendix). Finally, our results are also robust to measuring children's neighbourhood at birth. However, given that we observe birth indicators only for 28.17 percent of our sample, we measure throughout the chapter children's neighbourhood while children are still enrolled in primary education.

[12] The number of siblings accounts for the total number of brothers and sisters of the focal adult child (born in 1970–1980).

[13] However, our results hold even if we do not include number of siblings as an additional control variable.

[14] If adult children married more than once, we construct marriage duration by summing the total number of years that each marriage lasted. However, our results are robust to considering only first marriages and controlling for the duration of the first marriage alone.

[15] Note that since we employ the geographical distance between parents after divorce as a moderator, we do not include it as a control variable in our main analysis. However, our results are robust to additionally controlling for this variable (not reported). Further, because parental distance after divorce may depend on the age of the child at the time of divorce, our findings are also robust to additionally include control variables for the adult child's age at divorce and for the interaction between the adult child age's at divorce and parental distance (see Table 2.A1 in the appendix).

noncustodial parent, which are shown to play a role when deciding to enter into parenthood or to have subsequent children. (Aassve et al., 2012; Thomese and Liefbroer, 2013; Pessin et al., 2021; Rutigliano, 2020). In practice, we construct the moderator for parental involvement in two steps. First, for the sample of children from divorced families, we construct the average distance at which parents lived away from each other in the seven years following the divorce.[16] Second, for the full sample, we create a categorical variable coded 0 if adult children are from intact married families. If, by contrast, adult children are from divorced families, we define four categories of average distance (in kilometers) existing between parents after divorce: 0-5, 5-25, 25-75, and more than 75 kilometers. We assume that the noncustodial parent's level of involvement is lower when the parents live far from each other after the divorce and higher when the parents live closer to each other after the divorce.[17]

5 Methodology

5.1 Linear regression model

We estimate an Ordinary Least Squares (OLS) regression, which we specify as:

$$Y_i = \beta_0 + \beta_1 D_i + \alpha \mathbf{X_i} + \gamma_t + \epsilon_i \tag{2.1}$$

where Y_i indicates the fertility outcomes of individual i at age 40: completed fertility (i.e. the total number of children born to i, including childlessness), childlessness (1 is no children, 0 is at least one child), age at first child, and age at first marriage.[18][19][20] Because of all the possible sex differences in the mechanisms discussed earlier in Section 3, we estimate separate models for men and women. D_i denotes the variable of interest, and takes value 1 if the adult child has experienced parental divorce before age 18, 0 if the adult child has lived with married parents throughout childhood.[21]. Finally, $\mathbf{X_i}$ defines a series of individual control variables, which we include differently according to the model specification. We

[16] Note that our results are robust when we consider a shorter time window in which to measure parental distance, corresponding to five years after parents' divorce.

[17] Although it is possible that noncustodial parents who live far away from their children are nevertheless involved in children's lives (e.g. through phone calls or e-mails), Viry (2014) showed that a high frequency of these types of contact does not lead to an improvement in children's well-being.

[18] We estimate a linear probability model when the dependent variable is childlessness. However, our results are robust to estimating a logit model.

[19] As completed fertility is count data, we further estimate the influence of parental divorce on number of children using a Poisson regression. Our results are similar to what we obtain using a linear regression model in Table 2.2. Given the similarity in the findings and given that not all robustness checks' model specifications can be estimated using a Poisson regression, we employ throughout the chapter a linear regression model for completed fertility as outcome variable.

[20] With respect to age at first child, we also considered conducting a survival analysis using Cox proportional-hazards models. However, because we found no evidence to support the proportional-hazard hypothesis, which is necessary to estimate the model, we refrain from using proportional-hazard Cox models throughout the chapter.

[21] However, we also look at the influence of parental divorce by child age (see Section 6.3.1), and redefine the variable of interest to include children who experienced parental divorce after age 18 but before first parenthood (see Table 2.A4 in the online supplement).

always include indicators for year of birth and ethnicity. To account for heteroskedasticity in the error term, we use Huber-White standard errors.[22]

We should emphasize that identifying the association between parental divorce and adult children's fertility outcomes in adulthood, net of confounding variable, provides some challenges. First, to avoid controlling away part of the influence of parental divorce on our outcome variables and to avoid the risk of reverse causality, we do not account in our main analysis for individuals' socioeconomic status in adulthood. However, we find consistent results when we additionally include control variables for individuals' highest level of educational attainment and income measured at age 40 (see Table 2.A11 in the appendix). Second, given that we also observe fertility outcomes of single men and women, we do not include in our main analysis control variables for the characteristics of individuals' partners (e.g. partner's SES or family background). However, in Appendix B, we estimate a model in which we look at the fertility outcomes of couples at their first marriage. In this model, we control for both the wife's and the husband's background characteristics, and redefine the key variable of interest to capture whether at least one spouse has experienced divorce during childhood. The results obtained in the main analysis at the individual level are robust to the choice of not controlling for the partners' characteristics.

5.2 Parental conflict and parental death

A third important challenge rising in the estimation of the influence of parental divorce comes from disentangling the effect of parental divorce from the effect of unobserved variables such as parental conflict. To examine to what extent the coefficients estimated by means of a linear regression model might suffer from endogeneity bias we employ a natural experiment in which we compare adult children who experienced parental loss by divorce to adult children who experienced parental loss by death (for earlier studies using this methodology, see, e.g., Corak, 2001; Francesconi et al., 2010).[23] While intrinsically different, parental death and parental divorce have in common that adult children experienced

[22] Our results are robust to clustering standard errors at the neighbourhood level.

[23] We also considered Difference-in-Differences (DiD), instrumental variables (IV), and sibling fixed effects estimations, but these methodologies were discarded based on econometric issues. In particular, we discarded DiD and IV because, while the law introducing no-fault divorce in the Netherlands in 1971 is an exogenous change that affected divorce rates, it does not satisfy the methodological requirements needed for both DiD and IV. Regarding DiD, our data do not offer a suitable control group and we cannot test whether the parallel trend assumption holds. Regarding IV, the law does not provide us with a clear-cut instrument that is uncorrelated with the error term. Finally, we also discard the within-family variation model obtained by estimating sibling fixed effects (see e.g. Björklund and Sundström, 2006; Björklund et al., 2007). As explained in Amato and Anthony (2014), identifying the causal effect of parental divorce using sibling fixed effects relies on two assumptions. First, parental divorce should affect children differently according to the number of years they have lived in an disrupted family during childhood. Second, children experiencing parental divorce after age 18 (or after they leave home) should not be affected by the divorce of their parents. Because we cannot exclude that children aged more than 18 (or after they have left home) might be influenced by the divorce of parents in their fertility and marital outcomes, we do not consider the assumptions required for the sibling fixed effects models to be applicable to our context. In fact, our findings show that children experiencing parental divorce after age 18 are equally influenced by parental divorce (see Table 2.A4 in the online supplement), and that for some of our outcome variables no age category prevails over the others (see Figure 2.1).

a form of parental absence during childhood and likely a drop in household income.[24] Because parental death is reasonably less endogenous than parental divorce[25], the outcomes of bereaved adult children can be used as a benchmark against which to evaluate the endogeneity of parental divorce. In particular, the absence of significant differences in the outcome variables between adult children from bereaved families and adult children from divorced families may suggest a limited role of omitted variable bias in the estimation of β_1. More formally, we modify Equation 2.1 and estimate the following equation:

$$Y_i = \beta_0 + \beta_1 D_i + \beta_2 B_i + \alpha \mathbf{X_i} + \gamma_t + \epsilon_i \tag{2.2}$$

in which we include an indicator B_i for whether the individual i comes from a bereaved family. We code this indicator as 1 if the parent passed away before the child attained the age of 18, and 0 otherwise. Note that, as we have data on parental death only as of 1994, adult children in bereaved families can only be aged between 15 and 17 years old when their parent passed away. Therefore, we select the population of children born in 1979-1980, and compare adult bereaved children with adult children from divorced families who experienced parental divorce between 15 and 17 years old. By comparing β_2 (the effect of parental loss by death) to β_1 (the influence of parental divorce), we attempt to assess to what extent the coefficients estimated in Equation 2.1 might suffer from endogeneity bias (Corak, 2001). More specifically, if the difference between the estimators is not statistically different by means of a test of equality of coefficients, namely if $\beta_1 - \beta_2 = 0$, we are confident that our estimated coefficients reflect the effect of parental absence and selection is limited in driving our findings.

5.3 Coefficient stability test

Although in our analysis we control for a large set of covariates that capture adult children's family and socioeconomic background, the empirical strategy underlying Equation 2.1 is based on adding observables to account for heterogeneity. That is, we conclude that our results are robust if the coefficients obtained in the regression where we only control for few exogenous covariates are similar in magnitude and significance to the coefficients ob-

[24] We might expect that some of the mechanisms discussed earlier in Section 3 might also apply to the case of parental death. As there is evidence that parental death produces a drop in the income of the living parent and results in children's lower educational attainment (Bíró, 2013; Gimenez et al., 2013), we might expect the economic disruption hypothesis and the hypothesis of human capital and fertility to apply also in the case of parental death. Moreover, given that children who experience parental death are more likely to adopt risky behavior as a consequence of trauma (Kaplow et al., 2010), we might expect also the risk behavior hypothesis to play a role. Further, consistent with the parental distance hypothesis, we might expect parental absence due to parental loss to negatively affect fertility because of the reduced availability of grandparents to provide childcare. Finally, although parental death may interrupt the exposition of children to the model of a (presumably functioning) two-parent marital family, possibly reflecting in an increased likelihood of future relationship instability, we do not expect that the mechanisms underlying the socialization hypothesis or the union duration hypothesis will play a major role in the case of parental death.

[25] Note that this might not be the case if parents from certain socioeconomic groups engage more often in risky or unhealthy behaviors (e.g., heavy drinking), that might increase the likelihood of developing deadly deceases (Corak, 2001; McLanahan et al., 2013).

tained in the regressions where we include the full set of covariates (*selection on observables*), under the assumption that the selection on observables is informative about the selection on unobservables. Nonetheless, there may still be other unobserved variables that we do not observe and might be influencing our outcome variables rather than parental divorce per se (e.g. parental stress). Moreover, as illustrated by Oster (2019), coefficients may appear stable after the inclusion of control variables because they explain little of the dependent variable's variance (i.e. they are of poor quality), rather than because the model is robust to omitted variable bias. To account for this, Oster (2019) developed a test in which, before and after the inclusion of control variables, movements in R-squared are observed as well as movements in coefficients, thereby accounting for the quality of the control variables included.

We follow Oster (2019) and construct a bounding estimator for the influence of parental divorce on adult children's fertility and marital outcomes. We consider robust all point estimates that survive assuming an equal degree of selection on observed and unobserved variables, and we report the degree of selection that is needed to cancel out the estimated effect. More information on the methodology is provided in Appendix B.2 in the online supplement.

5.4 Coarsened Exact Matching

Although our population data are rich enough to identify and follow into adulthood a large number of adult children who experienced parental divorce during childhood, the sample of adult children from divorced families is considerably smaller than the sample of adult children from intact, married families. An excessively large control group (in our case, adult children from intact, married families) compared to the treatment group (in our case, adult children from divorced families) may lead to biased estimates of the treatment effect (in our case, the influence of parental divorce), as the estimation of the treatment effect may become dependent on the statistical specification of the model (Ho et al., 2007). To achieve better balance in the distribution of covariates and reduce model dependence, we follow Iacus et al. (2011, 2012) and pre-process our data with Coarsened Exact Matching (CEM). The aim of the CEM is to make the treatment group as similar as possible to the control group by seeking to match each unit in the treatment group with at least one unit in the control group that shares exactly the same or very similar observable characteristics. Given our large sample, for all control variables we look for an exact match.[26]

[26]Note that to maximize the number of matched children from divorced families, we do not match based on child neighbourhood at age 11. However, our findings are robust to including neighbourhood (at age 11) FE as additional control variable on the data pre-processed by CEM (see Table 2.A8 in the online supplement).

6 Results

6.1 Descriptive statistics

Our sample consists of 1,814,820 adult children (875,930 women and 938,890 men), of whom 197,195 (94,944 women and 102,251 men) experienced parental divorce before age 18, corresponding to about 10.87 percent of the total sample. For comparison, the number of divorces per 1000 couples in the Netherlands ranged between 3.3 in 1970 and 7.5 in 1980 (StatLine, 2022). Table 2.1 below compares men and women in divorced families (columns 1 and 3) to men and women in stable, intact families (columns 2 and 4).

Table 2.1: Descriptive statistics

	Women		Men	
	Intact families (1)	Divorced families (2)	Intact families (3)	Divorced families (4)
Age of mother at birth	27.302	25.690#	27.28	25.649#
Family size	2.746	2.564#	2.735	2.551#
Parental distance (in km)	0.000	18.764#	0.000	18.474#
Ethnicity				
Both parents NL	0.799	0.694#	0.797	0.695#
One parent NL, other not NL	0.120	0.192#	0.120	0.190#
Both parents not NL	0.081	0.114#	0.083	0.115#
Outcome variables				
Age at first marriage	27.755	28.303#	29.976	30.568#
Childlessness (1 is yes)	0.213	0.234#	0.317	0.385#
Number of children	1.697	1.564#	1.430	1.188#
Age at first child	29.404	28.253#	31.318	30.723#
Observations	780,986 (89.16%)	94,944 (10.84%)	836,639 (89.11%)	102,251 (10.89%)
Total	875,930	875,930	938,890	938,890

#The mean for women (men) of divorce is significantly different at the 5 percent level from the mean for women (man) of intact families by means of a two-tailed z-test.

Men and women from divorced families appear to have younger mothers and fewer siblings than men and women from intact, married families. They are also more ethnically diverse, as they are less likely to have both parents born in the Netherlands, but more likely to have either one or both parents of foreign origins.[27]

Regarding the outcome variables, men and women from divorced families appear to marry later than men and women from intact, married families. Specifically, men (women) from divorced families marry for the first time at an average age of 30.6 (28.3) years old. This corresponds to a postponement of marriage by almost three quarters of a year compared to men (women) from married families. Men and women from divorced families also appear to remain more often childless. In particular about 23.4 percent of the women and 38.5 percent of the men from divorced families in our sample are childless at the age of 40.

[27] This is consistent with descriptive evidence from the Netherlands showing that marriages in which at least one partner is foreign born have higher divorce risks than marriages in which both partners are Dutch born (Kalmijn et al., 2005; van Huis and Steenhof, 2003).

Relative to their comparison groups from intact, married families, these averages are 2.1 percentage points higher in the case of women, and 6.8 percentage points higher in the case of men. The data on childlessness are generally consistent with fertility statistics reported by Statistics Netherlands, which show that approximately one in four women and one in three men are childless by the age of 45 (CBS, 2017).[28] Further, within the sample of parents, both genders from divorced families appear to become parents at a younger age than their comparison groups from intact, married families. Men and women from divorced families become parents at an average age of 30.7 and 28.3 years respectively, i.e. about 6 months and 1 year earlier than men and women from intact, married families. Finally, both men and women from divorced families appear to have lower completed fertility than men and women from intact, married families. Women from divorced families have on average 1.56 children, while men from divorced families have on average 1.19 children. Relative to their comparison groups from intact, married families, the reduction in the number of children amounts to 7.84 percent in the case of women and 16.92 percent in the case of men.

6.2 Fertility and marital outcomes

Using a linear regression model, Table 2.2 compares men and women from divorced families to men and women from intact, married families on their fertility and marital outcomes. Our results show that parental divorce significantly influences the outcomes of both genders. When the full set of covariates is included in column 2, women from divorced families are 0.340 years older than women from intact families at their first marriage (corresponding to a postponement of the marriage of about four months), and a similar result holds for men.[29]

Turning to the fertility outcomes, both men and women from divorced families appear to have a lower number of children than men and women from intact, married families. First, they remain childless more often. The estimated association is particularly large for men from divorced families who appear to remain childless 5.9 percentage points more often than men from intact families in column 4, whereas for women this association amounts to 2.0 percentage points. Second, they have lower completed fertility than their comparison group from intact, married families. Again, this reduction appears larger for men than for women, as it amounts to 0.180 less children in the case of men and 0.082 less children in the case of women (corresponding to a decrease of about 13 and 5 percent relative to men and women from married, intact families). These gender differentials do not stem from men not having reached the end of their reproductive life at age 40, as we find consistent differentials also when fertility outcomes are measured at age 45 (see Table 2.A7). To place these results in perspective, Cygan-Rehm and Maeder (2013) find that in Germany, a Western

[28] The differences between men and women are generally due to the greater difficulty that men have in finding a partner than women (CBS, 2017).

[29] However, men and women from divorced families are also found to begin cohabiting at a younger age than men and women from intact, married families (see Table 2.A5 in the online supplement).

European country like the Netherlands, one additional year of schooling reduces the number of children of women by 0.1 and increases their likelihood of remaining childless by 2–5 percentage points. Thus, for women, our results appear to be comparable to the decline in fertility produced in Western European countries by one additional year of education.

Within the sample of parents, our findings also show that women (and to a less extent men) from divorced families have their first child sooner than women (men) from intact, married families. In column 8, for women from divorced families, this association amounts to 0.790 years and corresponds to the latter entering motherhood about one full year earlier than their comparison group from intact, married families. It is possible that entry into parenthood at a younger age is partly driven by the lower adult socioeconomic status of men and women from divorced families compared to men and women from intact, married families. Indeed, as shown in Table 2.A11 in the appendix, additionally controlling for adult children's educational attainment at age 40 reduces the estimated associations between parental divorce and first parenthood timing. Moreover, additional analyses (see Table 2.A5 in the online supplement) reveal that parental divorce is also positively correlated with teenage pregnancy when the full set of covariates is controlled for.[30]

We further explore whether men and women at the top and at the bottom of the distribution of age at first child are affected differently by parental divorce by looking at the influence of parental divorce by quantiles of the age distribution. Figure 2.A1 in the online supplement shows that, when the full set of covariates is accounted for, parental divorce has the largest (negative) influence among younger parents at the bottom of the distribution. By contrast, comparing older parents even reveals a positive association between parental divorce and age at first child. It might be that among individuals at the top of the distribution who postpone parenthood, parental divorce results in even greater postponement of parenthood because it increases the risk of not establishing a stable relationship with a partner.

To check whether our findings are robust to omitted variable bias, first, we construct the bounding estimator developed by Oster (2019). We show our results in Table 2.3 below, where we report for each outcome variable the bounding estimator constructed assuming an equal degree of selection on observed and unobserved variables on the one hand, and the degree of selection needed to explain away the result on the other. Table 2.3 reveals that, even if we assumed the unobserved variables (such as parental stress) to be as important as the observed variables in determining our outcome variables, adult children from divorced families are still found to postpone marriage, to remain childless more often, to have a lower number of children, and to become parents earlier than adult children from intact families.

[30] We should note that although adult children from divorced families appear to have their first child earlier than adult children from divorced families, this is not reflected in the former having higher fertility than the latter. Thus, we find no support for the economic disruption hypothesis and the human capital and fertility hypothesis. Besides, additional analyses (reported in Table 2.A4 in the appendix) show that parental divorce is highly influential on marital and fertility outcomes even when it occurs when children have finished their education at age 25, thus contracting the hypotheses posited by both theories.

Table 2.2: Parental divorce and offspring's fertility and marital outcomes

Women	Age at first marriage		Childlessness		Number of children		Age at first child	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parental divorce (1 is yes)	0.445***	0.340***	0.019***	0.020***	−0.129***	−0.082***	−1.084***	−0.790***
	(0.024)	(0.024)	(0.001)	(0.002)	(0.004)	(0.004)	(0.020)	(0.020)
Family background	No	Yes	No	Yes	No	Yes	No	Yes
Neighbourhood (age 11) FE	No	Yes	No	Yes	No	Yes	No	Yes
Ethnicity, birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	576,172	576,172	875,930	875,930	875,930	875,930	687,137	687,137
Children from DF	53,646	53,646	94,944	94,944	94,944	94,944	72,684	72,684
R-squared	0.047	0.139	0.002	0.029	0.004	0.073	0.029	0.123
Men	**Age at first marriage**		**Childlessness**		**Number of children**		**Age at first child**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parental divorce (1 is yes)	0.528***	0.412***	0.064***	0.059***	−0.234***	−0.180***	−0.542***	−0.333***
	(0.023)	(0.024)	(0.002)	(0.002)	(0.004)	(0.004)	(0.020)	(0.020)
Family background	No	Yes	No	Yes	No	Yes	No	Yes
Neighbourhood (age 11) FE	No	Yes	No	Yes	No	Yes	No	Yes
Ethnicity, birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	534,879	534,879	938,890	938,890	938,890	938,890	634,642	634,642
Children from DF	47,238	47,238	102,251	102,251	102,251	102,251	62,904	62,904
R-squared	0.036	0.119	0.003	0.027	0.006	0.061	0.018	0.089

Notes. This table shows the estimated association between having experienced parental divorce in childhood (before age 18) and fertility and marital outcomes in adulthood. DF indicates divorced families. Family background includes age of mother at birth and family size. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

As a second test for omitted variable bias, we compare adult children who experienced parental loss by divorce to adult children who experienced parental loss by death, under the assumption that parental death is more exogenous than parental divorce and thus can provide a benchmark against which to compare the outcomes of adult children from divorced families (Corak, 2001). We show our results in Table 2.A2 in the online supplement. Our findings reveal that parental death is correlated with most of our outcome variables along the same direction as parental divorce (albeit often not significantly due to the lower power). When the full set of covariates is included, a test of equality of coefficients fails to reject the null hypothesis of no significant difference between the bereaved and the divorced group for the regressions on childlessness, age at first child, and number of children. This suggests a limited role of unobservables in determining the influence of parental divorce on these outcome variables. A significant difference between the two groups appears to persist relative to age at first marriage. However, it is possible that regarding marital behavior, parental divorce may affect adult children differently than parental death, for example by changing adult children's perspectives on marriage. Previous research that used death as a natural experiment consistently found a significant difference between the bereaved and the divorce groups in marriage and divorce likelihoods (e.g. Corak, 2001). Moreover, the bounding analysis earlier discussed further suggested a limited role of unobservables in driving the influence of parental divorce on age at first marriage.

As a third test, we check whether the estimated coefficient measuring the influence of parental divorce on fertility and marital outcomes may depend on the statistical specification

Table 2.3: Parental divorce, fertility and marital outcomes, and coefficient stability test

Women	Age at first marriage		Childlessness		Number of children		Age at first child	
	$\delta = 1$	δ^{max}	$\delta = 1$	δ^{max}	$\delta = 1$	δ^{max}	$\delta = 1$	δ^{max}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parental divorce (1 is yes)	**0.270**	4.368	**0.020**	17.860	**−0.065**	4.239	**−0.662**	5.047
Observations	576,172	576,172	875,930	875,930	875,930	875,930	687,137	687,137
Children from DF	53,646	53,646	94,944	94,944	94,944	94,944	72,684	72,684
Men	Age at first marriage		Childlessness		Number of children		Age at first child	
	$\delta = 1$	δ^{max}	$\delta = 1$	δ^{max}	$\delta = 1$	δ^{max}	$\delta = 1$	δ^{max}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parental divorce (1 is yes)	**0.352**	5.881	**0.055**	10.182	**−0.158**	6.316	**−0.244**	3.445
Observations	534,879	534,879	938,890	938,890	938,890	938,890	634,642	634,642
Children from DF	47,238	47,238	102,251	102,251	102,251	102,251	62,904	62,904

Notes: The table shows the estimated association between having experienced parental divorce in childhood and fertility and marital outcomes in adulthood, using the bounding estimator developed by Oster (2019). The table shows the estimated coefficients assuming a unit degree of selection on observed and unobserved variables ($\delta = 1$), and the degree of selection that is needed to explain away the results (δ^{max}). Following the suggestion by Oster (2019), we set $R_{max} = 1.3\tilde{R}$, where $\tilde{R}$ is the R-squared from the OLS regression in which we include the full set of control variables (namely ethnicity, age of mother, family size, birth year FE, and neighbourhood (age 11) FE). Estimated coefficients that are robust to a unit degree of selection on observed and unobserved variables are indicated in bold.

of the model due to the very large control group compared to the treatment group. In Table 2.A8 in the online supplement, we show the estimated associations between parental divorce and fertility and marital outcomes when the data are pre-processed using CEM. Table 2.A8 reveals that even when the sample is trimmed to reduce imbalances in covariates, both men and women from divorced families remain more frequently childless, have lower number of children, postpone first marriage, and become parents earlier than men and women from intact, married families.

Finally, we also check whether our results are robust to controlling for genetic factors by including cousins fixed effects (see Table 2.A10 in the online supplement), and find consistent findings as the ones reported in the main analysis in Table 2.2.

6.3 Mechanisms

6.3.1 Heterogeneous analyses

By age at parental divorce: divorce-adjustment stress hypothesis. So far, we have shown the average association between parental divorce and adult children's adult fertility and marital outcomes. As predicted by the divorce-adjustment stress hypothesis, however, parental divorce may be more or less detrimental depending on the age of the child at the time of divorce, and children attending secondary school at the time of divorce should be less affected by their parents' divorce than children attending preschool or primary school. To test this hypothesis, we redefine the key variable of interest and examine differences by adult child age at the time of the divorce. We recode our key variable of interest as 0 if adult children are from intact married families. If adult children are from divorced families, we further distinguish three categories of children's ages at the time of divorce: 0-4, 5-12, and 13-17 years old. These stages correspond to the preschool, primary school, and secondary

school years. We show in Figure 2.1 (panel a) the estimated coefficients when the full set of covariates is controlled for.

Overall, our findings reveal mixed support for the divorce-adjustment stress hypothesis. On the one hand, the hypothesis is weakly supported in the sample of men. In the regressions on age at first child and childlessness, indeed, parental divorce appears to have a significantly larger influence–in absolute values–if it occurs while adult children are in preschool (age group 0-4) than in secondary school (age group 13-17).[31] On the other hand, the hypothesis is mostly not supported in the sample of women. Except for age at first child, for which results are consistent with those obtained in the sample of men, regressions on women's age at first marriage and number of children reveal that parental divorce has a smaller influence–in absolute values–if it occurs while adult children are in preschool (age group 0-4) than in secondary school (age group 13-17).[32] Thus, because of the heterogeneity found in the age patterns shown, we do not conclude that child age at the time of the divorce is a meaningful moderator of the relationship between parental divorce and adult marital and fertility outcomes in the direction predicted by the divorce-adjustment stress hypothesis.

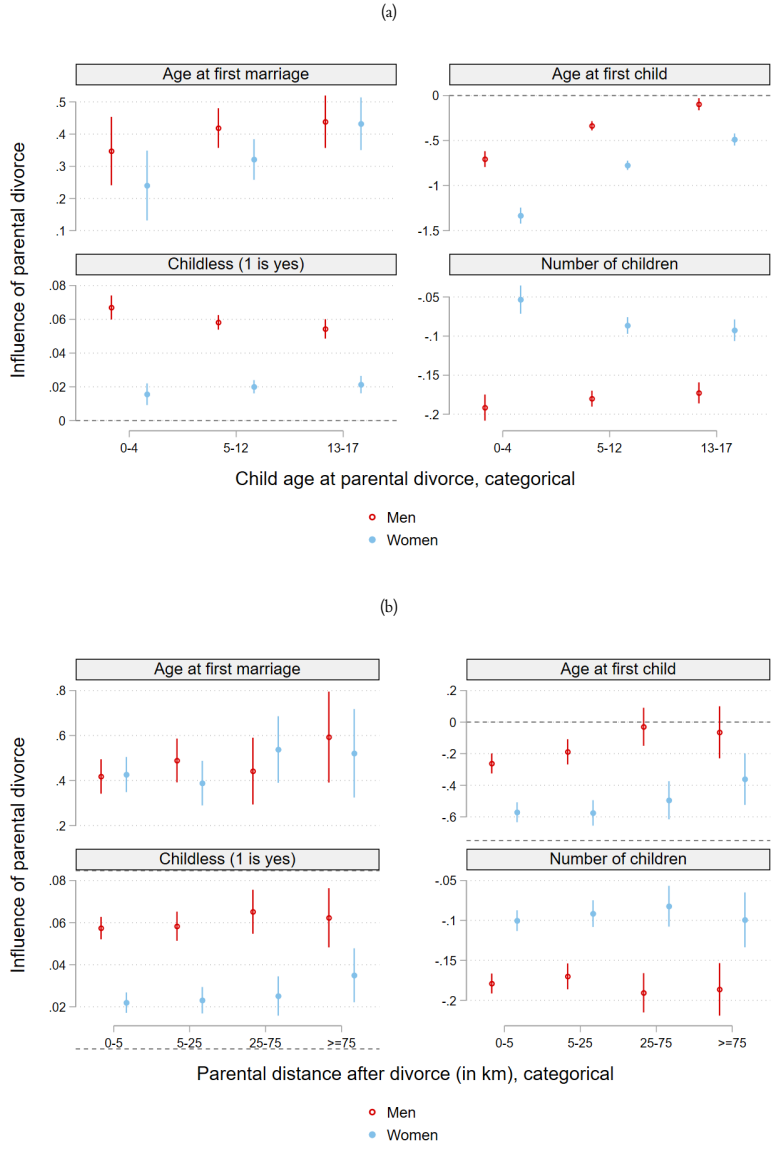
By levels of parental distance: parental distance hypothesis. Parental divorce may also influence adult children's outcomes differently depending on the level of involvement of the noncustodial parent after the divorce. According to the parental distance hypothesis, adult children whose parents lived farther away from each other after the divorce should be affected to a larger extent by parental divorce than adult children whose parents lived closer to each other after the divorce, presumably because they could enjoy a greater degree of involvement of the noncustodial parent. We investigate this aspect further by looking at how parental divorce shapes fertility and marital outcomes at different levels of parental distance after the divorce. Similar to the heterogeneous analysis by age, we recode our key variable of interest into a categorical variable as explained earlier in Section 4. We show our results in Panel (b) of Figure 2.1.

Overall, our analyses do not reveal a strong role played by the parental distance hypothesis. When marital timing, childlessness, and number of children are considered, the estimated coefficients are similar across distance categories and virtually none of them are statistically different from each other by means of a test of equality of coefficients. Some evidence of heterogeneities across distance categories is detected in the regression of age at first child. Perhaps surprisingly, parental divorce appears to have a larger negative influence on the timing of the first child if parents reside closer to each other after the divorce. However, these differences are not meaningful in terms of the coefficients' magnitude. Thus, our findings do not provide evidence to support the parental distance hypothesis.

[31] The estimated coefficients are not significantly different from each other in the regressions of age at first marriage and number of children.

[32] The estimated coefficients are not significantly different from each other in the regressions of childlessness.

Figure 2.1: The influence of parental divorce, by child age at divorce (panel a) and distance between parents after divorce (panel b)



Notes: Control variables include ethnicity, age of mother at birth, family size, birth year FE, and neighbourhood (age 11) FE.

By birth year cohorts: trend hypothesis. According to the trend hypothesis, several mechanisms may have led the influence of parental divorce on fertility and marital outcomes to increase or decrease over time. We examine this aspect by investigating differences by birth cohorts. We construct three categories for adult children's birth years: 1970–1972; 1973–1976; 1977–1980. We show our results in Table 2.A3 in the online supplement. Our findings partly support the existence of a decreasing trend concerning the influence of parental divorce on fertility outcomes. Women from divorced families born in 1977–1980 are found to have 0.050 more children than their comparison group in 1970–1973 (corresponding to an increase of 42.37 percent), and are less likely to remain childless by 0.8 percentage points. These differences are statistically significant by means of a *t*-test of equality of coefficients. Similarly, men from divorced families born in 1977–1980 are found to have 0.044 more children than their comparison group in 1970–1973 (corresponding to an increase of 26.35 percent) and are less likely to remain childless by 0.7 percentage points, albeit the latter difference is not statistically significant by means of a *t*-test of equality of coefficients. Overall, these results suggest that mothers' economic empowerment, higher divorce rates, and increased attention paid to children after divorce may all have played a role in decreasing the influence of parental divorce on especially individuals' fertility outcomes.

6.3.2 Union duration hypothesis

A possible explanation for the difference in fertility outcomes found in Table 2.2 is provided by the union duration hypothesis. This theory posits that men and (especially) women from divorced families are less likely to start a family or to have higher-order births because their cohabitation or marriage union will on average be more unstable and last less than the union of men and women from intact, married families. To test this hypothesis, we follow the mediation strategy developed by Baron and Kenny (1986) in testing the role of marriage duration as a mediator of the relationship between having experienced parental divorce in childhood and fertility outcomes in adulthood. Note that, to test this theory, we focus on the sample of men and women who have been married at least once. However, our results are robust to extending the analysis to individuals who have not married and controlling for the total number of years spent in a cohabiting relationship from age 18 up to age 40 (see Table 2.A6 in the online supplement). We show our results in Table 2.4 below.

To establish mediation, three conditions must be fulfilled in the estimation of three regression models. First, the key variable of interest (in our case, parental divorce) should significantly correlate with the dependent variables in the regressions without mediator. We have shown this condition to be satisfied in columns 4 and 8 of Table 2.2, and we find similar results (albeit smaller in absolute value) when looking at the sample of married individuals. Second, the key variable of interest should significantly correlate with the mediator in the regression of the mediator on the key variable of interest. Our findings show this condition to be fulfilled given that men and women from divorced families are found to be

Table 2.4: Children from divorced families, union duration hypothesis, and fertility outcomes

Women	Marriage duration		Childlessness			Number of children		
	(1)	(2)	(3)	(4)	Indirect influence (5)	(6)	(7)	Indirect influence (8)
Parental divorce (1 is yes)	-1.342***	-1.070***	0.018***	0.005***	0.0137***	-0.074***	-0.004	-0.0705***
	(0.023)	(0.024)	(0.001)	(0.001)	[0.0131, 0.0143]	(0.005)	(0.005)	[-0.0736, -0.0673]
Marriage duration				-0.013***			0.066***	
				(0.000)			(0.000)	
Family background	No	Yes	Yes	Yes		Yes	Yes	
Neighbourhood (age 11) FE	No	Yes	Yes	Yes		Yes	Yes	
Ethnicity, birth year FE	Yes	Yes	Yes	Yes		Yes	Yes	
Observations	576,172	576,172	576,172	576,172	576,172	576,172	576,172	576,172
Children from DF	53,646	53,646	53,646	53,646	53,646	53,646	53,646	53,646
R-squared	0.0343	0.120	0.0259	0.0687		0.101	0.186	

Men	Marriage duration		Childlessness			Number of children		
	(1)	(2)	(3)	(4)	Indirect influence (5)	(6)	(7)	Indirect influence (8)
Parental divorce (1 is yes)	-1.044***	-0.819***	0.026***	0.013***	0.0138***	-0.121***	-0.053***	-0.0671***
	(0.022)	(0.023)	(0.002)	(0.002)	[0.0130, 0.0145]	(0.005)	(0.005)	[-0.0705, -0.0634]
Marriage duration				-0.017***			0.082***	
				(0.000)			(0.000)	
Family background	No	Yes	Yes	Yes		Yes	Yes	
Neighbourhood (age 11) FE	No	Yes	Yes	Yes		Yes	Yes	
Ethnicity, birth year FE	Yes	Yes	Yes	Yes		Yes	Yes	
Observations	534,879	534,879	534,879	534,879	534,879	534,879	534,879	534,879
Children from DF	47,238	47,238	47,238	47,238	47,238	47,238	47,238	47,238
R-squared	0.0286	0.110	0.0271	0.0806		0.0895	0.202	

Notes. This table shows the mediation model estimated to test the union duration hypothesis using marriage duration as a mediator. Marriage duration is computed as the total number of years the focal child has been married or in a registered partnership until age 40. Columns 1 and 2 show the estimated association between parental divorce and marriage duration; columns 4 to 5 and 6 to 7 show the estimated association between parental divorce and fertility outcomes when controlling for marriage duration. Columns 5 and 8 report the indirect association (the influence of parental divorce that works through marriage duration), with 95% percentile bootstrap confidence intervals reported in square brackets (R=1000). Family background includes age of mother at birth and family size. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

married for about one year less than men and women from intact, married families. Finally, the estimated association between the key variable of interest and the dependent variable should be insignificant (*complete mediation*) or lower (*partial mediation*) in the regressions with the mediator than in the regression without the mediator. When marriage duration is controlled for, we no longer find a meaningfully significant association between parental divorce and fertility outcomes in the case of women. In the case of men, this association is reduced by roughly half in both the regressions of childlessness and number of children. Therefore, although the union duration hypothesis appears to have a slightly stronger role for women than for men from divorced families, both groups are significantly influenced by parental divorce according to the mechanism posited by the theory. We formally test the significance of the indirect effect (the influence that parental divorce exerts through marriage duration) using the bootstrap method suggested by Preacher and Hayes (2004). Our results (shown in columns 5 and 8) confirm a significant mediating role played by marriage duration for both men and women from divorced families.

It might be argued that men and women from divorced families have shorter marriage durations also because they postpone marriage, which in turn exposes them to fewer fertile years than their comparison groups from intact, married families (and thus to smaller families). However, we exclude this alternative explanation. First, we do not find that married men and women from divorced families postpone entry into parenthood (see Table 2.B2 in the online supplement), which suggests that the decision to become parents does not depend on their marital status. Second, our results are robust to testing the union duration hypothesis on cohabitation agreements (see Table 2.A6 in the online supplement), which are generally found to be started by adult children from divorced families at a younger age than by adult children from intact, married families (see Table 2.A5 in the online supplement).

More generally, we also exclude that men and women from divorced families may have lower fertility because they are less likely to find a partner during lifetime. In Table 2.A9 in the online supplement, we look at the association between parental divorce and partnership status, that is, the likelihood of having at least one (cohabiting or marriage) partner before the age of 40. Our results do not suggest that men and women from divorced families are more likely to remain single for lifetime. On the contrary, men and women from divorced families even seem to be in a cohabiting or married relationship more frequently than their comparison groups from divorced families (although the associations are small in magnitude and mostly not meaningful). Taken together, all our analyses thus support the union duration hypothesis as a possible mechanism behind the fertility outcomes of adult children from divorced families.

7 Discussion

This chapter offered first evidence on population data of childlessness and completed fertility of adult children who experienced parental divorce during childhood. Regarding fertility outcomes, previous literature either compared adult children from divorced families to children from intact, married families on early childbearing likelihoods (e.g. Ermisch and Francesconi, 2001; Ermisch et al., 2004), or looked at other fertility outcomes (e.g. number of children in Rijken and Liefbroer, 2009) using surveys, which are prone to nonresponse errors that might lead to incorrect inference (Mitchell, 2010). Using a longitudinal administrative dataset from the Netherlands, we reconstructed the family, marital, and fertility history of the population of men and women born between 1970 and 1980, that we followed from birth until 2020. As such, we compared adult children from divorced families to adult children from intact married families on a range of fertility and marital outcomes measured at completed fertility (namely age at first child, childlessness, number of children, and age at first marriage).

Our results showed that parental divorce significantly influences offspring's marital and

fertility outcomes. Adult children from divorced families are found to postpone marriage relative to adult children from intact, married families, although this association corresponds to a marriage postponement of about four months. Women (and to a lesser extent men) from divorced families are also found to enter into parenthood earlier than their comparison group from intact families. Nonetheless, adult children from divorced families are also importantly shown to have lower fertility relative to adult children from intact, married families. First, they remain childless more frequently. In particular, men from divorced families are about 5.9 percentage points more likely to remain without children than men from intact, married families. Second, adult children from divorced families have lower completed fertility than children from intact, married families. This decline in completed fertility amounts to 0.08 less children in the case of women and 0.18 less children in the case of men (corresponding to a decrease of about 5 and 13 percent respectively). Regarding the decline in fertility, no clear pattern is detected by adult children's age at divorce, suggesting that the latter does not play an univocal role in determining adult children's fertility declines.

Regarding the mechanisms that might explain our findings, our results mostly corroborate the hypothesis that, by being more likely to separate from their partners, adult children from divorced families are not in a stable relationship for long enough to start a family or expand their existing family with higher-order births. Hence, they remain childless more often and have a lower number of children. We call this mechanism the union duration hypothesis and we find it to hold true for both men and women from divorced families. Given that we do not find a positive association between parental divorce and fertility outcomes, our results mostly exclude the economic disruption hypothesis, the fertility and human capital hypothesis, and the risk behavior hypothesis as possible mechanisms. Besides, additional analyses (reported in Table 2.A4 in the online supplement) show that parental divorce is still influential on adult children's fertility and marital outcomes even if it occurs in young adulthood, thus contradicting the hypotheses posited by both the economic disruption theory and the theory of fertility and human capital.

From a policy perspective, this chapter suggests that parental divorce is an additional factor to consider when investigating the decline in fertility rates and possibly when designing policies to stimulate birth rates. In particular, our results provide evidence that the negative influence of parental divorce on fertility outcomes has somewhat diminished over time. This "counter-influence" amounts to about 0.05 more children had by women from divorced families born in late birth cohorts compared to women from divorced families born in early birth cohorts, and 0.04 more children had by men born in late birth cohorts compared to men born in early birth cohorts. This finding might be suggestive that, over time, mothers increasing labor market participation, less stringent divorce conditions, and a greater attention paid by parents to children's wellbeing after the divorce might have been relevant factors in helping offsetting the negative consequences of parental divorce on adult

fertility. Regarding the level of parental attention, however, we should note that additional analyses did not reveal a possible role played by the degree of parental involvement after divorce in moderating the negative associations between parental divorce and adult fertility. Nonetheless, taken together, our results might still be suggestive that policies providing more child support after divorce or stimulating mothers economic empowerment might help offset the decline in fertility experienced by adult children from divorced families relative to adult children from intact, married families.

Although we attempted to resolve the main issues related to estimating the relationship between parental divorce and children's outcomes, this study is not without limitations. First, our findings cannot be given a causal interpretation. Nonetheless, in our analysis we employed a number of methodologies with which we attempted to obtain an unbiased estimate of the influence of parental divorce. These methods included the comparison of adult children from divorced families with adult children from bereaved families, the development of a bounding estimator as illustrated by Oster (2019), and the pre-processing of data with Coarsened Exact Matching. The consistency of findings across model specifications give us more confidence that, if anything, a causal relationship between parental divorce and fertility outcomes would mostly be negative. Second, we only focused on parental divorce and did not consider parental separation. Similar and additional mechanisms may apply when family disruption is defined to include also the dissolution of cohabitation agreements. Third, our quantitative research does not allow to test all theories predicting changes in children's fertility because of changes in children's beliefs about marriage and family. Further quantitative and qualitative research should complement our findings. As divorce rates are still high in most industrialized countries, the strong negative correlation found may have important policy implications as it may further enforce the decline in fertility rates already experienced in virtually all developed countries.

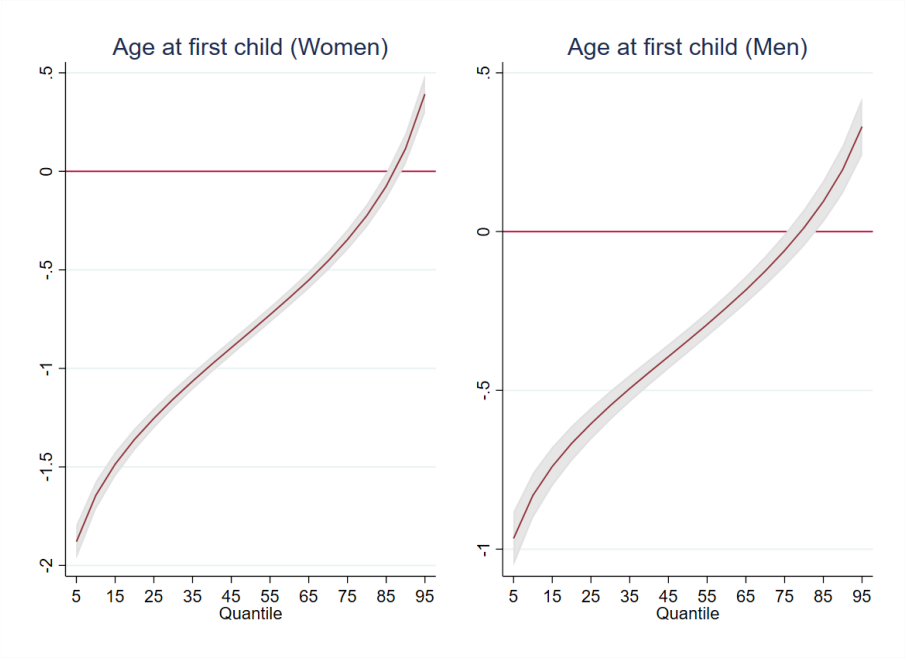
Appendix A: Supplementary Tables and Figures

Table 2.A1: Parental divorce and offspring's fertility and marital outcomes, controlling for the influence of parental distance at different ages at the time of divorce

	Women				Men			
	Age at first marriage (1)	Childlessness (2)	Number of children (3)	Age at first child (4)	Age at first marriage (5)	Childlessness (6)	Number of children (7)	Age at first child (8)
Parental divorce (1 is yes)	0.310***	0.019***	−0.089***	−0.838***	0.375***	0.080***	−0.190***	−0.637***
	(0.092)	(0.006)	(0.015)	(0.074)	(0.091)	(0.006)	(0.015)	(0.075)
Parental distance (in km)	0.005*	0.000*	−0.001	0.002	0.001	−0.000	0.000	0.006**
	(0.002)	(0.000)	(0.000)	(0.002)	(0.002)	(0.000)	(0.000)	(0.002)
Child age at divorce	0.010	0.000	−0.001	0.024***	0.005	−0.001*	0.001	0.037***
	(0.008)	(0.000)	(0.001)	(0.006)	(0.008)	(0.001)	(0.001)	(0.006)
Parental distance × child age at divorce	−0.000	−0.000	0.000	−0.000	0.000	0.000	−0.000	−0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	558,181	844,476	844,476	662,710	518,874	904,218	904,218	613,199
Children from DF	35,655	63,490	63,490	48,257	31,233	67,579	67,579	41,461
R-squared	0.142	0.029	0.074	0.120	0.121	0.010	0.062	0.089

Notes. This table shows the estimated association between having experienced parental divorce in childhood and fertility and marital outcomes in adulthood, when the regression models additionally control for child age at divorce and the interaction between child age at divorce and parental distance. DF indicates divorced families. Additional controls include age of mother at birth, family size, ethnicity, indicators for year of birth, indicators for child neighbourhood (age 11). Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 2.A1: The influence of parental divorce by quantiles of age at first child



Notes: The figure shows the estimated quantiles of the influence of parental divorce on age at first child. Control variables include ethnicity, age of mother at birth, family size, birth year FE, and neighbourhood (age 11) FE.

Table 2.A2: Fertility and marital outcomes of children from divorced and bereaved families

Women	Age at first marriage		Childlessness		Number of children		Age at first child	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Family disruption (ref: intact, married family)								
Parental divorce (1 is yes)	0.466***	0.626***	0.027***	0.027***	−0.132***	−0.097***	−0.571***	−0.109
	(0.075)	(0.073)	(0.005)	(0.005)	(0.013)	(0.012)	(0.062)	(0.056)
Parental death (1 is yes)	−0.321	−0.500**	0.036**	0.027*	−0.054	−0.036	−0.585***	−0.730***
	(0.177)	(0.178)	(0.012)	(0.012)	(0.035)	(0.036)	(0.151)	(0.154)
Family background	No	Yes	No	Yes	No	Yes	No	Yes
SES status (age 40)	No	Yes	No	Yes	No	Yes	No	Yes
Neighbourhood (age 11) FE	No	Yes	No	Yes	No	Yes	No	Yes
Ethnicity, birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value, *t*-test	**0.000**	**0.000**	0.515	0.921	**0.040**	0.127	0.638	0.082
Observations	93,226	93,226	149,261	149,261	149,261	149,261	117,782	117,782
Children from DF	2,253	2,253	4,145	4,145	4,145	4,145	3,148	3,148
Children from BF	836	836	1,337	1,337	1,337	1,337	1,004	1,004
R-squared	0.053	0.225	0.004	0.086	0.003	0.129	0.022	0.178

Men	Age at first marriage		Childlessness		Number of children		Age at first child	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Family disruption (ref: intact, married family)								
Parental divorce (1 is yes)	0.656***	0.542***	0.051***	0.050***	−0.181***	−0.142***	−0.030	−0.013
	(0.111)	(0.117)	(0.008)	(0.008)	(0.018)	(0.019)	(0.092)	(0.094)
Parental death (1 is yes)	−0.210	−0.200	0.040**	0.036**	−0.120***	−0.113***	−0.240	−0.311*
	(0.179)	(0.186)	(0.013)	(0.013)	(0.032)	(0.032)	(0.148)	(0.151)
Family background	No	Yes	No	Yes	No	Yes	No	Yes
SES status (age 40)	No	Yes	No	Yes	No	Yes	No	Yes
Neighbourhood (age 11) FE	No	Yes	No	Yes	No	Yes	No	Yes
Ethnicity, birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value, *t*-test	**0.000**	**0.000**	0.429	0.371	0.092	0.433	0.226	0.093
Observations	85,629	85,629	156,321	156,321	156,321	156,321	106,346	106,346
Children from DF	1,887	1,887	4,108	4,108	4,108	4,108	2,572	2,572
Children from BF	731	731	1,445	1,445	1,445	1,445	925	925
R-squared	0.033	0.207	0.004	0.081	0.003	0.114	0.011	0.154

Notes. This table compares children who experienced parental loss by divorce in childhood to children who experienced parental loss by death in childhood on their adult fertility and marital outcomes. For each regression we report the *p*-value resulting from testing the equality of coefficients between the influence of parental divorce and the effect of parental death. Family background include age of mother and family size. DF indicates divorced families; BF indicates bereaved families. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A3: The influence of parental divorce, by birth year cohorts

	Women			Men		
	1970–1973 (1)	1974–1976 (2)	1977–1980 (3)	1970–1973 (4)	1974–1976 (5)	1977–1980 (6)
Panel A. Age at first marriage						
Parental divorce (1 is yes)	0.390***	0.327***	0.326***	0.542***	0.344***	0.410***
	(0.051)	(0.044)	(0.036)	(0.046)	(0.045)	(0.037)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	207,834	162,219	206,119	200,539	146,647	187,693
R-squared	0.150	0.181	0.172	0.138	0.163	0.154
Panel B. Childlessness						
Parental divorce (1 is yes)	0.026***	0.019***	0.018***	0.065***	0.053***	0.058***
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	296,630	247,316	331,984	330,199	261,039	347,652
R-squared	0.0515	0.0579	0.0499	0.0472	0.0550	0.0475
Panel C. Number of children						
Parental divorce (1 is yes)	−0.118***	−0.082***	−0.068***	−0.211***	−0.172***	−0.167***
	(0.009)	(0.008)	(0.006)	(0.008)	(0.007)	(0.006)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	296,630	247,316	331,984	330,199	261,039	347,652
R-squared	0.0960	0.105	0.0911	0.0823	0.0914	0.0795
Panel D. Age at first child						
Parental divorce (1 is yes)	−0.749***	−0.739***	−0.808***	−0.200***	−0.391***	−0.342***
	(0.043)	(0.036)	(0.029)	(0.039)	(0.037)	(0.030)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	231,843	194,085	261,209	224,467	176,254	233,921
R-squared	0.148	0.160	0.145	0.121	0.127	0.113

Notes: The table shows the estimated association between having experienced parental divorce in childhood and marital and fertility outcomes in adulthood, by birth year cohorts. Additional controls include ethnicity, age of mother, family size, birth year FE, and neighbourhood (at age 11) FE. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A4: Parental divorce and offspring's fertility and marital outcomes, for extended child age categories

	Women				Men			
	Age at first marriage (1)	Childless-ness (2)	Number of children (3)	Age at first child (4)	Age at first marriage (5)	Childless-ness (6)	Number of children (7)	Age at first child (8)
Parental divorce (ref: intact, married family)								
child age 0–17	0.416***	0.024***	−0.098***	−0.719***	0.476***	0.064***	−0.198***	−0.275***
	(0.024)	(0.002)	(0.004)	(0.020)	(0.024)	(0.002)	(0.004)	(0.020)
child age 18–24	0.789***	0.044***	−0.169***	0.380***	0.691***	0.066***	−0.204***	0.294***
	(0.037)	(0.003)	(0.007)	(0.029)	(0.037)	(0.003)	(0.006)	(0.030)
child age ≥ 25	2.069***	0.067***	−0.309***	2.627***	1.463***	0.057***	−0.250***	1.881***
	(0.055)	(0.004)	(0.009)	(0.038)	(0.048)	(0.004)	(0.008)	(0.035)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	576,172	875,930	875,930	687,137	534,879	938,890	938,890	634,642
Children from DF	78,877	139,206	139,206	105,394	71,928	152,882	152,882	94,422
R-squared	0.142	0.029	0.074	0.128	0.121	0.028	0.063	0.092

Notes. This table shows the estimated association between parental divorce and men and women's fertility and marital outcomes, when children from intact married families are compared to children from divorced families experiencing parental divorce at ages younger than 18 years old, between 18 and 24 years old, and older than 25 years old. Note that children are defined to belong to a divorced families if parental divorce occurred before children had their first child. If children remained childless, they are considered from divorced families if parental divorce occurred when they were under age 30 (corresponding to the average age at which the individuals in our sample become parent). Children whose parents never divorced or whose parents divorced after children became parents (or divorced after children's 30th birthday when childless), are defined as having lived in an intact, married family. DF indicates divorced families. Additional controls include ethnicity, age of mother, family size, birth year FE, and neighbourhood (at age 11) FE. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A5: Parental divorce and offspring's teenage parenthood and cohabitation timing

	Women				Men			
	Teenage parenthood		Age at cohabitation		Teenage parenthood		Age at cohabitation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parental divorce (1 is yes)	0.033***	0.028***	-0.822***	-0.706***	0.005***	0.004***	-0.932***	-0.760***
	(0.001)	(0.001)	(0.014)	(0.015)	(0.000)	(0.000)	(0.016)	(0.017)
Family background	No	Yes	No	Yes	No	Yes	No	Yes
Neighbourhood (age 11) FE	No	Yes	No	Yes	No	Yes	No	Yes
Ethnicity, birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	687,137	687,137	826,302	826,302	634,642	634,642	844,152	844,152
Children from DF	72,684	72,684	90,197	90,197	62,904	62,904	92,532	92,532
R-squared	0.020	0.049	0.039	0.072	0.004	0.023	0.018	0.049

Notes. This table shows the estimated association between parental divorce and men and women's likelihood to become parents before age 20 (teenage parenthood) and age at first cohabitation. DF indicates divorced families. Family background includes age of mother at birth and family size. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A6: Children from divorced families, union duration hypothesis on cohabitation agreements, and fertility outcomes

Women	Cohabitation duration		Childlessness			Number of children		
	(1)	(2)	(3)	(4)	Indirect influence (5)	(6)	(7)	Indirect influence (8)
Parental divorce (1 is yes)	-1.118***	-1.041***	0.035***	0.005**	0.0291***	-0.106***	-0.016**	-0.0893***
	(0.030)	(0.032)	(0.002)	(0.002)	[0.0276, 0.0308]	(0.006)	(0.006)	[-0.0946, -0.0843]
Cohabitation duration				-0.028***			0.086***	
				(0.000)			(0.000)	
Family background	No	Yes	Yes	Yes		Yes	Yes	
Neighbourhood (age 11) FE	No	Yes	Yes	Yes		Yes	Yes	
Ethnicity, birth year FE	Yes	Yes	Yes	Yes		Yes	Yes	
Observations	310,960	310,960	310,960	310,960	310,960	310,960	310,960	310,960
Children from DF	43,356	43,356	43,356	43,356	43,356	43,356	43,356	43,356
R-squared	0.020	0.078	0.048	0.202		0.102	0.260	

Men	Cohabitation duration		Childlessness			Number of children		
	(1)	(2)	(3)	(4)	Indirect influence (5)	(6)	(7)	Indirect influence (8)
Parental divorce (1 is yes)	-1.026***	-0.939***	0.077***	0.036***	0.0407***	-0.211***	-0.100***	-0.1106***
	(0.029)	(0.030)	(0.003)	(0.002)	[0.0382, 0.0430]	(0.006)	(0.005)	[-0.1176, -0.1040]
Cohabitation duration				-0.043***			0.118***	
				(0.000)			(0.000)	
Family background	No	Yes	Yes	Yes		Yes	Yes	
Neighbourhood (age 11) FE	No	Yes	Yes	Yes		Yes	Yes	
Ethnicity, birth year FE	Yes	Yes	Yes	Yes		Yes	Yes	
Observations	308,328	308,328	308,328	308,328	308,328	308,328	308,328	308,328
Children from DF	42,349	42,349	42,349	42,349	42,349	42,349	42,349	42,349
R-squared	0.019	0.068	0.049	0.310		0.090	0.354	

Notes. This table shows the mediation model estimated to test the union duration hypothesis using cohabitation duration as a mediator. Cohabitation duration is computed as the total number of years the focal child has been in a cohabiting relationship from age 18 until age 40. Partners are identified as cohabiting if they have filed taxes jointly or have signed a written cohabitation contract. Since data on cohabitation agreements start in 1995, we perform this analysis only on the subpopulation of children born between 1977 and 1980, so that we can observe the cohabitation history of all individuals from the age of 18. Table 2.A6 confirms the significant role played by cohabitation duration in mediating the relationship between parental divorce and fertility outcomes. Columns 1 and 2 show the the estimated association between parental divorce and cohabitation duration, revealing that children who experienced parental divorce have shorter cohabiting relationships. Columns 4 to 5 and 6 to 7 show the estimated association between parental divorce and fertility outcomes when controlling for cohabitation duration. These regressions reveal that once cohabitation duration is included as an additional control variable, the association between parental divorce and fertility outcomes is greatly reduced in magnitude in absolute values. Finally, Columns 5 and 8 report the indirect association (the influence of parental divorce that works through cohabitation duration), with 95% percentile bootstrap confidence intervals reported in square brackets (R=1000), revealing the significant role played by cohabiting duration in mediating the relationship between parental divorce and fertility outcomes. Family background includes age of mother at birth and family size. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A7: Parental divorce and offspring's fertility and marital outcomes observed at age 45

Women	Age at first marriage		Childlessness		Number of children		Age at first child	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parental divorce (1 is yes)	0.698***	0.565***	0.023***	0.024***	−0.151***	−0.101***	−1.063***	−0.739***
	(0.041)	(0.042)	(0.002)	(0.002)	(0.006)	(0.006)	(0.032)	(0.032)
Family background	No	Yes	No	Yes	No	Yes	No	Yes
Neighbourhood (age 11) FE	No	Yes	No	Yes	No	Yes	No	Yes
Ethnicity, birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	328,105	328,105	461,422	461,422	461,422	461,422	365,437	365,437
Children from DF	24,310	24,310	38,766	38,766	38,766	38,766	29,840	29,840
R-squared	0.034	0.129	0.001	0.039	0.005	0.087	0.027	0.130
Men	Age at first marriage		Childlessness		Number of children		Age at first child	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parental divorce (1 is yes)	0.809***	0.663***	0.064***	0.058***	−0.254***	−0.195***	−0.433***	−0.216***
	(0.039)	(0.040)	(0.002)	(0.002)	(0.006)	(0.006)	(0.032)	(0.032)
Family background	No	Yes	No	Yes	No	Yes	No	Yes
Neighbourhood (age 11) FE	No	Yes	No	Yes	No	Yes	No	Yes
Ethnicity, birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	318,553	318,553	504,125	504,125	504,125	504,125	357,530	357,530
Children from DF	23,378	23,378	44,064	44,064	44,064	44,064	28,589	28,589
R-squared	0.028	0.114	0.002	0.035	0.008	0.075	0.016	0.094

Notes. This table shows the estimated association between having experienced parental divorce in childhood (before age 18) and fertility and marital outcomes in adulthood, when fertility and marital outcomes are measured at age 45. DF indicates divorced families. Family background includes age of mother at birth and family size. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A8: Parental divorce and offspring's fertility and marital outcomes using Coarsened Exact Matching (CEM)

Women	Age at first marriage		Childlessness		Number of children		Age at first child	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parental divorce (1 is yes)	0.555***	0.408***	0.021***	0.017***	−0.108***	−0.077***	−0.858***	−0.680***
	(0.025)	(0.025)	(0.002)	(0.002)	(0.004)	(0.004)	(0.021)	(0.021)
Additional controls[a]	No	Yes	No	Yes	No	Yes	No	Yes
Observations	557,571	557,571	851,495	851,495	851,495	851,495	667,723	667,723
Children from DF	53,524	53,524	94,735	94,735	94,735	94,735	72,516	72,516
Men	**Age at first marriage**		**Childlessness**		**Number of children**		**Age at first child**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parental divorce (1 is yes)	0.610***	0.473***	0.064***	0.055***	−0.213***	−0.173***	−0.381***	−0.247***
	(0.024)	(0.024)	(0.002)	(0.002)	(0.004)	(0.004)	(0.020)	(0.020)
Additional controls[a]	No	Yes	No	Yes	No	Yes	No	Yes
Observations	519,092	519,092	915,613	915,613	915,613	915,613	618,079	618,079
Children from DF	47,147	47,147	102,034	102,034	102,034	102,034	62,767	62,767

Notes. This table shows the estimated association between having experienced parental divorce in childhood (before age 18) and fertility and marital outcomes in adulthood, with data processed with CEM. DF indicates divorced families. Robust standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
[a] Additional controls include age of mother at birth, family size, neighbourhood (age 11) FE, birth year FE, and ethnicity.

Table 2.A9: Parental divorce and adult children's partnership status

	Women		Men	
	(1)	(2)	(3)	(4)
Parental divorce (1 is yes)	0.012***	0.012***	0.011***	0.009***
	(0.001)	(0.001)	(0.001)	(0.001)
Family background	No	Yes	No	Yes
Neighbourhood (age 11) FE	No	Yes	No	Yes
Ethnicity, birth year FE	Yes	Yes	Yes	Yes
Observations	875,930	875,930	938,890	938,890
Children from DF	94,944	94,944	102,251	102,251
R-squared	0.008	0.031	0.003	0.024

Notes. This table shows the estimated association between having experienced parental divorce in childhood (before age 18) and partnership status. Partnership status is a binary variable coded 1 if the adult child has been in a cohabiting relationship or has been married before age 40, and 0 otherwise. DF indicates divorced families. Family background includes age of mother at birth and family size. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 2.A10: Parental divorce, offspring's fertility and marital outcomes, and cousin fixed effects

Maternal cousins	Age at first marriage		Childlessness		Number of children		Age at first child	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parental divorce (1 is yes)	0.287***	0.294***	0.025***	0.025***	−0.059***	−0.066***	−0.512***	−0.485***
	(0.081)	(0.081)	(0.005)	(0.005)	(0.013)	(0.013)	(0.063)	(0.063)
Family background	No	Yes	No	Yes	No	Yes	No	Yes
Ethnicity, gender, birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	168,962	168,962	335,953	335,953	335,953	335,953	221,101	221,101
Children from DF	14,757	14,757	39,074	39,074	39,074	39,074	23,446	23,446
R-squared	0.553	0.554	0.436	0.436	0.463	0.464	0.538	0.539
Paternal cousins	Age at first marriage		Childlessness		Number of children		Age at first child	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parental divorce (1 is yes)	0.196*	0.226**	0.024***	0.024***	−0.049***	−0.062***	−0.465***	−0.423***
	(0.086)	(0.086)	(0.005)	(0.005)	(0.013)	(0.013)	(0.066)	(0.066)
Family background	No	Yes	No	Yes	No	Yes	No	Yes
Ethnicity, gender, birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	157,638	157,638	311,598	311,598	311,598	311,598	206,212	206,212
Children from DF	12,738	12,738	33,699	33,699	33,699	33,699	20,399	20,399
R-squared	0.552	0.553	0.429	0.429	0.457	0.460	0.533	0.535

Notes. This table shows the estimated association between having experienced parental divorce in childhood (before age 18) and fertility and marital outcomes in adulthood, when controlling for cousin fixed effects. To avoid identification on only few observations, we pool men and women and do not condition additionally on neighborhood fixed effects. DF indicates divorced families. Family background includes age of mother at birth and family size. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.A11: Parental divorce and offspring's fertility and marital outcomes, with different levels of controls for SES status

Women	Age at first marriage			Childlessness			Number of children			Age at first child		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Parental divorce (1 is yes)	0.363***	0.540***	0.403***	0.018***	0.020***	0.023***	−0.081***	−0.077***	−0.116***	−0.757***	−0.453***	−0.982***
	(0.024)	(0.027)	(0.021)	(0.002)	(0.002)	(0.001)	(0.004)	(0.005)	(0.004)	(0.020)	(0.021)	(0.017)
Neighborhood FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Higher education	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No
Log net income	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Ethnicity, birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	576,151	416,567	647,775	875,904	645,178	978,393	875,904	645,178	978,393	687,113	509,162	768,721
Children from DF	53,644	41,173	69,329	94,942	73,913	119,745	94,942	73,913	119,745	72,682	57,381	92,006
R-squared	0.147	0.190	0.089	0.039	0.033	0.010	0.074	0.065	0.047	0.139	0.240	0.078
Men	**Age at first marriage**			**Childlessness**			**Number of children**			**Age at first child**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Parental divorce (1 is yes)	0.429***	0.546***	0.428***	0.047***	0.036***	0.068***	−0.153***	−0.123***	−0.223***	−0.301***	−0.117***	−0.544***
	(0.024)	(0.028)	(0.021)	(0.002)	(0.002)	(0.001)	(0.004)	(0.005)	(0.004)	(0.020)	(0.023)	(0.018)
Neighborhood FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Higher education	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No
Log net income	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Ethnicity, birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Family controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	534,848	377,914	587,397	938,843	670,467	1,026,759	938,843	670,467	1,026,759	634,612	453,417	694,391
Children from DF	47,235	34,161	58,220	102,247	75,086	123,956	102,247	75,086	123,956	62,903	46,262	76,493
R-squared	0.121	0.133	0.071	0.097	0.088	0.010	0.114	0.107	0.035	0.092	0.131	0.048

Notes. This table shows the estimated association between having experienced parental divorce in childhood (before age 18) and fertility and marital outcomes in adulthood, when controls for adult children's income level, educational attainment, and neighbourhood are gradually included. Family background includes age of mother at birth and family size. Higher education is a categorical variable indicating the highest level of education achieved by the individual – 0 for dropping out of school without a secondary education diploma; 1 for a secondary education diploma; 2 for a bachelor's degree; and 3 for a master's degree or Ph.D. Log net income accounts for individuals' net annual income at age 40. It is constructed by, first, calculating the individual's gross annual income at age 40. Next, the total amount of taxes paid by each individual in the reference year is collected and subtracted to individuals' gross annual income. As last step, logarithms of this quantity are taken to smooth the extremes of the distribution. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix B: Couple-level analysis

B.1 Sample construction

So far, we obtained the main findings of this study from an individual-level analysis. However, the fertility outcomes of adult men and women likely depend on the partner's background characteristics, and may even reflect whether the partner has lived in an intact family during childhood or has experienced divorce. We account for partners' characteristics using couple-level data. To reconstruct couples, first, we link every adult child born in 1970–1980 to their spouse (or spouses, if the adult child married more than once). Then, we retain couples in which both spouses are at their first marriage. This is necessary to ensure that adult children who were married multiple times will appear only once in the sample. Further, we focus on couples in which both spouses are born between 1970 and 1980, to facilitate comparison with the results obtained from our main analysis at individual level.

To account for the structure of the data at couple level, we redefine the control and outcome variables. As outcome variables, we look at the average age of spouses at marriage, the average age of spouses at the birth of their first child, whether the couple remained childless or had at least one child, and the number of children born to the couple. Note that, if the couple divorced, we measure fertility outcomes in the last available year prior to the divorce. If the couple has not divorced, we measure fertility outcomes in the year in which the wife is aged 40 years old. As key variable of interest, we construct a binary indicator coded 1 if at least one spouse experienced divorce during childhood (i.e. before age 18), 0 if both spouses lived in an intact, married family. As control variables, we include the same covariates used in the analysis at the individual level. We control for number of siblings, ethnicity, age of mother, birth year, and neighbourhood of residence (at age 11) of both wife and husband.

B.2 Results

Descriptive statistics. Table 2.B1 below shows the descriptive statistics of the couple-level sample. We identify 302,853 married couples, of which 15.51 percent are couples in which at least one spouse has experienced divorce during childhood. The descriptive statistics for the couple-level data are consistent with those shown in Table 2.1 for the individual-level data. Couples from divorced families are ethnically more diverse, have fewer siblings, and younger mother than couples from intact families. They also appear to marry at older age, to remain childless more often, to have their first child earlier, and to have a lower number of children than couples from intact families.

Fertility outcomes. Turning to the estimated model, Table 2.B2 compares couples in which

Table 2.B1: Descriptive statistics of couple-level data

	Couples from intact families (1)	Couples from divorced families (2)
Wife's ethnicity		
Both parents NL	0.852	0.769#
One parent NL, other not NL	0.108	0.162#
Both parents not NL	0.041	0.069#
Husband's ethnicity		
Both parents NL	0.853	0.782#
One parent NL, other not NL	0.108	0.154#
Both parents not NL	0.039	0.065#
Wife's family size	2.754	2.533#
Husband's family size	2.772	2.550#
Wife's age of mother	27.179	26.165#
Husband's age of mother	27.166	26.131#
Outcome variables		
Mean age of spouses at first marriage	28.821	29.446#
Childlessness	0.096	0.130#
Number of children	2.029	1.773#
Mean age of spouses at first child	30.013	29.585#
Observations	255,890 (84.49%)	46,963 (15.51%)
Total	302,853	302,853

#The mean for couples in which at least one spouse experienced divorce during childhood is significantly different at the 5 percent level from the mean for couples in which both spouses come from intact families by means of a two-tailed *z*-test.

at least one spouse experienced divorce during childhood to couples in which both spouses come from intact, married families. Our findings confirm our results from the main analysis at the individual level. When the full set of covariates is included, couples from divorced families are found to postpone marriage relative to couples from intact, married families. They are also more likely to have their first child earlier than couples from intact, married families. Finally, columns 4 and 6 reveal that couples from divorced families are more likely to remain childless and have smaller families than couples from intact, married families.

Table 2.B2: Parental divorce and offspring's fertility and marital outcomes with couple-level analysis

	Mean age at first marriage		Childlessness		Number of children		Mean age at first child	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parental divorce (1 is yes for at least one spouse)	0.532***	0.357***	0.033***	0.026***	-0.250***	-0.135***	-0.433***	-0.316***
	(0.023)	(0.024)	(0.002)	(0.002)	(0.005)	(0.005)	(0.021)	(0.022)
Wifes ethnicity (ref: both parents born in NL)								
One parent NL, other not NL	0.732***	0.301***	0.016***	0.010***	-0.138***	-0.041***	0.402***	0.115***
	(0.025)	(0.026)	(0.002)	(0.002)	(0.006)	(0.006)	(0.022)	(0.023)
Both parents not NL	-0.607***	-0.108*	0.034***	0.035***	-0.100***	-0.182***	-0.077	0.601***
	(0.050)	(0.051)	(0.003)	(0.004)	(0.011)	(0.011)	(0.047)	(0.048)
Husbands ethnicity (ref: both parents born in NL)								
One parent NL, other not NL	0.707***	0.290***	0.009***	0.003	-0.128***	-0.026***	0.390***	0.106***
	(0.025)	(0.026)	(0.002)	(0.002)	(0.006)	(0.006)	(0.022)	(0.023)
Both parents not NL	-1.004***	-0.296***	-0.003	-0.006	0.063***	-0.023*	-0.901***	-0.055
	(0.052)	(0.053)	(0.003)	(0.004)	(0.011)	(0.012)	(0.047)	(0.048)
Wifes age of mother at birth		0.078***		0.002***		-0.005***		0.106***
		(0.002)		(0.000)		(0.000)		(0.002)
Husbands age of mother at birth		0.063***		0.000**		-0.002***		0.080***
		(0.002)		(0.000)		(0.000)		(0.002)
Wifes family size		-0.395***		-0.009***		0.144***		-0.357***
		(0.007)		(0.000)		(0.002)		(0.006)
Husbands family size		-0.416***		-0.005***		0.110***		-0.347***
		(0.007)		(0.000)		(0.002)		(0.006)
Constant	28.738***	27.197***	0.092***	0.080***	2.061***	1.528***	29.964***	26.854***
	(0.010)	(0.070)	(0.001)	(0.005)	(0.002)	(0.018)	(0.008)	(0.064)
Wifes birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Husbands birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wifes neighbourhood (age 11) FE	No	Yes	No	Yes	No	Yes	No	Yes
Husbands neighbourhood (age 11) FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	302,853	302,853	302,853	302,853	302,853	302,853	272,162	272,162
Couples from DF	46,963	46,963	46,963	46,963	46,963	46,963	40,879	40,879
R-squared	0.0187	0.206	0.00312	0.0799	0.0128	0.186	0.0117	0.206

The tables shows the estimated association between parental divorce and offspring's fertility and marital outcomes on couple-level data. Couples in which at least one spouse experienced parental divorce during childhood are compared to couples in which both spouses come from intact, married families. Robust standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix C: Details on the bounding estimator methodology

To assess whether our results are robust to omitted variable bias, we follow Oster (2019) and construct the bounding estimator β^* for the bias-corrected effect of parental divorce on children's fertility and marital outcomes as follows:

$$\beta^* \approx \tilde{\beta} - \delta[\dot{\beta} - \tilde{\beta}]\frac{R_{max} - \tilde{R}}{\tilde{R} - \dot{R}} \tag{2.C1}$$

where $(\tilde{\beta}, \tilde{R})$ are the influence of parental divorce and R-squared estimated in the regression with the full set of observed control variables included (*controlled regression)*; $(\dot{\beta}, \dot{R})$ are the influence of parental divorce and R-squared estimated in the regression without additional control variables (*uncontrolled regression*); R_{max} is the unknown R-squared estimated in the regression with the full set of observed and unobserved control variables included; δ is the degree of selection, i.e. the relative importance of the unobservables with respect to the observables in driving the effect of parental divorce on the outcome variables.

Following Oster (2019), we test the robustness of our findings by computing the degree of selection that is needed to explain away the effect of parental divorce on our outcome variables. That is, we compute the value δ^{max} for which $\beta = 0$. Note that, because R_{max} is unknown in Equation 2.C1, to compute δ^{max} we first need to specify R_{max}. Because of measurements errors, it is unlikely that R_{max} would be 1 even if all observed and unobserved control variables could be included. Oster (2019) suggests that a reasonable cutoff for this parameter is $R_{max} = 1.3\tilde{R}$. Therefore, we set $R_{max} = 1.3\tilde{R}$ and solve Equation 2.C1 for the value δ^{max} at which $\beta = 0$. As suggested by Oster (2019), we consider robust all specifications in which δ^{max} is at least equal to 1, and we report the bias-corrected estimator β^* computed at $\delta = 1$.

Obtaining access to the data from Statistics Netherlands

The data used in this chapter include detailed information on the entire population of the Netherlands and are therefore highly confidential. To access these data, a researcher should submit a proposal that outlines the objectives of the project as well as what datasets the researcher deems necessary. The full data catalogue can be found here: https://www.cbs.nl/en-gb/our-services/customised-services-microdata/microdata-conducting-your-own-research/microdata-catalogue. Each project outside the European Union must include a researcher who is affiliated with a research institution within the Netherlands and most of the individual data files are solely described in Dutch. The costs of the projects vary and are mostly dependent on the duration of the project, the number of researchers, and the data files requested. More information about the costs can be found here: https://www.cbs.nl/en-gb/our-services/customised-services-microdata/microdata-conducting-your-own-research/services-and-costs.

Once the project is approved, the researchers must pass a test provided by Statistics Netherlands that includes 20 questions on what actions are allowed regarding the privacy issues. Upon passing the test, the authorized researchers can access the data both within and outside the Netherlands through a highly secured remote-access environment. Note that the results can only be used outside the environment once they have been approved and inspected by Statistics Netherlands for potential privacy breaches. For additional questions about the data access, please contact Statistics Netherlands at the following e-mail address: microdata@cbs.nl. Interested readers can also contact the corresponding author for material to replicate this chapter. We would be happy to provide assistance and Stata code necessary for replication.

Chapter 3

Parental separation and family reconstruction: The effect of the birth of a half-sibling and the presence of stepsiblings on children's educational achievements.[1]

Abstract

Over the past few decades, there has been an increase in the number of children who first experienced the separation of their parents and then the transition to a stepfamily. Yet, it is empirically underinvestigated whether exposure to similar complex sibling structures affects the educational outcomes of children in stepfamilies. Using population data from the Netherlands, we provide evidence of the educational outcomes of children born in 1995-2009 who experienced the birth of a half-sibling or the move of stepsiblings into the household. To account for endogeneity, we exploit variations in complex sibling structure between full siblings at the time of the standardized test in primary education. The results indicate that complex sibling structures do not have a significant impact on the academic performance of children in stepfamilies. Both the birth of a half-sibling and the presence of stepsiblings do not significantly affect children's outcomes in primary and secondary school. No further heterogeneity is found according to the duration of exposition to complex sibling structures. Overall, the findings suggest that complex sibling structures are not more detrimental to children's educational outcomes than parental separation and stepfamily formation.

[1]This chapter is based on: Palmaccio, S., Mazrekaj, D., & De Witte, K. (2023). Parental separation and family reconstruction: The effect of the birth of a half-sibling and the presence of stepsiblings on children's educational achievements.

For their helpful comments and suggestions, we would like to thank Christiaan Monden, and seminar and workshop participants at KU Leuven, Cologne, and Gent University. The data for this study are protected by a confidentiality agreement and we are precluded from sharing the data with others. Interested readers can consult the online supplement for information on how to obtain access to the data.

1 Introduction

In recent decades, family structures have changed notably (OECD, 2011). The normalization of divorce has led to a decrease in the number of children living in biological two-parent families and an increase in the number of children living in single-parent or stepparent families. The rise of stepfamilies, in particular, has resulted in increasing complexity also in the sibling composition within the household (Sanner et al., 2018). In the US, 3 out of 10 respondents in a survey conducted by Pew Research Center revealed that they had at least one half-sibling or stepsibling (Sanner et al., 2018; Pew Research Center, 2011), and a similar trend is true also in the EU. For example, a recent report revealed that in the Netherlands 19 percent of households have minor children living with a steprelative (stepparent, half-sibling, or stepsibling) (van Gaalen and van Roon, 2020). Despite the extensive research examining the impact of complex family structures on children, the effects of complex sibling structures formed at or after the start of a stepfamily are underinvestigated.

In this chapter, we investigate the effects of the birth of a half-sibling and the presence of stepsiblings on children's educational outcomes. To the best of our knowledge, this is among the first studies to identify how children, who have been exposed to complex sibling structures after parental separation, perform in school net of the effect of complex family structures. Only Gennetian (2005) previously examined pure half-sibling effects using survey data, and found weak negative effects of having (young) half-sibling on children's school grades in the US. Her study, however, suffered from data limitations that prevented the identification of the full sample of stepfamilies, and allowed the observation of only married stepfather families. Given that most stepfamilies are initially formed through cohabitation rather than marriage (Bumpass et al., 1995; Stewart, 2005), the inability to observe half-sibling or stepsiblings in cohabiting stepfamilies might result in selectivity of the sample of 'nontraditional' siblings observed.

The majority of other studies on the subject have expanded the definition of complex family to include considerations about half-sibling and (to a lesser degree) stepsibling relationships (Sundström, 2013; Ginther and Pollak, 2004; Gennetian, 2005; Halpern-Meekin and Tach, 2008). These studies conceptualized 'blended' families as those in which the parent and stepparent have a *joint child*, in opposition to 'nonblended' families, which consist of intact two-parent families where children have, at most, full siblings. Although the definition of blended families is intrinsically related to the presence of half-sibling and stepsiblings in the household, this literature suffered from two limitations. First, it did not explicitly examine how the birth of a half-sibling impacted those children who already went through parental separation and stepfamily formation (*stepchildren).* On the contrary, earlier research most often grouped the experiences of stepchildren with those of joint children and com-

pared children in blended families with children in nonblended families.[2][3] The second limitation of the previous literature is that hardly any study has explicitly investigated the potential extra impact that the presence of stepsiblings may have on children following the formation of a stepfamily. One exception is Tillman 2008, who showed that the presence of stepsiblings is responsible for the negative associations between living in a stepfamily and school performance. However, this study was subject to significant data limitations, and the author was only able to observe sibling structures cross-sectionally, with no information available regarding past sibling compositions.

This chapter contributes to the literature by analyzing the effects of complex sibling compositions on a range of educational outcomes using unique administrative data from the Netherlands. We look at the population of children born in 1995–2009 for whom we observe standardized test scores at the end of primary education. For the older birth cohorts (born in 1995–2003) we also observe diploma attainment. Our administrative dataset offers three important advantages over most previous studies. First, the study offers precise and objective data on addresses, marital and cohabitation statuses, and child-parent links, which are utilized to reconstruct relationships between children, parents, half-siblings, and stepsiblings. Moreover, the residential address at which children, (step)parents, and nontraditional siblings are registered is also used to reconstruct households as the set of individuals living together. Most previous studies reconstructed these family and sibling structures using surveys. Surveys, however, are prone to discrepancies in how family and sibling relationships are reported by different family members, especially in the context of complex family structures (Brown and Manning, 2009; White, 1998). These discrepancies, in turn, might prevent the correct identification of family and sibling structures (Brown and Manning, 2009). Second, unlike previous studies that could observe only stepfather families and half-sibling born in married stepfamilies (e.g. Gennetian, 2005), our dataset allows us to observe the full population of stepfamilies. This results in a more nuanced and detailed understanding of the relationships between children, parents, and siblings in these families. Further, we can reconstruct each child's family and sibling structure longitudinally from birth until the end of primary education. This allows for the observation of changes in the family and sibling composition over time, including the addition of half-siblings or the introduction of stepsiblings into the household, shedding light on how these changes affect educational outcomes for children in stepfamilies.

As a third advantage over earlier literature, we observe a sufficient number of observations to account for common family characteristics using sibling fixed-effect models. Our identification strategy exploits variations in sibling structures among full siblings at the time of the test in primary education. To identify half-sibling effects, we exploit stepfami-

[2] Note that stepchildren and joint children are biologically related to each other as they are half-sibling.

[3] Alternatively, within blended families, these studies have compared stepchildren with joint children (Sundström, 2013; Halpern-Meekin and Tach, 2008; Ginther and Pollak, 2004), being unable to examine the independent effect of a half-sibling's birth given that both joint children and stepchildren have, by definition, a half-sibling.

lies where an older full sibling took the standardized test before the birth of a half-sibling, and a younger full sibling took the standardized test after the birth of a half-sibling. To identify stepsibling effects, we compare stepsibling structures across families where an older full sibling took the standardized test while still in a nonblended family and a younger full sibling took the test while in a stepfamily.

Our findings show that complex sibling structures have no impact on the educational attainments of children in stepfamilies. Children in stepfamilies who experienced the birth of a half-sibling are found to perform just as well on standardized test scores at the end of primary education as children in stepfamilies who remained without half-sibling. Similarly, children in stepfamilies who lived with a stepsibling are found to obtain similar test scores as children in stepfamilies who never lived with stepsiblings. The analyses at the sibling level confirm the null effects found in the analyses at the individual level. Furthermore, no heterogeneity is found according to the duration of exposure to complex sibling structures.

Our results can be interpreted as evidence that children living in complex family structures do not experience additional hardships due to the presence of 'nontraditional' siblings. These findings have important policy implications as they suggest that policies aimed at supporting children in complex family structures should not discriminate against or stigmatize the inclusion of new non-biological ties. For instance, some adoption policies may prioritize traditional nuclear families over other family structures, which can prevent children in need of adoption from finding loving and supportive homes. Additionally, state-level child support guidelines may not adequately account for the financial obligations of non-biological parents or siblings in blended families, which can create financial hardships for the family. In the US, for instance, one of the legal concepts underlying family law is that stepparents and stepchildren are "legal strangers" to each other, and even if the stepparent is married to the child's biological parent, most states do not mandate that stepparents provide financial support to stepchildren (Stewart and Timothy, 2020). Our results suggest that these differences between biological and non-biological ties may be unfounded.

2 Background

2.1 Previous literature

Intrinsically intertwined with research on complex family structures is research on complex sibling structures. Early research initially investigated the effects of complex family structures on children's educational outcomes by observing whether parents separated during childhood (for reviews, see Amato, 2000; McLanahan et al., 2013; Palmaccio et al., 2023). These studies generally compared children in separated families to children in intact two-parent families, finding that children with separated parents have lower educational attainments than children in intact families. The definition of family structures on the basis of child-parent relationships alone, however, was shown to be not exhaustive of the complex-

ity of family relationships (Ginther and Pollak, 2004; Gennetian, 2005). A child who has always lived in a biological two-parent family may still belong to a complex family if at least one parent had children with a previous partner. As a result, an increasing number of studies started classifying family structures taking into account also sibling relationships (Sundström, 2013; Ginther and Pollak, 2004; Gennetian, 2005; Tillman, 2008; Halpern-Meekin and Tach, 2008). This literature generally distinguished between 'blended' families (stepparent family where the resident parent and the stepparent have a joint child), stepfamilies (stepparent family where the resident parent and the stepparent do not have children together), and 'nonblended' families (biological two-parent family with at most full siblings). Although the conceptualization of blended families is closely related to the presence of half-sibling and stepsiblings, previous literature still suffers from two shortcomings.

The first limitation is that prior research has not thoroughly examined the impact of complex sibling structures on children who have undergone parental separation and the formation of stepfamilies. By contrast, they either compared children in blended families to children in nonblended families, finding that having a half-sibling negatively correlates with educational attainments in cross-sectional models (Sundström, 2013; Ginther and Pollak, 2004; Gennetian, 2005; Halpern-Meekin and Tach, 2008).[4] Alternatively, conditionally on having a half-sibling, previous research compared stepchildren to joint children within blended families, finding that stepchildren perform worse in school than joint children (Sundström, 2013; Halpern-Meekin and Tach, 2008, although not significantly in Ginther and Pollak, 2004). Although informative, such comparisons do not enable us to extract information on the hypothetical outcomes of stepchildren in the absence of a half-sibling. On the one hand, grouping stepchildren and joint children together and comparing them to children in nonblended families confound the very different life trajectories experienced by the two types of half-sibling. Joint children are born having already a half-sibling, while stepchildren have first experienced the separation of their biological parents, then the formation of a stepfamily, and finally the birth of a sibling with whom they share only one biological parent. On the other hand, comparing stepchildren with joint children is not informative either given that both siblings have, by definition, a half-sibling.

Among the few studies that distinctively examined the consequences of having younger half-sibling, Gennetian (2005) used child-fixed effect models and found weak negative effects of having a young half-sibling on children's school grades in the US. However, data limitations allowed the author to identify only married stepfather families and prevented the observation of cohabiting stepfamilies and stepmother families. This might have resulted in negative estimates of half-sibling effects that are downward biased since they are based on rather close bond stepfamilies, that is, stepfamilies in which (step)parents married Turunen (2014) examined the association between the birth of half-sibling and children's educational

[4]Negative correlations become weak or insignificant in the family fixed-effects models estimated by Gennetian (2005); Sundström (2013).

outcomes in Sweden and found that children with separated parents and younger half-sibling perform worse in school than children with separated parents and no half-sibling. Nonetheless, the author could not distinguish the family arrangement in which children lived after parental separation (e.g. single family or stepfamily), limiting the interpretability of the coefficients found as pure half-sibling effects.

As a second limitation, earlier literature centered the definition of blended families around the presence of half-sibling and did not examine the effects of stepsibling composition. Some studies included having a stepsibling as a control variable when more broadly analyzing the effects of complex family and sibling structures (e.g. Sundström, 2013). These studies, however, did not discuss the magnitude of the observed coefficients. To the best of our knowledge, only Tillman (2008) directly examined stepsibling composition correlations, finding evidence that the presence of stepsiblings mediates the negative relationship between living in a stepfamily and educational outcomes. However, data limitations are present in this study as well, given that the author could measure sibling structure only cross-sectionally at one point in time, and had no information on past sibling composition nor on nonresident siblings.

In the present chapter, we extend earlier literature by examining how the birth of a half-sibling and the presence of stepsiblings affect the educational outcomes of those children who already went through parental separation and stepfamily formation. Our unique dataset allows us to overcome the data limitations of previous research, while our estimation strategy enables us to address endogeneity concerns arising from selection on unobservables using sibling fixed effect models.

2.2 Potential mechanisms

So far, the existing literature mostly suggested a negative relationship between complex sibling structures and stepchildren's educational outcomes. Several mechanisms may provide an explanation for these educational differentials. A first mechanism is related to the possibility that stepchildren with nontraditional siblings may receive fewer economic and time resources from their caregivers than their counterparts having only full siblings. This might occur for two reasons. First, by increasing family size, the presence of nontraditional siblings might reduce the share of resources allocated by caregivers to stepchildren (*resource dilution theory*). The resource dilution theory indeed posits that a family's resources are fixed and that parents have to compromise between the quantity and the quality of their children (Becker and Tomes, 1976; Downey, 2001). Thus, the birth of a half-sibling or the presence of stepsiblings might be detrimental for stepchildren, as it would deprive them of resources otherwise allocated to them. However, recent studies investigating family size effects have found no evidence of a quantity-quality trade-off when children's birth order is taken into account (Black et al., 2005; Price, 2008), making the resource dilution theory an unlikely explanation for the effects of complex sibling compositions.

Second, children with nontraditional siblings may receive fewer resources from their caregivers also due to another mechanism, according to which parents invest more in biological children than in nonbiological children (*kin selection theory*, Mazrekaj et al., 2020). The presence of stepchildren and especially the birth of a half-sibling might therefore, for one thing, take financial resources away from stepchildren, as stepparents might prefer to save them for or invest them in their biological offspring. Further, it might reduce the time devoted by stepparents to stepchildren, as stepparents might prefer spending quality time with their biological children.

A final mechanism predicting poorer school performance of children with nontraditional siblings compared to children with only full siblings lies in the emotional difficulties and the sense of abandonment stepchildren may face when nontraditional siblings enter the household (*estrangement hypothesis*). This mechanism might be particularly strong in the case of the birth of a half-sibling, as stepchildren may feel detached both from their resident and nonresident parent. On the one side, the birth of a half-sibling may lead stepchildren to feel that they do not belong to the new family unit that is being formed (Mostafa et al., 2018).[5] On the other hand, the birth of a half-sibling may increase the estrangement of stepchildren also with the nonresident parent. Earlier research found that nonresident parents reduce contact with their offspring after they or their former partners have a child with another person (Tach et al., 2010; Berger et al., 2012). Consequently, if the estrangement hypothesis holds, we would expect the sense of nonbelonging to be highest if stepchildren experience the birth of a half-sibling from both the resident and nonresident parent, intermediate if only from the resident parent, and lowest if only from the nonresident parent. A testable hypothesis is that these emotional gradients are reflected in corresponding educational deficits that differ according to the half-sibling composition.

3 Data

3.1 Sample construction

We use population data from the Netherlands that consist of a collection of digital and automated municipal registers covering the entire Dutch population annually from 1995 until 2021 (Prins, 2017; Palmaccio et al., 2023). The municipal registers are individually held by each municipality in the Netherlands and contain information about all individuals living within the territory of competence of the municipality. Each inhabitant is assigned a unique identifier number with which it is possible to uniquely link the data of the inhabitant to that of parents, children, and partners. Children are identified given that parents are required to register the birth of any child no later than three days from childbirth. Partners are identi-

[5] Although a contrasting hypothesis is that the birth of a half-sibling help 'cement' stepfamily ties, previous research has found no evidence of better adaptation of stepchildren to stepfamily formation in the presence of half-sibling (Stewart, 2005; Ganong and Coleman, 2004).

fied given that the local municipal registers keep track of the marital and cohabitation status of each inhabitant.[6]. Two individuals are classified as married if they are related by a marital bond, while they are classified as cohabiting if they have signed a written agreement of cohabitation or have filed the taxes jointly. The local registers also provide annually updated information about divorces and end dates of marital and cohabiting unions. By observing parents' relationship status with each other and possibly with other partners, we are able to reconstruct the relationship history of each of the parents. Further, we observe annually updated information about individuals' addresses, with which we reconstruct households as sets of individuals living together. Taken together, this information allows us to reconstruct children's family structure at birth and throughout childhood. We are able to identify year by year whether children are residing with their two biological parents, with a single parent, or with a parent and a stepparent.

Our data also allow us to precisely reconstruct children's sibling composition during childhood. Using the child-parent identification linkages, we are able to observe all the children born to each parent and stepparent. From the focal child's perspective, these siblings may either be full siblings (i.e. they share both biological parents), half-sibling (they share only one biological parent), or stepsiblings (they have no blood connection but are legally linked because their parents are in a relationship). Additionally, using the address information, we further distinguish between resident and nonresident half and stepsiblings depending on whether the nontraditional sibling resides at the same address as the focal child. It should be noted that individuals in the Netherlands can only be registered at one address, and we do not observe custody arrangements between parents after divorce. Reconstructing resident sibling structures might therefore be particularly challenging in the case of shared custody agreements, where focal children (and/or their stepsiblings) might be residing for half of their time with one parent and the other half with the other parent. However, descriptive evidence shows that shared custody agreements in the Netherlands are still rare, as they amounted to 5% in 1998, and although having increased over time, they still amounted to only 16% in 2008 (Spruijt and Duindam, 2009). Therefore, we expect shared residence arrangements to apply to only a small percentage of the children in our sample, from which it follows that the majority of the children in our sample are presumably subject most of their time to the family structure corresponding to their residence address.

To examine the educational outcomes of children in complex sibling structures, we select children born between 1995 and 2009 such that they are old enough to have taken the standardized high-stakes test (CITO-test) at the end of primary education (around age 11-12). We observe 1,764,015 children with complete information on test scores. Because

[6]In addition, the data also provide information about whether two partners are in a registered partnership. As of 1998, in fact, in the Netherlands it is possible for two partners to form a registered partnership as an additional legal recognition of a relationship. Registered partnerships, however, were mainly introduced to legally recognize same-sex unions, and are perceived as alternative to marriage. Therefore, throughout the analysis, we will not distinguish between registered partnerships from marriages but we will consider them in the same category.

we aim to isolate the effect of living in nontraditional sibling structures, we further restrict the sample as follows. First, we require that all children in our sample are born in a non-blended family, that is, a family with both biological parents and at most full siblings (88.59 percent of the sample). By doing so, we avoid that children may be born already in complex family structures or have nontraditional siblings at birth. Second, we require that children have either remained in the nonblended family throughout childhood or have experienced family recontruction. Family reconstruction is defined as having experienced first the separation of the biological parents and then the formation of a resident stepfamily.[7] As such, we exclude children who have experienced parental separation, but whose resident parent remained single and did not subsequently re-partner (10.47 percent of the sample). Third, we require reconstructed families to be 'stable'. That is, we exclude children in stepfamilies where the parent and the stepparent separated before the focal children took the standarized high-stakes test at the end of primary education, or where the focal child switched residence from a stepfather family to a stepmother family or viceversa (3.07 percent of the remaining sample). Fourth, we exclude children in family units where we observe that siblings are residing with different parents, e.g. one sibling with the mother and the other sibling with the father (1.77 percent of the stepfamily sample). Because siblings are almost always kept together after parental separation, these family arrangements may reflect cases in which the residence is split by parents on paper, while in reality it is evenly shared. To avoid possible misclassifications[8], we exclude these children from the analysis. Nonetheless, our results are robust to estimating our main models on a redefined sample that includes siblings in split residence arrangements. Finally, we exclude children with missing covariates (2.45 percent of the remaining sample). Our final sample consists of 1,313,984 children, of whom 62,665 experienced parental separation and stepfamily formation during childhood.

3.2 Variable construction

Educational variables. We examine a range of outcomes that depict the key defining moments of the Dutch educational system (standardized test scores at the end of primary education and diploma attainment in secondary education). In the Netherlands, students must attend school until they are 18 years old or until they have earned a secondary education diploma. After completing primary education, children enroll in secondary school and select one of three tracks (i.e. general, pre-university, or pre-vocational). These programs differ in educational content, duration, and future job perspectives. The pre-university track lasts 6

[7] Throughout the analysis, we will focus on the effects of complex sibling structures defined from the perspective of the resident parent. Although children may also have half-sibling and stepsiblings from the nonresident parent, examining the effects of these family structures is beyond the scope of this study. Nonetheless, to test the estangement hypothesis, we dig deeper into heterogeneous halfsibling effects according to whether halfsiblings are born to the resident parent only, the nonresident parent only, or both the resident and the nonresident parent (see Table 3.A4 in the online supplement).

[8] In the estimation of sibling fixed-effect models, these siblings would be compared to each other as if they were experiencing different life trajectories when in fact they are exposed to the same family and sibling structures.

years and prepares pupils to enroll in academic universities. The general track lasts 5 years and prepares pupils to enroll in universities of applied sciences, where knowledge on specific professions is learned. Finally, the pre-vocational track lasts 4 years and prepares pupils to enroll in the vocational track, where specific manual jobs are learned.

At the end of primary education, at around age twelve (Grade 8), students take part in a national high-stakes standardized test that is administered throughout the Netherlands by the Central Institute of Test Development (CITO). Although the CITO-test is not mandatory at the school level and schools can decide not to participate, 85% of schools in the Netherlands usually take part in the test (Kabátek and Perales, 2021; Kortlever and Lemmens, 2012). Once a school decides to participate in the CITO exam, the test is mandatory for all pupils enrolled in that school. The test is considered high-stakes, as the scores are relevant for the admission of students to high school. The test is usually taken around February and consists of multiple-choice questions regarding Dutch language, mathematics, and learning skills. As of 2015, other institutes have developed their own tests which are alternatively provided to the CITO-tests. However, the majority of schools still provide CITO-tests (75.34 percent of the children in our sample took the CITO-tests). To maximize sample size, in the analysis we pool the scores of all test types, and to make them comparable, we standardize test scores by test type and year.[9].

For the older birth cohorts that we can follow up until the end of high school, we construct an additional outcome variable for diploma attainment. Specifically, we create a binary variable coded 1 if the student passed the high school exit exam on the first attempt, and 0 if the student failed the exam or dropped out of school before taking the exam.

Control variables. To account for children's socioeconomic background, we include continuous variables for the mother's and the father's gross annual income. To construct these variables, first, we observe the gross annual income of each parent when the child was born.[10] Next, we take logarithms to smooth out the extremes of the distribution. We also include a binary variable for parental education, which we code 1 if both parents have completed secondary education, and 0 otherwise. Finally, we include indicators for children's neighborhood of residence at birth.

To account for children's family characteristics, we include a range of control variables denoting the characteristics of both the *original family* and the *stepfamily*. With respect to the original family unit, first, we control for children's foreign background. We create a categorical variable and code it 0 if both parents were born in the Netherlands; 1 if one parent was born in the Netherlands, the other outside the Netherlands; 2 if both parents were born outside the Netherlands. Further, we account for the total number of full siblings in the family at the time of the test by including a discrete variable for family size. We also

[9]Results do not change if we only use CITO-test scores.

[10]As income data are available from 2003, for the birth cohorts 1995-2002 we measure parents' gross annual income in the first available year rather than at the child's birth.

account for maternal age by including a continuous variable for mother's age at childbirth.

Turning to the stepfamily, we include a categorical variable for the stepparent parental status before joining the stepfamily. We code this variable 0 if the stepparent does not exist because the child belongs to a nonblended family, 1 if the stepparent did not have children with previous partners; 2 if the stepparent had at least one child with previous partners.[11] Further, to account for the possible birth of one or more half-sibling from the nonresident parent, we construct a discrete variable for the total number of nonresident half-sibling the child had at the time of the standardized tests at the end of primary education. Next, to purge the half-sibling effect from the effect of any new childbirth, we account for the total number of blood-related children in the household and include a discrete variable for the resident family size. We construct this variable by observing the number of full siblings and resident half-sibling in the household at the time of the standardized tests. Finally, we account for duration of time spent by children in a nonblended family by including a discrete variable for the number of years children lived with both their biological parents.

At the individual level, we control for children's sex (1 is male, 0 is female), year of birth, and birth order. We construct the latter by including a discrete variable for the birth order of children relative to their full siblings.

4 Methodology

To examine the effect of complex sibling structures on children's educational outcomes, we consider the birth of a half-sibling and the presence of stepsibling(s) into the household separately, thereby constructing different model specifications.

4.1 Halfsibling effects

To examine half-sibling effects, we focus on the sample of stepfamilies and compare children in stepfamilies who experienced the birth of a resident half-sibling to children in stepfamilies who did not experience the birth of a half-sibling during childhood. In the baseline empirical specification, we estimate variations of the following linear regression model estimated by ordinary least squares (OLS):

$$Y_i = \beta_0 + \beta_1 \mathrm{H}_i + \boldsymbol{\theta X_i} + \epsilon_i \tag{4.1}$$

where Y_i indicates the educational outcomes of child i: standardized test scores for all birth cohorts; diploma attainment for birth cohorts 1995-2003. H_i is a binary indicator given value 1 if the child had a younger resident half-sibling at the moment of the test and value 0 otherwise. $\mathbf{X_i}$ defines a series of individual, socioeconomic, and family characteristics that we include differently according to the model specification. In all baseline regressions, we

[11] Our findings are robust to not including this control variable when estimating stepsibling composition effects (see Panel A in Table 3.A9 in the appendix).

include individual control variables for children's sex and ethnicity, and indicators for years of birth. To account for correlations of the error term among full siblings, we cluster the standard errors at the full sibling level.

Although our rich dataset allows us to control for an extensive range of family and socioeconomic characteristics at the individual level, we may still fail to account for unobserved family characteristics that may be associated with the key regressor of interest and directly correlated with educational outcomes. Unobserved factors such as the level of conflict between parents before separation or the level of stress within the family are likely to negatively affect children's performance in school. If these factors also influence the likelihood that parents have children with other partners after separation, the OLS coefficient estimated in Equation (4.1) will be biased.[12] To account for omitted variable bias, we estimate the following family fixed effect model:

$$Y_{ij} = \beta_0 + \beta_1 \mathrm{H}_{ij} + \boldsymbol{\delta Z_{ij}} + \mu_j + \epsilon_{ij} \tag{4.2}$$

where Y_{ij} indicates the outcome variables (standardized test scores) of child i in family j. Z_{ij} denotes a series of individual and socioeconomic characteristics that are similar to those included in X_i in Equation (4.1), but restricted to the characteristics that vary between siblings at the time of the test. μ_j denotes sibling fixed effects and accounts for all observed and unobserved characteristics that are common among full siblings. Standard errors are clustered at the full sibling level.

To identify half-sibling effects, we exploit the variation in complex sibling structure between full siblings at the time of the test in given family units. In these family units, we observe at least one older full sibling taking the test before the birth of a half-sibling, and one younger full sibling taking the test after the birth of the half-sibling. The variation is observed for 759 full siblings, corresponding to 3.80 percent of the sibling sample. Because by requirement all full siblings are observed in a stepfamily at the time of the test, variations in standardized test scores between full siblings due to earlier life-disruption events (such as parental divorce, stepfamily formation, or living in a single-parented family before being in a stepfamily), are absorbed by the family fixed effects.[13]

4.2 Stepsibling effects

We also examine stepsibling effects, namely the effect of the presence of a sibling with whom children do not share any blood ties but are legally related because their parents are in a partnership. To this end, we consider both the sample of children in stepfamilies

[12] For instance, a resident parent in a poor relationship with the nonresident parent may receive less physical or financial support from the nonresident parent in the care of the children, which in turn may negatively influence the likelihood that the resident parent have another child.

[13] Nonetheless, full siblings may have different educational outcomes also due to different characteristics of the primary school they attended. In Table 3.A5 in the online supplement, we re-estimate our models by additionally including school fixed effects, and find consistent results with those obtained in our main analysis.

already used to estimate half-sibling effects and the sample of children from nonblended families. In the basic empirical specification, we estimate different versions of the following linear regression model estimated by OLS:

$$Y_i = \alpha + \sum_{k=1}^{2} \beta_k S_{ki} + \boldsymbol{\theta X_i} + \epsilon_i \tag{4.3}$$

where Y_i and X_i denote the same educational outcome variables and almost identical individual, family, and socioeconomic control variables as those included in Equation (4.1). S_{ki} is the key variable of interest and is a categorical variable accounting for the stepsibling structure within the family, as experienced from the focal child's perspective. We construct three categories: 0 for having lived in a nonblended family (reference category); 1 for never having lived with a stepsibling whilst in a stepfamily[14]; 2 for having lived with a stepsibling whilst in a stepfamily.[15] Note that, to be falling in the category of having lived with a stepsibling whilst in a stepfamily, children should have either lived with a stepsibling for one full calendar year or, in case of multiple residential moves within a year, for a time period that is large enough to be their longest spell. Standard errors are clustered at the full sibling level.

Similarly to what was earlier discussed in Section 4.1, the coefficients of interest estimated in Equation (4.3) may be biased if the corresponding variables are correlated with influential but unobserved family characteristics. For example, the level of stress or conflict within the family experienced prior to separation may influence the characteristics sought by the resident parent in future partners.[16] To account for family observed and unobserved characteristics, we estimate the following family fixed effect model:

$$Y_{ij} = \alpha + \sum_{k=1}^{2} \beta_k S_{kij} + \theta \mathbf{Z_{ij}} + \mu_j + \epsilon_{ij} \tag{4.4}$$

where, Y_{ij} denotes standardized test scores of child i in family j; Z_{ij} a series of individual and socioeconomic control variables that vary between siblings; μ_j sibling fixed effects. Standard errors are clustered at the full sibling level.

[14] Note that children may never have lived with stepsiblings either because the stepparent did not have children with previous partners or because stepchildren never moved into the household. In Table 3.A9 in the appendix, we further differentiate between these two categories and find consistent results as those reported in our main specification in Table 4.4 where we combine both categories.

[15] Note that children in stepfamilies who fall into category 1 (never having lived with a stepsibling whilst in a stepfamily) or 2 (having lived with a stepsibling whilst in a stepfamily) may or may not have experienced the birth of a half-sibling.

[16] A resident parent in a poor relationship with the nonresident parent may be more inclined to look for a new partner who can devote full physical and financial support to him/her, for example, by not having previous children or not having children who would move into the household. As such, the individual-level coefficient β_1 estimated in Equation 4.3 may be downward biased, as it may overstate the (negative) effect of living in a stepfamily without stepsiblings. By contrast, the individual-level coefficient β_2 estimated in Equation (4.3) may be upward biased, as it may understate the (negative) effect of living in a stepfamily without stepsiblings. Taken together, accounting for sibling fixed effects may yield larger or smaller differences between the two coefficients depending on how much variation is explained by the omitted variables.

Similarly to before, the identification strategy employed to examine stepsibling effects is based on variations in stepsibling composition between full siblings at the time of the standardized test in primary education. As source of variation, we exploit those family units where an older full sibling took the test when still in a nonblended family (thus without stepsiblings) and a younger full sibling took the test after having entered a stepfamily. The variation is observed for 4,796 children (corresponding to the 0.57 percent of the sibling sample). By comparing β_1 (the effect of living in a stepfamily with no resident stepsiblings) with β_2 (the effect of living in a stepfamily with a resident stepsibling), we attempt to single out the effect of the presence of stepsiblings on children's standardized test scores. More specifically, if the difference between the estimated coefficients is statistically significant by means of a test of equality of coefficients (i.e. $\beta_1 \neq \beta_2$), we conclude that the presence of stepsiblings in the household significantly impacted children's outcomes in primary education net of stepfamily effects.[17]

It should be noted that in our estimation strategy, both in the half-sibling and in the stepsibling analyses, we assume that there are no anticipation effects, that is, that the announcement of a sibling 'change' within the household does not have strong effects on children. To test this assumption, we examine the association between the expectation of the birth of a half-sibling and test scores in Table 3.A11 in the appendix. More specifically, we compare children in stepfamilies who will experience the birth of a half-sibling within nine months after having taken standardized test scores with children in stepfamilies who never experienced the birth of a half-sibling or will experience the birth of a half-sibling as of nine months after their test dates. Our results show no evidence of anticipation effects, as when we include the full set of covariates, we do not find a meaningful association between the expectation of the birth of a half-sibling and test scores. Thus, although we cannot test this assumption in a similar fashion for stepsibling effects, we expect that the also bias stemming from anticipation effects in the stepsibling analysis is limited and at most waning over time.

5 Results

5.1 Descriptive statistics

Table 3.1 reports the descriptive statistics of the sample at the individual level. First, we compare children in nonblended families (column 1) to the full sample of children in stepfamilies (column 2). Then, we further distinguish the sample of children in stepfamilies according

[17]It should be noted that children in stepfamilies may differ in the number of years spent in a single-parented family in the period between the separation of the parents and the formation of a stepfamily. To examine whether our results may be driven by children living for a long period in a single family before living in a stepfamily, we re-estimate our models on a subsample of children who lived for at most three years in a single family before the transition to a stepfamily. The results (shown in Table 3.A6 in the online supplement) are consistent with our main findings reported in Table 3.3, although the models appear to have lower explanatory power, especially when estimating sibling fixed effects while accounting for the full set of covariates in the last column of the table.

to whether children had a half-sibling (column 3 and 4) or lived with a stepsibling in childhood (columns 5 and 6). Table 3.A1 in the online supplement reports the corresponding statistics for the sibling sample, where each child is matched with at least one full sibling.

Table 3.1: Descriptive statistics of individual level data

			Stepfamilies			
	Nonblended families	Stepfamilies	Resident half-sibling		Resident stepsibling	
			No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
Sex (1 is male)	0.503	0.489#	0.489	0.488	0.488	0.494
Age of mother at birth	31.243	29.002#	30.031	26.889†	28.874	29.954‡
Family size	2.587	1.966#	2.086	1.720†	1.947	2.109‡
Resident family size	2.587	2.38#	2.086	2.984†	2.388	2.323‡
Birth order	1.81	1.599#	1.684	1.423†	1.582	1.723‡
Ethnicity						
Both parents born in NL	0.756	0.779#	0.793	0.75†	0.775	0.808‡
One parent NL, other not NL	0.128	0.173#	0.167	0.185†	0.174	0.165
Both parents not NL	0.116	0.048#	0.041	0.065†	0.051	0.027
Father gross income at birth	58,789.045	48,466.01#	50,662.24	43,955.95†	48,004.469	51,905.566‡
Mother gross income at birth	21,090.119	21,891.77#	22,520.65	20,600.32†	21,734.617	23,062.869‡
Parental education (1 diploma SE)	0.901	0.879#	0.89	0.856†	0.877	0.893
No. of years in an intact family at test	13.164	6.034#	6.727	4.611†	5.989	6.37‡
No. of nonsident halfsiblings at test	0	0.315#	0.219	0.513†	0.324	0.249‡
No. of stepparent's own children						
Zero	0	0.513#	0.417	0.766†	0.602	0‡
At least one child	0	0.469#	0.583	0.234†	0.398	1‡
Stepsibling structure:						
Never lived with stepsibling	0	0.882#	0.857	0.933†	1	0‡
Lived with stepsibling	0	0.118#	0.143	0.067†	0	1‡
Birth of halfsibling (1 is yes)	0	0.327#	0	1†	0.347	0.185‡
Outcome variables						
Standardized test scores	0.084	−0.153#	−0.133	−0.195†	−0.156	−0.131‡
Diploma attainment (1 is yes)	0.929	0.902#	0.908	0.89	0.901	0.911
Observations	1,251,319	62,665	42,143	20,522	55,251	7,414

Notes: #: the mean for children in column (2) is significantly different at the 5 percent level from the mean for children in column (1) by means of a two-tailed z-test. †: the mean for children in column (4) is significantly different at the 5 percent level from the mean for children in column (3) by means of a two-tailed z-test. ‡: the mean for children in column (6) is significantly different at the 5 percent level from the mean for children in column (5) by means of a two-tailed z-test.

Our individual sample consists of 1,313,984 children, of whom 62,665 children experienced parental separation and stepfamily formation during childhood (corresponding to the 4.77 percent of the sample). Consistent with previous literature (e.g. Gennetian, 2005; Ginther and Pollak, 2004), children in stepfamilies generally have lower socioeconomic status, younger mothers, and fewer siblings than children in nonblended families. Within the sample of stepfamilies, about one in three children experienced the birth of a half-sibling (20,522 children, corresponding to 32.7 percent of the stepfamily sample), whereas somewhat more than one child in ten lived with a stepsibling in childhood (7,414 children, corresponding to 11.8 percent of the stepfamily sample).

Focusing on the differences between stepfamilies based on half-sibling composition, Table 3.1 shows that children in stepfamilies with half-sibling belong to smaller family units than children in stepfamilies without half-sibling, as they have fewer full siblings and are

lower in the birth order. Children in stepfamilies with half-sibling also have a lower socioeconomic status, given that they have younger mothers at childbirth, and parents with a lower gross annual income and a lower level of education. Further, they are more ethnically diverse, as they are less likely to have both parents born in the Netherlands, but more likely to have either one or both parents born outside the Netherlands. Finally, children in stepfamilies with half-sibling are more likely to have nonresident half-sibling born to the nonresident parent, whereas they are less likely to have lived with a stepsibling in childhood.

Turning to the description of stepfamilies based on stepsibling composition, children in stepfamilies with resident stepsiblings belong to larger family units than children in stepfamilies without resident stepsiblings, as they have more full siblings and are higher in the birth order. They also have higher socioeconomic status, as their mothers are older at birth and both their parents have higher gross incomes. Further, they are less ethnically diverse, as they are more likely to have both parents born in the Netherlands. Finally, children in stepfamilies with resident stepsiblings are less likely to experience the birth of a half-sibling than children in stepfamilies who never lived with stepsiblings, and are also less likely to have nonresident half-sibling.

With respect to the outcome variables, children in the full sample of stepfamilies appear to generally perform worse in school than children in nonblended families, as they score lower on standardized test scores at the end of primary education and are less likely to obtain a high school diploma. In particular, children in stepfamilies score on average -.153 standard deviations on standardized tests, which corresponds to a decrease of 0.237 standard deviations relative to the score of .084 standard deviations obtained by children in nonblended families. Within stepfamilies, having a half-sibling appears to be correlated with higher deficits in primary education. Children in stepfamilies who experienced the birth of a half-sibling score on average -.195 standard deviations whereas children in stepfamilies without half-sibling score on average -.133 standard deviations (corresponding to a negative difference of .06 standard deviations). Nonetheless, children in stepfamilies with half-sibling do not appear to obtain a high school diploma at a different frequency than children in stepfamilies without half-sibling.

Turning to differences in outcome variables by stepsibling composition, the descriptive statistics show that children who lived with a stepsibling in childhood have better scores in primary school than children who never lived with a stepsibling. The former achieve on average -.131 standard deviations in standardized tests, while the latter achieve on average -.156 standard deviations (corresponding to a positive difference of about .25 standard deviations). Nonetheless, as in the case of the distinction based on half-sibling composition, children in stepfamilies who lived with a stepsibling do not appear to obtain a high school diploma at different frequencies than children in stepfamilies who never lived with a stepsibling.

5.2 Complex sibling structures and primary school outcomes

5.2.1 Halfsibling effects

Table 3.2 reports the effect of the birth of a half-sibling on children's standardized test scores at the end of primary education. Before getting into the details of the estimation results, it should be noted that further analyses (reported in Table 3.A2 in the online supplement) reveal that children who entered a stepfamily after parental separation are already performing poorly in school compared to children who belong to an intact, nonblended family. In Table 3.A2, we investigate further the relationship between complex family structures formed after parental separation and standardized test scores, finding high and meaningful deficits of children in stepfamilies compared to children in nonblended families. However, it is important to note that because no meaningful differences are found between children who lived in a stepfamily after parental separation and children who lived in a single family after parental separation, it is likely that the school deficits of children in stepfamilies are driven by parental separation rather than by the family composition experienced after parental separation.

Turning to the estimation results, columns 1 and 2 of Table 3.2 show the results estimated at the individual level, while columns 3 and 4 report those estimated at the sibling level. Our findings suggest that the birth of a half-sibling does not significantly affect children's test scores in primary education. In column 1, children in stepfamilies who experienced the birth of a half-sibling are found to have test scores .060 standard deviations lower than children in stepfamilies who remained without half-sibling. However, when their lower socioeconomic status is accounted for in column 2, the coefficient of interest becomes close to 0 and not significant. Accounting for family fixed effects in columns 3 and 4 yields consistent findings as those obtained from our OLS analysis at the individual level, suggesting a limited role of omitted variables in driving the individual-level coefficients. The birth of a half-sibling does not appear to significantly affect children's test scores, as the coefficients of interest are again close to 0 and not statistically significant.

5.2.2 Stepsibling structures

Table 3.3 compares the standardized test scores obtained by children in nonblended families with those obtained by children in stepfamilies who either never lived with a stepsibling in childhood or had a resident stepsibling. Individual-level analyses are reported in columns 1 and 2, while sibling fixed-effect models are shown in columns 3 and 4. It should be noted that children may not experience living with stepsiblings either because the stepparent does not have own children or because the stepsiblings never join the household. We distinguish between these two categories in Panel B of Table 3.A9 in the appendix and find results that are consistent with those reported in our main specification in Table 4.4.

Our results suggest no impact of having resident stepsiblings on children's test scores.

Table 3.2: Halfsibling's birth and test scores

	Individual sample		Sibling sample	
	(1)	(2)	(3)	(4)
Half-sibling (1 is yes)	−0.060***	−0.012	−0.011	0.003
	(0.009)	(0.020)	(0.065)	(0.085)
Sex (1 is male)	0.003	−0.001		0.027
	(0.008)	(0.008)		(0.016)
Ethnicity (ref: both parents born in NL)				
One parent NL, other not NL	0.033**	0.014		
	(0.011)	(0.011)		
Both parents not NL	−0.129***	−0.026		
	(0.020)	(0.022)		
Age of mother at birth		0.026***		
		(0.001)		
Family size		0.071***		
		(0.016)		
Birth order		−0.159***		−0.094**
		(0.009)		(0.029)
Fathers log gross income		0.010***		0.001
		(0.002)		(0.005)
Mothers log gross income		0.010***		0.001
		(0.002)		(0.004)
Parental education (1 diploma SE)		0.449***		
		(0.014)		
Stepparent parental status				
(ref: stepparent has at least one child)				
Stepparent has no children		0.030**		
		(0.009)		
No. of nonresident half-siblings		0.002		−0.051
		(0.007)		(0.045)
No. of years in nonblended family		−0.010***		
		(0.002)		
Resident family size		0.019		−0.015
		(0.014)		(0.060)
Constant	−0.134***	−1.374***	−0.114***	0.064
	(0.007)	(0.046)	(0.019)	(0.169)
Birth year FE	Yes	Yes	Yes	Yes
Neighbourhood FE	No	Yes	No	Yes
Sibling FE	No	No	Yes	Yes
Observations	62,665	62,665	19,913	19,913
R-squared	0.007	0.077	0.343	0.344

Notes: The table shows the effect of having a half-sibling on the standardized test scores of children in stepfamilies. Socioeconomic control variables (age of mother, father's log gross income, mother's log gross income, parental education, neighborhood FE) are measured at birth, while stepfamily control variables (stepparent parental status, resident family size, number of years in a nonblended family, number of nonresident half-siblings), as well as family size, are measured at the time children took the standardized test scores at the end of primary education. Standard errors are clustered at the full sibling level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In column 1, children who lived with stepsiblings appear to perform somewhat better in school compared to children in stepfamilies without resident stepsiblings. However, the difference is not statistically significant by means of a test of equality of coefficients. When their higher socioeconomic status is accounted for in column 2, children in stepfamilies with resident stepsiblings still appear to obtain somewhat better scores than children in stepfamilies without resident stepsiblings. While the latter show deficits in test scores of 0.163 standard deviations, the former report deficits in test scores of .146 standard deviations. Nonetheless, the positive difference between the two groups (amounting to .017 standard deviations) is not meaningful and is not statistically significant by means of a test of equality of coefficients.

Accounting for common family characteristics in the fixed-effects models also yields

Table 3.3: Stepsiblings composition and test scores

	Individual sample		Sibling sample	
	(1)	(2)	(3)	(4)
Stepsibling structure (ref: nonblended family)				
Never lived with stepsibling	−0.264***	−0.163***	−0.110***	−0.094**
	(0.005)	(0.007)	(0.025)	(0.033)
Lived with stepsibling	−0.247***	−0.146***	−0.122*	−0.112*
	(0.012)	(0.012)	(0.050)	(0.050)
Sex (1 is male)	−0.005**	−0.008***	−0.005*	−0.005*
	(0.002)	(0.002)	(0.002)	(0.002)
Ethnicity (ref: both parents born in NL)				
One parent NL, other not NL	0.057***	0.002		
	(0.003)	(0.003)		
Both parents not NL	−0.396***	−0.121***		
	(0.003)	(0.004)		
Age of mother at birth		0.027***		
		(0.000)		
Family size		0.054***		
		(0.013)		
Birth order		−0.140***		−0.101***
		(0.001)		(0.003)
Fathers log gross income		0.020***		0.001
		(0.001)		(0.001)
Mothers log gross income		0.004***		0.001
		(0.000)		(0.001)
Parental education (1 diploma SE)		0.536***		
		(0.003)		
Half-sibling (1 is yes)		−0.002		−0.019
		(0.018)		(0.077)
No. of nonresident half-siblings		0.013*		−0.062
		(0.006)		(0.040)
Resident family size		0.036**		−0.023
		(0.013)		(0.057)
Stepparent parental status (ref: nonblended family)				
Stepparent has no children		0.040***		−0.035
		(0.009)		(0.051)
Constant	0.125***	−1.457***	0.120***	0.359*
	(0.001)	(0.011)	(0.001)	(0.157)
Birth year FE	Yes	Yes	Yes	Yes
Neighbourhood FE	No	Yes	No	Yes
Sibling FE	No	No	Yes	Yes
Observations	1,313,984	1,313,984	839,514	839,514
p-value, t-test	0.177	0.220	0.822	0.746
R-squared	0.026	0.101	0.383	0.384

Notes: The table shows the effect of having lived with a stepsibling in childhood on the standardized test scores of children in stepfamilies. Stepsibling structure is a categorical variable coded 0 if children have lived in a nonblended family until having taken the standardized test scores at the end of primary education (reference category); 1 if children never lived with a stepsibling whilst in a stepfamily; 2 if children lived with a stepsibling for at least one year whilst in a stepfamily. Socioeconomic control variables (age of mother, father's log gross income, mother's log gross income, parental education, neighborhood FE) are measured at birth, while stepfamily control variables (stepparent parental status, resident family size, number of years in a nonblended family, number of nonresident half-siblings), as well as family size, are measured at the time children took the standardized test scores at the end of primary education. Note that the variable stepparent parental status is a categorical variable but the category stepparent has at least one child is dropped because of multicollinearity with stepsibling structure. Standard errors are clustered at the full sibling level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

small and not significant differences across stepsibling structures. When the full set of covariates is accounted for in column 4, having a resident stepsiblings appears to be associated with higher deficits than having lived in stepfamilies without resident stepsiblings.[18]

[18]It might be that stepsiblings more likely move into households where there was a low level of stress or conflict

Nonetheless, this difference is not large in magnitude and is not significant by means of a test of equality of coefficients. Therefore, both individual-level and sibling-level estimations are suggestive that stepsibling composition does not play a meaningful role in influencing the test scores obtained by children in stepfamilies.

We should highlight that the sample of children in nonblended families is much larger and more heterogeneous than the sample of children in stepfamilies, which might cause bias in the estimation of the coefficients of interest since the estimated effects might depend on the statistical specification of the model (Ho et al., 2007). As a robustness check, we pre-process our data using Coarsened Exact Matching (Iacus et al., 2011, 2012), and match children in stepfamilies to children in nonblended families with similar or exact same characteristics at birth (for more details, see Table 3.A3 in the online supplement). Using CEM, we are able to match 59,945 children in stepfamilies with 707,192 children in nonblended families. Our results, shown in Table 3.A3 in the online supplement, suggest no significant difference in standardized test scores between children in stepfamilies who lived with a stepsibling during childhood and their counterparts who never lived with a stepsibling, thus confirming our main findings of Table 3.3.

5.3 Additional analyses

5.3.1 Heterogeneity by exposition length

Although children in stepfamilies subjected to complex sibling structures do not report, on average, larger deficits in test scores than children in stepfamilies subjected to simpler sibling compositions, heterogeneity might arise depending on how long children were exposed to the complex sibling structure. To test heterogeneity by time length, we redefine the key variable of interest in both the half-sibling and the stepsibling analyses as follows. In the halfsibing analysis, we construct a categorical variable for the age at which children experienced the birth of a half-sibling. If no resident half-sibling was born, we code this variable 0; if a half-sibling was born, we create three categories accounting for whether children were aged 1-4, 5-8, or 9-14 years old at childbirth.[19] In the case of the stepsibling analysis, we redefine the main categorical variable of interest by further dividing children who lived with a stepsibling into two categories: i) always lived with a stepsibling while in a stepfamily; ii) lived for a certain period (but not always) with a stepsibling while in a stepfamily. We show our results in Table 3.4 below, where we report in Panel A the findings regarding the half-sibling analysis and in Panel B the findings regarding the stepsibling analysis.

Our results do not reveal large heterogeneity by the length of time spent in each sibling

before the separation, so that accounting for unobserved characteristics produces a negative difference between the two groups of interest.

[19]Note that our results are robust to including dummies for the age at which children experienced parental separation.

Table 3.4: Complex sibling structures and children's outcomes, by length of exposition

Panel A	Individual sample		Sibling sample	
	(1)	(2)	(3)	(4)
Age at half-siblings birth (ref: no half-siblings)				
(1-4)	-0.080***	-0.037	-0.030	-0.001
	(0.022)	(0.030)	(0.091)	(0.108)
(5-8)	-0.055***	-0.013	-0.019	-0.001
	(0.012)	(0.023)	(0.073)	(0.093)
(9-14)	-0.078***	-0.015	-0.039	-0.026
	(0.012)	(0.020)	(0.065)	(0.084)
Individual controls	Yes	Yes	Yes	Yes
Family controls	No	Yes	No	Yes
Stepfamily controls	No	Yes	No	Yes
Sibling FE	No	No	Yes	Yes
Observations	62,665	62,665	19,913	19,913
R-squared adjusted	0.009	0.078	0.345	0.345

Panel B	Individual sample		Sibling sample	
	(1)	(2)	(3)	(4)
Stepsibling structure (ref: nonblended family)				
Never with stepsibling (i)	-0.264***	-0.163***	-0.112***	-0.096**
	(0.005)	(0.007)	(0.025)	(0.034)
Always with stepsibling (ii)	-0.217***	-0.132***	-0.101	-0.097
	(0.016)	(0.016)	(0.060)	(0.060)
Some time with stepsibling (iii)	-0.284***	-0.164***	-0.151*	-0.133
	(0.018)	(0.017)	(0.069)	(0.070)
Individual controls	Yes	Yes	Yes	Yes
Family controls	No	Yes	No	Yes
Stepfamily controls	No	Yes	No	Yes
Sibling FE	No	No	Yes	Yes
p-value from tests of equality of coefficients:				
$H_0 : \hat{\beta}_i = \hat{\beta}_{ii}$	**0.004**	0.068	0.864	0.991
$H_0 : \hat{\beta}_{ii} = \hat{\beta}_{iii}$	**0.005**	0.160	0.538	0.656
$H_0 : \hat{\beta}_i = \hat{\beta}_{iii}$	0.278	0.939	0.572	0.601
Observations	1,313,984	1,313,984	839,514	839,514
R-squared adjusted	0.026	0.101	0.383	0.384

Notes: The table shows the effect of complex sibling structures on the standardized test scores of children in stepfamilies, by the length of exposition to the complex sibling structures. Panel A reports half-sibling effects by child age at half-sibling's birth. Panel B reports stepsibling effects distinguishing between children who always lived with stepsiblings while in a stepfamily and children who lived for a certain period (but not always) with stepsiblings while in a stepfamily. Individual controls include sex and indicators for year of birth in sibling-level models; sibling-level covariates plus ethnicity in individual-level models. Family controls include birth order, mother's log gross income, father's log gross income in sibling-level models; sibling-level covariates plus age of mother, family size, parental education, and indicators for neighborhood at birth in individual-level models. Stepfamily controls include no. of nonresident half-siblings, and resident family size in sibling-level models; sibling-level covatiates plus no. of years in nonblended family, and stepparent parental status in individual-level models. Standard errors are clustered at the full sibling level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

structure. In Panel A, when the full set of covariates is included in column 2, children in stepfamilies with half-sibling do not obtain significantly different scores depending on the age in which the half-sibling was born. All estimated coefficients are close to zero and insignificant. These findings are confirmed by the sibling fixed-effect models reported in the last two columns of the panel, which show no significant effect of half-sibling birth on any age category.

Turning to Panel B, when the full set of covariates is accounted for in the individual-level model, children in stepfamilies who always lived with stepsiblings while in a stepfamily appear to obtain smaller deficits in test scores than children in stepfamilies who lived for

a certain period (but not always) with stepsiblings. Nonetheless, this difference (amounting to .032 standard deviations in absolute value) is not statistically significant by means of a test of equality of coefficients (p-value=0.160). When common family characteristics are accounted for in the last two columns of the panel, the difference remains virtually unchanged (albeit coefficients are often statistically insignificant because of lower power) and statistically insignificant. Overall, our results are suggestive that even when different exposition lengths are considered, children in stepfamilies subjected to complex sibling structures do not perform significantly different than children in stepfamilies subjected to simple sibling structures.

5.3.2 The estrangement hypothesis

So far, our results suggested no impact of the birth of a (resident) half-sibling on children's educational outcomes in primary education. However, children might react not only to the births occurring within the family unit but also to the births taking place outside the family unit to the nonresident parent. According to the estrangement hypothesis, stepchildren having half-sibling from both parents will have the largest learning deficits in school, followed by stepchildren with half-sibling only from the resident parent and stepchildren with half-sibling only from the nonresident parent. To test this hypothesis, we constructed a new variable of interest, which we coded 0 if stepchildren do not have half-sibling, 1 if stepchildren have at least one half-sibling from the resident parent, 2 if stepchildren have at least one half-sibling from the nonresident parent, 3 if stepchildren have half-sibling from both parents. The results are presented in Table 3.A4 in the online supplement.

Our findings do not reveal heterogeneity according to half-sibling composition. When the full set of covariates is included in column 2, all estimated coefficients appear close to 0 and are not meaningfully different from each another. Accounting for observed and unobserved family characteristics in the last two columns of Table 3.A4 confirms the findings obtained at the individual level, as differences in effect sizes are not meaningful (nor statistically significant by means of a test of equality of coefficients). Overall, our results suggest no role of the estrangement hypothesis in driving the school performance of children in stepfamilies with half-sibling.

5.3.3 The birth of a half-sibling versus the birth of a full sibling

Although having a half-sibling does not significantly impact the test scores of children in stepfamilies, it is interesting to bound the half-sibling effect by further investigating the effect that having a sibling normally exerts on children in nonblended families. For this purpose, we construct a new binary variable of interest coded 1 if the focal child had a younger full sibling at the time of the test, and 0 otherwise. We show our results in Panel A of Table 3.A7 in the online supplement, where we report in columns 1 and 2 the models estimated on the full sample of children in nonblended families, and in columns 3 to 5 the

models estimated on subgroups constructed according to birth order and family size. More specifically, in the last three columns of the panel we compare the standardized test scores of the last-born child in a family of size (n) to the standardized test scores of the second-to-last born child in a family of size ($n+1$).

Consistent with previous literature examining birth order effects (e.g. Black et al., 2005), our findings suggest a positive association between having a younger full sibling and standardized test scores. When the full set of covariates is accounted for in column 2, the test scores of children with younger full siblings are .184 standard deviations higher than the test scores of children without younger full siblings (i.e. last in the birth order). Columns 3 to 5 further suggest that children benefit from 'not being the last ones' in the family, with the positive effect of having a younger sibling monotonically decreasing as birth order increase.

To compare full sibling and half-sibling effects, Panel B of Table 3.A7 replicates the estimation models discussed above to the sample of children in stepfamilies. The first two columns of the panel report the coefficients obtained in our main analysis in Table 3.2. The last three columns show half-sibling effects on subgroups once more constructed according to family size and birth order. Unlike in nonblended families, the presence of a (half) sibling in stepfamilies is not associated with a meaningful improvement in children's standardized test scores. In particular, the birth of a half-sibling has no effect on the school performance of first-born children, only slightly positively affects second-born children, and negatively affects third-born children.[20] Overall, our results are suggestive that having a younger sibling with whom only one biological parent is shared does not generate the same positive effects on test scores as having a sibling with whom both biological parents are shared. While in principle these effects might indicate a negative difference between half-siblings' and full siblings' effects, in practice, they should be read bearing in mind that children from stepfamilies will likely never have another younger full sibling. Therefore, the effect of the birth of a half-sibling should be interpreted as not detrimental compared with the counterfactual case in which no half-siblings are born.

5.4 Complex sibling structures and secondary school outcomes

Table 3.5 reports the influence of complex sibling structures on children's likelihood to obtain a high school diploma. Columns 1 and 2 show the association between diploma attainment and half-sibling birth, whereas columns 3 and 4 report the association between diploma attainment and stepsibling composition. Note that these outcomes are estimated for the subsample of children that are old enough to have finished high school (born in 1995-2003) and that only individual sample analyses are reported due to the inability to

[20] With respect to the latter estimate, however, it is possible that the effect may be due to multiple testing. Indeed, constructing the q-values as in Anderson (2008) using the p-values obtained from regressions that include all covariates in Table 3.2, Table 3.3, Table 3.4, Table 3.5., Table 3.A4, and Table 3.A7 reveals that the coefficient is no longer significant at a 5% significance level (q-value=0.067).

estimate sibling fixed effect models. Our findings align with the main findings reported in Tables 3.2 and 3.3. When the full set of covariates is included, having had a resident half-sibling in childhood does not significantly influence the likelihood of graduating from high school on first attempt (column 2). Similarly, having lived in a stepfamily with a stepsibling in childhood is not reflected in significantly different scores than having lived in a stepfamily without stepsiblings (column 4).[21] In the appendix, we also explore the association between complex sibling structures and dropout likelihoods (see Table 3.A8), finding consistent results as the ones shown in Table 3.5.

Overall, our estimates suggest that living in complex sibling structures is no more harmful to children than experiencing complex family structures on both primary and secondary school outcomes. In additional analyses, we also explore the association between complex sibling structures and the likelihood of enrolling in higher education shortly after high school (see Table 3.A10). Our results show no effect of complex sibling structures on tertiary education enrolment.

Table 3.5: Complex sibling structures and diploma attainment in secondary education

Panel A	Diploma attainment (1)	(2)	*Panel B*	Diploma attainment (3)	(4)
			Stepsibling structure (ref: nonblended family)		
Halfsibling (1 is yes)	-0.019***	-0.012	Never lived with stepsibling	-0.035***	-0.022***
	(0.004)	(0.010)		(0.002)	(0.003)
			Lived with stepsibling	-0.027***	-0.018***
				(0.005)	(0.005)
Individual controls	Yes	Yes	Individual controls	Yes	Yes
Family controls	No	Yes	Family controls	No	Yes
Stepfamily controls	No	Yes	Stepfamily controls	No	Yes
Observations	32,385	32,385	Observations	647,602	647,124
R-squared adjusted	0.006	0.013	R-squared adjusted	0.009	0.031
			p-values, *t-test*	0.096	0.419

Notes: The table shows the effect of complex sibling structures formed in childhood (observed up to the year children took the standardized test at the end of primary education) on the likelihood that children in stepfamilies obtain a high school diploma. Panel A shows the association between half-sibling birth and diploma likelihoods, whereas Panel B reports the association between stepsibling composition and diploma likelihoods. Individual controls include sex, ethinicity, and indicators for year of birth in Panel A and in Panel B. Family controls include birth order, mother's log gross income, father's log gross income, age of mother, family size, parental education, and indicators for neighborhood at birth in Panel A and in Panel B. Stepfamily controls include no. of nonresident half-siblings, stepparent parental status, no. of years in nonblended family, and resident family size in Panel A; no. of nonresident half-siblings, resident family size, binary indicator for whether stepparent has own children in Panel B. Standard errors are clustered at full sibling level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

6 Conclusion

This chapter examined the effect of the birth of a half-sibling and the presence of stepsiblings on children's educational outcomes in primary (using standardized test scores) and secondary education (using diploma attainment). Relying on detailed and objective popu-

[21] As an additional analysis, it might be interesting to estimate the effect on diploma attainment of nontraditional siblings born/entering the family while the stepchildren attend secondary education, taking into account children's test scores in primary education. Because this analysis would estimate the effects of sibling composition on children aged 13-17 at the time of the 'event' (whereas in our analysis stepchildren all attend primary education when 'treated'), we refrain from including the analysis in this chapter. However, future research exploring the effects on stepchildren of nontraditional siblings born/entered during secondary education might consider implementing this test.

lation data from the Netherlands, we reconstructed the family and the sibling structures of children born in 1995–2009 from birth until 2021. We compared children in stepfamilies without half-sibling to children in stepfamilies with half-sibling, and distinguished stepfamilies according to their stepsibling composition. In addition to individual-level models, the setting allowed us to estimate sibling fixed effect models by exploiting family units where full siblings were subjected to different sibling structures at the time of the standardized tests in primary education.

Our findings showed no role played by complex sibling structures in influencing children's school performance. Once the socioeconomic background was taken into account, children in stepfamilies who experienced the birth of a resident half-sibling were shown to obtain scores in the standardized tests that were not statistically different than those obtained by children in stepfamilies without half-sibling. This null effect was confirmed by sibling fixed effect models, which showed no impact of the birth of a half-sibling on children's test scores. Further, children in stepfamilies who lived with stepsiblings in childhood were found to perform as well on standardized test scores as children in stepfamilies who never lived with stepsiblings, in both individual- and sibling-level analyses. Complex sibling structures also did not appear to influence children's educational achievements in secondary school, as neither the presence of half-sibling nor the presence of stepsiblings was found to significantly impact the frequency at which children in stepfamilies obtained a high school diploma.

Regarding the possible mechanisms behind our findings, our analysis provides a number of insights into possible explanations. Because we do not find educational deficits associated with having nontraditional siblings, we rule out that stepchildren receive fewer resources from their caregivers once half-sibling or stepsiblings enter the household. Further, because we do not find significant differences in effect sizes according to whether half-sibling were born to nonresident parents, resident parents, or both resident and nonresident parents, we also rule out that stepchildren may feel estranged from their family once a half-sibling is born. Nonetheless, examining how the birth of a full sibling impacts older children in nonblended families reveals that having a younger full sibling is positively associated with older children's standardized test scores. This suggests that the birth of a sibling with whom one shares one biological parent does not benefit the existing children in the same way as the birth of a sibling with whom one shares both biological parents.

It might be that children who experienced parental separation develop an emotional detachment that leads to weak emotional bonds with any new nontraditional sibling entering the family unit. This emotional detachment may be consequent to parental separation and independent of the presence of any nontraditional sibling. Previous literature showed that children with divorced parents have weaker emotional bonds with both their mother and father (Amato and Sobolewski, 2001). The deterioration of the child-parent relationship might lead children in stepfamilies not to feel an attachment to the newly born

half-sibling, and to be indifferent to the presence of stepsiblings in the household. By contrast, children in nonblended families may benefit from having a younger sibling, as they may develop social and interpersonal skills by acting as mentors or helpers towards their younger siblings (Downey and Condron, 2004). Unfortunately, our data do not provide us with information enabling us to test this hypothesis. Nonetheless, given the increase in family complexity witnessed by virtually all developed countries, our findings are reassuring in that adding sibling complexity to family units does not lead to greater educational difficulties for children.

From a policy perspective, our findings provide useful elements to consider when discussing policies that might regulate new types of family structures such as stepfamilies. So far, stepfamilies have been either completely disregarded at the institutional level or have been recognized but inadequately (Stewart and Timothy, 2020). This may be due to social stigma and the perception of stepfamilies as dysfunctional, which stems partly from cultural influences from literature and folklore, and partly from social influences due to the deviation of stepfamilies from the archetypal two-parent family where children only have full siblings (Saint-Jacques et al., 2020; Valiquette-Tessier et al., 2016). Our findings show that deviating from the normative model of having only full siblings (or of living with biological parents only) does not bring additional hardships to children. This might be key to consider when addressing the rights and needs of stepfamily members in regulations that will become essential given how prominent stepfamilies are in all Western countries and how likely they are to become even more widespread because of their increasing social acceptance by youngsters (Stewart and Timothy, 2020; Taylor and Keeter, 2010).

Appendix A: Supplementary Tables and Figures

Table 3.A1: Descriptive statistics of sibling level data

		Stepfamilies			
	Nonblended families	Resident half-sibling		Resident stepsibling	
		No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)
Sex (1 is male)	0.505	0.493	0.491	0.492	0.493
Age of mother at birth	31.094	29.741	27.227†	29.146	30.048‡
Family size	2.774	2.372	2.305†	2.36	2.475‡
Resident family size	2.774	2.372	3.521†	2.709	2.657‡
Birth order	1.877	1.738	1.726	1.783	1.92‡
Ethnicity					
Both parents born in NL	0.773	0.828	0.79†	0.814	0.827
One parent NL, other not NL	0.119	0.144	0.159	0.149	0.147
Both parents not NL	0.108	0.028	0.051	0.037	0.026
Father gross income at birth	60,540.612	51,938.207	46,501.916†	50,780.121	52,997.877‡
Mother gross income at birth	21,202.954	21,320.01	18,895.536†	20,522.076	21,903.561‡
Parental education (1 diploma SE)	0.911	0.904	0.877	0.894	0.914
No. of years in an intact family at test	13.18	6.662	5.273†	6.611	6.704‡
No. of nonsident halfsiblings at test	0	0.214	0.428†	0.26	0.224
No. of stepparent's own children					
Zero	0	0.4	0.753†	0.561	0‡
At least one child	0	0.6	0.247†	0.439	1‡
Stepsibling structure:					
Never lived with stepsibling	0	0.855	0.931†	1	0‡
Lived with stepsibling	0	0.145	0.069†	0	1‡
Birth of halfsibling (1 is yes)	0	0	1†	0.287	0.151‡
Outcome variables					
Std test scores	0.121	−0.096	−0.169†	−0.131	−0.089‡
Diploma attainment (1 is yes)	0.935	0.918	0.896	0.913	0.915
Observations	817,624	14,125	5,788	19,166	2,724

Notes: †: the mean for children in column (3) is significantly different at the 5 percent level from the mean for children in column (2) by means of a two-tailed z-test. ‡: the mean for children in column (5) is significantly different at the 5 percent level from the mean for children in column (4) by means of a two-tailed z-test.

Table 3.A2: Complex family structures and standardized test scores

	Individual sample		Sibling sample	
	(1)	(2)	(3)	(4)
Family structure (ref: nonblended family)				
Single family	-0.203***	-0.151***	-0.111***	-0.112***
	(0.003)	(0.003)	(0.009)	(0.009)
Stepfamily	-0.272***	-0.131***	-0.121***	-0.120***
	(0.004)	(0.004)	(0.016)	(0.016)
Sex (1 is male)	-0.006***	-0.009***	-0.005*	-0.005*
	(0.002)	(0.002)	(0.002)	(0.002)
Ethnicity (ref: both parents born in NL)				
One parent NL, other not NL	0.051***	0.001		
	(0.003)	(0.003)		
Both parents not NL	-0.392***	-0.118***		
	(0.003)	(0.004)		
Age of mother at birth		0.027***		
		(0.000)		
Family size		0.087***		
		(0.001)		
Birth order		-0.138***		-0.100***
		(0.001)		(0.003)
Fathers log gross income		0.017***		-0.000
		(0.000)		(0.001)
Mothers log gross income		0.004***		0.001*
		(0.000)		(0.001)
Parental education (1 diploma SE)		0.527***		
		(0.003)		
Constant	0.126***	-1.401***	0.112***	0.292***
	(0.001)	(0.010)	(0.001)	(0.012)
Birth year FE	Yes	Yes	Yes	Yes
Neighbourhood FE	No	Yes	No	Yes
Sibling FE	No	No	Yes	Yes
Observations	1,445,046	1,445,046	919,193	919,193
R-squared adjusted	0.030	0.103	0.383	0.384

Notes: The table shows the effect of complex family structures (i.e. single families or stepfamilies) experienced after parental separation on children's standardized test scores at the end of primary education. Sibling fixed effect models are estimated by exploiting differences in family structures among full siblings in family units where an older full sibling took the standardized test while in a nonblended family, and a younger full sibling took the standardized test while in a single family or a stepfamily. Standard errors are clustered at full sibling level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.A3: Stepsibling composition and standardized test scores using Coarsened Exact Matching (CEM)

	Without covariates (1)	With covariates (2)
Stepsibling structure (ref: nonblended family)		
Never lived with stepsibling	−0.145***	−0.138***
	(0.005)	(0.007)
Lived with stepsibling	−0.125***	−0.129***
	(0.012)	(0.012)
Observations	767,137	767,137
p-value, *t*-test	0.749	0.304
R-squared adjusted	0.002	0.126

Notes: The table shows the effect of having had a resident stepsibling on standardized test scores, when data are pre-processed using coursed exact matching. Column 1 shows results with no control variable included, whereas column 2 reports the results obtained when including all the covariates of Table 3.3. Children in nonblended families are matched to children in stepfamilies on characteristics observed at birth (and thus preceding parental separation). We coarsen father's log gross income and mother's gross income into deciles of the distribution; age of mother at birth into 5 categories (less than 25, 25–30, 30–35, 40–45, and more than 45), family size into three categories (1, 2, 3 or more); birth order into three categories (1, 2, 3 or more). For sex, ethnicity, year of birth, and parental education, we look for an exact match. To maximize the number of children in stepfamilies that we are able to match to children in nonblended families, we do not match on childneighbourhood at birth. Standard errors are clustered at full sibling level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.A4: Heterogeneity by half-sibling composition and children's outcomes

	Individual sample		Sibling sample	
	(1)	(2)	(3)	(4)
Halfsibling composition (ref: no half-sibling)				
Half-sibling composition (ref: no half-siblings)				
HS from nonresident parent	−0.001	0.014	−0.041	−0.037
	(0.013)	(0.014)	(0.070)	(0.070)
HS from resident parent	−0.046***	−0.004	−0.020	−0.014
	(0.010)	(0.020)	(0.074)	(0.095)
HS from both parents	−0.087***	−0.021	−0.033	−0.024
	(0.013)	(0.023)	(0.090)	(0.102)
Individual controls	Yes	Yes	Yes	Yes
Family controls	No	Yes	No	Yes
Stepfamily controls	No	Yes	No	Yes
Sibling FE	No	No	Yes	Yes
Observations	62,665	62,665	19,913	19,913
R-squared adjusted	0.009	0.078	0.345	0.345

Notes: The table shows the effect on standardized test scores of having a half-sibling depending on whether the half-sibling is born to the nonresident parent, the resident parent, or both. Sibling fixed effect models are estimated by exploiting differences in half-sibling structures among full siblings at the time of the standardized test in primary education. Individual controls include sex and indicators for year of birth in sibling-level models; sibling-level covariates plus ethnicity in individual-level models. Family controls include birth order, mother's log gross income, father's log gross income in sibling-level models; sibling-level covariates plus age of mother, family size, parental education, and indicators for neighborhood at birth in individual-level models. Stepfamily controls include resident family size in sibling-level models; sibling-level covatiates plus no. of years in nonblended family, and stepparent parental status in individual-level models. Standard errors are clustered at full sibling level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.A5: Complex sibling structures and standardized test scores controlling for school fixed effects

Panel A	Individual sample (1)	Sibling sample (2)	*Panel B*	Individual sample (3)	Sibling sample (4)
			Stepsibling structure (ref: nonblended family)		
Halfsibling (1 is yes)	-0.008	-0.069	Never lived with stepsibling	-0.155***	-0.098***
	(0.020)	(0.104)		(0.007)	(0.032)
			Lived with stepsibling	-0.141***	-0.116***
				(0.011)	(0.051)
Individual controls	Yes	Yes	Individual controls	Yes	Yes
Family controls	Yes	Yes	Family controls	Yes	Yes
Stepfamily controls	Yes	Yes	Stepfamily controls	Yes	Yes
School FE	Yes	Yes	School FE	Yes	Yes
Observations	62,665	18,696	Observations	1,313,984	839,444
R-squared	0.335	0.702	R-squared	0.146	0.674
			p-values, *t-test*	0.304	0.750

Notes: The table shows the effect of complex sibling structures on children's standardized test scores when including indicators of the school that children attended at the time of the test. Standard errors are clustered at the fullsibling level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.A6: Stepsibling effects and standardized test scores estimated on the subpopulation of children who lived for at most three years in a single family before stepfamily formation

	Individual sample		Sibling sample	
	(1)	(2)	(3)	(4)
Stepsibling structure (ref: nonblended family)				
Never lived with stepsibling	−0.263***	−0.170***	−0.108***	−0.087**
	(0.005)	(0.008)	(0.026)	(0.034)
Lived with stepsibling	−0.248***	−0.144***	−0.106*	−0.097*
	(0.014)	(0.014)	(0.052)	(0.053)
Individual controls	Yes	Yes	Yes	Yes
Family controls	No	Yes	No	Yes
Stepfamily controls	No	Yes	No	Yes
Sibling FE	No	No	Yes	Yes
p-value, *t*-test	0.300	0.085	0.969	0.866
Observations	1,298,996	1,298,996	835,954	835,954
R-squared	0.026	0.100	0.383	0.384

Notes: The table shows the effect of having lived with a stepsibling in childhood on the standardized test scores of children in stepfamilies when excluding children who lived for more than three years in a single family before entering a stepfamily. Standard errors are clustered at the full sibling level. Individual controls include sex and indicators for year of birth in sibling-level models; sibling-level covariates plus ethnicity in individual-level models. Family controls include birth order, mother's log gross income, father's log gross income in sibling-level models; sibling-level covariates plus age of mother, family size, parental education, and indicators for neighborhood at birth in individual-level models. Stepfamily controls include resident family size in sibling-level models; sibling-level covatiates plus no. of years in nonblended family, and binary indicator for whether stepparent has own children in individual-level models. Standard errors are clustered at full sibling level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.A7: Complex sibling structures and children's outcomes, by family size and birth order

Panel A	Full sample		Birth order = 1 Family size ∈ [1,2]	Birth order = 2 Family size ∈ [2,3]	Birth order = 3 Family size ∈ [3,≥4)
	(1)	(2)	(3)	(4)	(5)
Younger full sibling's birth (1 is yes)	0.111***	0.184***	0.169***	0.146***	0.088***
	(0.002)	(0.002)	(0.004)	(0.003)	(0.007)
Individual controls	Yes	Yes	Yes	Yes	Yes
Family controls	No	Yes	Yes	Yes	Yes
Observations	1,251,319	1,251,319	404,369	441,338	157,177
R-squared	0.028	0.107	0.112	0.112	0.167
Panel B	Full sample		Birthorder = 1 Family size = 1 Res family size ∈ [1,2]	Birthorder = 2 Family size = 2 Res family size ∈ [2,3]	Birthorder = 3 Family size = 3 Res family size ∈ [3,≥4)
	(1)	(2)	(3)	(4)	(5)
Younger half-sibling's birth (1 is yes)	-0.060***	-0.012	0.002	0.050**	-0.082*
	(0.009)	(0.020)	(0.018)	(0.019)	(0.037)
Individual controls	Yes	Yes	Yes	Yes	Yes
Family controls	No	Yes	Yes	Yes	Yes
Stepfamily controls	No	Yes	Yes	Yes	Yes
Observations	62,665	62,665	13,962	18,894	5,295
R-squared	0.007	0.199	0.058	0.055	0.070

Notes: The table shows the association between having a younger sibling and standardized test scores at the end of primary education. Panel A reports the associations for the birth of a full sibling. Panel B reports the associations for the birth of a half-sibling. Individual controls include sex, ethinicity, and indicators for year of birth in Panel A and in Panel B. Family controls include mother's log gross income, father's log gross income, age of mother, parental education, and indicators for neighborhood at birth in Panel A and in columns 3 to 5 of Panel B; column 2 of Panel B additionally includes family size and birth order. Stepfamily controls include no. of nonresident half-siblings, stepparent parental status, and no. of years in nonblended family in columns 3 to 5 of Panel B; column 2 of Panel B additionally includes resident family size. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.A8: Complex sibling structures and dropout likelihood in secondary education

Panel A	Dropout likelihood (1)	(2)	*Panel B*	Dropout likelihood (3)	(4)
			Stepsibling structure (ref: nonblended family)		
Halfsibling (1 is yes)	0.012***	0.004	Never lived with stepsibling	0.029***	0.020***
	(0.003)	(0.007)		(0.001)	(0.002)
			Lived with stepsibling	0.019***	0.013***
				(0.003)	(0.003)
Individual controls	Yes	Yes	Individual controls	Yes	Yes
Family controls	No	Yes	Family controls	No	Yes
Stepfamily controls	No	Yes	Stepfamily controls	No	Yes
Observations	36,734	36,734	Observations	741,817	741,817
R-squared adjusted	0.004	0.020	R-squared adjusted	0.004	0.013
			p-values, *t-test*	0.005	0.057

Notes: The table shows the effect of complex sibling structures on the likelihood that children in stepfamilies drop out of school before obtaining a high school diploma. Dropout likelihood is a binary variable coded 1 if the student dropped out of school before obtaining a high school diploma and 0 otherwise. Panel A shows the association between half-sibling birth and dropout likelihoods, whereas Panel B reports the association between stepsibling composition and dropout likelihoods. Individual controls include sex, ethinicity, and indicators for year of birth in Panel A and in Panel B. Family controls include birth order, mother's log gross income, father's log gross income, age of mother, family size, parental education, and indicators for neighborhood at birth in Panel A and in Panel B. Stepfamily controls include no. of nonresident half-siblings, binary indicator for whether stepparent has own children, no. of years in nonblended family, and resident family size in Panel A; no. of nonresident half-siblings, resident family size, binary indicator for whether stepparent has own children in Panel B. Standard errors are clustered at full sibling level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.A9: Stepsibling composition and standardized test scores, by different levels of stepparent parental status

Panel A	Individual sample (1)	Sibling sample (2)	*Panel B*	Individual sample (3)	Sibling sample (4)
Stepsibling structure (ref: nonblended family)			Stepsibling structure (ref: nonblended family)		
(i) Never lived with stepsibling	-0.143***	-0.109***	(i) Do not have stepsiblings	-0.123***	-0.129***
	(0.006)	(0.025)		(0.007)	(0.039)
(ii) Lived with stepsibling	-0.149***	-0.116*	(ii) Lived with stepsibling	-0.146***	-0.112*
	(0.012)	(0.050)		(0.012)	(0.050)
			(iii) Never lived with stepsibling	-0.163***	-0.094**
				(0.007)	(0.033)
Stepparent parental status' controls	No	No	Stepparent parental status' controls	No	No
Additional controls	Yes	Yes	Additional controls	Yes	Yes
Sibling FE	No	Yes	Sibling FE	No	Yes
p-value from tests of equality of coefficients:			*p*-value from tests of equality of coefficients:		
$H_0 : \hat{\beta}_i = \hat{\beta}_{ii}$	0.649	0.892	$H_0 : \hat{\beta}_i = \hat{\beta}_{ii}$	0.072	0.790
			$H_0 : \hat{\beta}_{ii} = \hat{\beta}_{iii}$	0.221	0.746
Observations	1,313,984	839,514	Observations	1,313,984	839,514
R-squared adjusted	0.101	0.384	R-squared adjusted	0.101	0.384

Notes: The table shows the effect of stepsibling composition on standardized test scores when the model is specified differently relative to the categorical variable that accounts for the stepparent parental status. Specifically, *Panel A* shows the effect of living with stepsiblings when the categorical variable 'stepparent parental status' is not included. *Panel B* shows the effect of living with stepsibings when the variable of interest 'Never lived with stepsibling' defined in Equation 4.3 is further split according to whether the stepparent did not have children with previous partners (i) or had children with previous partners but the children never moved into the household (iii). Additional controls include in the sibling sample: sex, indicators for year of birth, birth order, mother's log gross income, father's log gross income, resident family size, indicator for halfsibling's birth, no. of non-resident half-siblings. Additional controls include in the individual sample the sibling-level covariates plus: ethnicity, age of mother, family size, parental education, indicators for neighborhood at birth, and no. of years in nonblended family. Standard errors are clustered at full sibling level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.A10: Complex sibling structures and higher education enrolment likelihood

Panel A	Higher education enrolment (1)	(2)	*Panel B*	Higher education enrolment (3)	(4)
			Stepsibling structure (ref: nonblended family)		
Half-sibling's birth (1 is yes)	-0.026**	0.021	Never lived with stepsibling	-0.176***	-0.124***
	(0.009)	(0.026)		(0.004)	(0.007)
			Lived with stepsibling	-0.158***	-0.117***
				(0.012)	(0.012)
Individual controls	Yes	Yes	Individual controls	Yes	Yes
Family controls	No	Yes	Family controls	No	Yes
Stepfamily controls	No	Yes	Stepfamily controls	No	Yes
Observations	13,414	13,414	Observations	265,038	265,038
R-squared adjusted	0.007	0.077	R-squared adjusted	0.019	0.105
			p-values, *t-test*	0.159	0.609

Notes: The table shows the effect of complex sibling structures on the likelihood that children in stepfamilies enrol in higher education at age 19. Higher education enrolment is a binary variable coded 1 if the student, at age 19, is enrolled in a study program in a research-oriented higher education (WO) or a professionally-oriented higher education (HBO), and 0 otherwise. Panel A shows the association between half-sibling birth and higher education enrolment, whereas Panel B reports the association between stepsibling composition and higher education enrolment. Individual controls include sex, ethinicity, and indicators for year of birth in Panel A and in Panel B. Family controls include birth order, mother's log gross income, father's log gross income, age of mother, family size, parental education, and indicators for neighborhood at birth in Panel A and in Panel B. Stepfamily controls include no. of nonresident half-siblings, binary indicator for whether stepparent has own children, no. of years in nonblended family, and resident family size in Panel A; no. of nonresident half-siblings, resident family size, binary indicator for whether stepparent has own children in Panel B. Standard errors are clustered at full sibling level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.A11: Half-sibling birth's expectation and standardized test scores

	Standardized test scores	
	(1)	(2)
Expected half-sibling's birth (1 is yes)	-0.060	-0.007
	(0.076)	(0.074)
Individual controls	Yes	Yes
Family controls	No	Yes
Stepfamily controls	No	Yes
Observations	42,143	42,143
R-squared adjusted	0.006	0.052

Notes: The table shows the influence of expecting a half-sibling's birth on standardized test scores. Expected half-sibling's birth is coded 1 if focal children do not have half-siblings at the test date but will have a half-sibling within the next 270 days (i.e. 9 months) from the test date; 0 if focal children do not have half-siblings at the test date or they will have a half-sibling after 270 days have passed from the test date. 126 children are observed as expecting a half-sibling's birth. Individual controls include sex, ethinicity, and indicators for year of birth. Family controls include birth order, mother's log gross income, father's log gross income, age of mother, family size, parental education, and indicators for neighborhood at birth in Panel A. Stepfamily controls include no. of nonresident half-siblings, binary indicator for whether stepparent has own children, no. of years in nonblended family, and resident family size. Standard errors are clustered at full sibling level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Chapter 4

Early Labor Market Outcomes of Young Adults from Same-Sex Families: Evidence from Population Data.[1]

Abstract

Although most studies suggest that children from same-sex families perform at least as well as children from different-sex families in childhood or adolescence, data limitations have prevented investigation of how children from same-sex families perform in adulthood. Using Dutch longitudinal tax registers, we offer first population-based evidence on how young adults from same-sex families fare when entering the labor market after high school. Our data allow us to follow five cohorts of young adults (born in 1995-1999) from same-sex families each year since birth all the way after high school until 2020. We observe 239,313 young adults who have not enrolled into higher education, of which 605 are young adults from same-sex families (558 from female same-sex families and 47 from male same-sex families). Our results suggest that young adults who lived in a same-sex family perform just as well early on the labor market as young adults from different-sex families. However, we also find considerable differences by the time spent in a same-sex family. Young adults raised by same-sex parents from birth are more employed in part-time occupations and more often choose nontraditional industries (i.e. industries dominated by the opposite sex). Especially boys from same-sex families are more employed in nontraditional industries than boys from different-sex families. We test various mechanisms that may explain our results.

[1]This chapter is based on: Palmaccio, S., Mazrekaj, D., & De Witte, K. (2023). Early Labor Market Outcomes of Young Adults from Same-Sex Families: Evidence from Population Data.

The data for this study are protected by a confidentiality agreement and we are precluded from sharing the data with others. Interested readers can consult the online supplement for information on how to obtain access to the data. For their helpful comments and suggestions, we would like to thank Douglas Allen, Walter Schumm, Paul Sullins, Marie Evertsson, Dieter Verhaest, Kitt Carpenter, participants of the University of Oxford Department of Sociology reading group, and participants of the conferences of the American Sociological Association, the European Sociological Association, the Society of Labor Economists, the European Economic Association, the European Society for Population Economics, the Society of Economics of the Household, and the Lisbon Economics and Statistics of Education, as well as seminar and workshop participants at KU Leuven, University of Birmingham, Vanderbilt University, Hertie School, and participants at the American Economic Association Economics of LGBTQ+ Virtual Seminar.

1 Introduction

Whether same-sex couples should be granted equal parenting rights as different-sex couples is a debated topic in many countries. In the US, despite the legalization of same-sex marriage in 2015 (U.S. Supreme Court case Obergefell v. Hodges, 2015), same-sex couples still experience considerable discrimination in parenting. For instance, private foster care agencies refused to work with same-sex couples in Pennsylvania (Fulton v. City of Philadelphia, 2020), and lesbian married couples were denied motherhood on the child birth's certificate in Indiana (Box v. Henderson, 2020). Uneven support for same-sex couples' parenting rights also occurs in the European Union, where eight countries do not allow same-sex couples to adopt children, and one country – Hungary – recently introduced a constitutional ban on adoptions by same-sex couples (ILGA Europe, 2021). Opponents of same-sex parenting claim that children raised in same-sex families experience substantial hardships during childhood that translate to adverse outcomes in adulthood. However, most studies using either nationally representative census data or administrative population data indicate that children from same-sex families perform at least as well as children from different-sex families on a range of psychological, health and school outcomes (Aldén et al., 2017; Boertien and Bernardi, 2019; Cenegy et al., 2018; Mazrekaj et al., 2020; Reczek et al., 2017; Rosenfeld, 2010; Watkins, 2018). Despite the considerable evidence about how children from same-sex families perform during their childhood or adolescent years, however, few studies have been able to collect data about how children from same-sex families perform in adulthood.

In this chapter, we investigate how young adults who have lived in a same-sex family in their childhood perform on the labor market shortly after they leave high school. Early labor market outcomes are particularly important as young adults are conventionally considered a vulnerable population that is more exposed to difficulties on the labor market (Caliendo and Schmidl, 2016). For instance, in the EU, the unemployment rate in 2021 was more than twice as large among workers aged 15-24 years old (amounting to 16.6%) than for the full population of workers aged 15-75 years old (amounting to 7.0%) (Eurostat, 2022b). Previous research has shown that poor experience early on is likely to have scarring effects on mental health and psychological well-being (McKee-Ryan et al., 2005; Korpi, 1997). In turn, this may negatively affect subsequent labor market performance, leading to lower future wages and a higher probability of long-term unemployment (Gregg and Tominey, 2005; Nordstrom Skans, 2011). Given that compensatory policy measures are still possible for workers of young age, examining the labor market outcomes of young adults from same-sex families at especially this age can be key to designing policies that facilitate their transition into the labor market.

To the best of our knowledge, only Regnerus (2012) has included labor market information in his comparison of children from same-sex and different-sex families on 40 outcomes

using The New Family Structures Study (NFSS) survey. He found that, in the US, children from same-sex families were less full-time employed and more unemployed than children who lived in an intact biological family until the age of 18. However, self-reported information – prone to misreporting and recall bias – spreads doubt on these findings (Cheng and Powell, 2015; Rosenfeld, 2015; Perrin et al., 2013). Cheng and Powell (2015) showed that half of the children from same-sex families never lived with the partner of the same-sex parent, and one tenth never lived with the same-sex parents themselves. Only 51 of the 236 adult children originally included in the sample could be confidently defined as raised for at least one year in a same-sex family. Once the analyses were performed on the newly defined sample, Cheng and Powell (2015) found no significant difference in labor market performance between children from same-sex and different-sex families. Nonetheless, this small number of only 51 children casts doubt on whether the results found in these studies can be generalized to the population of children from same-sex families.

This chapter contributes to the literature by analyzing a range of early labor market outcomes (namely annual earnings, hourly wages, employment, full-time employment and industry choices) using a population dataset of young adults who lived in a same-sex family in their childhood. We use unique administrative longitudinal data from the Netherlands, in which we observe the entire population of children born between 1995 and 1999, which we follow until 2020. We focus on young adults who have not enrolled in higher education after high school, and observe their labor market outcomes at age 21. We identify 605 young adults who lived in a same-sex family in childhood (558 in female same-sex families and 47 in male same-sex families), of which 112 are born in a same-sex family. Unlike the small self-reported sample used in Regnerus (2012), our data allow us to look at the population of children from same-sex families while observing labor market information in the tax registers. Moreover, the Netherlands is a particularly interesting country to study because of the extensive support – cultural and legal – it provides to the LGBTQ+ community. In fact, the Netherlands was among the first countries to recognize registered partnerships for same-sex couples in 1998, and the first country to legalize same-sex marriage in 2001.

As we consider the labor market performance of young workers who have not enrolled in higher education, it is important to already reflect on how we test for the non-random composition of our sample. First, we compare young adults from same-sex families to young adults from different-sex families on the likelihood of enrolling into higher education, finding no significant differences between the two groups. Second, we also study older birth cohorts for which we can observe labor market outcomes after graduating from tertiary education, finding similar results as for the main sample. Both analyses confirm the external validity of the findings towards other cohorts. The lack of significant differences in the probability of enrolling into higher education suggests no selection in the composition of our sample, while the analysis on the earliest birth cohorts suggests that our results are valid for both older ages and for young adults who worked immediately after graduating

from tertiary education.

Our results suggest that young adults from same-sex families earn just as much and are just as likely to be (full-time) employed as young adults from different-sex families. However, these results hide considerable heterogeneity by the time spent in same-sex families. Young adults born in same-sex families (and who spent their entire childhood in a same-sex family) tend to earn less than young adults from different-sex families. These differences occur in part because young adults born in same-sex families are more likely to work part-time. Additionally, they are also more likely to be employed in nontraditional industries (i.e. industries dominated by the opposite sex). This finding is driven by boys born in same-sex families who are predominantly employed in nontraditional industries – traditionally less profitable than male-dominated industries. Our data reject other explanations as predicted by the sex identity theory, discrimination theory, human capital/signalling theory, and family formation theory. We also reject the hypotheses that young adults born in same-sex families follow their mothers' industry choices, lack complementary role models, or lack human capital and are less capable than young adults from different-sex families. Our results are robust to data pre-processing by using Coarsened Exact Matching, and to the inclusion of omitted variable bias by means of a coefficient stability test by Oster (2019).

2 Background

2.1 Earlier research and methodological challenges

The prevailing view in the social science literature is that children from same-sex families perform at least as well as children from different-sex families on a range of psychological, health, and school outcomes (for reviews of earlier studies, see Allen 2013, Manning et al. 2014b, Marks 2012). However, earlier research suffered from two important limitations (Manning et al., 2014b). First, data on same-sex families was often collected through convenience or snowball sampling. As a result, these studies analyzed same-sex parents – mostly white, wealthy and well-educated lesbian parents – who were not representative of the general population. Second, earlier research often relied on samples that were too small to detect statistically significant differences for correct inference.

To overcome these methodological issues, a growing literature has used nationally representative census data (Allen, 2013; Allen et al., 2013; Boertien and Bernardi, 2019; Cenegy et al., 2018; Reczek et al., 2017; Rosenfeld, 2010; Sullins, 2015; Watkins, 2018) or administrative data (Aldén et al., 2017; Kabátek and Perales, 2021; Mazrekaj et al., 2020) to offer population-based evidence of children's performance in same-sex families. With few exceptions (see Allen, 2013; Allen et al., 2013; Sullins, 2015), findings from these studies support the hypothesis that children from same-sex families do not experience significant disadvantages relative to children from different-sex families. Children from same-sex families are found to progress through school at the same pace as children from different-sex fami-

lies (Bernardi et al., 2019; Rosenfeld, 2010; Watkins, 2018), to outperform the latter on test scores in primary education, and are more likely to graduate from high school (Mazrekaj et al., 2020; Kabátek and Perales, 2021). Children from same-sex families also appear to be equally healthy as children from different-sex families (Cenegy et al., 2018; Reczek et al., 2017), have similar behavior problems (Reczek et al., 2017), and may even have a lower probability of developing respiratory diseases when raised by female same-sex parents than children from different-sex families (Aldén et al., 2017). Note that Reczek et al. (2017) and Sullins (2015) find that children from same-sex families experience somewhat higher rates of emotional difficulties relative to children from different-sex families. However, Reczek et al. (2017) speculate that this difference might be driven by the stress caused by social and institutional discrimination, or by higher adoption rates among same-sex couples relative to different-sex couples. Given the high support enjoyed by the LGBT community in the Netherlands, and the low proportion of children adopted by same-sex couples in our sample (only 4), we do not expect this outcome to play a major role in our context.

Nevertheless, earlier research has mainly focused on children's performance in childhood or adolescence. This is because obtaining data on adult performance of children from same-sex families is exceptionally challenging. First, the researcher needs to have enough information to distinguish same-sex families from different-sex families. Second, the data need to cover a sufficiently large period into the future to identify children's adult performance. Finally, the number of observed children from same-sex families must be sufficiently high to produce representative and generalizable findings. Fulfilling these data conditions is especially problematic given that most countries have legally recognized same-sex families as a family structure only in recent years.

Our administrative population data allow us to overcome these issues. Particularly, they include a rich set of labor market outcomes that are measured until 2020. Given that we can reconstruct the family structure of young adults born since 1995, this long time span allows us to observe birth cohorts that are old enough to earn the adult Dutch minimum wage (i.e. age 21 years old or older). Further, our data allow us to observe the entire population of the Netherlands, a country where registered partnerships for same-sex couples have been legal for more than two decades. As such, this chapter provides first evidence using population data on how young adults from same-sex families perform on the labor market.

As a final note, we should emphasize that comparing same-sex to different-sex families implicitly ascribes to different-sex families the role of normative family type (Stacey and Biblarz, 2001). This means that any deviation from different-sex families might implicitly be conceptualized as negative or deficient. While we acknowledge that enforcing such comparison may contribute to reproducing the heteronormativity of different-sex families relative to same-sex families, we also note that it allows to discuss the aspects by which children in same-sex families are somehow expected to be different than children in different-sex families according to heteronormative beliefs.

3 Theoretical mechanisms

Various theories from sociology, psychology and economics predict that young adults from same-sex families are likely to perform either better or worse than young adults from different-sex families on the labor market.[2] These theories are based on internal aspects that commonly characterize same-sex families (i.e. an egalitarian division of household labor among same-sex parents, a non-conforming behavior of children to sex stereotypes, a small family size, and a high human capital), or on external aspects that might affect young adults' labor market performance (such as discrimination on the labor market). Table 4.1 below provides an overview of the theoretical mechanisms discussed in this section and the (positive or negative) relationship they predict between having lived in same-sex families and labor market outcomes.

Table 4.1: Overview of the theoretical mechanisms

Theoretical mechanism	Log gross earnings (1)	Log gross hourly wages (2)	Employment (3)	Full-time employment (4)	Nontraditional industry (5)
Sex identity theory	+ (driven by females)	+ (driven by females)	n/a	n/a	n/a
Family formation theory	+	+	+	+	n/a
human capital/signalling theory	+	+	+	+	n/a
Discrimination theory	-	-	-	-	n/a
Sector-selection theory	+ or -	+ or -	n/a	n/a	+

Notes. This table shows an overview of the relationship between living in same-sex versus different-sex families and the outcome variables, as predicted by the theoretical mechanism discussed in Section 3. Note that we do not expect every mechanism to affect all outcome variables, and we denote unaffected outcome variables with "n/a".

Young adults from same-sex families may perform better than young adults from different-sex families on the labor market because they are likely to grow up in an environment in which gender roles and sex stereotypes are less pronounced (*sex identity theory*). In a review of the literature, Stacey and Biblarz (2001) showed that, unlike in different-sex families, girls and boys in same-sex families frequently play and behave in ways that do not conform to cultural gender norms. Instead, they tend to develop greater interests in more gender-neutral activities. Sex stereotypes have been shown to produce differences in adults' self-confidence, with women generally having lower confidence in their own abilities than men (Bordalo et al., 2019). In turn, distorted perceptions of own abilities affect wages and contribute to the gender pay gap, as overconfident men will earn more than underconfident women, all else equal (Santos-Pinto, 2012). Given that young adults from same-sex families are less exposed to sex stereotypes during childhood, the sex identity theory predicts reduced wage differentials between female and male young adults from

[2]Note that the sex-identity theory, the family formation theory, and the sector-selection theory discussed in this section are named by the authors.

same-sex families relative to wage differentials between female and male young adults from different-sex families. These reduced wage differentials stem from higher performance of females rather than from a lower performance of males. Consequently, overall earnings of young adults from same-sex families would be higher than the earnings of young adults from different-sex families. Given these implications of the sex identity theory, we will analyze earnings separately for females and males from same-sex families. If the difference in earnings is significantly higher when comparing females from same-sex families to females from different-sex families, than when comparing males from same-sex families to males from different-sex families, this would suggest that the sex identity theory holds.

Young adults from same-sex families may also perform better than young adults from different-sex families on the labor market as a result of the way same-sex couples have children (*family formation theory*). Given that the procedures to obtain children for same-sex couples such as adoption, artificial insemination and surrogacy are time-consuming and expensive, the decision to conceive a child may not be as spontaneous for same-sex couples as it may be for different-sex couples. As a result, same-sex families are likely to be of smaller size than different-sex families (Black et al., 2007). This is true also in the Netherlands, where children from same-sex families have significantly fewer siblings than children from different-sex families (Mazrekaj et al., 2020). The relationship between family size and young adults' outcomes is mostly shown to be negative by earlier literature. According to the resource dilution theory, parental resources are finite and the proportion of such resources that can be devoted to every child decreases as the number of children increases (Downey, 2001; Cáceres-Delpiano, 2006), leading to a negative correlation between family size and children's outcomes including, among the others, labor market outcomes (Lampi and Nordblom, 2012). Nevertheless, some studies find that birth order is more important than the number of siblings in determining young adults' outcomes. For example, Black et al. (2005) showed that controlling for birth order makes family size effects negligible, and that young adults with higher birth order tend to earn less and be less full-time employed in adulthood than young adults with lower birth order. Either way, the family formation theory combines the two approaches and predicts that young adults from same-sex families perform better on the labor market than young adults from different-sex families (e.g. by having higher earnings, being more employed or more full-time employed). We hypothesize that the anticipated positive labor market performance is mediated by the higher likelihood of young adults from same-sex families to have lived in smaller families and to be early in the birth order relative to young adults from different-sex families.

A final mechanism by which young adults from same-sex families can be expected to perform better than young adults from different-sex families is related to their school performance. Previous literature has found that children from same-sex families obtain higher test scores in primary education (Aldén et al., 2017; Mazrekaj et al., 2020; Kabátek and Perales, 2021), and are just as likely to progress through school as children from different-

sex families (Boertien and Bernardi, 2019; Rosenfeld, 2010; Watkins, 2018). It is well documented that higher schooling leads to better performance on the labor market (Oreopoulos and Salvanes, 2011), either as a result of a higher human capital (Becker, 1993) (*human capital theory*) or as a result of signalling (Spence, 1973) (*signalling theory*). Therefore, according to human capital/signalling theory, young adults from same-sex families would perform better on the labor market than their peers from different-sex families as a direct consequence of their high school performance. Thus, we hypothesize that the anticipated positive relationship between having lived in a same-sex family and labor market outcomes is mediated by their good high school performance.

Although the aforementioned theories suggest that young adults from same-sex families should perform at least as well, and likely better, than young adults from different-sex families on the labor market, the discrimination and the sector-selection theory provide a contrasting view. The discrimination theory posits that same-sex parents experience substantial prejudice due to their sexual orientation (Stacey and Biblarz, 2001). The discrimination of same-sex parents on the workplace may weaken their possibility to construct family networks on the labor market, which are shown to be relevant for young workers (Kramarz and Skans, 2007; Corak and Piraino, 2011).[3] The social stigma same-sex parents are exposed to may also directly be reflected upon their children. Employers who are against same-sex parenting may discriminate against a job applicant if they are aware that the job applicant has same-sex parents. Moreover, it is a common belief among straight individuals that young adults from same-sex families are more likely to be in a same-sex relationship than young adults from different-sex families (Gato and Fontaine, 2013; Savin-Williams and Ream, 2003), and recent research on the topic confirms this belief to be true (Gartrell et al., 2019). Thus, young adults from same-sex families may be subjected to an additional source of discrimination on the labor market, this time due to their own sexual orientation. We might expect these mechanisms to be particularly strong in rural and small communities, where personal information on sexual orientation and family composition can easily circulate by word of mouth. Although the Netherlands is a highly tolerant society toward LGBTQ rights, it is possible that LGB individuals might still experience minority stress. In particular, there is evidence that, in the Netherlands, minority stress is reported especially by those individuals who do not feel connected with the LGB community (Franssens, 2010; Kuyper and Fokkema, 2011). Given that in rural areas the LBGT community might be less prevalent than in urban areas, it is possible that minority stress might occur more frequently in such areas, thereby reflecting in labor market performance differentials. Therefore, a testable implication of the discrimination theory is that young adults from same-sex families would perform worse on the labor market (e.g. by earning lower wages, being less employed or less full-time employed) in rural communities rather

[3]For instance, young workers are found to frequently work in factories where their parents are also employed or to work with employers for which their parent(s) also worked (Kramarz and Skans, 2007; Corak and Piraino, 2011).

than in urban cities compared to young adults from different-sex families. Nevertheless, we should emphasize that this study is situated in the Netherlands, the first country to have legalized same-sex marriage in 2001, and where the LGBT community enjoys an extensive legal and cultural support. For example, a survey conducted in 2019 revealed that 97% of the Dutch population agreed that gay, lesbian and bisexual individuals should enjoy the same rights as heterosexual individuals, and 92% believed that same-sex marriage should be allowed throughout Europe (Eurobarometer, 2019). Thus, we do not expect that the discrimination theory will constitute a major mechanism affecting the performance of young adults from same-sex families on the labor market.

An important alternative mechanism that predicts nontraditional industry choices on the labor market of young adults from same-sex families relative to young adults from different-sex families is the *sector-selection theory*. As previously discussed, young adults from same-sex families are less exposed to sex stereotypes while growing up (Stacey and Biblarz, 2001). Moreover, earlier literature has shown that same-sex parents are likely to share paid and unpaid work (e.g. housework and childcare) equally between each other, both in female and male same-sex families (Fulcher et al., 2008; Biblarz and Stacey, 2010; Brewster, 2017). By contrast, parents in different-sex families are likely to unevenly divide these commitments (Cotter et al., 2011). Although the differences have become less pronounced throughout the years, the mother figure is still more involved with domestic work and childcare than the father figure, who is instead more invested in paid work (Dribe and Stanfors, 2009). These features are also true in the Netherlands, where a traditional gendered division of labor is still commonly supported (Poortman and Van Der Lippe, 2009), most women only work part-time (OECD, 2019b), and same-sex couples divide the time they spend on paid work more equally than different-sex couples (Jaspers and Verbakel, 2013). Earlier research has found that an egalitarian parental division of household labor and liberal parental attitudes on gender roles are correlated with nontraditional occupational preferences of children (Fulcher et al., 2008; Fulcher, 2011; only within the sample of daughters in Croft et al., 2014).[4] Not conforming to sex-typed cultural norms in childhood and having parents with paid employments who equally share household tasks, we hypothesize that young adults from same-sex families may be more likely employed in nontraditional industries, that is, industries that are atypical from their sex. In practice, this means that boys from same-sex families might be more likely employed in what are traditionally considered as female-dominated industries while girls from same-sex families might be more likely employed in male-dominated industries. Earlier research showed that wage levels are considerably lower in jobs that are mostly occupied by females, while higher in jobs that are predominantly occupied by males (Cohen and Huffman, 2003; Macpherson and Hirsch,

[4]Note that, while also Fulcher et al. (2008) relate parental sexual orientation to children's occupational preferences, their analysis suffers from important limitations such as the use of a small convenience sample–their sample of lesbian families consists of only 33 observations. Moreover, Fulcher et al. (2008) observe the occupational aspirations of children aged 4-6 years old, but do not observe their occupational choices in adulthood.

1995). Therefore, according to this theory the differences in wage levels between boys and girls from same-sex families are theoretically ambiguous, as young adults from same-sex families might earn lower or higher wages than young adults from different-sex families depending on whether boys and girls from same-sex families are influenced differently by the mechanism behind the sector-selection theory.

In sum, the sex identity theory, the birth order theory, and the human capital/signalling theory suggest that young adults from same-sex families will perform at least as well as young adults from different-sex families on the labor market. On the other hand, the discrimination theory predicts a lower labor market performance of young adults from same-sex families. Finally, the sector-selection theory predicts no difference in wage levels but a discrepancy in industry choices between young adults from same-sex and different-sex families. Ultimately, it is an empirical question as to which of these mechanisms dominates. Alternatively, these counterveiling mechanisms may offset each other, leading to no differences in labor market performance between young adults from same-sex and children in different-sex families. Note, however, that we do not expect every mechanism to affect all outcome variables.

4 Data

4.1 Sample construction

We use administrative data collected by Statistics Netherlands that cover the entire Dutch population annually from 1995 to 2020. Each Dutch resident is associated with a unique identifier with which we retrieve demographic characteristics, parental identifiers, address and spousal information. This allows us to reconstruct households and family structure. Specifically, each child is associated to a mother (either female or male) and a father (either male or female) through an identifier. Moreover, every individual is associated to their address of residence, which is annually updated. As we observe the address where every individual lives year by year, we can identify households as the set of individuals sharing the same address in a particular year. Further, we observe identifiers for each individual's partner. We have information on whether two individuals are married, in a registered partnership or are cohabiting. The latter category includes partners who live together and have signed a written agreement of cohabitation, or are registered as tax partners in the Dutch Tax and Customs Administration. We can also distinguish whether children have lived with their biological parents during childhood, with a parent and a stepparent, or with a single parent.[5] Finally, by matching parental and partner identifiers with sex, we are able

[5]Note that two partners might be married or in a registered partnership but live (temporarily) at two separate addresses, for instance if for work reasons they officially reside in different cities. This concerns a small percentage of families in our sample, both for same-sex and different-sex families (around 3% of different-sex families and 3% of same-sex families). Therefore, in this chapter we consider partners having a formal relationship but living at different addresses as part of the same household.

to distinguish whether and how long the child has resided in a same-sex or a different-sex family during childhood.

To study labor market outcomes of young adults who lived in a same-sex family in childhood, we restrict the sample as follows. First, consistent with most of recent literature (Aldén et al., 2017; Boertien and Bernardi, 2019; Mazrekaj et al., 2020; Reczek et al., 2016; Watkins, 2018), we only study young adults raised by couples as our administrative data do not have a measure of sexual orientation.[6][7] Second, we restrict the sample to birth cohorts that are old enough to be entitled to the (adult) statutory minimum wage, that applies in the Netherlands to all employees aged 21 or more. Further, as our panel ends in 2020, we only consider labor market outcomes of young adults aged 21. As such, we study labor market outcomes of young adults born between 1995 and 1999, and aged 21 between 2016 and 2020.[8] Third, we drop observations with missing covariates (17.08 percent of the sample).[9] Finally, to limit the likelihood that young adults might be involved in student jobs and internships, we exclude individuals who have enrolled in higher education after high school. In practice, this means that our sample mostly includes individuals with a vocational educational background rather than a general educational background (more information on the Dutch educational system is provided in Appendix A in the online supplement). However, we find similar results when we observe labor market outcomes at age 23 and we include young adults who finished higher education at that age (see Appendix A and Table 4.A2 in the online supplement for more information).[10] The final sample consists of 239,313 young adults who have never enrolled in higher education before age 21.

4.2 Young adults from same-sex families

As mentioned earlier, our data do not include information on sexual orientation. Rather, we derive whether parents are in a same-sex male or female relationship by the sex of the partner they are residing with.[11] Given that we only observe couples, a household in a given year during childhood can consist of young adults who lived with their biological parents, with adoptive parents or with a parent and a stepparent. If we observe that the

[6] Specifically, our data allows to compare same-sex to different-sex couples, while it does not allow to compare sexual minority to non-sexual minority parents. While all persons in same-sex relationships are sexual minorities, not all persons in different-sex relationships are heterosexual/not sexual minorities.

[7] Nonetheless, we include young adults who have lived with a single parent up to one year to account for potential bureaucratic delays in the registration of the new relationship status of the parents.

[8] To test whether our results are sensitive to the choice of observing labor market outcomes of young adults aged 21, we also perform our analysis on young adults aged 19 and 20 who have not enrolled in higher education (see Table 4.B9 in the online supplement). Our results are virtually unchanged with respect to our main specification in Table 4.2.

[9] However, our results are robust to imputing missing values using Multiple Imputation by Chained Equations (MICE).

[10] We also find similar results when we observe labor market outcomes at age 21 and we include young adults who enrolled in higher education after high school diploma (see Table 4.B1 in the online supplement).

[11] Note that we refrain from attributing sexual identities to families. This means that, for example, we do not label different-sex families as heterosexual families or female same-sex families as lesbian families, since there may be other sexual identities that we do not observe (such as bisexuality).

parents (or the parent and stepparent) the young adult resided with are two men or two women, we conclude that the young adult has lived in a same-sex household in that year. In the analyses, we consider young adults to be from a same-sex family if they have lived for at least one full year in a same-sex family, before turning 16 years old. According to the Dutch law, minors who have reached the age of 16 can request to be emancipated, i.e. to be legally responsible for themselves before they have attained the age of 18. To avoid misclassification because emancipated young adults might no longer live with their parents, we set the age limit at 16 years old.

There are two possible ways young adults may have resided in a same-sex family. On the one hand, they may have entered a same-sex family after having lived part of their childhood with their biological different-sex parents. As such, they necessarily lived through the dissolution of their parents' partnership due to the coming out of one of their parents. On the other hand, they may be born in a planned same-sex family of two male or female same-sex parents (e.g. through fertility treatments or adoption).[12]

In our sample, we identify 605 young adults from same-sex families, of whom around 20% are born in a same-sex family. More generally, one out of two young adults have lived at least 4 years in a same-sex family. It should be noted that, given the low proportion of adopted children among same-sex couples (only 4 in our sample), we do not further distinguish between adopted and biological children, which we include all together in our analysis. It should also be noted that, in line with earlier administrative data studies (Aldén et al., 2017; Kabátek and Perales, 2021; Mazrekaj et al., 2020), most young adults from same-sex families in our sample are from female same-sex families (558 children), whereas only 47 children are from male same-sex families. This is likely due to three reasons. First, the path to motherhood for female same-sex couples is legally easier than for gay couples. According to Dutch law, the mother who gives birth to the child is the legal mother of the child, regardless of any agreement prior to child's birth. If the biological mother is in a married relationship with another partner, the legal procedures for male same-sex couples to become legal parents of the child become even more complicated. Male same-sex couples first need the legal mother to renounce her legal rights and responsibilities towards the child. Later, they need to become the adopting parents of the child through a complex and time-consuming legal procedure. By contrast, this procedure is easier for female same-sex couples, as one of the two partners is already the legal mother of the child. A second reason why we observe few male same-sex families is related to the late introduction of surrogacy practices for gay couples in the Netherlands, namely in 2019. Finally, child custody following parental divorce is mostly granted to the mother of the child. If the mother was gay, the child would reside with the (lesbian) mother. If the father was gay, the child would still reside with the (heterosexual) mother.

[12]Note that we do not exclude that children born in same-sex families may experience the dissolution of their parents' partnership. However, in our sample, we observe no parental separation among children born in same-sex families.

4.3 Variable construction

Outcome variables. We measure labor market outcomes by looking at a range of variables of interest, all measured yearly: log gross earnings in euros, log gross hourly wage in euros, employment, and full-time employment (note that we also look at a fifth outcome variable, namely industry choices, which we discuss further below together with the other measures included in the analysis to test the theoretical mechanisms).[13] We construct log gross earnings by taking the sum of the total amount of earnings that each individual has earned in the reference year. Earnings are computed by summing income from salaried employment and income from self-employment. Next, we take logarithms of this quantity to 'smoothen' the extremes of the distribution and reduce its skewness.[14] Given that individuals observed at a young age may earn different wages due to different number of hours worked, we also look at their hourly wages. To construct this outcome variable, first, we sum the total amount of hours worked by each individual in the reference year. Then, we divide the total amount of earnings from salaried employment obtained by the individual in the reference year by the corresponding number of hours worked. As such, we obtain a measure of how much individuals have earned per hour, i.e. their gross hourly wage. Finally, we take logarithms of this quantity to have a smooth distribution.[15] To construct the third outcome variable, we consider as employed every individual who has earned a positive income from employment in the reference year. By contrast, we consider as unemployed every individual who has not earned a positive income from employment, is not working or is inactive in the reference year. Finally, we consider as full-time employed every individual who has worked more than 1,680 hours in the reference year, which is the equivalent of a full-time job with 35 hours a week. By contrast, we consider as part-time employed every individual who is employed but is working less than 1,680 hours a year.[16]

Control variables. As a proxy for the socioeconomic status and/or genetic transmission, we include an indicator for parental educational attainment (1 if both parents have at least completed secondary education, 0 otherwise).[17] We also include a continuous variable for the gross household income, measured when the child is 15 years old.[18] To obtain this, first,

[13] We also estimate our model in terms of log net earnings and log net hourly wage (see Table 4.B3 in the online supplement). Our results are virtually unchanged with respect to our main specification in Table 4.2.

[14] However, we also estimate our model in absolute terms. Our results are virtually unchanged with respect to our main (logarithmic) specification.

[15] Note that, as we do not have data on the number of hours worked by self-employed persons, we construct the hourly wage only for workers with salaried employment. However, most individuals in our sample (92.85 percent) receive income from salaried employment, whereas only a fraction (7.15 percent) receive income from self-employment.

[16] Note that, similarly to the definition of hourly wage, we can construct full-time employment only for workers with salaried employment.

[17] Note that we do not use in our main model specification a more refined control variable for parental education due to data limitations. Nevertheless, we conduct a robustness check where we include parental education as a categorical variable (retaining the 65.28 percent of our sample). Our results are robust to this alternative specification (see Table 4.B8 in the online supplement).

[18] Note that, as data on income are available only as of 2010, we have set the age limit at 15 to have a balanced

we collect the gross annual income of each parent (or step-parent). Next, we obtain the household income by summing the gross annual income of both parents (or stepparents). Finally, we take logarithms to have a smooth distribution. The household socioeconomic status has been shown to strongly correlate with children's early-labor market outcomes (Duncan et al., 1998), and to explain part of the differences between children's performance in same-sex and different-sex families (Rosenfeld, 2010; Mazrekaj et al., 2020).

We also control for family characteristics. We include a continuous variable for maternal age at birth to account for young adults from same-sex families usually having older parents than young adults from different-sex families (Black et al., 2007; Mazrekaj et al., 2020). Young adults from same-sex families are also more likely than young adults from different-sex families to have experienced parental separation, mostly because they become part of a same-sex family from a parental prior different-sex relationship (Cenegy et al., 2018). To account for family instability, first, we include a variable for the number of family transitions experienced in childhood: either a change in residence or a change in coresident partner by a parent. Second, we account for parental union status at child birth, arranged in three categories: married parents; cohabiting parents; other. The latter category includes married or cohabiting unions of a parent and a step-parent.[19] Family instability is traditionally associated with compromised developmental outcomes in childhood, including higher likelihood of early entry in the labor force (Fomby and Bosick, 2013). Together with the socioeconomic status, it often accounts for most of the differences in performance between individuals from same-sex and different-sex families (Gates, 2015; Manning et al., 2014b).

At the individual level, as labor market outcomes likely differ by sex (Blau and Kahn, 2017), we include an indicator for individuals' sex (1 if male, 0 if female). Further, given that migrants might face difficulties on the labor market (McCall, 2001; Dustmann et al., 2010), we account for individuals' ethnic background by constructing a categorical variable on the basis of parental country of birth. We include four categories: both parents born in the Netherlands; one parent born in the Netherlands, the other in a Western country (e.g. another European country or the United States); one parent born in the Netherlands, the other in a non-Western country; both parents born outside the Netherlands. As such, we account for the lower likelihood of young adults from same-sex families to have both parents of foreign origins relative to young adults from different-sex families (Mazrekaj et al., 2020). Finally, we always include in our model specification fixed effects for year of birth, accounting for both observed and unobserved heterogeneity in birth cohorts and the labor market situation (e.g. economic situation). We also always include indicators for individuals' municipality of birth, to account for regional differences and for the higher

measure of family income across birth cohorts.

[19] Given that most studies on same-sex families distinguish between marriage and cohabitation (Allen, 2013; Mazrekaj et al., 2020; Reczek et al., 2016; Rosenfeld, 2010; Watkins, 2018), and that in the Netherlands the registered partnership is perceived as almost identical to marriage (Trandafir, 2014), we do not further differentiate between married unions and registered partnerships, which we include altogether in the first category (*married parents*).

likelihood of young adults from same-sex families to have grown up in urban areas (Black et al., 2007).

Measures to test the theoretical mechanisms. To test the family formation theory, and as part of the family characteristics, we include discrete variables for individuals' birth order and family size. According to the family formation theory, these variables are possible mediators of the relationship between having lived in same-sex families and labor market outcomes. By including them in the analysis, we account for young adults from same-sex families usually having lived in small families and being early in the birth order (Black et al., 2007; Mazrekaj et al., 2020).[20][21]

Further, to test the human capital/signalling theory, we use diploma attainment in secondary education as a mediator. We construct diploma attainment as a binary indicator coded 1 if the young adult has passed high school exit exams on first attempt, and 0 if the young adult has not passed high school exit exams on first attempt or has dropped out of school.

Next, to test the discrimination theory, we construct a binary indicator for whether the young adult is residing in a rural or urban area at age 21. We distinguish between rural and urban areas by considering the number of residence addresses registered at the municipality where young adults are residing in the reference year. We label as rural (urban) all municipalities where the number of residence addresses per squared kilometer is lower (larger) than 1500.

Finally, to test the sector selection theory, we include a fifth outcome variable to measure young adults' industry choices. Specifically, we construct a binary indicator accounting for whether individuals are employed in a nontraditional (1) or traditional (0) industry. Because the (non) conventionality of a job is closely related to its stereotyping as masculine or feminine (McDowell, 2015), we construct this outcome variable by first distinguishing the sex composition of the sectors in which young adults are employed. We identify 69 sectors, as established by the Tax and Customs Administration in the Netherlands (Belastingdienst, 2021). We follow Bose and Rossi (1983) and define female-dominated (male-dominated) sectors as sectors in which the percentage of employed women is higher (lower) than the percentage of total women in the Dutch workforce, for every given year. Examples of female-dominated sectors so defined consist of health and social services, hospitality services and retail sales. Next, we define nontraditional industries based on the relationship between the sex composition of the sector where the individual is employed and the individual's own sex. In other words, nontraditional industries are coded as 1 if men (women) are

[20] Note that birth order and family size are intended to be mediators. However, as we do not conclude the mediating role of these variables in Appendix C, we employ them as control variables throughout the analysis.

[21] To account for the fact that first born individuals always have higher likelihoods of residing in small families than later born individuals, and to purge family size effects from birth order, we also estimate our model by constructing a birth order index as in Booth and Kee (2009) (see Table 4.B7 in the online supplement). Our results are robust to this alternative specification of birth order and family size.

employed in female- (male-)dominated sectors, and as 0 if men (women) are employed in male- (female-)dominated sectors. Note that, as we do not observe sectors for self-employed individuals, we can construct industry choices only for workers who receive income from salaried employment. Moreover, while we follow Bose and Rossi (1983) to define female- and male-dominated sectors, our results are robust to following Jacobs (1989) and defining female-dominated sectors as sectors in which the proportion of women is higher than 70 percent of the total Dutch workforce, and male-dominated sectors as sectors in which the proportion of women is lower than 30 percent of the total Dutch workforce (not reported).

5 Methods

5.1 Linear regression model

We estimate labor market outcomes of young adults from raised in same-sex families with a linear regression model estimated by ordinary least squares (OLS):

$$Y_i = \beta_0 + \beta_1 H_i + \alpha \mathbf{X_i} + \epsilon_i \tag{5.1}$$

where Y_i indicates the labor market outcome variables for individual i: log gross earnings in euros, log gross hourly wage, full-time employment (1 if full-time employed, 0 if part-time employed), employment (1 if employed, 0 if unemployed or inactive), and industry choice (1 if employed in nontraditional sector, 0 if employed in traditional sector).[22][23] Further, H_i defines the key variable of interest, and takes value 1 if the individual has lived for at least one year in a same-sex family during childhood, and 0 if the individual has lived in a different-sex family.[24] Finally, $\mathbf{X_i}$ defines a series of individual, family and socioeconomic control variables, which we include differently according to our specification. In all model specifications, we include an indicator for sex (1 if male, 0 if female), a categorical variable for the country of origin, indicators for birth years, and indicators for the municipality of birth. To account for heteroskedasticity in the error term, we use Huber-White robust standard errors.[25]

Although our population data cover a time span large enough to observe labor market performance of young adults from same-sex families, establishing the association between

[22] Note that we estimate a linear probability model when the dependent variable is binary. However, our results are robust to logit specification (see Table 4.B5 in the online supplement).

[23] We also considered estimating annual earnings and hourly wages using a Heckman selection model to account for earnings and wages being only partially observed. However, we discarded this methodology based on the unavailability of valid instruments to estimate the selection equation. For instance, while number of children and marital status have been used to model especially women's labor market participation (see, e.g., Mulligan and Rubinstein, 2008), these variable are unlikely to be applicable to our context given that our sample is made of young adults who are by majority unmarried and childless at the time of their labor market performance. Nonetheless, our results are robust to estimating a Heckman selection model using number of children and marital status as instruments (not reported).

[24] However, we also re-estimate our main model with a continuous variable that accounts for the number of years young adults have spent in a same-sex family, as we show in Table 4.B6.

[25] Clustering standard errors at the municipality level does not alter the main conclusions in Table 4.2.

residing in a same-sex family and labor market outcomes, net of possible confounding factors, is not trivial. A first challenge comes from identifying the sample of workers. To single out individuals who are on the labor market with a long-time perspective, we only focus on children who have never enrolled in higher education by the age of 21. That is, we exclude children who have attended HBO (higher vocational education) or WO (higher general education). However, this choice has two important implications. First, it roughly halves the number of young adults from same-sex families that we are able to identify from 1,287 (full sample) to 605 (current sample of young adults not enrolled in higher education). Second, although focusing on young adults not in higher education is a necessary choice to spot individuals who share similar observable and unobservable characteristics, it may lead to a non-random composition of the sample if young adults from same-sex families are enrolled in higher university at different frequencies than young adults from different-sex families. In the online supplement, we compare young adults from same-sex families to young adults from different-sex families on the likelihood of being enrolled in higher education, finding no significant differences between the two groups (see Table 4.A1). Additionally, we also estimate our models on the full sample (see Table 4.B1) and on the sample of older birth cohorts for whom we observe whether they finished tertiary education (see Table 4.A2). These estimations lead to results that are consistent with conclusions from our main model specification, suggesting no selection in the composition of our sample.

Estimating the association between living in same-sex families and labor market outcomes presents a second challenge. To observe early labor market performance of children in same-sex families, we include cohorts of children born before the legalization of same-sex partnerships in 1998. This results in heterogeneity in the birth cohorts included in this chapter, and heterogeneity in family types. On the one hand, the older birth cohorts for the vast majority entered a same-sex family later in life, and experienced parental break-ups while residing in a same-sex family. To limit the bias stemming from the (distinct) negative effect of parental separation, we include controls for family instability, as often done in the literature (Cenegy et al., 2018; Reczek et al., 2016). On the other hand, the labor market outcomes of young adults who entered a same-sex family later in life might be different than those of young adults who were born in planned same-sex families, for two main reasons. First, we may expect that the mechanisms discussed earlier in Section 3 are stronger for individuals who were nurtured by same-sex parents since birth. Second, planned same-families share distinctive features relative to later-formed same-sex families. For instance, given the costly and time-consuming procedures to obtain children for same-sex couples, only the motivated and wealthy same-sex couples are likely to engage in these procedures to obtain children. Therefore, the socioeconomic status of same-sex families will hide considerable heterogeneity as planned same-sex families will likely have higher resources than later-formed same-sex families. Moreover, same-sex (female) parents con-

ceiving their children through Donor Insemination (DI) may even choose their donor to be young and highly educated (Whyte et al., 2016). However, controlling for human capital formation in wage regressions is problematic due to endogeneity bias arising from the inability to account for unobserved (but influential) characteristics such as ability. To test whether the heterogeneity in family types may influence our results, first, we re-estimate our model using a continuous variable that accounts for the number of years spent in a same-sex family as key variable of interest (see Table 4.B6 in the online supplement). As such, we investigate the direction of the relationship between share of childhood spent in same-sex families and labor market outcomes. Second, we re-estimate our model only on the subsample of young adults born in same-sex families, as we discuss later in Section 6.3. Finally, we re-estimate our model on the last birth cohorts (see Table 4.B2 in the online supplement), and find similar results as in our main model specification.

5.2 Mediation analysis

To test the role of human capital in shaping young adults' performance on the labor market, we perform a mediation analysis using diploma attainment in secondary education as mediator. Similarly, we assess the role of family formation by performing a mediation analysis with family size and birth order as mediators. More details on the method and on the expected relationship between the key variable of interest, the outcome variables, and the mediators can be found in Appendix C in the online supplement.

5.3 Coarsened Exact Matching

In our analysis, we are able to observe the population of young adults from same-sex families. However, this sample is much smaller than the sample of young adults from different-sex families, and likely less heterogeneous, which may lead to bias in the estimation of our key coefficient of interest. As argued by Ho et al. (2007), if control units (in our case, young adults from different-sex families) are too heterogeneous with respect to treatment units (in our case, young adults from same-sex families), the estimation of the treatment effect may become dependent on the statistical specification of the model. Model dependence, in turn, can cause bias in the estimation of the treatment effect. Thus, the very large number of young adults from different-sex families relative to young adults from same-sex families may lead to a biased estimate of the relationship between having lived in same-sex families and labor market outcomes.

To improve the balance in our control variables and further test the robustness of our results, we employ coarsened exact matching (CEM) by Iacus et al. (2011, 2012). We coarsen the following control variables: annual household earnings to deciles of the income distribution, age of mother at birth to six categories (younger than 25, 26 to 30, 31 to 35, 36 to 40, 41 to 45, older than 46), family transition to three categories (never experienced a

family transition, experienced at least one family transition, experienced two or more family transitions), ethnicity to two categories (both parents born in the Netherlands, at least one parent born outside the Netherlands), family size to three categories (only child, two children, more than two children), and birth order to three categories (first born, second born, later than second born). As for the remaining covariates, we do not perform any coarsening but we seek an exact match.

5.4 Coefficient stability test

We should note that the identification strategy to assess young adults' performance on the labor market is mostly based on adding control variables to account for observables (*selection on observables*). Although many of the control variables included in this chapter may correlate with unobserved characteristics (for instance parental socioeconomic status and family characteristics are likely correlated with parental motivation Guryan et al., 2008), we do not observe other variables which might influence young adults' labor market outcomes, such as parental involvement in career planning (Feldman, 2003) (*selection on unobservables*). Stability of coefficients after the inclusion of control variables has often been interpreted as evidence that the bias from omitted variables is limited, under the hypothesis that the bias arising from the inclusion of observables is informative about the bias that would arise if the full set of control variables could be included – observables and unobservables. However, as shown by Oster (2019), the stability of coefficients alone is not enough to derive the robustness of results, as it may fail to acknowledge the quality of the controls included. In fact, covariates that do not move coefficients by much may fail to do so not because the treatment effect is robust to omitted variable bias, but because these variables explain little of the dependent variables variance. As such, movements in R-squared (which reflect the quality of the controls included) need to be accounted for as well to correctly infer whether estimates are effectively robust to omitted variable bias.

To this end, we follow Oster (2019) and construct a consistent estimator for omitted variable bias (β^*) relying on movements in coefficients, movements in R-squared, and assumptions on the degree of selection on observed and unobserved variables (δ). More details on the methodology can be found in Appendix E.

6 Results

6.1 Sample characteristics

Our sample consists of 239,313 young adults, of which 605 lived for at least one year in a same-sex family during childhood, and 112 were raised by same-sex parents from birth until age 15.[26] Table 4.1 reports the descriptive statistics as well as the p-values of a difference in

[26] As previously mentioned, 558 of these young adults lived in a female same-sex family, whereas 47 in a gay family. The low number of gay families prevents us from estimating our models separately for female and male

means test.[27]

Table 4.1: Descriptive statistics

	Young adults from different-sex families (1)	Young adults from same-sex families (2)	Young adults born in same-sex families (3)
Sex (1 is male)	0.557	0.549	0.625
Ethnicity (1 is both parents born in NL, 0 is at least one parent not born in NL)	0.752	0.686	0.804
Log household annual gross income	11.014	10.917#	11.3#
Household annual gross income	73,778.967	68,439.578#	92,421.274#
Parental education[a]	0.851	1.049#	1.890#
Family transitions	0.103	1.504#	0.036
Family structure at birth (1 is married parents, 0 is cohabiting parents or other)	0.874	0.593#	0.339#
Age of mother at birth	30.034	29.674#	35.143#
Family size	2.748	2.436#	2.134#
Birth order	1.953	1.641#	1.527#
Outcome variables			
Gross annual earnings in EUR	18,508.325	17,033.818#	12,500.8#
Log gross annual earnings	9.525	9.426#	9.047#
Gross hourly wage in EUR	10.531	10.44#	9.304#
Log gross hourly wage	2.274	2.265	2.149
Employment (1 is yes)	0.929	0.899	n.a.
Full-time employment (1 is yes)	0.348	0.291	0.196
Nontraditional industry (1 is yes)	0.283	0.326	0.500#
Number of children	238,708	605	112

Notes: #The mean for Young adults from same-sex families is significantly different at the 5 percent level from the mean for Young adults from different-sex families in column (1) by means of a two-tailed *z*-test.
[a]Note that because of missing values related to this variable, the number of young adults from different-sex families is 206,671, the number of young adults from same-sex families is 484, and the number of young adults born in same-sex families is 84. [b]Note that with respect to this variable, the number of young adults from different-sex families is 205,276, the number of young adults from same-sex families is 491, and the number of young adults born in same-sex families is 92, given that we define it only on the basis of salaried employment.

Consistent with earlier studies (Aldén et al., 2017; Boertien and Bernardi, 2019; Mazrekaj et al., 2020; Kabátek and Perales, 2021), young adults from same-sex families are more likely to have fewer siblings (hence they are more likely to be early in the birth order) in column 2. They also appear to have higher-educated parents than young adults from different-sex families, although not significantly. However, they have younger mothers and significantly lower household income than young adults from different-sex families. Young adults from same-sex families have also experienced significantly more family transitions during childhood, and are less likely to be born into married families. This is not surprising as most of the individuals in our sample entered a same-sex family later in life, having to deal with parental break-ups and changes in the household composition while growing up. This is because same-sex partnerships were legalized in the Netherlands in 1998, so most children

same-sex families.

[27]Because disclosure agreements prevent us from showing statistics based on small numbers, in the Table 4.1, we aggregate ethnicity and family structure at birth at a higher level and show parental education as a continuous variable (0 if at least one parent is a high school dropout, 1 if both parents have a high school diploma, 2 if at least one parent has a high school education, 3 if both parents have a higher education diploma). However, in the analyses, we include these control variables as described in 4.3. In addition, we do not report the average number of children born into same-sex families who are employed.

in our sample could be born in a planned same-sex family only if belonging to the latest birth cohorts. If, however, we only consider the subsample of young adults born in same-sex families in column 3, we notice that these young adults have lived in more stable families during childhood, as they experienced less family transitions while growing up (albeit not significantly). Nonetheless, young adults raised by same-sex parents from birth are still less likely to be born into married families but more likely to be born into cohabiting families. It is likely that same-sex couples, although living together since the birth of the child, delayed registered partnership and marriage until they felt the public attitude towards these family arrangements reached a certain threshold. We further observe that young adults who were raised by same-sex parents from birth enjoy significantly higher socioeconomic status (i.e. higher household income, higher parental education, and higher maternal age at birth) than young adults from different-sex families. Finally, young adults from same-sex families appear to be as ethnically diverse as young adults from different-sex families.

Based on descriptive statistics, young adults from same-sex families seem to earn significantly less annually than young adults from different-sex families, both in levels and in logarithmic terms, and they also appear to earn less per hour in terms of levels. Further, young adults from same-sex families appear to be as employed as young adults from different-sex families, although they are less employed in full-time occupations and more employed in nontraditional industries (especially when considering young adults born in same-sex families). These differences, however, are not significant, and descriptive comparisons do not control for possible differences in observed characteristics between young adulst from same-sex and different-sex families.

6.2 Young adults from same-sex families and labor market performance

Table 4.2 compares young adults from same-sex families to young adults from different-sex families on their early labor market performance. Keep in mind that these young adults have not enrolled in higher education by age 21. In Appendix A in the online supplement, we test for the non-random composition of our sample and dig deeper into higher education enrolment of young adults from same-sex versus different-sex families, and find no selection in the composition of our sample. Columns 1 and 2 in Table 4.2 show results for log gross earnings, columns 3 and 4 for log gross hourly wage, columns 5 and 6 for employment, and columns 7 and 8 for full-time employment.[28] Our findings suggest that young adults from same-sex families earn just as much and are just as likely to be employed and full-time employed as young adults from different-sex families.

To test the sensitivity of our results, we employ coarsened exact matching and match every young adult from a same-sex family to at least one young adult from a different-sex family with similar or exactly the same observed characteristics. As mentioned previously,

[28] We will separately discuss estimations on the likelihood of being employed in a nontraditional industry in Section 3, as it is one of the theoretical mechanisms reported in Section 3.

Table 4.2: Young adults from same-sex families and labor market outcomes

	Log gross earnings		Log gross hourly wage		Employment		Full-time employment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Young adult has lived in a SSF (1 is yes)	-0.047	-0.003	-0.006	0.010	-0.023	-0.002	-0.034	-0.007
	(0.038)	(0.038)	(0.020)	(0.020)	(0.012)	(0.012)	(0.020)	(0.020)
Sex (1 is male)	0.187***	0.193***	0.041***	0.043***	-0.004***	-0.004***	0.250***	0.251***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Ethnicity (ref: both parents born in NL)								
One NL parent, other Western	-0.161***	-0.143***	-0.041***	-0.036***	-0.018***	-0.016***	-0.069***	-0.063***
	(0.007)	(0.007)	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)
One NL parent, other non-Western	-0.260***	-0.255***	-0.050***	-0.047***	-0.026***	-0.021***	-0.124***	-0.121***
	(0.015)	(0.015)	(0.008)	(0.008)	(0.004)	(0.004)	(0.007)	(0.007)
Both parents not NL	-0.278***	-0.429***	-0.025***	-0.066***	-0.040***	-0.029***	-0.182***	-0.228***
	(0.007)	(0.007)	(0.004)	(0.004)	(0.002)	(0.002)	(0.003)	(0.003)
Log household income		-0.012***		0.004***		0.006***		-0.000
		(0.002)		(0.001)		(0.001)		(0.001)
Parental education (1 is diploma SE)		-0.122***		-0.029***		0.027***		-0.038***
		(0.004)		(0.002)		(0.001)		(0.002)
Family transitions		-0.005		-0.004		-0.012***		-0.010***
		(0.004)		(0.002)		(0.001)		(0.003)
Family structure (ref: married parents)								
Cohabiting parents		-0.081***		-0.015***		-0.018***		-0.033***
		(0.006)		(0.003)		(0.002)		(0.003)
Other		-0.120***		-0.036**		-0.026***		-0.059***
		(0.024)		(0.012)		(0.007)		(0.011)
Age of mother at birth		-0.016***		-0.005***		-0.002***		-0.005***
		(0.001)		(0.000)		(0.000)		(0.000)
Family size		0.031***		0.012***		-0.003***		0.010***
		(0.002)		(0.001)		(0.001)		(0.001)
Birth order		0.022***		0.010***		0.001		0.005***
		(0.003)		(0.001)		(0.001)		(0.001)
Constant	9.060***	9.624***	2.012***	2.072***	0.895***	0.873***	0.068*	0.215***
	(0.063)	(0.068)	(0.039)	(0.042)	(0.020)	(0.022)	(0.032)	(0.035)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality at birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of young adults	222,200	222,200	205,767	205,767	239,313	239,313	205,767	205,767
Number of young adults from SSF	544	544	491	491	605	605	491	491
R-squared	0.0829	0.0958	0.0699	0.0748	0.0125	0.0170	0.123	0.128

Notes: The table shows the estimated association between living in a same-sex family and labor market outcomes. SSF indicates same-sex families. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

the aim of the CEM matching procedure is to reduce the imbalance in the distribution of the control variables. Our matched sample consists of 21,400 young adults, of which 408 lived in a same-sex family in childhood. The results in Table 4.B4 in the online supplement confirm the results of our main analysis for all outcome variables. We also show that our results are robust to the inclusion of omitted variable bias by performing the coefficient stability test as in Oster (2019) in Table 4.E1 in the online supplement.

6.3 Young adults born in a same-sex family

So far, our sample of young adults from same-sex families has heterogeneously included both young adults who entered a same-sex family later in life and young adults who were born and raised in a planned same-sex family throughout childhood. However, these two groups are likely very different, both on observable and unobservable characteristics (Mazrekaj et al., 2020) As discussed earlier in Section 6.1, young adults born in same-sex families enjoy a higher socioeconomic status and are less likely to experience family instability than young adults who entered a same-sex family later in life. Moreover, the influence

of same-sex parenting is likely to be larger for young adults born in same-sex families as they are nurtured by same-sex parents during their entire childhood. Therefore, we check the sensitivity of our results by focusing on the subsample of young adults who have lived with same-sex parents since birth (and until age 15).[29] Around 20 percent of the young adults from same-sex families in our sample fall into this category, which corresponds to 112 individuals. We show our results in Table 4.3, where columns 1 and 2 report the estimates of our model for log gross earnings, columns 3 and 4 for log gross hourly wage, columns 5 and 6 for employment, and columns 7 and 8 for full-time employment.

Table 4.3: Young adults born in same-sex families and labor market outcomes

	Log gross earnings		Log gross hourly wage		Employment		Full-time employment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Young adult has lived in a SSF from birth (1 is yes)	−0.452***	−0.295**	−0.122*	−0.082	0.015	0.025	−0.177***	−0.125**
	(0.097)	(0.097)	(0.047)	(0.047)	(0.023)	(0.023)	(0.040)	(0.040)
Sex (1 is male)	0.188***	0.193***	0.041***	0.043***	−0.004***	−0.004***	0.250***	0.252***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Ethnicity (ref: both parents born in NL)								
One NL parent, other Western	−0.161***	−0.143***	−0.041***	−0.036***	−0.018***	−0.016***	−0.069***	−0.063***
	(0.007)	(0.007)	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)
One NL parent, other non-Western	−0.260***	−0.255***	−0.050***	−0.048***	−0.026***	−0.021***	−0.124***	−0.121***
	(0.015)	(0.015)	(0.008)	(0.008)	(0.004)	(0.004)	(0.007)	(0.007)
Both parents not NL	−0.278***	−0.429***	−0.026***	−0.066***	−0.040***	−0.029***	−0.183***	−0.229***
	(0.007)	(0.007)	(0.004)	(0.004)	(0.002)	(0.002)	(0.003)	(0.003)
Log household income		−0.012***		0.004**		0.006***		−0.000
		(0.002)		(0.001)		(0.001)		(0.001)
Parental education (1 is diploma SE)		−0.122***		−0.029***		0.027***		−0.039***
		(0.004)		(0.002)		(0.001)		(0.002)
Family transitions		−0.007		−0.004		−0.012***		−0.011***
		(0.004)		(0.002)		(0.002)		(0.003)
Family structure (ref: married parents)								
Cohabiting parents		−0.080***		−0.014***		−0.018***		−0.032***
		(0.006)		(0.003)		(0.002)		(0.003)
Other		−0.119***		−0.035**		−0.021**		−0.062***
		(0.024)		(0.012)		(0.008)		(0.011)
Age of mother at birth		−0.016***		−0.005***		−0.002***		−0.005***
		(0.001)		(0.000)		(0.000)		(0.000)
Family size		0.031***		0.012***		−0.003***		0.010***
		(0.002)		(0.001)		(0.001)		(0.001)
Birth order		0.022***		0.010***		0.001		0.005***
		(0.003)		(0.001)		(0.001)		(0.001)
Constant	9.059***	9.622***	2.013***	2.073***	0.895***	0.873***	0.069*	0.216***
	(0.063)	(0.069)	(0.040)	(0.042)	(0.020)	(0.022)	(0.032)	(0.035)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality at birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of young adults	221,761	221,761	205,368	205,368	238,820	238,820	205,368	205,368
Number of young adults from SSF	105	105	92	92	112	112	92	92
R-squared	0.0831	0.0958	0.0700	0.0749	0.0125	0.0169	0.123	0.128

The table shows the estimated associations between living in same-sex families from birth and log gross earnings, log hourly wage, employment, and full-time employment. SSF indicates same-sex families. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.3 reveals that young adults born in same-sex families tend to earn significantly

[29] We also estimated our models on a redefined variable of interest that accounts for the number of years spent in a same-sex family, ranging from 0 (never lived in a same-sex family) to 15 (always lived in a same-sex family). We show our results in Table 4.B6 in the online supplement. Our findings are in line with findings from our main specification in Table 4.2. When the full set of covariates is included, young adults from same-sex families earn just as much and are just as likely to be employed as young adults from different-sex families. Young adults from same-sex families appear to be significantly less employed in full-time occupations, but this coefficient is very close to zero and not meaningful.

less than young adults from different-sex families. When the full set of covariates is controlled for in column 2, young adults born in same-sex families are found to earn 29.5 percent less throughout the year than their peers from different-sex families, which corresponds to an earnings differential of around 5,200 euros. This negative association on annual earnings is partly explained by the sorting of young adults born in same-sex families in part-time occupations. Our results show that when the full set of covariates is controlled for in column 8, young adults from same-sex families are 12.5 percent less likely to be full-time employed than young adults from different-sex families. Nevertheless, young adults born in same-sex families are also found to earn 12.2 percent less per hour than young adults from same-sex families in column 3, although this association is reduced to 8 percent and becomes non significant when the full set of control variables in included in column 4. The negative associations on hourly earnings suggest that young adults from same-sex families tend to earn less even when their shorter work hours are taken into account. These results are robust to CEM, to the inclusion of omitted variable bias by means of a coefficient stability test as in Oster (2019) (see Table 4.E1 in the appendix), and to multiple hypothesis testing (see Table 4.B13 in the appendix).

6.4 Mechanisms

Sex identity theory and discrimination theory. According to the sex identity theory, males and females from same-sex families might perform differently on the labor market than their peers from different-sex families. To test this theory for the full sample of young adults from same-sex families (both born and not born in a same-sex family), we report in Panel A of Table 4.4 the estimated model separately for the subsample of males (columns 1 to 4) and females (columns 5 to 8). If the sex identity theory holds, the difference in earnings when comparing females from same-sex families to females from different-sex families should be significantly higher than the difference in earnings when comparing males from same-sex families to males from different-sex families.

Overall, our results indicate that young adults from same-sex families perform just as well as young adults from different-sex families on the labor market regardless of their sex. Although we find that log gross earnings are positively correlated with females from same-sex families (column 5), while negatively correlated with males from same-sex families (column 1), these associations are not significant and are only found for log gross earnings. Moreover, a test of equality of coefficients fails to reject the null hypothesis of equality of coefficients between females and males from same-sex families, for both log gross earnings and log gross hourly wage (p-value = 0.656 for log gross earnings, p-value = 0.130 for log gross hourly wage). We also find no heterogeneity by sex for the sample of young adults who were raised by same-sex parents since birth (see Table 4.B10 in the online supplement).[30] Therefore, we interpret our results as evidence that the sex identity theory does

[30] However, we should emphasize that the limited sample size of young adults born in same-sex families suggests

Table 4.4: Young adults from same-sex families and labor market outcomes, by sex and urbanicity

PANEL A	Males				Females			
	Log gross earnings (1)	Log gross hourly wage (2)	Employment (3)	Full-time employment (4)	Log gross earnings (5)	Log gross hourly wage (6)	Employment (7)	Full-time employment (8)
Young adult has lived in a SSF (1 is yes)	−0.027	0.014	−0.001	−0.021	0.024	0.005	−0.004	0.014
	(0.051)	(0.024)	(0.016)	(0.029)	(0.056)	(0.034)	(0.019)	(0.028)
Constant	9.990***	2.108***	0.860***	0.556***	9.420***	2.061***	0.886***	0.107*
	(0.104)	(0.066)	(0.032)	(0.057)	(0.090)	(0.051)	(0.029)	(0.042)
Additional controls[a]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of young adults	123,611	113,238	133,357	113,238	98,589	92,529	105,956	92,529
No. of young adults from SSF	299	272	332	272	245	219	273	219
R-squared	0.102	0.0739	0.0258	0.0946	0.0737	0.0774	0.0127	0.0396
PANEL B	Urban				Rural			
	Log gross earnings (1)	Log gross hourly wage (2)	Employment (3)	Full-time employment (4)	Log gross earnings (5)	Log gross hourly wage (6)	Employment (7)	Full-time employment (8)
Young adult has lived in a SSF (1 is yes)	−0.039	0.002	0.004	−0.029	0.055	0.018	−0.010	0.025
	(0.052)	(0.027)	(0.016)	(0.025)	(0.053)	(0.031)	(0.020)	(0.034)
Constant	9.738***	2.030***	0.842***	0.245**	9.556***	2.065***	0.886***	0.189***
	(0.159)	(0.140)	(0.060)	(0.088)	(0.078)	(0.044)	(0.024)	(0.040)
Additional controls[b]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of young adults	101,981	93,710	111,535	93,710	120,219	112,057	127,778	112,057
No. of young adults from SSF	337	299	376	299	207	192	229	192
R-squared	0.0708	0.0688	0.0197	0.100	0.108	0.0822	0.0175	0.139

The tables show the estimated associations between living in same-sex families and labor market outcomes, for males and females (Panel A), and young adults living in rural and urban municipalities (Panel B). Rural (urban) municipalities are defined as having less (more) than 1500 number of residences per kilometer squared. SSF indicates same-sex families. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

[a] Additional controls include ethnicity, log household income, parental education, family transitions, family structure at birth, age of mother at birth, family size, birth order, birth year fixed effects, municipality at birth fixed effects.

[b] Additional controls include sex, ethnicity, log household income, parental education, family transitions, family structure at birth, age of mother at birth, family size, birth order, birth year fixed effects, municipality at birth fixed effects

not play a role in modeling young adults' outcomes on the labor market.

In Panel B of Table 4.4, we test the discrimination theory by analysing labor market outcomes by urbanicity. If the discrimination theory holds, the potential discrepancy in labor market outcomes between young adults from same-sex and different-sex families should be smaller if positive or larger if negative in rural communities where discrimination against same-sex parents is likely to be higher than in urban areas that are typically very tolerant towards same-sex parenting. Overall, our findings indicate that young adults from same-sex families perform just as well as young adults from different-sex families on the labor market in both rural (columns 5 to 8) and urban municipalities (columns 1 to 4). In both subsamples, no significant difference is found for any of the outcome variables. The same is true for the sample of young adults who were raised by same-sex parents from birth (see Table 4.B10 in the online supplement). Thus, our results show no evidence that young adults from same-sex families may be discriminated against by employers on the labor market.

Human capital/signalling theory and family formation theory. Two additional mechanisms that may drive young adults from same-sex families to perform differently than young adults from different-sex families on the labor market relate to their differences in education (hu-

caution in the interpretation of particularly those mechanisms that are tested by further splitting the sample.

man capital/signalling theory), and family size (family formation theory). To test these theories, we perform separate mediation analyses.

To conserve space, we discuss the analyses in Appendix C in the online supplement and show the results in Table 4.C1 for the human capital/signalling theory and in Table 4.C2 for the family formation theory. We find no evidence that diploma attainment mediates the relationship between having resided in a same-sex family and labor market outcomes. Moreover, we find that the relationship between family size and birth order as mediators in the relationship between residing in a same-sex family and labor market outcomes does not go in the direction predicted by the family formation theory. Therefore, our results are suggestive that the human capital/signalling theory and the family formation theory do not play a significant role when comparing labor market outcomes of young adults from same-sex versus different-sex families.

Sector-selection theory. The sector-selection theory posits that young adults from same-sex families, by not conforming to sex-stereotypes in childhood and having parents who equally divide paid and unpaid labor among each other, will be more likely to sort into non-traditional industries. To test this mechanism, we estimate a model in which the outcome variable is a binary indicator given value of 1 if the individual is employed in a nontraditional sector and value of 0 if the individual is employed in a traditional sector. We show our results in Table 4.5.

Table 4.5: Young adults from same-sex families and nontraditional industries

Outcome: Employed in nontraditional sector	Full sample		Born	
	(1)	(2)	(3)	(4)
Young adult has lived in a SSF (1 is yes)	0.029	0.010	0.174***	0.150**
	(0.020)	(0.021)	(0.049)	(0.049)
Individual controls[a]	Yes	Yes	Yes	Yes
SES controls[b]	No	Yes	No	Yes
Family controls[c]	No	Yes	No	Yes
Number of young adults	205,767	205,767	205,368	205,368
Number of young adults from SSF	491	491	92	92
R-squared	0.0512	0.0532	0.0512	0.0532

The table shows the estimated association between living in same-sex families and sorting into non-traditional industries on the labor market. Full sample includes both young adults born and not born in same-sex families; born includes only young adults raised in same-sex families from birth. Sectors are defined on the basis of sector codes provided by the Tax and Customs Administration in the Netherlands (Belastingdienst, 2021). Nontraditional industries are defined by comparing the sector's sex composition to the individual's sex. Specifically, an individual is assigned to a nontraditional industry if the sector in which the individual is employed is predominantly occupied by the opposite sex. This means that if the individual is a men, a nontraditional industry is an occupation in a female-dominated sector; if the individual is a women, a nontraditional industry is an occupation in a male-dominated sector. Female-dominated (male-dominated) sectors are defined as sectors in which the percentage of employed women is higher (lower) than the percentage of total women in the Dutch workforce, for every given year. Robust standard errors, clustered at the individual level, in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[a] Individual controls include sex, ethnicity, birth year fixed effects and municipality at birth fixed effects.

[b] SES controls include log household income and parental education.

[c] Family controls include family transitions, family structure, age of mother at birth, family size, and birth order.

The findings for the full sample (both born and not born in a same-sex family) suggest

that young adults from same-sex families are just as likely as young adults from different-sex families to sort into nontraditional industries. However, examining the sample of young adults raised by same-sex parents from birth yields a different result. The last two columns of Table 4.5 show that, when the full set of covariates is controlled for, young adults born in same-sex families are about 15.0 percent more likely than young adults from different-sex families to choose an industry that is atypical from their sex. These findings are robust to CEM (not reported) and to accounting for multiple hypothesis testing (see Table 4.B13 in the appendix). They are also robust to additionally controlling for the municipalities where young adults live at age 21 to account for the possible correlation between individuals' current municipality of residence and labor market conditions (e.g. job opportunities or industry composition) within the municipality (not reported). Thus, the sector-selection theory appears to be particularly relevant for the subset of young adults who have lived with same-sex parents from birth.

Earlier, we found that young adults born in same-sex families earned less than young adults from different-sex families. This negative association was partly explained by the sorting of young adults born in same-sex families in part-time occupations. Nonetheless, a proportion of the association remained unexplained. Because a sector's sex composition is also reflected on its average wage level (with female-dominated sectors typically being lower-paying than male-dominated sectors), the overall negative association found in Table 4.3 might additionally reflect the degree to which boys and girls from same-sex families are differently affected by the mechanism behind the sector-selection theory. To test this, we examine the correlation between being raised by same-sex parents from birth and choosing for nontraditional industries by sex.[31] Specifically, we look at the likelihood of being employed in a female-dominated sector in the case of boys and in a male-dominated sector in the case of girls. Our findings (reported in Table 4.B12 in the online supplement) are suggestive that the sorting in nontraditional sectors is driven by boys from same-sex families, who are 20.3 percent more likely than boys from different-sex families to choose a female-dominated industry. No association is found for girls. Therefore, the sector-selection theory may explain the remaining part of the negative association on earnings, given that young adults born in same-sex families will earn lower wages than their comparison group from different-sex families partly because they are predominantly employed in female-dominated sectors. Nonetheless, these associations should be read with caution as they are computed on very small sample sizes.

Two other mechanisms may also explain our results. First, young adults from same-sex families (mostly female same-sex families in our data) might be 'following their mothers' and choosing female-dominated industries (*parent sector hypothesis*). Table 4.B11 in the online

[31] We also tested this assumption by including sector-fixed effects in the regression models estimating the influence of being born in same-sex families on earnings. We found that, when sector fixed effects were included, the estimated association on annual earnings decreased by 43.39%, whereas the estimated association on log hourly wages decreased by 17.07%.

supplement indeed shows a positive association between having resided in a same-sex family and choosing for female-dominated sectors, in samples that include both boys and girls. However, in appendix D in the online supplement we reject this hypothesis by linking young adults' industries to their parents' industries in our data. Second, it may be argued that mothers and fathers are not perfect substitutes but rather provide different inputs to a child's growth (*role model hypothesis*, Allen, 2013). By having two mothers or two fathers, young adults born in same-sex families might sort into nontraditional sectors because they miss the complementary parenting styles that opposite-sex parents provide. We reject this hypothesis by observing industry choices of young adults from different-sex families who lost a parent during their childhood in Appendix D in the online supplement. Finally, by testing the human capital/signalling theory we also indirectly rule out the explanation that young adults born in same-sex families have lower human capital than young adults from different-sex families and therefore cannot estimate their future income properly, leading to the choice of less lucrative occupations. On the contrary, we find that young adults born in same-sex families obtain a high school diploma more often than young adults from different-sex families, albeit not significantly.

7 Conclusion

This chapter provided first evidence on population data of how young adults who lived in a same-sex family in childhood fare on the labor market shortly after high school graduation. Using longitudinal administrative data from the Netherlands, we compared young adults from same-sex families and young adults from different-sex families who did not enrol into higher education by age 21 on a range of labor market outcomes. Our results showed no significant difference in labor market performance between the two groups. Specifically, young adults from same-sex families were found to earn just as much, to be equally employed, and to have equal probability of being full-time employed as young adults from different-sex families.

These findings, however, hide considerable heterogeneity by the time spent in same-sex families. Young adults who were born in same-sex families and raised by same-sex parents throughout childhood were found to earn less than young adults from different-sex families. This association is partly explained by the sorting of young adults born in same-sex families in part-time occupations. The other part of the association is likely explained by young adults born in same-sex families choosing for nontraditional industries (i.e. industries dominated by the opposite sex). In particular, boys from same-sex families are found to be predominantly employed in nontraditional industries, which are generally less lucrative than male-dominated industries. No significant association is found for girls. Our findings are consistent with what we have labeled as the sector-selection theory. This theory posits that because young adults born in same-sex families tend to conform less to gender roles

and are influenced since birth by their parents' egalitarian household labor division, they will be more likely to choose an industry that is atypical than their sex. Other hypotheses to explain the negative association between being raised by same-sex parents and earnings did not hold in our data including the sex identity theory, discrimination theory, human capital/signalling theory, and family formation theory. We also excluded additional explanations such as mimicking mothers' occupational behavior, lacking complementary role models, and having lower human capital than young adults from different-sex families.

From a policy perspective, our results suggest that parents' sex composition is reflected on young adults' labor market choices. This is possibly because family environment can influence young people's attitudes about sex roles (Stacey and Biblarz, 2001; Fulcher et al., 2008; Biblarz and Stacey, 2010; Brewster, 2017). Thus, in addition to the home environment, discussions about sex stereotypes could be stimulated in schools so that young people may make informed labor market choices free from preset beliefs about sex roles.

Some final considerations and limitations of this chapter are worth noting. First, we should emphasize that our analysis refers to the Netherlands, a country where the LGBTQ+ community enjoys broad cultural support and where same-sex partnerships have been legal for more than twenty years. Therefore, our results might be an upper bound on the relationship between having lived in same-sex families in childhood and labor market outcomes. Second, although this chapter is directly related to previous research by Mazrekaj et al. (2020) and Kabátek and Perales (2021), our sample is somewhat different than the samples employed in these studies. This difference is largely due to the focus on older birth cohorts to study early labor market outcomes of young adults from same-sex families. However, our results are robust to several sensitivity tests, including the estimation on later birth cohorts, the use of Coarsened Exact Matching to reduce imbalances between family types, and the analysis of coefficient stability as developed by Oster (2019). Third, our data do not provide a measure of individuals' sexual orientation. This means that while we are able to compare the sex of partners in a couple, we cannot assign sexual identities to parents. As such, our study is not suited to examine how young adults raised by sexual minority parents (which include single parents) perform relative to young adults raised by non-sexual minority parents. Further, our results mostly refer to female same-sex families as the number of male same-sex families is too small to derive correct inference.

Finally, we should emphasize that our analysis concerns individuals observed at a young age and who did not enroll in higher education. Although most theoretical mechanisms do not appear to be supported by our empirical findings, these theories may become more relevant as individuals get older or may apply differently to individuals who have attended higher education. However, in our analyses we noted that young adults from same-sex families were just as likely as young adults from different-sex families to enroll in higher education. Further, observing the labor market outcomes at a later age and including young adults who finished higher education yielded consistent findings with those obtained in our

main analysis. Finally, young adults born in same-sex families who did not enroll in higher education after high school share similar observed characteristics as young adults born in same-sex families who decided to continue their education (i.e. high socioeconomic status, low family size, old age of parents). Therefore, we expect that similar and additional mechanisms than those discussed in this chapter will also apply to the pool of college graduates raised in same-sex families. Examining labor market outcomes of individuals when they are older and further exploring the labor market performance of young adults who have graduated from tertiary education are avenues that are worth exploring in future research.

Appendix A: Education in the Netherlands

A.1 Dutch educational context

In the Netherlands, children start compulsory education at the age of five. At the end of primary education (around age twelve), children enter a tracking system and choose a secondary school in a pre-vocational track (VMBO), a general track (HAVO), and a pre-university track (VWO). Each track differs in the number of school years and type of education provided. The pre-university track lasts six years and prepares pupils for research-oriented higher education (WO). The general track lasts five years and prepares pupils for professionally oriented higher education (HBO). The HBO trains students for specific professions, and is the equivalent of college education in the US. Finally, the pre-vocational track lasts four years and prepares pupils for the vocational track (MBO), which, in turn, teaches practical occupations that can be immediately performed after high school graduation. The MBO consists of four levels, and lasts one to four years depending on the level selected.

Education in the Netherlands is compulsory until age 18, or until a diploma from MBO, HAVO or VWO is obtained. The majority of pupils acquires a vocational qualification. For instance, in 2017, 54 percent of high school graduates obtained their diploma from the vocational track, whereas the remaining 46 percent was divided between the general and the pre-university track (OECD, 2019a). More generally, while the majority of pupils attending the general or the pre-university track enroll in higher education after high school diploma, only a small proportion of vocational graduates continues with education. For comparison, in 2017, among the entrants to bachelor's programs, only 24 percent had a vocational qualification (OECD, 2019a). Therefore, in our analysis, by looking at labor market performances of young adults who did not enrol in higher education after high school, in practice, we are primarily looking at labor market outcomes of individuals with vocational background.

A.2 Young adults from same-sex families, higher education, and labor market

To better understand whether the sample of young adults from same-sex families not enrolled in higher education we observe in the main analysis may be representative of the full population of young adults from same-sex families, first, we examine the association between having lived in a same-sex versus a different-sex family and transition into higher education. Specifically, using the full sample of individuals born in the Netherlands between 1995 and 1999 (which includes both individuals who continued their education after high school graduation and those who discontinued it), we investigate the likelihood of young adults from same-sex families to be enrolled in higher education shortly after high

school diploma, i.e. at age 19. We show results in Table 4.A1 below, both for the full sample of young adults from same-sex families (column 1) and for the subsample of young adults born in same-sex families (column 2), when the full set of covariates is included. Our findings show that the estimated association between having lived in a same-sex family and tertiary education enrolment is close to zero and nonsignificant for both the full sample of young adults from same-sex families and the subsample of young adults born in same-sex families. These findings also hold when we additionally control for diploma attainment in secondary education to account for young adults in same-sex families' school performance (not reported). Thus, our findings suggest that young adults from same-sex families are just as likely as young adults from different-sex families to enroll in higher education after high school graduation.

Second, we examine the labor market outcomes of young adults from same-sex versus different-sex families at age 23. At this age, individuals are likely to have graduated from tertiary education. Hence, we examine the labor market performance of young adults who never enrolled in higher education as well as the labor market performance of young adults who finished higher education at age 23. We consider that individuals completed tertiary education if they did not enroll in a new degree program after academic graduation. We show our results in Table 4.A2, where we report the estimated coefficients when the full set of covariates is controlled for. The results are consistent with the findings of Table 4.2 and 4.3 in our main analysis. In the case of the full sample (that includes both young adults born and not born in same-sex families), all estimated coefficients are close to zero and not significant. In the case of the subsample (that includes only young adults born in same-sex families), we find a sizeable negative association on earnings and fulltime employment, and a positive association on nontraditional industries. Nonetheless, these associations are not significant, which may reflect the lower sample size of young adults born in same-sex families being identified.

Table 4.A1: Young adults from same-sex families and higher education enrolment at age 19

	Young adults from same-sex families (1)	Young adults born in same-sex families (2)
Young adult has lived in a SSF (1 is yes)	0.009	-0.018
	(0.013)	(0.024)
Sex (1 is male)	-0.071***	-0.071***
	(0.001)	(0.001)
Ethnicity (ref: both parents born in NL)		
One NL parent, other Western	-0.004	-0.004
	(0.002)	(0.002)
One NL parent, other non-Western	-0.007	-0.007
	(0.004)	(0.004)
Both parents not NL	0.014***	0.013***
	(0.002)	(0.002)
Log household income	0.060***	0.060***
	(0.001)	(0.001)
Parental education (1 is diploma SE)	0.226***	0.226***
	(0.002)	(0.002)
Family transitions	-0.050***	-0.050***
	(0.002)	(0.002)
Family structure (ref: married parents)		
Cohabiting parents	-0.036***	-0.035***
	(0.002)	(0.002)
Other	-0.029***	-0.030***
	(0.007)	(0.007)
Age of mother at birth	0.017***	0.017***
	(0.000)	(0.000)
Family size	0.035***	0.035***
	(0.001)	(0.001)
Birth order	-0.075***	-0.073***
	(0.001)	(0.001)
Constant	-0.795***	-0.793***
	(0.012)	(0.012)
Birth year FE	Yes	Yes
Municipality at birth FE	Yes	Yes
Number of young adults	568,130	567,230
Number of young adults from SSF	1,287	387
R-squared	0.108	0.107

Notes: The table shows the estimated associations between living in same-sex families and higher education choice. The outcome variable accounts for whether the individual is enrolled (1) or not enrolled (0) in higher education at age 19. Regressions are estimated by means of a linear probability model, for both the main sample of young adults from same-sex families (born and not born) and the subsample of young adults born in same-sex families. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.A2: Young adults from same-sex families and labor market outcomes at age 23

	Log gross earnings (1)	Log gross hourly wages (2)	Employment (3)	Full-time employment (4)	Nontraditional industry (5)
Panel A					
Young adult has lived in a					
SSF (1 is yes)	−0.004	0.023	−0.035	−0.012	−0.023
	(0.046)	(0.022)	(0.019)	(0.028)	(0.027)
Constant	10.129***	2.469***	0.794***	0.441***	−0.078
	(0.080)	(0.036)	(0.032)	(0.050)	(0.042)
Additional controls	Yes	Yes	Yes	Yes	Yes
Number of young adults	145,274	135,889	158,555	135,889	135,889
Number of young adults from SSF	311	288	368	288	288
Panel B					
Young adult has lived in a					
SSF from birth (1 is yes)	−0.150	−0.012	−0.042	−0.105	0.041
	(0.108)	(0.033)	(0.037)	(0.057)	(0.058)
Constant	10.129***	2.470***	0.793***	0.441***	−0.077
	(0.080)	(0.036)	(0.033)	(0.051)	(0.042)
Additional controls	Yes	Yes	Yes	Yes	Yes
Number of young adults	145,034	135,669	158,269	135,669	135,669
Number of young adults from SSF	71	68	82	68	68
R-squared	0.0590	0.0306	0.0284	0.105	0.0270

Notes. This table shows the estimated association between living in same-sex families and labor market outcomes of 23-year-olds who never enrolled in higher education by that age or finished higher education. Young adults have finished higher education if they graduated at age 23 or earlier and did not enroll in a new degree program. Panel A shows the results for the full sample (born and not born in same-sex families); Panel B shows the results for the subsample of young adults born in same-sex families. SSF indicates same-sex families. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix B: Supplementary Figures and Tables

Table 4.B1: Labor market outcomes of young adults from same-sex families for the full sample (both in higher education and not in higher education)

	Log gross earnings		Log gross hourly wage		Employment		Full-time employment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Young adult has lived in a SSF (1 is yes)	0.011	0.006	0.012	0.016	-0.011	0.003	-0.002	-0.000
	(0.031)	(0.030)	(0.013)	(0.013)	(0.008)	(0.008)	(0.011)	(0.011)
Sex (1 is male)	0.134***	0.133***	0.023***	0.023***	-0.017***	-0.017***	0.145***	0.145***
	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Ethnicity (ref: both parents born in NL)								
One NL parent, other Western	-0.153***	-0.126***	-0.016***	-0.011***	-0.019***	-0.017***	-0.039***	-0.033***
	(0.005)	(0.005)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
One NL parent, other non-Western	-0.125***	-0.157***	-0.014**	-0.015**	-0.019***	-0.018***	-0.054***	-0.062***
	(0.010)	(0.010)	(0.005)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)
Both parents not NL	0.132***	-0.253***	0.019***	-0.029***	-0.004**	-0.013***	-0.057***	-0.152***
	(0.005)	(0.005)	(0.002)	(0.003)	(0.001)	(0.001)	(0.002)	(0.002)
Log household income		-0.112***		0.000		-0.008***		-0.020***
		(0.002)		(0.001)		(0.000)		(0.001)
Parental education (1 is diploma SE)		-0.336***		-0.048***		0.015***		-0.098***
		(0.004)		(0.002)		(0.001)		(0.002)
Family transitions		0.050***		0.006***		-0.007***		0.010***
		(0.004)		(0.002)		(0.001)		(0.002)
Family structure (ref: married parents)								
Cohabiting parents		-0.032***		-0.004		-0.010***		-0.007***
		(0.005)		(0.002)		(0.001)		(0.002)
Other		-0.092***		-0.017*		-0.019***		-0.027***
		(0.018)		(0.008)		(0.005)		(0.006)
Age of mother at birth		-0.035***		-0.004***		-0.003***		-0.009***
		(0.000)		(0.000)		(0.000)		(0.000)
Family size		-0.002		0.013***		-0.002***		-0.003***
		(0.002)		(0.001)		(0.000)		(0.001)
Birth order		0.095***		0.007***		0.004***		0.026***
		(0.002)		(0.001)		(0.001)		(0.001)
Constant	8.851***	11.218***	2.015***	2.110***	0.945***	1.117***	0.040*	0.549***
	(0.054)	(0.058)	(0.029)	(0.030)	(0.014)	(0.015)	(0.019)	(0.021)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality at birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of young adults	527,083	527,083	486,466	486,466	568,545	568545	486,466	486,466
Number of young adults from SSF	1,167	1,167	1,062	1,062	1,288	1,288	1,062	1,062
R-squared	0.0455	0.0951	0.0438	0.0481	0.0101	0.0133	0.0643	0.0867

Notes: The table shows the estimated associations between living in same-sex families and labor market outcomes, for the full sample of young adults that includes young adults who enrolled in higher education after high school as well as young adults who discontinued their studies. SSF indicates same-sex families. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.B2: Labor market outcomes of young adults from same-sex families for selected later birth cohorts

	Log gross earnings		Log gross hourly wage		Employment		Full-time employment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Young adult has lived in a SSF (1 is yes)	−0.016	0.039	−0.027	−0.011	−0.002	0.016	−0.020	0.019
	(0.056)	(0.056)	(0.031)	(0.032)	(0.016)	(0.016)	(0.030)	(0.030)
Sex (1 is male)	0.187***	0.192***	0.037***	0.038***	−0.001	−0.002	0.261***	0.263***
	(0.005)	(0.005)	(0.003)	(0.003)	(0.001)	(0.001)	(0.003)	(0.003)
Ethnicity (ref: both parents born in NL)								
One NL parent, other Western	−0.180***	−0.163***	−0.042***	−0.037***	−0.020***	−0.019***	−0.083***	−0.076***
	(0.010)	(0.010)	(0.005)	(0.005)	(0.003)	(0.003)	(0.005)	(0.005)
One NL parent, other non-Western	−0.251***	−0.242***	−0.044***	−0.041***	−0.034***	−0.029***	−0.139***	−0.134***
	(0.021)	(0.021)	(0.010)	(0.010)	(0.006)	(0.006)	(0.010)	(0.010)
Both parents not NL	−0.289***	−0.419***	−0.037***	−0.069***	−0.041***	−0.032***	−0.192***	−0.233***
	(0.010)	(0.011)	(0.005)	(0.006)	(0.003)	(0.003)	(0.005)	(0.005)
Log household income		−0.008*		0.005***		0.005***		−0.000
		(0.003)		(0.002)		(0.001)		(0.002)
Parental education (1 is diploma SE)		−0.099***		−0.017***		0.029***		−0.028***
		(0.006)		(0.003)		(0.002)		(0.004)
Family transitions		−0.015*		−0.004		−0.010***		−0.018***
		(0.007)		(0.003)		(0.002)		(0.004)
Family structure (ref: married parents)								
Cohabiting parents		−0.070***		−0.012**		−0.015***		−0.031***
		(0.008)		(0.004)		(0.002)		(0.004)
Other		−0.165***		−0.047**		−0.030**		−0.074***
		(0.036)		(0.017)		(0.011)		(0.017)
Age of mother at birth		−0.016***		−0.004***		−0.002***		−0.006***
		(0.001)		(0.000)		(0.000)		(0.000)
Family size		0.031***		0.013***		−0.002*		0.009***
		(0.003)		(0.001)		(0.001)		(0.002)
Birth order		0.019***		0.010***		0.002		0.007***
		(0.004)		(0.002)		(0.001)		(0.002)
Constant	9.556***	10.089***	2.334***	2.364***	0.934***	0.922***	0.138**	0.304***
	(0.093)	(0.101)	(0.052)	(0.055)	(0.026)	(0.029)	(0.045)	(0.049)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality at birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of young adults	103,825	103,825	96,270	96,270	110,995	110,995	96,270	96,270
Number of young adults from SSF	257	257	238	238	277	277	238	238
R-squared	0.0647	0.0756	0.0246	0.0294	0.0116	0.0162	0.127	0.131

Notes: The table shows the estimated associations between living in same-sex families and labor market outcomes, for individuals born in 1998 and 1999. SSF indicates same-sex families. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.B3: Young adults from same-sex families and net earnings

	Log net earnings		Log net hourly wages	
	(1)	(2)	(3)	(4)
Young adult has lived in a SSF (1 is yes)	-0.046	-0.004	-0.002	0.010
	(0.038)	(0.037)	(0.019)	(0.019)
Sex (1 is male)	0.180***	0.186***	0.023***	0.025***
	(0.004)	(0.004)	(0.002)	(0.002)
Ethnicity (ref: both parents born in NL)				
One NL parent, other Western	-0.158***	-0.140***	-0.033***	-0.029***
	(0.007)	(0.007)	(0.003)	(0.003)
One NL parent, other non-Western	-0.257***	-0.251***	-0.041***	-0.039***
	(0.015)	(0.015)	(0.007)	(0.007)
Both parents not NL	-0.272***	-0.420***	-0.015***	-0.051***
	(0.007)	(0.007)	(0.003)	(0.004)
Log net household income		-0.009***		0.002
		(0.002)		(0.001)
Parental education (1 is diploma SE)		-0.122***		-0.026***
		(0.004)		(0.002)
Family transitions		-0.004		-0.002
		(0.004)		(0.002)
Family structure (ref: married parents)				
Cohabiting parents		-0.080***		-0.011***
		(0.006)		(0.003)
Other		-0.117***		-0.034**
		(0.023)		(0.011)
Age of mother at birth		-0.016***		-0.004***
		(0.001)		(0.000)
Family size		0.031***		0.010***
		(0.002)		(0.001)
Birth order		0.022***		0.011***
		(0.003)		(0.001)
Constant	9.054***	9.579***	1.915***	1.985***
	(0.062)	(0.068)	(0.037)	(0.039)
Birth year FE	Yes	Yes	Yes	Yes
Municipality at birth FE	Yes	Yes	Yes	Yes
Number of young adults	222,169	222,169	205,738	205,738
Number of young adults from SSF	544	544	491	491
R-squared	0.0824	0.0953	0.0712	0.0753

The table shows the estimated associations between living in same-sex families and log net earnings. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.B4: Young adults from same-sex families and labor market outcomes, with data processed by means of Coarsened Exact Matching (CEM)

	Log gross earnings (1)	Log gross hourly wages (2)	Employment (3)	Full-time employment (4)
Young adult has lived in a SSF (1 is yes)	-0.013	0.015	-0.017	-0.035
	(0.054)	(0.028)	(0.017)	(0.029)
Constant	9.493***	2.258***	0.912***	0.347***
	(0.024)	(0.014)	(0.009)	(0.014)
Number of young adults	20,219	18,884	21,400	18,884
Number of young adults from SSF	365	337	408	337

The table shows the estimated association between living in a same-sex family and labor market outcomes. The sample is defined by means of a CEM matching procedure. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.B5: Logistic regression on employment, full-time employment, and industry choices as outcome variables

	Employment		Full-time employment		Nontraditional industry	
	(1)	(2)	(3)	(4)	(5)	(6)
Young adult has lived in a SSF (1 is yes)	-0.021	-0.001	-0.034	-0.007	0.028	0.008
	(0.011)	(0.009)	(0.020)	(0.022)	(0.020)	(0.019)
Individual controls[a]	Yes	Yes	Yes	Yes	Yes	Yes
SES controls[b]	No	Yes	No	Yes	No	Yes
Family controls[c]	No	Yes	No	Yes	No	Yes
Number of young adults	239,292	239,292	205,767	205,767	205,767	205,767
Number of young adults from SSF	605	605	491	491	491	491

Notes. The table shows the estimated associations between living in same-sex families and our binary outcome variables (employment, full-time employment, and employment in a nontraditional sector) using a logit specification. The sample includes both young adults born and not born in a same-sex family. To facilitate comparison with the results obtained using a Linear Probability Model, we report the average marginal effects. Standard errors computed using the delta method are in parenthesis.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[a] Individual controls include ethnicity, birth year fixed effects and municipality at birth fixed effects.

[b] SES controls include log household income and parental education.

[c] Family controls include family transitions, family structure, age of mother at birth, family size, and birth order.

Table 4.B6: Number of years in same-sex families and labor market outcomes

	Log gross earnings		Log gross hourly wage		Employment		Full-time employment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of years in same-sex family	-0.014**	-0.007	-0.005	-0.003	-0.001	0.000	-0.008***	-0.005*
	(0.005)	(0.005)	(0.003)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Sex (1 is male)	0.187***	0.193***	0.041***	0.043***	-0.004***	-0.004***	0.250***	0.251***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Ethnicity (ref: both parents born in NL)								
One NL parent, other Western	-0.161***	-0.143***	-0.041***	-0.036***	-0.018***	-0.016***	-0.069***	-0.063***
	(0.007)	(0.007)	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)
One NL parent, other non-Western	-0.260***	-0.255***	-0.050***	-0.047***	-0.026***	-0.021***	-0.124***	-0.121***
	(0.015)	(0.015)	(0.008)	(0.008)	(0.004)	(0.004)	(0.007)	(0.007)
Both parents not NL	-0.278***	-0.429***	-0.026***	-0.066***	-0.040***	-0.029***	-0.182***	-0.228***
	(0.007)	(0.007)	(0.004)	(0.004)	(0.002)	(0.002)	(0.003)	(0.003)
Log household income		-0.012***		0.004***		0.006***		-0.000
		(0.002)		(0.001)		(0.001)		(0.001)
Parental education (1 is diploma SE)		-0.122***		-0.029***		0.027***		-0.038***
		(0.004)		(0.002)		(0.001)		(0.002)
Family transitions		-0.005		-0.003		-0.012***		-0.010***
		(0.004)		(0.002)		(0.001)		(0.002)
Family structure (ref: married parents)								
Cohabiting parents		-0.081***		-0.014***		-0.018***		-0.032***
		(0.006)		(0.003)		(0.002)		(0.003)
Other		-0.120***		-0.036**		-0.027***		-0.059***
		(0.024)		(0.012)		(0.007)		(0.011)
Age of mother at birth		-0.016***		-0.005***		-0.002***		-0.005***
		(0.001)		(0.000)		(0.000)		(0.000)
Family size		0.031***		0.012***		-0.003***		0.010***
		(0.002)		(0.001)		(0.001)		(0.001)
Birth order		0.022***		0.010***		0.001		0.005***
		(0.003)		(0.001)		(0.001)		(0.001)
Constant	9.060***	9.623***	2.013***	2.072***	0.895***	0.873***	0.068*	0.215***
	(0.063)	(0.068)	(0.039)	(0.042)	(0.020)	(0.022)	(0.032)	(0.035)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality at birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of young adults	222,200	222,200	205,767	205,767	239,313	239,313	205,767	205,767
Number of young adults from SSF	544	544	491	491	605	605	491	491
R-squared	0.0830	0.0958	0.0699	0.0748	0.0125	0.0170	0.123	0.128

Notes: The table shows the estimated associations between labor market outcomes and number of years spent in a same-sex family, ranging from 0 (never lived in a same-sex family) to 16 (always lived in a same-sex family from birth). SSF indicates same-sex families. Robust standard errors, clustered at the individual level, in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.B7: Young adults from same-sex families, labor market outcomes and birth order index

	Log gross earnings (1)	Log gross hourly wages (2)	Employ-ment (3)	Full-time employment (4)
Young adult has lived in a SSF (1 is yes)	-0.003	0.010	-0.002	-0.007
	(0.038)	(0.020)	(0.012)	(0.020)
Sex (1 is male)	0.193***	0.043***	-0.004***	0.252***
	(0.004)	(0.002)	(0.001)	(0.002)
Ethnicity (ref: both parents born in NL)				
One NL parent, other Western	-0.143***	-0.036***	-0.016***	-0.063***
	(0.007)	(0.003)	(0.002)	(0.004)
One NL parent, other non-Western	-0.255***	-0.047***	-0.021***	-0.121***
	(0.015)	(0.008)	(0.004)	(0.007)
Both parents not NL	-0.429***	-0.066***	-0.029***	-0.228***
	(0.007)	(0.004)	(0.002)	(0.003)
Log household income	-0.012***	0.004***	0.006***	-0.000
	(0.002)	(0.001)	(0.001)	(0.001)
Parental education (1 is diploma SE)	-0.121***	-0.029***	0.027***	-0.038***
	(0.004)	(0.002)	(0.001)	(0.002)
Family transitions	-0.006	-0.004	-0.012***	-0.010***
	(0.004)	(0.002)	(0.001)	(0.003)
Family structure (ref: married parents)				
Cohabiting parents	-0.080***	-0.014***	-0.018***	-0.032***
	(0.006)	(0.003)	(0.002)	(0.003)
Other	-0.118***	-0.035**	-0.026***	-0.059***
	(0.024)	(0.012)	(0.007)	(0.011)
Age of mother at birth	-0.016***	-0.005***	-0.002***	-0.005***
	(0.001)	(0.000)	(0.000)	(0.000)
Family size	0.043***	0.018***	-0.002***	0.013***
	(0.001)	(0.001)	(0.000)	(0.001)
Birth order index	0.054***	0.026***	0.003	0.014***
	(0.006)	(0.003)	(0.002)	(0.003)
Constant	9.582***	2.053***	0.871***	0.205***
	(0.068)	(0.041)	(0.021)	(0.035)
Birth year FE	Yes	Yes	Yes	Yes
Municipality at birth FE	Yes	Yes	Yes	Yes
Number of young adults	222,200	205,767	239,313	205,767
Number of young adults from SSF	544	491	605	491
R-squared	0.0958	0.0749	0.0170	0.128

Notes. The table shows the estimated associations between living in same-sex families and labor market outcomes, when family size effects are purged from birth order. Following booth2009birth, a birth order index is estimated in place of the discrete variable that accounts for the child's birth order. The birth order index is obtained as follows. First, the average birth order (A) is calculated as $A = (N+1)/2$, where N is family size; next, the birth order index (B) is obtained by dividing the absolute birth order (ϕ) by the average birth order (A). In formula: $B = \phi/A$. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.B8: Labor market outcomes of young adults from same-sex families and categorical parental education

	Log gross earnings (1)	Log gross hourly wages (2)	Employ-ment (3)	Full-time employment (4)
Young adult has lived in a SSF (1 is yes)	0.042	0.019	0.010	0.005
	(0.043)	(0.022)	(0.014)	(0.023)
Sex (1 is male)	0.173***	0.043***	-0.007***	0.234***
	(0.005)	(0.002)	(0.001)	(0.002)
Ethnicity (ref: both parents born in NL)				
One NL parent, other Western	-0.123***	-0.036***	-0.015***	-0.053***
	(0.008)	(0.004)	(0.003)	(0.004)
One NL parent, other non-Western	-0.226***	-0.037***	-0.012*	-0.109***
	(0.018)	(0.009)	(0.005)	(0.008)
Both parents not NL	-0.395***	-0.054***	-0.027***	-0.211***
	(0.008)	(0.004)	(0.003)	(0.004)
Log household income	-0.002	0.007***	0.009***	0.006***
	(0.003)	(0.002)	(0.001)	(0.001)
Parental education (ref: one parent is a school dropout)				
Both parents hold SE diploma	-0.045***	-0.014***	0.020***	-0.014***
	(0.006)	(0.003)	(0.002)	(0.003)
One parent has higher education diploma	-0.188***	-0.040***	0.005**	-0.059***
	(0.006)	(0.003)	(0.002)	(0.003)
Both parents have higher education diploma	-0.399***	-0.081***	-0.029***	-0.126***
	(0.013)	(0.006)	(0.004)	(0.006)
Family transitions	0.001	0.000	-0.012***	-0.004
	(0.005)	(0.003)	(0.002)	(0.003)
Family structure (ref: married parents)				
Cohabiting parents	-0.074***	-0.014***	-0.023***	-0.028***
	(0.008)	(0.004)	(0.002)	(0.004)
Other	-0.137***	-0.050***	-0.033***	-0.061***
	(0.029)	(0.015)	(0.009)	(0.013)
Age of mother at birth	-0.012***	-0.004***	-0.001***	-0.004***
	(0.001)	(0.000)	(0.000)	(0.000)
Family size	0.033***	0.012***	-0.002*	0.010***
	(0.003)	(0.001)	(0.001)	(0.001)
Birth order	0.016***	0.011***	-0.002	0.004*
	(0.003)	(0.002)	(0.001)	(0.002)
Constant	9.392***	2.000***	0.816***	0.116**
	(0.080)	(0.052)	(0.029)	(0.041)
Birth year FE	Yes	Yes	Yes	Yes
Municipality at birth FE	Yes	Yes	Yes	Yes
Number of young adults	142,582	131,530	156,241	131,530
Number of young adults from SSF	421	380	471	380
R-squared	0.0891	0.0675	0.0142	0.120

Notes. The table shows the estimated associations between living in same-sex families and labor market outcomes, with parental education included as a categorical variable. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.B9: Labor market outcomes of young adults aged 20 and 19 years old

	Log gross earnings		Log gross hourly wage		Employment		Full-time employment	
PANEL A: Age 20	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Young adult has lived in a SSF (1 is yes)	-0.027	0.014	-0.003	0.005	-0.020	-0.005	-0.018	0.003
	(0.038)	(0.038)	(0.021)	(0.021)	(0.011)	(0.011)	(0.017)	(0.018)
Constant	8.689***	9.553***	1.711***	1.807***	0.912***	0.878***	-0.013	0.134***
	(0.059)	(0.064)	(0.046)	(0.048)	(0.019)	(0.021)	(0.026)	(0.029)
Individual controls[a]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SES controls[b]	No	Yes	No	Yes	No	Yes	No	Yes
Family controls[c]	No	Yes	No	Yes	No	Yes	No	Yes
Number of young adults	226,437	226,437	207,362	207,362	239,313	239,313	207,362	207,362
Number of young adults from SSF	557	557	505	505	605	605	505	505
R-squared	0.0743	0.0946	0.0517	0.0583	0.00871	0.0125	0.118	0.125
PANEL B: Age 19								
Young adult has lived in a SSF (1 is yes)	-0.048	-0.011	-0.007	-0.007	-0.003	0.009	-0.018	-0.006
	(0.036)	(0.036)	(0.022)	(0.022)	(0.008)	(0.008)	(0.014)	(0.014)
Constant	8.331***	9.336***	1.496***	1.496***	0.948***	0.906***	-0.017	0.084***
	(0.052)	(0.058)	(0.044)	(0.044)	(0.016)	(0.017)	(0.022)	(0.024)
Individual controls[a]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SES controls[b]	No	Yes	No	Yes	No	Yes	No	Yes
Family controls[c]	No	Yes	No	Yes	No	Yes	No	Yes
Number of young adults	230,376	230,376	205,320	205,320	239,313	239,313	205,320	205,320
Number of young adults from SSF	579	579	503	503	605	605	503	503
R-squared	0.0544	0.0825	0.0299	0.0299	0.00577	0.00916	0.106	0.113

Notes: The table shows the estimated association between living in same-sex families and labor market outcomes, for young adults born between 1995 and 1999 and aged 20 (*PANEL A*) and 19 (*PANEL B*) years old. SSF indicates same-sex families. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[a]Individual controls include sex, ethnicity, birth year fixed effects and municipality at birth fixed effects.

[b]SES controls include log household income and parental education.

[c]Family controls include family transitions, family structure, family size, age of mother at birth and birth order.

Table 4.B10: Theoretical mechanisms for young adults raised by same-sex parents from birth

Panel A: Sex identity theory	Males				Females			
	Log gross earnings (1)	Log gross hourly wage (2)	Employ-ment (3)	Full-time empl. (4)	Log gross earnings (5)	Log gross hourly wage (6)	Employ-ment (7)	Full-time empl. (8)
Young adult has lived in a SSF (1 is yes)	−0.264*	−0.078	0.061**	−0.103	−0.319	−0.070	−0.035	−0.022
	(0.104)	(0.059)	(0.021)	(0.054)	(0.200)	(0.077)	(0.050)	(0.014)
Constant	9.989***	2.108***	0.860***	0.556***	9.418***	2.063***	0.887***	0.328***
	(0.104)	(0.066)	(0.032)	(0.057)	(0.091)	(0.052)	(0.029)	(0.022)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of young adults	123,380	113,029	133,095	113,029	98,381	92,339	105,725	243,715
No. of young adults from SSF	68	63	70	63	37	29	42	29
R-squared	0.102	0.0740	0.0258	0.0946	0.0737	0.0775	0.0127	0.0283
Panel B: Discrimination theory	Urban				Rural			
	Log gross earnings (1)	Log gross hourly wage (2)	Employ-ment (3)	Full-time empl. (4)	Log gross earnings (5)	Log gross hourly wage (6)	Employ-ment (7)	Full-time empl. (8)
Young adult has lived in a SSF (1 is yes)	−0.266*	−0.051	0.041	−0.134**	−0.342*	−0.152	−0.002	−0.091
	(0.126)	(0.056)	(0.026)	(0.046)	(0.147)	(0.085)	(0.047)	(0.077)
Constant	9.735***	2.030***	0.842***	0.243**	9.556***	2.066***	0.887***	0.191***
	(0.159)	(0.140)	(0.060)	(0.088)	(0.078)	(0.045)	(0.024)	(0.040)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of young adults	101,716	93,473	111,235	93,473	120,045	111,895	127,585	111,895
No. of young adults from SSF	72	62	76	62	33	30	36	30
R-squared	0.0708	0.0689	0.0196	0.100	0.108	0.0823	0.0175	0.139

Notes. The table shows the results from testing the sex identity theory, and the discrimination theory on the subsample of young adults raised by same-sex parents from birth until age 15. SSF indicates same-sex families. Panel A shows labor market outcomes separately for males and females born in same-sex families (sex identity theory); Panel B shows labor market outcomes of young adults born in same-sex families according to whether they are residing in urban or rural areas (discrimination theory). We start by describing Panel A. Although a negative association on earnings (in most cases not significant) is found both for females and males from same-sex families, females from same-sex families appear to suffer smaller losses (both in terms of log gross earnings and log gross hourly wages) than males from same-sex families. However, a test of equality of coefficients fails to reject the null hypothesis of equality of coefficients between females and males from same-sex families, suggesting no role of the sex identity theory in shaping young adults' outcomes on the labor market. Turning to Panel B, although the negative association (in most cases not significant) on both annual earnings and hourly wages appears to be larger for young adults born in same-sex families who live in rural areas than for young adults born in same-sex families who live in urban areas, a test of equality of coefficients fails to reject the null hypothesis of equality of coefficients for all the outcome variables. However, we should emphasize the low sample size of young adults born in same-sex families when the sample is splitted by sex and urbanicity, which suggests caution in the interpretation of results.

Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.B11: Young adults from same-sex families and female-dominated industries

Outcome: Employed in female-dominated sector	Full sample		Born	
	(1)	(2)	(3)	(4)
Young adult has lived in a SSF (1 is yes)	0.046*	0.041*	0.175***	0.159***
	(0.020)	(0.020)	(0.048)	(0.048)
Individual controls[a]	Yes	Yes	Yes	Yes
SES controls[b]	No	Yes	No	Yes
Family controls[c]	No	Yes	No	Yes
Number of young adults	205,767	205,767	205,368	205,368
Number of young adults from SSF	491	491	92	92
R-squared	0.219	0.220	0.219	0.220

The table shows the estimated association between living in same-sex families and sorting into female-dominated sectors on the labor market. Full sample includes both young adults born and not born in same-sex families; born includes only young adults raised in same-sex families from birth until age 15. Sectors are defined on the basis of sector codes provided by the Tax and Customs Administration in the Netherlands (Belastingdienst, 2021). Female-dominated (male-dominated) sectors are sectors in which the percentage of employed women is higher (lower) than the percentage of total women in the Dutch workforce, for every given year. Robust standard errors, clustered at the individual level, in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[a]Individual controls include sex, ethnicity, birth year fixed effects and municipality at birth fixed effects.

[b]SES controls include log household income and parental education.

[c]Family controls include family transitions, family structure, age of mother at birth, family size, and birth order.

Table 4.B12: Young adults born in same-sex families and nontraditional industries by sex

Outcome: Employed in a nontraditional industry	Males		Females	
	(1)	(2)	(3)	(4)
Young adult has lived in a SSF from birth (1 is yes)	0.240***	0.203***	-0.000	-0.009
	(0.060)	(0.061)	(0.070)	(0.070)
Individual controls[a]	Yes	Yes	Yes	Yes
SES controls[b]	No	Yes	No	Yes
Family controls[c]	No	Yes	No	Yes
Number of young adults	113,029	113,029	92,339	92,339
Number of young adults from SSF	63	63	29	29
R-squared	0.0336	0.0382	0.00830	0.00885

Notes. The table shows the estimated associations between living in same-sex families from birth and the likelihood of being employed in a nontraditional industry by sex. Nontraditional industries are defined by comparing the sector's sex composition to the individual's sex. Specifically, an individual is assigned to a nontraditional industry if the sector in which the individual is employed is predominantly occupied by the opposite sex. This means that in the subsample of males, a nontraditional industry is an occupation in a female-dominated sector; in the subsample of females, a nontraditional industry is an occupation in a male-dominated sector. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[a] Individual controls include ethnicity, birth year fixed effects and municipality at birth fixed effects.

[b] SES controls include log household income and parental education.

[c] Family controls include family transitions, family structure, age of mother at birth, family size, and birth order.

Table 4.B13: Multiple hypothesis testing

	Log gross earnings		Log gross hourly wage		Employ-ment		Full-time employment		Nontraditional sector	
Regression model	*p*-value (1)	*q*-value (2)	*p*-value (3)	*q*-value (4)	*p*-value (5)	*q*-value (6)	*p*-value (7)	*q*-value (8)	*p*-value (9)	*q*-value (10)
Main regression (born and not born)	0.9399	1.000	0.6301	1.000	0.7469	1.000	0.8935	1.000	0.6363	1.000
By sex: male	0.6006	1.000	0.5574	1.000	0.9712	1.000	0.4642	1.000		
By sex: female	0.6667	1.000	0.8912	1.000	0.8109	1.000	0.6245	1.000		
By urbanicity: urban	0.4533	1.000	0.9514	1.000	0.7967	1.000	0.2367	1.000		
By urbanicity: rural	0.3023	1.000	0.5637	1.000	0.6028	1.000	0.4598	1.000		
Main regression (born)	0.0024**	0.022*	0.0822	0.897	0.0019**	1.000	0.2796	0.022*	0.0023	0.022*

Notes. The table shows *p*-values and *q*-values constructed from the coefficient showing the influence of same-sex families on selected outcomes, as shown in Table 4.2, Table 4.4, Table 4.3, and Table 4.5. *q*-values are constructed accounting for the False Discovery Rate (FDR) following Anderson (2008) and using *p*-values in this table. All regressions include controls for ethnicity, sex (except for regressions estimating heterogeneity by sex), birth year fixed effects, municipality at birth fixed effects, log household income, parental education, family transitions, family structure, age of mother at birth, family size, and birth order. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix C: Details on human capital/signalling theory and family formation theory

C.1 Details on the mediation analyses

According to Baron and Kenny (1986), testing for mediation involves the estimation of three separate regressions. First, a regression of the dependent variables on the key independent variable of interest (in our case H_i, i.e. residing in a same-sex versus a different-sex family); second, a regression of each mediator on the key independent variable of interest; third, a regression of the dependent variables on both the key independent variable of interest and the mediator(s). Mediation occurs if (1) the key independent variable significantly correlates with the dependent variable in the first regression; (2) the key independent variable significantly correlates with the mediator in the second regression (thus establishing the relevance of the mediator); (3) the association between the key independent variable and the dependent variable is lower in the third regression than in the first one. If all conditions hold, then the mediator is sweeping away part (or all in case of complete mediation) of the estimated association between the key variable of interest and the outcome variables. Note that, in line with Hayes (2009); Zhao et al. (2010), we estimate mediation models regardless of whether we find that residing in same-sex families significantly correlates with labor market outcomes in our main model. Moreover, we formally test the significance of the mediation indirect effect using the bootstrap method suggested by Preacher and Hayes (2004).

To establish the role of diploma attainment as a mediator, the following conditions should hold: first, young adults from same-sex families should be significantly more likely to graduate from high school than young adults from different-sex families. This was found in Mazrekaj et al. (2020); Kabátek and Perales (2021) in the Netherlands. Second, diploma attainment should be positively and significantly associated with labor market outcomes. Third, the estimated association between residing in same-sex families and labor market outcomes should be lower in the third regression than in the second one, thus proving that controlling for human capital formation cancels out part of the positive (negative) association between living in same-sex families and subsequent positive (negative) labor market outcomes.

Similarly, to establish the role of family size and birth order as mediators, first, young adults from same-sex families should be significantly more likely to live in small families and be early in the birth order.[32] Second, small family size and low birth order should be positively and significantly correlated with labor market outcomes. Third, the estimated association between residing in same-sex versus different-sex families should be lower in the third than in the second regression, therefore proving that living in smaller families

[32] Note that we estimate the relationship between living in same-sex families, family size and birth order through an OLS model. However, our results are robust to the use of the Poisson specification to account for count data.

partly accounts for the positive (negative) association between living in same-sex families and subsequent positive (negative) labor market outcomes.

C.2 Estimation results

To test the human capital/signalling theory and the family formation theory, we perform separate mediation analyses. We show our results for the human capital/signalling theory in Table 4.C1, while we refer to Table 4.C2 for the family formation theory. In both Tables 4.C1 and 4.C2 we report in Panel A the mediation analysis for the full sample (both born and not born in same-sex families), and in Panel B the mediation analysis for the subsample of young adults born in same-sex families. To save space, in almost all regressions we control for the full set of covariates, but similar results are found also when we control only for individual characteristics.

We start by describing Table 4.C1. In Panel A and Panel B we show the estimated first (columns 1 and 2) and third (columns 3 to 6) regressions, while we refer to Table 4.2 and Table 4.3 for the estimated second regressions. A first requirement to establish mediation is that the key variable of interest is significantly correlated in the predicted direction with the mediator. The first two columns of Table 4.C1 compare young adults from same-sex families to young adults from different-sex families on the likelihood of obtaining a diploma at the end of secondary education. Our results suggest that young adults from same-sex families are not differently likely than young adults from different-sex families to graduate from high school. In column 2, where we control for the full set of covariates, the coefficient of interest (namely whether the individual has resided in a same-sex family at least once) is positive but not significant both for the full sample and for the sample of young adults born in same-sex families.[33] Given that we find no evidence supporting the relevance of the mediator, our results are suggestive that the human capital/signalling theory does not play a role in shaping young adults' outcomes on the labor market. These results are robust to testing the significance of the indirect effect using the bootstrap method suggested by Preacher and Hayes (2004). As an additional remark, we should note that in the last four columns of Table 4.C1, we compare labor market outcomes of young adults from same-sex versus different-sex families while additionally controlling for diploma attainment. Findings from these regressions show that young adults from same-sex families do not perform differently than young adults from different-sex families on the labor market also when the model is specified to include a control variable for human capital. The coefficient of interest (i.e. having resided in a same-sex versus a different-sex family) is again not significant and close in size to what estimated in Table 4.2 and in Table 4.3, signalling that our results are not sensitive to the choice of not controlling for human capital formation in our main model specification.

[33] Note that our results are consistent with what reported by Mazrekaj et al. (2020), who show that children who resided in a same-sex family at least once perform as well as their peers in different-sex families on diploma attainment.

We now turn to describe Panel A and Panel B of Table 4.C2, where we show the estimated first (columns 1 and 2) and second (columns 3 to 6) regressions, while we refer to Table 4.2 and Table 4.3 for the estimated third regression. Again, we first check whether the key variable of interest is significantly correlated in the predicted direction with the mediators. Our results show that young adults from same-sex families are significantly more likely to live in small families and to have a low birth order than young adults from different-sex families, thus confirming the relevance of the mediator. Next, we check whether the associations between family size, birth order and labor market outcomes hold in the predicted direction in our main regressions in Table 4.2 and Table 4.3. Our results indicate that family size and birth order, contrarily to our expectations, positively correlate in most cases with labor market outcomes. That is, young adults from larger families and with a higher birth order tend to have higher earnings and are more likely employed in full-time occupations than young adults from smaller families or with a lower birth order. By contrast, we find a negative relationship only between birth order and employment likelihoods. Given that the relationship between the mediators and the dependent variables by majority does not hold in the direction predicted by the theory[34], we exclude that the family formation theory might be a relevant mechanism influencing young adults' performance on the labor market. It should be noted that although using bootstrap as in Preacher and Hayes (2004) to test for the significance of the indirect effect yields a significant indirect effect of family size and birth order, given that the conditions to establish mediation developed by Baron and Kenny (1986) are not satisfied, we refrain from concluding that family size and birth order are mediators in the relationship between having lived in same-sex families and labor market outcomes as predicted by the family formation theory. Besides, the last four columns of Panel B show that regression coefficients are very similar to those reported in Table 4.2 and Table 4.3 where family size and birth order are included as additional covariates.

[34] We also re-estimate Table 4.2 by including a categorical variable instead than a discrete variable for birth order (not reported). We construct the latter variable by distinguishing first born, second born, third born, and later than third born. Our results do not show that young adults high in the birth order perform worse on the labor market than young adults low on the birth order, thus confirming the results from our main model specification.

Table 4.C1: Young adults from same-sex families, human capital theory and labor market outcomes

Panel A	Diploma attainment			Log gross earnings	Log gross hourly wages	Employ-ment	Full-time employment
	(1)	(2)		(3)	(4)	(5)	(6)
Young adult has lived in a SSF (1 is yes)	−0.033	0.011	Young adult has lived in a SSF (1 is yes)	0.027	0.004	−0.000	0.003
	(0.018)	(0.018)		(0.040)	(0.023)	(0.011)	(0.023)
			Diploma attainment	0.184***	0.018***	0.116***	0.062***
				(0.007)	(0.003)	(0.002)	(0.003)
Constant	0.843***	0.755***	Constant	9.651***	2.151***	0.810***	0.283***
	(0.002)	(0.012)		(0.031)	(0.017)	(0.009)	(0.016)
Individual Controls[a]	Yes	Yes	Individual Controls[a]	Yes	Yes	Yes	Yes
SES controls[b]	No	Yes	SES controls[b]	Yes	Yes	Yes	Yes
Family controls[c]	No	Yes	Family controls[c]	Yes	Yes	Yes	Yes
Number of young adults	206,882	206,882	Number of young adults	197,136	183,234	206,882	183,234
Number of young adults from SSF	482	482	Number of young adults from SSF	449	407	482	407
R-squared	0.0216	0.0254	R-squared	0.103	0.0760	0.0477	0.131

Panel B	Diploma attainment			Log gross earnings	Log gross hourly wages	Employ-ment	Full-time employment
	(1)	(2)		(3)	(4)	(5)	(6)
Young adult has lived in a SSF from birth (1 is yes)	0.050	0.051	Young adult has lived in a SSF from birth (1 is yes)	−0.267**	−0.099	−0.010	−0.128**
	(0.032)	(0.032)		(0.098)	(0.057)	(0.027)	(0.048)
			Diploma attainment	0.184***	0.018***	0.116***	0.061***
				(0.007)	(0.003)	(0.002)	(0.003)
Constant	0.843***	0.756***	Constant	9.650***	2.151***	0.811***	0.283***
	(0.002)	(0.012)		(0.031)	(0.017)	(0.009)	(0.016)
Individual Controls[a]	Yes	Yes	Individual Controls[a]	Yes	Yes	Yes	Yes
SES controls[b]	No	Yes	SES controls[b]	Yes	Yes	Yes	Yes
Family controls[c]	No	Yes	Family controls[c]	Yes	Yes	Yes	Yes
Number of young adults	206,483	206,483	Number of young adults	196,765	182,896	206,483	182,896
Number of young adults from SSF	83	83	Number of young adults from SSF	78	69	83	69
R-squared	0.0216	0.0254	R-squared	0.103	0.0761	0.0477	0.131

Notes: The table shows the mediation analyses employed to test the human capital/signalling theory. Panel A reports the results for the full sample of young adults from same-sex families (both born and not born in a same-sex family); Panel B reports the results for the subsample of young adults raised in same-sex families from birth. Columns 1 and 2 report the estimated association between having lived in a same-sex family and diploma attainment; columns 3 to 6 report the estimated association between having lived in a same-sex family and labor market outcomes when controlling for diploma attainment. Diploma attainment is a binary indicator coded 1 if the individual has passed high school exit exams at first attempt, and 0 if the individual has not passed high school exit exams at first attempt or has dropped out of school. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[a]Individual controls include sex, ethnicity, birth year fixed effects and municipality at birth fixed effects.

[b]SES controls include log household income and parental education.

[c]Family controls include family transitions, family structure, age of mother at birth, family size, and birth order.

Table 4.C2: Young adults in same-sex families, family formation theory and labor market outcomes

Panel A	Family size	Birth order		Log gross earnings	Log gross hourly wages	Employ-ment	Full-time employment
	(1)	(2)		(3)	(4)	(5)	(6)
Young adult has lived in a SSF (1 is yes)	−0.219***	−0.216***	Young adult has lived in a SSF from birth (1 is yes)	−0.013	0.006	−0.001	−0.009
	(0.043)	(0.034)		(0.038)	(0.020)	(0.012)	(0.020)
Constant	3.042***	−0.797***	Constant	9.703***	2.101***	0.864***	0.242***
	(0.087)	(0.070)		(0.068)	(0.042)	(0.021)	(0.035)
Individual Controls[a]	Yes	Yes	Individual Controls[a]	Yes	Yes	Yes	Yes
SES controls[b]	No	Yes	SES controls[b]	Yes	Yes	Yes	Yes
Family controls[c1]	No	Yes	Family controls[c2]	Yes	Yes	Yes	Yes
Number of young adults	239,313	239,313	Number of young adults	222,200	205,767	239,313	205,767
Number of young adults from SSF	605	605	Number of young adults from SSF	544	491	605	491
R-squared	0.159	0.260	R-squared	0.0919	0.0720	0.0169	0.127

Panel B	Family size	Birth order		Log gross earnings	Log gross hourly wages	Employ-ment	Full-time employment
	(1)	(2)		(3)	(4)	(5)	(6)
Young adult has lived in a SSF from birth (1 is yes)	−0.327***	−0.652***	Young adult has lived in a SSF from birth (1 is yes)	−0.318**	−0.092	0.025	−0.131**
	(0.082)	(0.069)		(0.097)	(0.047)	(0.023)	(0.040)
Constant	3.040***	−0.803***	Constant	9.701***	2.102***	0.865***	0.243***
	(0.087)	(0.071)		(0.068)	(0.042)	(0.021)	(0.035)
Individual Controls[a]	Yes	Yes	Individual Controls[a]	Yes	Yes	Yes	Yes
SES controls[b]	No	Yes	SES controls[b]	Yes	Yes	Yes	Yes
Family controls[c1]	No	Yes	Family controls[c2]	Yes	Yes	Yes	Yes
Number of young adults	238,820	238,820	Number of young adults	221,761	205,368	238,820	205,368
Number of young adults from SSF	112	112	Number of young adults from SSF	105	92	112	92
R-squared	0.159	0.261	R-squared	0.0920	0.0721	0.0168	0.127

Notes: The table shows the mediation analyses employed to test the family formation theory. Panel A reports the results for the full sample of young adults from same-sex families (both born and not born in a same-sex family); Panel B reports the results for the subsample of young adults raised in same-sex families from birth. Column 1 reports the estimated association between having lived in a same-sex family and family size; column 2 reports the estimated association between having lived in a same-sex family and birth order; columns 3 to 6 report the estimated association between having lived in a same-sex family and labor market outcomes when the full set of covariates does not include family size and birth order. Note that regressions in column 1 and 2 are estimated by means of an OLS model. However, similar results are obtained with a Poisson model. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[a] Individual controls include sex, ethnicity, birth year fixed effects and municipality at birth fixed effects.

[b] SES controls include log household income and parental education.

[c1] Family controls include family transitions, family structure, age of mother at birth, family size, and birth order.

[c2] Family controls include family transitions, family structure, and age of mother at birth.

Appendix D: Details on parent sector and role model hypotheses

D.1 Parent sector hypothesis

As our sample of same-sex families predominantly consists of female same-sex families, and most women in the Netherlands are employed in part-time rather than in full-time occupations (OECD, 2019a), it might be argued that young adults from same-sex families may be 'following their mothers' in preferring part-time to full-time occupations, therefore sorting into jobs with high female representation. To test this, we run an additional analysis in which we look at the likelihood that young adults are employed in the same sector in which at least one of their parents was employed while they were in their teen years. To link young adults' sectors to parent sectors, first, we observe the household composition in which young adults were living at age 15. This corresponds to the last year in which we require individuals to have lived with both their parents (or with a parent and a step-parent). Next, we look at the sector history of parents and define a new binary outcome variable for the sample of young adults whose parents were both employed when young adults were aged 15 years old. We code this variable 1 if the young adult (at age 21) is employed in the same sector in which at least one of the parents (or step-parents) was employed when the young adult was aged 15 years old; 0 if the young adult (at age 21) is employed in a different sector than both parents.[35] We display our results in Table 4.D1 below. Our findings show, consistently across model specifications, that young adults born in same-sex families are not statistically different than young adults from different-sex families to be employed in the same sector as their parents. Therefore, we conclude that young adults born in same-sex families do not follow their mothers in choosing the industry in which they will be employed in the future.

[35]Note that we test the robustness of our findings to the definition of our outcome variable as follows. First, given that requiring both parents to be employed restricts the sample size on which our analysis is estimated, we redefine our outcome variable to include young adults for whom one parent was unemployed or inactive when they were in their teen years. More precisely, we re-code our outcome variable as 1 if the young adult is employed in the same sector in which at least one parent (or step-parents) was employed; 0 if the young adult is employed in a different sector than both parents, or if the young adult is employed in a different sector than one parent and the other parent is inactive/unemployed (young adults for whom both parents are unemployed or inactive are not included). Second, we check whether our results are sensitive to the time frame in which we measure parent sectors. That is, we measure parent sectors contemporaneously to young adults' sectors, and re-code the outcome variable accordingly. Such estimations can be found in columns 3 to 8 of Table 4.D1, and confirm that young adults in same-sex families are not statistically different than young adults in different-sex families to be employed in a sector where at least one of their parents is employed.

Table 4.D1: Young adults born in same-sex families and intergenerational transmission of sector choices

Outcome variable: family sector (1 is yes)	*Panel A*: parent sectors are measured when young adults are 15 years old				*Panel B*: parent sectors are measured when young adults are 21 years old			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Young adult has lived in a SSF from birth (1 is yes)	-0.027	-0.001	0.006	0.023	0.006	0.029	0.036	0.055
	(0.047)	(0.047)	(0.038)	(0.038)	(0.053)	(0.054)	(0.042)	(0.042)
Sex (1 is male)	-0.093***	-0.092***	-0.063***	-0.063***	-0.096***	-0.095***	-0.069***	-0.069***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Ethnicity (ref: both parents born in NL)								
One NL parent, other Western	-0.026***	-0.024***	-0.020***	-0.019***	-0.025***	-0.023***	-0.020***	-0.018***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)
One NL parent, other non-Western	-0.042***	-0.042***	-0.033***	-0.032***	-0.040***	-0.040***	-0.032***	-0.030***
	(0.007)	(0.007)	(0.005)	(0.005)	(0.008)	(0.008)	(0.006)	(0.006)
Both parents not NL	-0.038***	-0.058***	-0.042***	-0.045***	-0.029***	-0.052***	-0.036***	-0.045***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.005)	(0.005)	(0.003)	(0.003)
Log household income		-0.016***		0.011***		-0.012***	0.002	0.002
		(0.003)		(0.001)		(0.002)	(0.001)	(0.001)
Parental education (1 is diploma SE)		-0.004		-0.000		-0.013***	-0.004	-0.001
		(0.003)		(0.002)		(0.003)	(0.002)	(0.002)
Family transitions		-0.019***		-0.011***		-0.020***		-0.014***
		(0.003)		(0.002)		(0.003)		(0.002)
Family structure (ref: married parents)								
Cohabiting parents		-0.007*		-0.006*		-0.002		-0.005
		(0.004)		(0.003)		(0.004)		(0.003)
Other		0.003		-0.007		0.006		-0.013
		(0.014)		(0.010)		(0.016)		(0.010)
Age of mother at birth		-0.004***		-0.004***		-0.004***		-0.004***
		(0.000)		(0.000)		(0.000)		(0.000)
Family size		0.000		-0.007***		-0.001		-0.005***
		(0.002)		(0.001)		(0.002)		(0.001)
Birth order		0.004*		0.004**		0.004*		0.002
		(0.002)		(0.001)		(0.002)		(0.001)
Constant	0.303***	0.603***	0.238***	0.237***	0.305***	0.575***	0.188***	0.307***
	(0.044)	(0.054)	(0.033)	(0.037)	(0.047)	(0.053)	(0.033)	(0.034)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality at birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Restricted sample[a]	Yes	Yes	No	No	Yes	Yes	No	No
Number of young adults	118,812	118,812	182,518	182,518	113,908	113,908	181,631	181,631
Number of young adults from SSF	55	55	87	87	52	52	84	84
R-squared	0.0211	0.0234	0.0132	0.0151	0.0205	0.0230	0.0138	0.0157

Notes: The table shows the estimated associations between living in same-sex families (SSF) from birth and the likelihood of being employed in a sector where at least one parent (or step-parent) is employed. Household composition (i.e. whether young adults were living with both parents, or with a parent and a step-parent) is observed when young adults were aged 15 years old, while parent sectors are observed when young adults were aged 15 years old in Panel A, and 21 years old in Panel B.

[a]In the restricted sample, we require both parents to be employed. Hence, the outcome variable 'family sector' is coded 1 if the young adult is employed in the same sector of (at least) one parent; 0 if the young adult is employed in a different sector than both parents. In the non-restricted sample, we require (at least) one parent to be employed. Hence, the outcome variable is coded 1 if the young adult is employed in the same sector of (at least) one parent; 0 if the young adult is employed in a different sector than both parents, or if the young adult is employed in a different sector than one parent, and the other parent is inactive or unemployed. Note that young adults for whom both parents are inactive or unemployed are not included in the estimations. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

D.2 Role model hypothesis

Mothers and fathers might not perfect substitutes but rather provide different inputs to a child's growth (*role model hypothesis*, Allen, 2013). By having two mothers or two fathers, therefore, young adults born in same-sex families might sort into nontraditional sectors because they miss the complementary parenting styles that opposite-sex parents provide. To test this hypothesis, we perform an additional analysis in which we compare young adults born in same-sex families to young adults who experienced the loss of a parent by death during childhood (i.e. before age 16).[36] We hypothesize that, if the role model argument plays a role, we should observe similar results – in magnitude and significance – between young adults born in same-sex families and young adults who grew up with a widowed parent, given that young adults from bereaved families also lacked of the parenting style provided by the parent who prematurely passed away. In the estimations, we ensure that young adults from bereaved families are not exposed to parental inputs coming from potential subsequent partner(s) of the widowed parent by focusing on young adults whose live parent did not marry or cohabit again (before young adults were 18 years old) after the loss of the spouse. Further, as we observe parental loss by death only as a reason of interruption of the parents' marriage contract, we restrict our analysis to married families.[37] In sum, we create a new categorical variable of interest, which takes value 0 if the young adult lived in an intact married different-sex family until age 16; 1 if the young adult experienced parental loss by death while living in an intact married different-sex family; 2 if the young adult was born and lived throughout childhood in a same-sex family.[38] Note that, as young adults from bereaved families may perform poorly on the labor market because of the possibly lower human capital accumulated after family disruption, we include diploma attainment in our model as an additional control variable. We display our results in Table 4.D2 below. Our results show that young adults from bereaved families have different outcomes on the labor market than young adults from same-sex families. These outcomes do not appear to be consistent with the outcomes estimated in Table 4.3 for young adults born in same-sex families. Particularly, in our preferred specifications that include the full set of covariates, we find no evidence that young adults from bereaved families sort into nontraditional sectors (column 10). Moreover, contrary to young adults born in same-sex families, we do not find that young adults from bereaved families earn significantly different than young adults from intact, married families, nor that they are full-time employed at different frequen-

[36] However, our results are robust to choosing a stricter threshold for the age at which young adults experienced parental loss by death. For instance, even when we only consider young adults whose parent passed away in their childhood years (i.e. before age 6), our results are similar to those we present in Table 4.D2.

[37] This means that young adults whose parents never got married are not included in the sample. However, to maximize the number of observation for young adults born in same-sex families, we do not impose such restriction on same-sex families.

[38] Note that, by construction, none of the young adults in different-sex and same-sex families included in our main analysis in Table 4.2 have experienced parental loss by death during childhood. Hence, none of the young adult born in same-sex families that constitute the comparison group in Table 4.D2 have experienced parental loss by death.

cies. Overall, our results are thus suggestive that the role model argument does not play a role in driving the results of Table 4.3. This might indicate that female same-sex (male same-sex) parents manage to provide male (female) involvement in the life of their children outside the household, confirming recent literature on the topic relative to lesbian families (Goldberg and Allen, 2007; Tasker, 2010).

Table 4.D2: Young adults born in same-sex families, parental loss by death, and labor market outcomes

	Log gross earnings		Log gross hourly wages		Employment		Full-time employment		Nontraditional industry	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Family structure (ref: intact married different-sex families)										
Young adults from bereaved different-sex families	0.023	0.004	−0.026*	−0.014	−0.029***	−0.015*	−0.043***	−0.023	0.012	−0.009
	(0.017)	(0.023)	(0.010)	(0.012)	(0.005)	(0.006)	(0.009)	(0.012)	(0.010)	(0.012)
Young adult is born in a same-sex family	−0.428***	−0.320**	−0.143*	−0.110	−0.013	−0.015	−0.181***	−0.147**	0.151**	0.133*
	(0.099)	(0.098)	(0.058)	(0.057)	(0.026)	(0.027)	(0.047)	(0.048)	(0.056)	(0.057)
Sex (1 is male)	0.192***	0.201***	0.042***	0.044***	−0.001	0.001	0.259***	0.262***	0.182***	0.180***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Ethnicity (ref: both parents born in NL)										
One NL parent, other Western	−0.169***	−0.149***	−0.051***	−0.045***	−0.013***	−0.010***	−0.073***	−0.066***	0.035***	0.032***
	(0.008)	(0.008)	(0.004)	(0.004)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)	(0.004)
One NL parent, other non-Western	−0.270***	−0.259***	−0.061***	−0.059***	−0.024***	−0.017***	−0.126***	−0.122***	0.043***	0.042***
	(0.019)	(0.019)	(0.010)	(0.010)	(0.005)	(0.005)	(0.009)	(0.009)	(0.009)	(0.009)
Both parents not NL	−0.289***	−0.438***	−0.034***	−0.078***	−0.037***	−0.024***	−0.191***	−0.238***	0.019***	0.049***
	(0.008)	(0.008)	(0.004)	(0.005)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)	(0.004)
Log household income		−0.016***		0.003*		0.005***		−0.003*		0.005***
		(0.003)		(0.001)		(0.001)		(0.001)		(0.001)
Parental education (1 is diploma SE)		−0.129***		−0.036***		0.007***		−0.041***		0.027***
		(0.005)		(0.003)		(0.001)		(0.003)		(0.003)
Family transitions		0.054**		−0.002		−0.004		−0.010		0.022*
		(0.018)		(0.008)		(0.004)		(0.008)		(0.009)
Age of mother at birth		−0.016***		−0.005***		−0.001***		−0.005***		0.003***
		(0.001)		(0.000)		(0.000)		(0.000)		(0.000)
Family size		0.037***		0.014***		−0.002**		0.013***		−0.007***
		(0.002)		(0.001)		(0.001)		(0.001)		(0.001)
Birth order		0.019***		0.009***		0.001		0.003*		−0.002
		(0.003)		(0.002)		(0.001)		(0.002)		(0.002)
Diploma attainment (1 is yes)		0.189***		0.019***		0.113***		0.062***		−0.019***
		(0.007)		(0.003)		(0.002)		(0.004)		(0.004)
Constant	9.134***	9.580***	2.021***	2.083***	0.941***	0.823***	0.108*	0.228***	0.155***	0.048
	(0.081)	(0.087)	(0.053)	(0.055)	(0.020)	(0.022)	(0.043)	(0.046)	(0.042)	(0.045)
Birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality at birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of young adults	165,728	165,728	154,158	154,158	173,464	173,464	154,158	154,158	154,158	154,158
Number of young adults from SSF	78	78	69	69	83	83	69	69	69	69
Number of young adults from bereaved families	2,239	2,239	2,058	2,058	2,427	2,427	2,058	2,058	2,058	2,058
R-squared	0.0911	0.109	0.0729	0.0784	0.0124	0.0471	0.132	0.138	0.0518	0.0538

Notes. This table compares the labor market performance of young adults from intact, married families, young adults from married families who experienced parental loss by death before age 16, and young adults born in same-sex families. Young adults whose parents were never married (or were never in a registered partnership) are excluded from the sample. However, to retain the highest number of observations of young adults born in same-sex families, we do not impose such restriction on same-sex families. Robust standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix E: Details on coefficient stability test

E.1 Details on the methodology

To test the sensitivity of our results to omitted variable bias, we construct the bias-adjusted estimator β^* developed by Oster (2019), thereby re-estimating the association between living in same-sex families and labor market outcomes. Then, we compare β^* to the coefficient $\hat{\beta_1}$ obtained in Equation 5.1.

We construct β^* using values of coefficients ($\tilde{\beta}$, $\dot{\beta}$), R-squared ($\tilde{R}$, $\dot{R}$, R_{max}), and the degree of selection δ. More formally:

$$\beta^* \approx \tilde{\beta} - \delta\left[\dot{\beta} - \tilde{\beta}\right] \frac{R_{max} - \tilde{R}}{\tilde{R} - \dot{R}} \tag{4.E1}$$

where the pair ($\dot{\beta}$, $\dot{R}$) represents the key coefficient of interest and the R-squared resulting from the regression in which no control variable is included (*uncontrolled regression*), the pair ($\tilde{\beta}$, $\tilde{R}$) represents the key coefficient of interest and the R-squared resulting from the regression where all observed control variables are included (*controlled regression*), δ is the degree of selection, and R_{max} is the unknown R-squared that would result from the regression of the dependent variable on the full set of controls (observables and unobservables). While the set of parameters ($\tilde{\beta}$, $\tilde{R}$, $\dot{\beta}$, $\dot{R}$) is observed, δ and R_{max} are unknown and need to be specified. By defining bounds for δ and R_{max}, in turn, we can construct bounds for the estimator β^*.

Oster (2019) argues that one bound for β^* is $\tilde{\beta}$ (the coefficient from the controlled regression) that is obtained when the degree of selection δ is zero, or equivalently, when R_{max} is $\tilde{R}$ (meaning that only the observed variables are important to explain the relationship between the key regressor of interest and the dependent variable, or, equivalently, that the R-squared from the regression with observables and unobservables is the same as the R-squared from the regression with only observables). She also argues that an appropriate cutoff for δ is $\delta = 1$, given that control variables are intentionally chosen to capture the most important determinants of the dependent variables, and it is therefore unlikely that unobserved variables are more important than observed variables in determining the outcome (which would be the case for $\delta > 1$). Turning to bounds on R_{max}, as mentioned, R_{max} cannot be estimated but a natural upper bound might seem to be $R_{max} = 1$, i.e. the R-squared of a regression where the dependent variable is fully explained by the set of controls (observed and unobserved). However, as explained by Oster (2019), it is unlikely that in empirical applications the variance of the outcome variable can be fully explained by the controls (e.g. due to measurement error in the dependent variable). Therefore, R_{max} is likely to be lower than 1. Oster (2019) suggests that an appropriate value for R_{max} is $1.3\tilde{R}$, where $\tilde{R}$ is the R-squared from the controlled regression.

In sum, given the estimator $\beta^*(R_{max}, \delta)$ and given bounding values for δ and R_{max}, a bounding set Δ_s for the effect of residing in same-sex families on labor market outcomes can

be constructed. To define the unknown parameters, we will follow Oster's suggestions and set $\delta = 1$ and $R_{max} = 1.3\tilde{R}$.[39] Namely, $\Delta_s = \left[\tilde{\beta}, \beta^*(1.3\tilde{R}, 1)\right]$. To derive the robustness of our results, first, we will check whether the constructed set of bounds Δ_s includes zero. Second, we will check whether the identified bounding set Δ_s falls in the confidence interval on $\tilde{\beta}$.

E.2 Estimation results

In this section, we show the robustness of our findings to the coefficient stability test developed by Oster (2019). We show our results in Table 4.E1 below,[40] where we first report, for each outcome variable (including industry choices), the key coefficient of interest previously obtained from the controlled regressions in Table 4.2 and Table 4.3 (column 1). Then, we report the identified bounding set for the association of living in same-sex families and labor market outcomes, setting $R_{max} = 1.3\tilde{R}$, and $\delta = 1$ (column 2). Further, we report whether the identified bounding set excludes zero (column 3), and whether it is included in the 95% confidence intervals estimated for the regression coefficient $\tilde{\beta}$ (column 4). We test the sensitivity of our results both for the main sample of young adults who have lived in a same-sex family for at least one year (*Panel A*), and for the subsample of young adults raised by same-sex parents from birth until age 15 (*Panel B*).

Table 4.E1 reveals that the coefficients estimated in our main analyses are robust to omitted variable bias. In Panel A, all identified bounding sets are included in the 95% confidence intervals reported in column 1. Moreover, although the identified sets for log gross earnings, employment, full-time employment, and nontraditional industries do not exclude the zero, we note that the identified bounding sets are close to zero in column 2, which might suggest a zero association of living in same-sex families on these outcome variables. In Panel B, we also note that the identified bounding sets appear to be narrow and close in magnitude to the coefficients reported in column 1. Overall, the bounding analysis confirms that young adults born in same-sex families tend to have lower earnings, are less involved in full-time occupations and more employed in nontraditional industries than young adults from different-sex families. On a final note, we test whether our bounds are robust to choosing a higher degree of selection (namely $\delta = 2$, which corresponds to assuming that the unobservables are twice as important as the observables in explaining the relationship between living in same-sex families and subsequent labor market outcomes), and show the bias-adjusted estimator so obtained in column 5. Findings from column 5 suggest that our results are robust to a more restrictive assumption on δ, as all bounding sets constructed with $\delta = 2$ are included in the 95% confidence intervals reported in column 1, and almost all of them exclude zero.

[39] However, we also check whether our results are robust to assuming a higher degree of selection on observable and unobservables, i.e. $\delta = 2$. Our results are consistent with what we obtain when δ is equal to 1 (see Section D.2 for more details).

[40] Note that the structure of Table 4.E1 follows an application of Oster's coefficient stability test by Hener et al. (2016).

Table 4.E1: Young adults from same-sex families, labor market outcomes, and coefficient stability

Dependent variable	Controlled Regression $\tilde{\beta}$, [95% CI] (1)	Identified Set $[\tilde{\beta}, \beta^*[1.3\tilde{R}, 1]]$ (2)	Exclude zero? (3)	Within Conf. Interval? (4)	Bias-adjusted β^* with $\delta = 2$ (5)
Panel A: Young adult has lived in same-sex family					
Log gross earnings	-0.0029 [-0.0763,0.0706]	**[-0.0029, 0.0275]**	No	Yes	0.0585
Log gross hourly wage	0.0097 [-0.0280, 0.0474]	**[0.0097, 0.0158]**	Yes	Yes	0.0220
Employment	-0.0016 [-0.0224, 0.0191]	**[-0.0016, 0.0070]**	No	Yes	0.0159
Full-time employment	-0.0066 [-0.0466, 0.0335]	**[-0.0066, 0.0093]**	No	Yes	0.0254
Nontraditional industry	0.0097 [-0.0298, 0.0491]	**[0.0097, -0.0007]**	No	Yes	-0.0113
Panel B: Young adult has lived in same-sex family from birth					
Log gross earnings	-0.2949 [-0.4596, -0.1302]	**[-0.2949, -0.2394]**	Yes	Yes	-0.1837
Log gross hourly wage	-0.0818 [-0.1675, 0.0039]	**[-0.0818, -0.0685]**	Yes	Yes	-0.0553
Employment	0.0251 [-0.0223, 0.0725]	**[0.0251, 0.0230]**	Yes	Yes	0.0349
Full-time employment	-0.1249 [-0.2161, -0.0337]	**[-0.1249, -0.1165]**	Yes	Yes	-0.1081
Nontraditional industry	0.1496 [0.0598, 0.2395]	**[0.1496, 0.1293]**	Yes	Yes	0.1088

Notes: Following Oster (2019) (and Hener et al. (2016)), the table shows bounding sets for the association between living in same-sex families and labor market outcomes. The table reports bounding sets for both the main sample of young adults from same-sex families (Panel A) and for the subsample of young adults raised by same-sex families from birth (Panel B). For each outcome variable, we check whether the identified bounding set includes the zero and whether it is included in the confidence interval of the coefficient estimated in the main regression. Bounding sets are constructed by setting $R_{max} = 1.3\tilde{R}$, and $\delta = 1$, although we also check whether our results are robust to the more restrictive choice of δ equal to 2 (see Section 5.4 for more details on the role of R_{max} and δ). Column 1 reports OLS coefficients from our main analysis when the full set of covariates (sex, ethnicity, log household income, parental education, family transitions, family structure, family size, age of mother at birth, birth order, birth year fixed effects and municipality at birth fixed effects) is included.

General conclusion

Changes in family structures are hot topics in today's debate. Whether same-sex couples should be allowed to marry and form families or whether stepfamilies should be institutionalized is a subject of debate and opposing views in many countries.

Social changes are however happening. In the United States, the U.S. Supreme Court ruled in a landmark case in 2015 that same-sex couples shall be granted the same marriage rights as different-sex couples in all U.S. states. This ruling occurred even though public opinion was still divided in the years leading up to the legalization. In fact, as a Pew Research Center poll shows, 40 percent of Americans were not in favor of same-sex marriage the year before the ruling (Pew Research Center, 2019). Similarly, at the European level, there exists great heterogeneity among countries in terms of recognition of marriage and parental rights for same-sex couples, with Hungary and Poland being at the forefront of the opposition. Nonetheless, in the 2020 State of the Union address, the President of the European Commission declared that the mutual recognition of family relationships in the EU will be pursued, as 'if you are a parent in one country, you are a parent in all countries' (European Commission, 2022a). This statement referred to the need that parental ties created between same-sex parents and their children in one Member State are acknowledged by all other Member States (European Commission, 2022a).

Despite the great interest aroused by changes in family environments, there is still much we do not know about how these families function and through which channels they can influence children's outcomes. So far, the literature has indeed mainly relied on cross-sectional survey data, which are limited in the possibility of reconstructing family structures longitudinally. This has motivated the present doctoral dissertation, which has employed administrative datasets from Flanders and the Netherlands to examine the short-term and long-term consequences of heterogeneous family compositions on children's outcomes. Due to the longitudinal structure of our datasets, this doctoral dissertation has advanced existing knowledge on four family environments: 1) heterogeneous socioeconomic background; 2) parental divorce; 3) complex sibling structures; 4) same-sex parenting.

The remainder of this chapter is structured as follows. Section 1 provides, for each chapter, a summary of the research questions outlined in the general introduction, discusses the findings, and answers the research hypotheses. In Section 2, we discuss the policy implications of our findings and provide policy recommendations. Finally, Section 3 outlines the limitation of this doctoral dissertation and discusses avenues for future research.

1 Summary of findings

Chapter 1

The first chapter of this doctoral dissertation evaluated whether and under which circumstances providing parents with information on school quality influences the school choices they make for their children in primary education. To answer this question, we exploited an open government initiative in Belgium that consisted of the online publication of school inspection reports, and compared inspected and non-inspected schools on school enrolments and socioeconomic composition. Using a difference-in-differences framework, first, we assessed whether inspected schools attracted more first-grade students than non-inspected schools. Second, we assessed whether schools' socioeconomic composition changed after the publication of reports. The research question answered in this chapter was the following:

RQ1: *Do families with heterogeneous socioeconomic backgrounds respond differently to information provision on school quality in primary education?*

Our results showed that inspected schools in primary education see an increase in first-grade enrolment after the publication of the reports. The effect is driven by positively evaluated schools receiving 2.3 more students than non-inspected schools, corresponding to a 7% increase in first-grade enrolments. In perspective, this means that good-performing schools attract 1 more student in a class with an average class size of 18 students. By contrast, negatively evaluated schools do not significantly lose students. Additionally, our results showed no change in the schools' socioeconomic composition after the publication of the reports, neither in positively nor in negatively evaluated schools. These findings are robust to using Generalized Synthetic Control (GSC) to model the underlying unobserved school heterogeneities that lead schools to receive positive and negative evaluations in a non-random fashion.

In the general introduction, it was argued that highly educated parents would have been more likely to use the information disclosed in the inspection reports, leading therefore to a change in the school socioeconomic composition after the publication of school inspection reports. This statement is not supported by our findings. On the contrary, our results suggested that both high-educated and low-educated parents made use of the school quality information embedded in the school inspection reports. As a result, the socioeconomic composition of the school student body did not change after the publication of the reports. In summary, Chapter 1 concluded that parents make use of school quality indicators to choose the school for their children and that this use is not moderated by family socioeconomic background.

Chapter 2

In *Chapter 2*, we delved deeper into the life consequences of family events experienced during childhood. In particular, this chapter assessed the long-term implications of parental divorce by examining the marital and fertility behavior of children who experienced their parents' divorce during childhood. Using population data from the Netherlands, we compared more than 1,600,000 adult children from intact families with 197,195 adult children from divorced families, all of whom we followed from birth until the age of 40. *Chapter 2* answered the following research question:

RQ2: *Does parental divorce influence children's realized fertility in adulthood?*

Our findings showed that parental divorce significantly influences children's marital and fertility outcomes in adulthood. Children from divorced families were found to postpone marriage compared to children from intact families. They were also found to have their first child earlier, although this association was mostly found in the sample of women and was likely due to adult children from divorced families having lower socioeconomic status than adult children from intact, married families. Importantly, adult children from divorced families were found to have fewer children in adulthood than adult children from intact families. First, they were more likely to remain childless. Women from divorced families were 2.0 percentage points more likely not to become parents than their comparison group from intact families, whereas this association amounted to 5.9 percentage points in the case of men. Second, they had lower completed fertility. Women from divorced families were indeed found to have .082 fewer children than their comparison group (a decrease of 5 percent), whereas men from divorced families were found to have .180 fewer children (a decrease of 13 percent). These findings were robust to Coarsened Exact Matching, Oster's bounding analysis, and using parental death as an exogenous source of parental absence.

In the general introduction, it was stated that it was unclear whether parental divorce would have led to higher or lower fertility in adulthood. Our results however suggested a strong negative association between parental divorce and adult fertility. Furthermore, our results also provided evidence of one possible mechanism underlying this association, which we called "union duration" hypothesis. By means of a mediation analysis, we found that since adult children from divorced families are less likely to be in stable relationships and more likely to separate from their partners, they will have fewer children because they will not be in a relationship with a partner for long enough to become parents or have subsequent children. In summary, **Chapter 2** concluded a negative association between parental divorce and adult fertility, partly mediated by children from divorced families' higher relationship instability in adulthood.

Chapter 3

Instead of considering changes in family composition from a parental perspective, *Chapter 3* focused on changes in family composition from a sibling perspective. Specifically, this chapter examined the effect of the birth of a halfsibling and the move of stepsiblings into the household after parental separation. Using population data from the Netherlands, we compared more than one million children from intact (nonblended) families to 62,665 children from stepfamilies, of whom 20,522 children experienced the birth of a young halfsibling while in a stepfamily whereas 7,414 children lived at least one year with a stepsibling. Our large sample size allowed us to account for family unobserved characteristics using a sibling fixed effect design that exploited variations in sibling compositions between fullsiblings at the time in which standardized test scores at the end of primary education were taken. The research question raised in Chapter 3 was the following:

RQ3: *Does the birth of a halfsibling or the presence of stepsiblings after parental separation affects children's educational outcomes?*

Our findings indicated no effect of complex sibling structures on children's educational outcomes. Both our individual-level and our sibling-level analyses showed no role played by the birth of a halfsibling in determining children's standardized test scores at the end of primary education. Similarly, no significant impact on test scores was found according to whether children in stepfamilies lived with a stepsibling or not. Further analyses confirmed these null effects also on secondary school outcomes, as children with nontraditional siblings were found to have similar diploma and dropout likelihoods than their comparison groups with fullsiblings only.

In the general introduction, it was speculated a negative effect of complex sibling structures on children's school performance. In light of the above-mentioned results, this statement is not supported by our findings. Our findings are thus suggestive that children with nontraditional siblings do not receive less (financial or time) resources from their (step) parents, and are not likely to feel abandoned from their parents. Nonetheless, further analyses also showed that, in nonblended families, the birth of a younger fullsiblings is positively correlated with children's outcomes. This means that, in contrast to the birth of a halfsibling, children benefit from the birth of a sibling with whom they share both biological parents. We concluded that this might be because when a fullsibling is born, children develop a sentimental attachment to the newborn that does not arise in the case of birth of a halfsibling. In summary, *Chapter 3* concluded that having halfsiblings or stepsiblings after parental separation is not more harmful for children than parental separation per se.

Chapter 4

Finally, *Chapter 4* examined the influence on children's outcomes of yet another family composition, namely same-sex family. Specifically, this chapter evaluated whether young

adults who lived with same-sex parents in childhood perform differently early on in the labor market than young adults who lived in a 'traditional' mother-and-father family. Using population data from the Netherlands, we observed from birth until age 21 almost 240,000 young adults who did not enrol in higher education after high school, of whom 605 lived in a same-sex family in childhood. In chapter 4, the following research questions were posed:

RQ3: *How do children who lived in a same-sex family in childhood perform early on in the labor market? Is there a difference between children who lived in 'planned' same-sex families and in 'later-formed' same-sex families?*

Our findings indicated a null association between having lived in a same-sex family in childhood and labor market outcomes early on after having left high school. Our findings also revealed large heterogeneities by the time spent in same-sex families. Indeed, young adults from a 'planned' same-sex family –i.e. born and raised in a same-sex family until age 16– were found to have lower earnings (measured by lower annual wages and lower hourly wages) than young adults from different-sex families. Moreover, while no difference was found in terms of employment likelihoods, young adults from 'planned' same-sex families appeared to be less frequently employed on a full-time basis but more frequently employed on a part-time basis than young adults from different-sex families. Our findings were robust to reducing imbalances in family types using Coarsened Exact Matching and to assessing the sensitivity of the estimated coefficients to unobservables by constructing the bias-corrected estimator proposed by Oster (2019).

It was mentioned in the general introduction that it is theoretically unclear in which direction having same-sex parents will influence children's performance on the labor market. On the basis of our results, we concluded a negative association between having grown up with same-sex parents and wage levels. Nonetheless, our results also allowed us to dig deeper into the mechanisms that might be playing a role in determining this association. First, as mentioned, young adults from same-sex families were found less likely to be employed full-time but more likely to be employed part-time. Second, our findings supported the *sector-selection hypothesis* as a possible mechanism driving the industry choices of young adults from same-sex families. Our results indeed showed that young adults from same-sex families are more likely to sort into industries that are atypical from their sex. This was particularly the case for boys who were more likely to sort into female-dominated industries, which are traditionally lower-paying than male-dominated industries. In summary, **Chapter 4** concluded no difference in the labor market performance between young adults from same-sex families (born and not born), with young adults born in a same-sex families however showing a trend towards lower wage levels, part-time occupations, and nontraditional occupations.

2 Policy implications

Our results suggest that family compositions matter for children's outcomes. Both high-SES and low-SES parents care about children's school choices, and life events experienced during childhood have short-term and long-term implications that affect multiple outcomes (fertility, labor market choices, educational attainment). Below are specific policy suggestions to deal with the consequences of changing family compositions.

Chapter 1 concluded that parents make use of the school inspection reports published online on the governmental website and that this use is not moderated by family socioeconomic background. The first set of policy implications thus relates to the use of performance indicators in education. First, online tools such as government websites can be useful tools for disseminating information about school performance. Our results indeed showed that publishing school-quality information on an official online source was effective in reaching the targeted audience. The absence of a heterogeneous response by family socioeconomic background further suggested no barriers experienced by especially low-SES parents when consulting online school performance indicators. Second, governments should ensure that school performance indicators, such as school inspection reports, are of high standards. This is crucial since these indicators play an important role in determining parents' school choice for their children. Third, it is incumbent upon the government to ensure that, once school quality disclosure policies such as the one examined are implemented, all parents are made aware of the availability of performance indicators. Additional analyses indeed revealed that parents made use of the information embedded in the reports only if they were compelled to evaluate alternatives. It might be that parents who could choose the primary school for their children on the basis of other easy-to-determine characteristics (such as building continuity with kindergarten) were not aware of the existence of the reports or did not actively seek them out. Fourth, although conducting inspections in a random manner prevents inspections from becoming predictable and teachers from 'teaching to the inspection', it is worth noting that this could leave many schools uninspected or with a long period elapsing since the last inspection, thus reducing information asymmetries only to a certain extent. From a policy perspective, it is therefore advisable that governments carefully reflect on the frequency of inspections and the number of schools inspected each year. Finally, our findings suggest that providing more information is beneficial as it leads parents to enroll their children in positively-evaluated schools without resulting in increased school segregation. This suggests that in countries like Belgium, where certain performance indicators (e.g., central testing) are not available, their introduction may be beneficial in that it may help reduce information asymmetries between schools and parents. However, we should note that once performance indicators are made public, it is essential that governments ensure that no further socioeconomic segregation is created in schools because of the publication of the performance indicators. This can be achieved by, first, ensuring that all parents are

informed about the publication of the performance indicators. As discussed in Chapter 1, online tools may prove effective for this purpose. Additionally, information campaigns may also be conducted to raise awareness about the importance of school choice. Second, to avoid cream-skimming, schools might reserve a sufficient number of school places for students from vulnerable backgrounds (e.g., low-SES students, students with disabilities). Finally, governments might couple the publication of school performance indicators with the implementation of school intervention programs for under-performing schools, where school principals receive (financial or managerial) assistance in improving educational practices. This might ensure that, in the long term, all children would receive a good-quality education.

Chapter 2 compared children from divorced families with children from intact, married families on adult fertility, and found a negative association between having experienced parental divorce in childhood and fertility outcomes in adulthood. These findings provide useful insights when designing social policies aimed at stimulating birth rates, the effects of which are most often shown to be either positive but weak, or insignificant (Hoem, 2008; Gauthier, 2007; Sleebos, 2003; for contrasting evidence, see Bergsvik et al., 2021). This chapter has explored and provided evidence of a channel so far ignored by the literature, namely that fertility rates may continue to decline as a result of the intergenerational transmission of separation. If the decline in fertility is indeed also due to higher union instability resulting from life events experienced in childhood, it is possible that policies designed to boost birth rates will not have the desired effects for part of the population. In light of the high levels of divorce rates and the more accepting attitudes toward divorce of youngsters (Stewart and Timothy, 2020; Taylor and Keeter, 2010), if the goal for governments is to ensure the sustainability of the welfare state, it might be advisable that governments reflect on compensating the missing tax revenues by implementing other types of policies in addition to pronatalist policies. One example might be to regulate the legal and working conditions of certain groups of workers (e.g., immigrant workers) who are already available for employment in a country but cannot do so because of their irregular status. Although this is a hot topic in today's political debate, there is evidence from recent research that immigration can be a valuable resource to promote in order to help industrialized countries deal with the consequences of low fertility rates (e.g., Peri, 2020).

Chapter 3 examined the effects on school outcomes of complex sibling compositions and found no effects of having halfsiblings or living with stepsiblings on children's school performance. These findings offer some important policy recommendations. As discussed in the general introduction and in the chapter, rising divorce rates have worried scholars and policymakers about the negative effects that family and sibling complexity might exert on children (Raley and Sweeney, 2020). Moreover, public opinion continues to portray stepfamilies less positively than two-parent biological families, with nonbiological relationships still subject to stereotypes (Saint-Jacques et al., 2020; Valiquette-Tessier et al., 2016).

Perhaps as a result of these worries and attitudes, stepfamilies are to date still only partially institutionalized (Stewart and Timothy, 2020). For instance, as discussed in the general introduction, in the Netherlands financial aid for higher education is granted only to children from low-income families based on the income of the biological parents, without taking into account children's legal parents. Moreover, regardless of the number of years cohabiting, the Dutch Civil Code establishes that stepparents have maintenance obligations towards stepchildren only if they are married or in a registered partnership with the parent of the focal child.[41] Our findings showed that the presence of nonbiological ties in the family does not translate into additional hardships for children. Neither the presence of nontraditional siblings nor (in further robustness checks) the presence of a stepparent were found to bring additional hardships to children than parental divorce. On the basis of these findings, it is recommended that policy carefully reflects on legally defining nonbiological relationships so that the legal responsibilities of stepfamily members towards each other are well defined. This is especially important given that, for instance, nonmarital cohabitation after parental separation is becoming increasingly common in Northern Europe as an alternative to remarriage (De Jong Gierveld, 2004).

Chapter 2 and **Chapter 3** also provide some further joint insights. When looking at the similarities between the chapters, Chapter 2 and especially Chapter 3 revealed that parental separation negatively affects children's educational outcomes. Chapter 3 also showed in the descriptive statistics that parental separation more frequently occurs among children from disadvantaged families, where the level of income and the educational attainment of the parents is lower. Thus, these findings reveal that children from separated families are an extremely fragile group that needs special attention from policymakers. From a policy perspective, group interventions could be fostered in schools to help children and their parents cope with separation. Among possible interventions, those based on counseling and prevention have most often proven to yield positive results (Poli et al., 2017).

Chapter 4 compared young adults from same-sex families with young adults from different-sex families and, among other things, found a preference for nontraditional industries by young adults born in same-sex families compared to young adults from different-sex families. In the chapter, it was argued that a more egalitarian division of household labor and a reduced exposition to sex stereotypes would have influenced children's industry choices towards industries that are typically not selected by individuals of their own sex. Our findings confirmed this mechanism, highlighting how the family environment in childhood can importantly shape children's industry choices in adulthood. Thus, from a policy perspective, our findings are suggestive that encouraging discussions about sex stereotypes may help reduce occupational segregation by sex, which is one of the main drivers of wage inequality between men and women (Blau et al., 2013). In addition to the home environment, discussions about sex stereotypes could be stimulated in schools, so that

[41] Article 1:395; maintenance obligation of a stepparent.

young adults are able to make informed labor market decisions without being conditioned by preconstituted notions of sex roles.

The findings from this doctoral dissertation also provide more general recommendations. First, policy should aim at removing barriers for same-sex families and at creating safe environments for the LGBTQ community. **Chapter 4** showed that children in same-sex families perform as well on the labor market as children from different-sex families. Although children born in planned same-sex families were found to earn lower wages than children in different-sex families, the chapter concluded that these wage differentials likely stemmed from children in same-sex families' higher frequencies in part-time occupations and in non-traditional industries. Further, no evidence was found of lower human capital in children in same-sex versus different-sex families. These findings add up to the flourishing literature showing no deficiencies of same-sex families compared to different-sex families (see, e.g., Aldén et al., 2017, Kabátek and Perales, 2021, Mazrekaj et al., 2020). Based on these considerations, it is recommended that barriers that prevent same-sex couples from marrying and forming families are removed. Nonetheless, since findings pertain to a country –the Netherlands– where attitudes and perceptions toward LGBTQ rights are largely favorable, it is also recommended that in parallel with the extension of same-sex couples' rights, safe environments for LBGTQ individuals are promoted.

The second general recommendation is that policy should focus on reducing the stigma surrounding nonbiological family relationships. Throughout this doctoral dissertation, it was discussed how a considerable number of children are exposed to nonbiological family relationships, both in different-sex and in same-sex families. The findings of this doctoral dissertation suggested that nonbiological relationships do not harmfully impact children's outcomes. **Chapter 3** found that the presence of stepparents and stepsiblings does not negatively impact children's educational outcomes, whereas **Chapter 4** found that children in same-sex families, which by the majority are children in later-formed stepfamilies, perform as well as children from different-sex families on the labor market. Based on these findings, it is recommended that social barriers that prevent stepparents (stepsiblings) from being perceived as good parental (sibling) figures are broken down, both in same-sex and in different-sex families. A first step in this direction might be to promote the use within countries of neutral, nondiscriminatory terminology with which to refer to nonbiological relatives. For instance, unlike in English, in Italian, there is no corresponding word that uniquely denotes a 'stepfamily'. Moreover, the English words 'half-sibling' and 'stepparent' are translated by adding pejorative suffices to the corresponding Italian words of 'sibling' and 'parent', and it is unclear among linguists which terms should be used to refer to similar complex family and sibling structures (D'Achille, 2022). Because labels are shown to be not only descriptive, but to have effects of recognition and normalization of social phenomena (Swennen and Croce, 2016), the introduction and promotion of terminologies with neutral connotations might help reduce the stigma that still surrounds nonbiological relationships.

As a final general recommendation, policy should aim at including information on complex family structures in the household composition indicators provided by statistical offices (e.g. Eurostat). Findings from Chapters 2, 3, and 4 indicated how deviating from the household composition consisting of two married different-sex parents mattered for children's outcomes. Based on our findings, we often concluded that family members in complex family structures have different needs and concerns that need to be addressed with specific policies. For this purpose, it is necessary to improve the statistical indicators with which household composition is monitored across countries. To date, statistical indicators of household composition inadequately capture family and sibling complexity. For instance, Eurostat's main household composition indicator, which is based on data from the Labour Force Survey[42], categorizes households with children according to whether households are headed by a single adult or a couple. Yet, the type of relationship between children and the household headers is not provided, and therefore it is not possible to distinguish whether children are living with their biological parents or stepparents. Further, in the case of a couple heading the household, no information is provided on whether they are a same-sex or a different-sex couple. Given how prominent complex family structures are becoming, it is crucial that adequate indicators are introduced to monitor their evolution over time. This is essential both to understand how family structures evolve and to develop policy responses tailored to their needs.

3 Limitations and avenues for future research

Notwithstanding the attempts put forward in this doctoral dissertation to examine the role of family compositions on child outcomes by combining rich and longitudinal data sources with rigorous quantitative approaches, some limitations of this dissertation are worth noticing. First, the very nature of the research question makes causal evidence difficult to obtain. From a theoretical perspective, in the impossibility of randomly assigning life events such as parental divorce or same-sex parenting to children, causal evidence can be obtained by conditioning on all relevant characteristics (*conditional mean independent assumption*) or by employing quasi-experiments (Angrist and Pischke, 2009). However, social phenomena such as the ones under investigation are influenced by several factors that, even in a rich dataset such as the one we used, are difficult to observe. Furthermore, with respect to the same social phenomena, exogenous circumstances that randomly assign the population to a treatment or a control group in a natural experiment framework are rare. Besides Chapters 1 and 3, where explicit models that took unobserved heterogeneity into account were possible, this thesis attempted to overcome these limitations by assessing the extent to which the results could be subject to bias and by combining different quasi-experimental methodologies as robustness checks (Chapters 2 and 4). Nonetheless, results should be read bearing

[42] See, e.g., Eurostat (2022a).

in mind the challenges of obtaining true causal evidence.

Related to the previous point, another limitation of this doctoral dissertation lies in that, while population datasets allow providing rigorous and representative evidence of family composition effects, even better data is needed to fully get a grasp on how family environment affects children's outcomes. For instance, administrative datasets do not provide information on individuals' sexual orientation nor on individuals' attitudes towards life dimensions such as divorce, family size, or sex roles. However, this information is useful especially to investigate the theoretical mechanisms driving quantitative findings. To overcome this challenge, a promising avenue is to employ survey data linked to register data which are recently becoming available (see, e.g., the OKiN[43] survey developed by Kalmijn et al., 2018). Using this type of data, future research can complement the quantitative findings obtained with administrative data with the information on individuals' attitudes and perceptions collected in survey data. Alternatively, future research can deepen the theoretical mechanisms that emerge from quantitative research using qualitative analyses. With regard to this doctoral thesis, for example, qualitative analyses could be conducted to dig deeper into the mechanisms found in Chapter 2 (the *union-duration hypothesis*) and Chapter 4 (the *sector-selection theory*).

The findings reported in this doctoral dissertation also provide several insights into new possible streams of research. First, there is the need to dig deeper into the long-term consequences of heterogeneous family compositions. In Chapter 4, we explored the labor market outcomes of young adults not enrolled in higher education shortly after leaving high school. A direct extension of the chapter is thus to include high school graduates in the pool of the observed individuals, and examine the labor market outcomes of all young adults from same-sex families at an older age. Another extension is to examine young adults from same-sex families' fertility and marital outcomes, e.g. by looking at the age at which they leave home, the likelihood of becoming parents at a young age, or the likelihood that they themselves are in a same-sex cohabitation or marital relationship. Although these objectives are challenging due to the need to combine large samples and long-term perspectives, we believe that they will soon be feasible given how prominent these families are becoming.

Other two interesting research topics stem from the findings of Chapter 3. Although we did not find any effect of complex sibling structures on children's educational outcomes, it is possible that children in stepfamilies experienced some psychological difficulties that were compensated for by their parents through, for example, psychological counseling. As the first potential research topic, future research might therefore dig deeper into the mental health and psychological outcomes of children who experienced the birth of halfsiblings or the presence of stepsiblings in the household. If the aforementioned hypothesis were to prove true, it would provide further evidence favoring the need for intervention programs to help children cope with parental separation. The second potential research topic lies at

[43] Ouders en Kinderen in Nederland; Parents and Children in the Netherlands.

the intersection of the complex siblings literature and the same-sex parenting literature. When examining the educational outcomes of children with nontraditional siblings, for simplicity, we focused on different-sex stepfamilies and excluded same-sex stepfamilies. It might however be interesting to dig deeper into the sibling composition of 'later formed' same-sex stepfamilies, and explore whether the potential presence of nontraditional siblings might play a role in explaining the traditional underperformance of children in later-formed same-sex families relative to children born in same-sex families (see, e.g., Mazrekaj et al., 2020).

There is also a need for evidence in other countries. Virtually everything we know about children in same-sex families and most of what we know about children in divorced families comes from the United States or Northern European countries. In fact, this thesis has contributed to earlier literature by providing evidence of family composition effects in a Northern European country such as the Netherlands. As discussed in the general introduction, however, the Netherlands is a country that is highly tolerant with respect to LGBT rights and quite liberal with respect to attitudes toward divorce and remarriage. Therefore, we can expect our results to be, in absolute value, a lower bound of family composition effects, as the negative effects of family composition might be larger in other countries within Europe where family structures are regulated and perceived by the public more heterogeneously. Although providing evidence in such countries using administrative data may not be feasible, future research could exploit surveys or qualitative data to further investigate the outcomes of children in heterogeneous family compositions. This research could be relevant in understanding the role of legal frameworks and public opinion in moderating the relationship between family environments and children's outcomes. Indeed, in this doctoral dissertation, we often found no evidence to support many of the theoretical mechanisms discussed and tested. As this might be in part due to the particular context of the Netherlands, future work might also consider testing the mechanisms outlined in this thesis in other contexts. Nevertheless, we must emphasize that the findings of this doctoral dissertation indicate what other countries may attain when they develop attitudes similar to the Netherlands toward nontraditional family compositions.

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