



A classic that wasn't: *Statistical Geography* and paths only later taken

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journals.sagepub.com/home/phg**Ron Johnston and Kelvyn Jones**

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Abstract

Science is a cumulative activity, a body of knowledge sedimented in its publications, which form the foundation for further activity. Some items attract more attention than others; some are largely ignored. This paper looks at a largely overlooked book – *Statistical Geography* – published by three US sociologists at a time when geographers were launching their ‘quantitative revolution’. There was little literature within the discipline on which that revolution could be based, and a book with that title could have been seminal. But it was not, and as a consequence – as illustrated with three examples – major issues in spatial analysis were not addressed in the revolution’s early years. The paper explores why.

Keywordsquantitative revolution, scientific progress, *Statistical Geography***I Introduction**

Science is a cumulative activity, with each generation of scholars standing on the shoulders of those who preceded them and with its progress very largely preserved through the published record – the books, chapters and articles reporting material that others build on. And yet there are many examples of publications that fail to attract the attention they probably deserved. Potentially important material may not be immediately taken up and will only get wide appreciation either when ‘discovered’ at some time after its publication (what some call ‘sleeping beauties’: Fang, 2018) or when ‘re-invented’ by others, perhaps in ignorance of the earlier source. In human geography, for example, Duncan (1974) has shown that recognition of Hägerstrand’s pioneering work on spatial diffusion was delayed in part because it did not fit

into the dominant paradigm of geographic research at the time of its initial publication and in part because recognition of its innovative nature was only fully realised by those seeking a paradigm shift during his visit to Seattle in 1959/60: before then he was a ‘productive isolate’. After that visit he received wide recognition and enjoyed a very considerable reputation for his work on diffusion and subsequent studies.

Such a substantial oversight of a potentially important source also occurred with a book entitled *Statistical Geography: Problems in Analyzing Areal Data* by three American sociologists (Duncan et al., 1961: henceforth *DCD*), which

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attracted very little attention from geographers. Disciplinary myopia meant that, as far as citation analysis can show, relatively few geographers accessed it – certainly in the decade after its publication – let alone followed up some of its important suggestions regarding quantitative analyses of spatial data. According to Google Scholar[®] it had received 476 citations by early 2018 (accessed 26 April 2018), of which only 70 were by individuals readily identified as geographers.

That failure is somewhat surprising given the ferment of change that characterised human geography at the time of its publication. According to Burton (1963) the discipline had ‘undergone a radical transformation of spirit and purpose’ in the preceding decade, that he considered best portrayed as the ‘quantitative revolution’ which ‘had reached its culmination in the period from 1957 to 1960, and is now over’. That conclusion may have been presumptuous; indeed, in retrospect there probably never was a complete revolution as that term is generally understood – the overthrow of an established order. Nevertheless, there was a rapidly-growing increase in the application of statistical methods to geographical data (Johnston et al., 2018), in the UK as well as in the USA, and a book with that title should have attracted considerable attention among those seeking to promote the revolution/paradigm shift. But it did not – even though one of the revolution’s leaders, Brian Berry (pers. comm.), was a colleague of Dudley and Beverley Duncan at the University of Chicago when the book was being written and encouraged them to give it a title including the word geography; the Duncans worked in the university’s Population Research and Training Center and were not closely linked to the Department of Sociology, home of the world renowned ‘Chicago School’ of urban sociology.

II The book

As with many books, *DCD*’s subtitle is more informative about its contents than is the title;

Statistical Geography does not cover the entire field then being pioneered by geographers (there is nothing on point and line patterns, for example, and no mention of theoretical models of location such as central place theory) and its entire focus is on data for areal units. This reflects its origins in a series of studies on ‘Natural Resources and Regional Economic Growth’ undertaken for the Resources for the Future Inc. thinktank which involved exploring a set of methodological problems associated with the available data for such studies – most of them collected by the US Bureau of the Census and similar bodies. The authors’ Preface indicated their ‘manifestly incomplete’ knowledge of the geographical literature and their only becoming aware of some of it when the work was nearly complete, but also their belief that the problems they identified did not apply just to ‘any unique body of subject matter which representatives of a particular discipline are best equipped to investigate’ (p. vi). They expressed the hope that ‘geographers may learn from our efforts’; the argument we develop here is that very largely they did not, and important issues raised were only appreciated later – in some cases much later – in apparent ignorance that *DCD* had identified them as important, indeed crucial to the proper quantitative analysis of areal data.

The book is in three unequal parts: a short section (25 pages) on ‘Preliminaries’; a slightly longer one (29 pages) on ‘Areal Units and Areal Data’; and a much longer one (115 pages) on ‘Analysis of Areal Data’. The first identified four main ‘perspectives on areal differentiation’ within the extant literature: chorography, or areal differentiation, including regionalisation (Richard Hartshorne and Preston James are both quoted); analyses of areal distributions; analyses of spatial structures; and explaining areal variation. Use of regions in some analyses attracts criticism (pp. 140–1):

... some investigators think of regional differentiation as playing a role in the explanation of areal variation. In our opinion, this view is to be accepted only with grave reservations. In common parlance, of course, we talk as if ‘regions’ constitute an influence on social and economic phenomena... [but this may be] only a pseudo-explanation, at best a clue to the discovery... of some heuristic value for an investigator familiar with conditions prevailing in the region

leading to the later conclusion that (p. 146):

... the situation in regard to the use of ‘region’ as an explanatory factor is, at best, obscure, unless one simply chooses to follow out a computing routine more or less mechanically, without regard to the meaning of the results. In fact, there is much to be said for the view that using ‘region’ to ‘explain’ areal variation merely signifies that the investigator has not finished his [sic] problem.

Those four perspectives provide the context for their focus on methodological problems that ‘spread over quite a range of conceptual and mathematical complexity. Some of the simplest questions that can be addressed to areal data may harbour unsuspected difficulties and thus may afford suitable pretexts for methodological discussion’ (p. 29). Before proceeding with those discussions, however, the book’s second section outlines the nature of areal data and the spatial units for which they are compiled, since ‘the investigator must be cognizant of certain of their characteristics... [since these] may affect the conclusions drawn’ (p. 32; see Johnston et al., 2019).

The book’s core is in its third section, which identified four main objectives in the analysis of areal data (pp. 60–1):

- *The aggregation of data* to obtain a datum for a territorial universe;
- *The measurement of an areal distribution* – is the subject of interest spatially concentrated or dispersed, for example?;

- *The analysis of a spatial structure* – as in migration patterns; and
- *The explanation of areal variation* which ‘ordinarily involves description of the ways in which and degrees to which... phenomena vary among areal units, together with the application of some model which is supposed to account for such inter-unit differences’.

Most attention is given to the fourth of these.

A wide range of issues is raised, few of which were immediately taken up by geographers – see, for example, the discussion of compositional effects (pp. 99ff). The problems of ecological inference are covered, for example (pp. 69ff.; for a recent review of this topic see Gnaldi et al., 2018), and there are hints regarding both shift-share analysis (p. 63) and trend surface analysis (p. 134). We pay particular attention in the next section to three major issues facing the analysis of areal data that were discussed in some detail, two of which were realised by geographers within two decades of *DCD*’s publication, but without any reference to it; the full import of the third was not realised for several decades.

III The hints not taken

In the first decades of human geography’s ‘quantitative revolution’ it was generally accepted that standard statistical procedures deployed in other social sciences – notably but not only those comprising the general linear model – could be applied to geographical/spatial data without any particular problems. In 1956, however, Reynolds raised a number of specific issues regarding the geographical analysis of statistical data, in a brief paper that received little attention, with only 20 citations according to Google Scholar[®]: Barnes (1998: 216) termed it an ‘anodyne review of the potential of statistics in geography... which included a few mild cautionary remarks’. Reynolds

claimed that geographers needed to develop ‘distinctive tools’ specific to their discipline, but Garrison (1956: 428) responded that the ‘present methods of statistical inference are applicable to spatial-type problems’ such as correlation and regression, that ‘In short, there is ample evidence that present tools are adequate to our present state of development. No type of problem has been proposed that could not be treated with available tools’. He concluded that ‘The logical methods of science are universal. If assertions that geography is somehow different are accepted without proof, we may lose the benefits derived from the findings of others’ (p. 429). Indeed, that geography was not considered different is indicated by Hepple (2001: 385) noting that Yule ‘in the 1890s . . . constructed both the theory and application of multiple regression analysis, using geographical’ rather than individual biometric data (see also Denis and Docherty, 2007).

By the 1970s, however, it was clear to some that spatial data of various types needed bespoke methods and developments were put in train. *Statistical Geography* had earlier contained hints, in some cases strong hints, of those needs, but they were not taken up.

I Spatial autocorrelation

In one of the most-cited aphorisms of human geography’s post-1950s paradigm shift, Waldo Tobler in a 1969 conference paper propounded the ‘First Law of Geography’ that ‘everything is related to everything else, but near things are more related than distant things’ (Tobler, 1970: 236). Many like things cluster spatially, and some analysts realised that because the general linear model assumes that observations are independent of each other it could not properly be applied to geographical data, since neighbouring pairs of places were more likely to be similar than more distant pairs (see, for example, Dacey, 1968; Harvey, 1969; Gould, 1970; Curry, 1972; Berry, 1973). This spatial

dependence means that there is not as much information as appears, and unless analyses take this into account Type I errors are likely to occur – finding ‘significant’ results when there are none; this issue was recognised at least as early as 1888 in Galton’s critique of Tylor’s work drawing inferences from cross-cultural data (Hepple, 1998) – indeed, anthropologists refer to it as ‘Galton’s problem’ (Naroll, 1965). The first American textbook on statistical methods for geographers (King, 1969) includes few references to *Statistical Geography*; its first two chapters are included in a list of suggested reading for the chapter on ‘Numerical data in geographical research’. King does include a substantial discussion of spatial autocorrelation in a section on ‘Some related technical problems in geographical research’ in the chapter on ‘Analysis of spatial relationships and areal associations’ (King, 1969: 157–62). He notes that despite extensive work by econometricians on autocorrelation, ‘geographers have not progressed as far in handling the problem of spatial autocorrelation’ (p. 158). Alongside reference to Geary (1954), the main focus is on Dacey’s work (which is largely concerned with autocorrelation in point patterns and not in applications of the general linear model): *DCD* is not mentioned.¹

And yet *DCD* raise the issue very early in their book. They pointed out (p. 10) that Stephan observed in 1934 that neighbouring places are more likely to be alike than are distant places, raising concerns regarding statistical inference when areal data are being analysed; they also pointed to the analogy with autocorrelation in time series raised by Anderson (1954). But whereas ‘the variate of a time series is influenced only by past values, . . . for a spatial process the dependence extends in all directions’ (p. 11 – quoting Whittle, 1954). They cited Geary’s (1954) paper on contiguity ratios as one way of addressing this fundamental problem: later sections of the book also alluded to the problem (e.g. pp. 78, 111 – where there is a hint

of what later became known as geographical weighted regression – and 131 ff.) *DCD* does refer to the small early body of work by geographers on areal analysis that had some relationship to what became later appreciated as part of the spatial autocorrelation issue: Robinson's (1956) argument for weighting observations according to their spatial area, later generalised by Thomas and Anderson (1965), is mentioned but not recommended (*DCD* (1961: 47) noted that it was unclear what weighting system would be used if the data units did not vary by area) – and in any case it had little apparent impact on future geographical practice.²

Although several geographers were aware of the spatial autocorrelation issue by the late 1960s (Berry and Marble (1968: 2–3) refer to *DCD*'s coverage of it in their pioneering *Reader in Statistical Geography*, as well as to Matern's (1960) pioneering volume), the main work introducing it, and its resolution, to the discipline was undertaken at Bristol by Andrew Cliff (who had studied as a graduate student with Dacey in the early 1960s) and a statistician, Keith Ord. Their first paper was given at a conference in 1968 (Cliff and Ord, 1969), with a further paper at another conference a year later (Cliff and Ord, 1970), followed by two major monographs (Cliff and Ord, 1973, 1981) and a series of other papers. *DCD* is not mentioned in any of these works (nor are Anderson and Stephan).³ In a special issue of *Geographical Analysis* commemorating the fortieth anniversary of their first publication (Griffith, 2009), Cliff and Ord (2009) reflected on the origins and nature of their work, noting the major initial influence of Cliff's mentor at Northwestern University, Dacey, and their focus on the use of contiguity matrices as developed by Geary and Moran (1950): *DCD* is not mentioned but Gould's (1970) paper, presented at the same 1969 conference as their own, is. That commemorative issue contains 13 other papers, none of which mentions *DCD*.⁴

2 The modifiable areal unit problem (MAUP)

One area of then-contemporary geography that attracted *DCD*'s attention was its concern with regions; their definition was a dominant feature of the reigning areal differentiation paradigm. Regions were being identified using a range of areal data, but they noted (*DCD*, p. 25) that:

... it has to be recognized that some techniques of manipulating areal data produce results which have meaning only in relation to the particular set of areal units on which the results are based.

This was stressed further later in the book (pp. 98–9) in a statement that:

Many researchers on areal differentiation are forced to work with pre-fabricated areal units which they accept for reasons of convenience and expediency; moreover, as we have indicated, the results of manipulating areal data often are to some degree dependent on the choice of a set of areal units... students of areal structure must take into account the discrepancy between their hypothetical constructs and their actual results which is generated by the necessity of working with systems of areal units for which data are available.

This problem of 'modifiable units' (a term they adopted from Yule and Kendall (1950);⁵ the problem was first recorded in a short note by Gehlke and Biehl (1934) which is not cited in *DCD*⁶) in regional definition can be addressed either by choosing among a set of alternative regionalisations, which might produce different results, or by creating a bespoke set, but in that case it would be impossible to know whether it was the optimum.⁷ (The term had been used previously, in the geographic literature, by Reynolds (1956: 130),⁸ who wrote of 'a complicating factor that often disturbs the results, even when otherwise valid methods are employed. This is the modifiable nature of areal units and their varying size'.⁹)

As with spatial autocorrelation, King (1969: 154–7) recognised the existence of MAUP but it was only formally taken up – following Yule and Kendall but not *DCD* – by two early British converts to the quantitative revolution then working at the University of Newcastle upon Tyne, Stan Openshaw and Peter Taylor. Openshaw was interested in regionalisation using areal data and – following Yule and Kendall – recognised that both scale and aggregation issues were involved in what he termed ‘zone design’ (Openshaw, 1977).¹⁰ As *DCD* had recognised, different patterns may result from analyses at different scales (the scale effect: Bird (1956) made the same point in a very different context), and different aggregations of smaller areal units can also produce different results at the same scale (the aggregation effect). Taylor became interested in similar issues when he initiated studies of electoral geography in the United States, noting that different sets of boundaries for both US Congressional Districts and UK Parliamentary constituencies, but with their size held constant, could produce different election results – an example of the aggregation problem (Taylor, 1973; Taylor and Gudgin, 1976a, 1976b; Gudgin and Taylor, 1979), though this was not recognised as such then, with no citations of Yule and Kendall, let alone *DCD*. Neither Yule and Kendall nor *DCD* feature in their major critique of geographers’ (until then) little appreciation of the problem (Openshaw and Taylor, 1981; an earlier piece – Openshaw and Taylor (1979) – refers to Gehlke and Biehl (1934) but not to Yule and Kendall); neither is referenced in Openshaw’s (n.d.) undergraduate primer on the problem.¹¹

DCD’s identification of the ‘distortional influence of the manner of aggregation’ is, however, cited in an early paper which noted that the danger of ignoring this had been identified by *DCD* but ‘these comments are, apparently, not widely known’ (Lloyd and Dicken, 1968¹²) – and that remained the case for the next decade.

They further noted, significantly pre-dating later work on MAUP, that

it is theoretically possible to obtain almost any desired index value merely by tinkering with the size and shape of areal units... [and that] the application of a wide range of valuable descriptive and analytical techniques is rendered extremely difficult by the spatially chaotic system of areal subdivisions upon which all published statistical data are based. (p. 309)¹³

Openshaw and Taylor (1981: 67) identified three reasons why the MAUP might be ignored in geographical analyses: it is insoluble; it is of trivial importance; and acknowledgement of its existence would cast doubts on much geographical analysis. Most have adopted either the first or the second of these, and perhaps because of this it gets little attention in many statistics texts for geographers,¹⁴ though some have recognised that appreciation of the MAUP enables sophisticated appreciation of the spatial scale of many geographical processes (Jones et al., 2018).

3 Spatial scale

Scale has always been a key geographical concept, with its origins in cartography, and some of the earliest quantitative analyses recognised the issues it raised. McCarty et al. (1956, referenced by *DCD*), for example, argued that conclusions drawn from analyses at one spatial scale should not be expected to apply to other scales. Others (e.g. Haggett, 1964a – see also Haggett, 1964b, 1965; Chorley et al., 1966) made the same point, referring in that case to *DCD*.¹⁵ A major argument in *Statistical Geography* went much further, however, but it was almost totally ignored for some 55 years.

Although they did not initiate the method, in several key publications Duncan and Duncan (1955, 1957; Duncan and Lieberman, 1959) brought the indexes of dissimilarity and segregation as means of comparing two distributions

across a set of areas to wide attention. That work was surprisingly not specifically discussed in *DCD*, but a long section on ‘Measurement of areal distribution’ (pp. 81 ff.) introduced an ‘index of concentration [that] represents a specific application of the more general index of dissimilarity’ (p. 83: the formula is exactly the same). They applied it to the distribution of population across the United States at six successive censuses and five spatial scales – ranging from geographic divisions through states, economic subregions and state economic areas to counties. Their results showed that ‘In general, the smaller the average size of areal unit, the larger the index value’ (p. 84), and they followed that statement with this important extension:

... if one system of areal units is derived by subdivision of the units of another system, the index computed for the former can be no smaller than the index for the latter. Thus the index of concentration on a county basis will exceed the index on a State basis, because the county index takes into account intrastate concentration.¹⁶

They didn’t go further in suggesting how the degree of concentration could be calculated at each scale independent of the others, however.

In their example, *DCD* had a set of nested units in a five-level spatial hierarchy but at the time – and for some decades thereafter – that was a rare situation: most analysts had to accept data for whatever areal units were published and in many cases could not explore scale variations. Hence *DCD* concluded that section of their discussion with (pp. 98–9):

Many researchers on areal differentiation are forced to work with prefabricated areal units which they accept for reasons of convenience and expediency: moreover, as we have indicated, the results of manipulating areal data often are to some degree dependent on the choice of a set of areal units. Consequently, present practice in research can be fully satisfactory neither from the extreme ‘nominalist’ viewpoint (because the

description can be given in terms of a particular set of areal units) nor from the extreme ‘realist’ viewpoint (since prefabricated areal units are not ‘real’ regions). How this problem may be resolved cannot be foreseen. But it seems that men [sic] trying to develop cogent theories of areal structure will have to reckon with it for some time to come. Meanwhile, students of areal structure must take into account the discrepancy between their hypothetical constructs and their actual results which is generated by the necessity of working with systems of areal units for which data are available.¹⁷

This remained the situation for several decades, notably in the study of ethnic residential segregation in cities; there were few opportunities for exploring its intensity at a range of scales, and those that did (e.g. Peach, 1996; Woods, 1976) reported – as did *DCD* – that the indices of dissimilarity/segregation were larger the smaller the areal units, without any recognition of the interdependence of scales issue; indeed, Logan et al. (2015: 1077) later indicated that their purpose was ‘not to demonstrate that segregation is higher at a finer spatial scale, which is already well known’; if *DCD*’s argument about this had been appreciated when, or soon after, they published their book rather than over five decades later, the portrayal of residential segregation may have been very different.

The last decade has seen both a recognition that ethnic residential segregation patterns result from multi-scalar decision-making processes – many households choose which sector of a city they want to live in and then, within that, which local neighbourhood (Fowler, 2015) – and the realisation that geocoded data allow measurement of their intensity at multiple scales (e.g. Lee et al., 2008; Reardon et al., 2008, 2009; Wright et al., 2011; Östh et al., 2015). But their analyses made no reference to the issue raised by *DCD* regarding nested scales. Nor did they refer to the only other paper that did raise the issue – though without reference to *DCD*. Tranmer and Steel (2001), focusing on

population distributions more generally (as did *DCD* with their index of concentration), observed that, when analysing a three-scale nested data set, if one of the levels is excluded from the analysis ‘the variation that occurs at the level not included in the models is redistributed to the levels that the models do include’ – and although it is not always clear how much is allocated to which other level they conclude from their theoretical and empirical analyses that (p. 947):

The results suggest that the effects of levels above the highest level included in the analysis will be reflected in estimated components for the highest level included in the model. If the individual level [i.e. individuals who are aggregated up to the lowest level analysed] is ignored it will affect the estimates for the lowest level included in the analysis. The effects of an ignored level that lies between the two levels included in the analysis are redistributed between these two levels.

The importance of both *DCD*’s and Tranmer and Steel’s argument was belatedly recognised in the development of a multilevel modelling approach to measuring segregation at a variety of spatial scales (Jones et al., 2015),¹⁸ which has since been applied in studies of a number of cities and linked to the multi-scalar processes underpinning residential location decisions (Manley et al., 2015) as well as to analyses of other multi-scale aspects of spatial polarisation (Johnston et al., 2016). The key feature of this approach is its capacity to distinguish variation at one scale net of variation at others, and thereby illustrate significant differences in residential location patterns between groups that might otherwise not be identified that are scale specific (Manley et al., 2019). That modelling strategy has additional benefits in that it corrects for other problems in segregation measurement reflecting the influence of stochastic and measurement error on the magnitude of the traditional indices – issues recognised (as in Carrington and Troske’s (1997) illustration of

the tendency for the indices to be exaggerated when relatively small numbers are involved, as in many studies at micro-scales¹⁹) but very largely ignored in the extensive literature on changing patterns of segregation produced in the last two decades. (A paper by Kish (1954) does identify several of these issues – heterogeneity, dependence, multiple scales, natural-stochastic variation – but few followed them up: *DCD* (p. 38) – in a discussion of homogeneity/heterogeneity in areal units – suggest that Kish’s paper is ‘a significant example of a kind of thinking that should be taken up’;²⁰ see also the discussion of scales and processes in Manley et al. (2006).)

IV Selective progress

Science progresses through the accumulation of knowledge. Central to this process is the dissemination of new findings and arguments through printed media, in some cases enhanced by presentations at conferences, symposia, seminars and other events. Individuals and groups hear/read what others have done and build their future work on those foundations. But, as noted above in reference to Duncan’s (1974) study of Hägerstrand’s influence, some published works may not have a substantial impact even though they are innovative in one or more ways. *Statistical Geography* is one such work: it has a large number of citations but, despite the noun in the title, only some 15 per cent of them from geographers. Further, few of those items in which *DCD* was cited take up any of the major issues facing spatial analysis that they raised: most just make relatively bland statements about them – as in Simmons’ (1967: 389) footnote that ‘Problems of analysing areal data are discussed’ in that book.²¹

So why did *Statistical Geography* attract so little attention among geographers, especially in the years immediately after its publication when such a title should have been a magnet for the small, but rapidly increasing, number of

geography's practitioners for whom the topic was central to the changes they wished to make to their discipline? In his exploration of the delayed recognition of Hägerstrand's work on spatial diffusion Duncan (1974) suggests three possible causes of such a situation: (i) general social resistance, perhaps linked to linguistic or political barriers; (ii) what he terms a 'paradigmatic effect', with the work being largely ignored because it did not fit into the discipline's established practices; and (iii) a 'Matthew effect' – science is practised in interacting communities, groups of individuals who share common interests (research subjects, methods, etc.), and the work of outsiders – especially those with few if any links to one or more of those communities – may be ignored because of the lack of contacts. (Granovetter's (1973) work on social networks is relevant in this last case. Each scientific community is characterised by strong ties among its members, which sustain its activity. But one or more members may have weak ties with extra-community members, and these can be the source of new ideas. Without such weak ties, however, the community may either not recognise or just ignore such work.)

The first of those possibilities does not apply to *DCD*. The authors were American, Dudley and Beverly Duncan were by then well-established scholars working at a prestigious university,²² and their book was produced by a reputable publisher, based largely on work for a well-known think-tank. With regard to the second possibility, *Statistical Geography* clearly did not fit into the then-established geographical paradigm (as clearly set out by both Hartshorne (1939, 1959) and James and Jones (1954): both are cited by *DCD*). But, as Burton (1963) had argued, the book was published at a time of considerable ferment within geography, with a growing number of its scholars seeking to change its practices fundamentally – indeed to ensure a revolution (Johnston and Sidaway, 2016). But citation analysis suggests that most

of those involved in promoting that tradition, within which the adoption of the rigour of statistical analysis was a central goal, didn't find stimuli within *DCD*.²³

To some extent, that is understandable. Although the adoption of quantification was core to the revolutionaries' agenda, most of their attention in the early years of their activity was not on *areal* analysis. Following the lead set by Schaefer (1953 – not cited by *DCD*), among others, their focus was on *spatial* analysis, on point and line patterns, much of it set within the context of central place theory. As exemplified by Bunge's (1962) pioneering case for this 'new geography', *areal* analysis was not central to its concerns and so the methodological issues raised by *DCD*, with most of their empirical attention on a single substantive topic (regional economic growth) that was also peripheral to much contemporary geographical study, were not relevant. They became so – as illustrated by Simmons' (1967) paper; indeed it could be argued that within little more than a decade *areal* analysis had supplanted spatial analysis as the preferred/practised methodology of much quantitative human geography, but by then *DCD* had attracted little attention and was not widely discussed within geography's relevant research communities.

Simon Duncan's paradigmatic argument has some relevance to an appreciation of *DCD*'s limited impact on human geography in the 1960s, therefore. So too does his 'Matthew effect' argument. *DCD*'s three authors were sociologists with few direct contacts with geographers (apart from Brian Berry, the only other geographers acknowledged in their Preface are Harold McCarty and Arthur Robinson, both of whose published works are cited in the bibliography, along with those by 12 other geographers). These were 'weak ties', in Granovetter's (1973) terms, but they seem to have contributed little to the emerging geographical practices. Geography was not widely recognised as a social science in the early 1960s: few other

social scientists developed relationships with geographers then and few geographers sought inspiration from their work – something that changed very substantially when there was a shift away from the geometric focus of spatial analysis towards areal analysis and the study of spatially varying population profiles (as in social area analysis: Berry and Rees, 1969; Johnston, 1969, 1971).²⁴ That sociologists in the United States were well ahead of geographers in their adoption of relatively sophisticated quantitative approaches is well illustrated by the contents of Hagood's (1941) textbook and its revised edition (Hagood and Price, 1952).

The history of scholarly disciplines often has a Whiggish connotation, emphasising continuity as the present builds on the past, with occasional interruptions – paradigmatic shocks. Those histories focus on the publications that have most impact. Citation analyses show – on that metric at least – that many publications have little or no impact and so do not appear in the written histories (or the unwritten ones, such as those presented to students). Does that matter; is it of value to future disciplinary adherents to know about such pieces, even if some think they might/should have been important? As exemplified here, if the ideas that they encapsulate are important they will surely be taken up by others a little later – progress may be delayed, but not substantially so. This was certainly the case with both spatial autocorrelation and MAUP: *DCD*'s identification of their importance to areal analysis was not followed up, but the former became a major research issue within a decade and the latter within two decades, although in that case resolution has never been reached. But the third exemplar deployed here – multilevel scale issues – went totally unrecognised for four decades, and when independently discovered then (by Tranmer and Steel, 2001) it again went largely disregarded until a further re-invention some 15 years later.²⁵

For a variety of reasons, good and bad and including those advanced here, some pieces and arguments are likely to go largely unrecognised, increasingly so as the volume of published work continues to expand exponentially. Researchers have to be selective in what they read and take account of, and to a large extent they work within the established parameters of their own sub-discipline (or set of practices and concerns that might be trans-disciplinary) and the formal and, especially, informal networks to which they belong. Some may, occasionally if not frequently or regularly, cast their net more widely and, perhaps serendipitously, encounter something that they can bring into, and potentially change, their paradigm. They are the 'weak ties' without which much science may atrophy, but strong intra-community ties dominate scientific practices and it may be that no firm links are made with a body of literature – even a single item – that could transform a set of practices. It is perhaps also relevant that, despite *DCD*, areal analysis did not come to occupy a major role in sociology in the 1970s–1980s either (or, for that matter, political science); if it had, there may have been more cross-fertilisation with geographers, but the growing emphasis on survey analysis by sociologists which geographers didn't immediately share meant that there was a major unbridged methodological gulf between the exponents of quantitative analysis in the two disciplines (see Converse, 1987).²⁶

Scientific practices, as illustrated here, are socially constructed and are conducted within relatively closed communities. Occasionally those communities are challenged by new ideas, that may be independently developed internally or, more likely, are introduced from an external source. The latter may be instrumental in changing a discipline, or at least one or more of its parts, but if their introduction is not undertaken, or is at best partial and not then followed up by other practitioners, the impact may be slight. That was the case with *DCD*'s *Statistical Geography*: despite its relevance to their cause,

geographers seeking to change their discipline in the 1960s didn't identify it as valuable material to advance their agenda, and its authors, although hoping that 'geographers may learn from our efforts', did very little to advance that.²⁷ In the history of statistical geography, *Statistical Geography* played a very minor role – and geography changed anyhow!

There is a growing literature on what is termed 'counterfactual history' (Evans, 2016; see also Fearon, 1991, and Tetlock and Belkin, 1996), which addresses 'what if' questions – conjectures on what did not happen, or which might have happened, that assist understanding of what did happen (Black and McRaid, 2007). These might also be addressed to the history of academic disciplines and their practices, allowing us to appreciate better the paths taken by exploring the paths not taken and why. Just as the quantitative revolution might have taken a rather different form – lacking its early commitment to logical positivism, for example – if its proponents had been influenced by the writings of Crowe (1938) and Jones (1956) on laws and tendencies, might its practices have developed differently if *DCD* had been widely read and built-upon in the 1960s? Crowe and Jones were geographers who published in geography journals, but those papers had little impact; *DCD* were not geographers but they published a book with geography in the title. The lack of impact for Crowe and Jones suggests a failure of bonding social capital within geography – is it the same now? And *DCD*'s weak impact suggests an absence of bridging social capital between disciplines six decades ago – raising the same question. A Romanian sociologist has represented his discipline as characterised by 'multi-paradigmaticity, scattered cumulativity and multi-localized ignorance' (Rusu, 2012), a representation that has been applied to the contemporary situation in human geography (Johnston et al., 2014). Its wider application, alongside the concepts of bonding and bridging capital and that of strong and weak ties between

communities, could shed much light on disciplinary history – of what has been and might have been. 'History is written by the winners' is a much-challenged aphorism that has more than a ring of truth when the history of geography – or the nature of progress in geography – is being debated; exploring the discipline's 'might-have-beens', the 'losers' who had little or no impact, could illuminate how and why it has progressed along its chosen paths.

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Notes

1. The first textbook on statistics for geographers by a geographer (Gregory, 1963) makes no reference to the issue, but it is briefly mentioned in the fourth edition (Gregory, 1978); it is not mentioned in Cole and King (1968). As more introductory quantitative texts appeared, so attention was drawn to spatial autocorrelation, in their later if not initial editions (e.g. Taylor, 1977; Hammond and McCullagh, 1974; Matthews, 1981; Norcliffe, 1977; Gaile and Willmott, 1984; Williams, 1984; Ebdon, 1977; Clark and Hosking, 1986); surprisingly, it gets only one short paragraph in a much later introductory volume (Earickson and Harlin, 1994).
2. Thomas and Anderson's work was also relevant to the identification of the MAUP problem, but this appears to have gone almost entirely unnoticed.
3. *DCD*'s raising of the spatial autocorrelation issue is referred to in an earlier paper by Goddard (1968: 72), who acknowledges the problem but then simply notes

- that, as a consequence, the results of his correlation analyses ‘underestimate the true amount of spatial association’.
4. Andrew Cliff (pers. comm.) records that he was unaware that the issue was raised in *DCD*. Following his work with Dacey he was encouraged by his Bristol PhD supervisor (Peter Haggett) to apply those ideas in a conference which Hägerstrand was attending. He encountered Moran’s (1950) paper during a literature search and applied it to some of Hägerstrand’s data (Cliff, 1970), after involving Keith Ord (then in Bristol’s economic department) in working out the relevant distribution theory.
 5. Berry and Marble (1968: 2) note Yule and Kendall’s identification of the modifiable units issue, but not its discussion by *DCD*.
 6. Moore (1969–70: 113) noted that ‘the average level of correlation is significantly higher’ at larger spatial scales, citing *DCD*, concluding only that studies should be undertaken at a variety of scales in order to identify that which is ‘appropriate’ (p. 120). Harvey (1968: 71–2), also citing *DCD*, noted that different processes may operate at different scales, but ‘we have no measure of the scale at which a particular process has most to contribute to the formation of a spatial pattern and our notions of the scale problem remain intuitively rather than empirically based’.
 7. A related issue, not concerned with the MAUP per se, concerns the use of sets of areal units of varying size and shape, as illustrated by Chisholm’s (1960) critique of Dickinson’s use of administrative data to portray commuting patterns (Dickinson, 1957, 1959); he argued that if the areal units deployed are not uniform in size and shape then ‘some degree of spurious variation is introduced’ (p. 187). Dickinson (1960: 296) responded that he found Chisholm’s comments ‘neither relevant nor helpful’.
 8. Reynolds’ paper is briefly acknowledged by *DCD* (p. 7), as one of a short list of people (not all of them geographers) who had taken ‘some initial steps toward codifying methods of statistical geography’.
 9. Norcliffe (1977) exemplifies the issue in the context of a discussion of ecological correlation and inference, with no references.
 10. Cliff et al. (1975), in their work on regionalisation as a combinatorial problem, identified the aggregation problem but did not link it to the MAUP more generally.
 11. Peter Taylor (pers. comm.) reports that they were not aware that the modifiable units issue was raised in *DCD*.
 12. Peter Dicken (pers. comm.) has reported that they became aware of the issue, and of *DCD*, from the reference to it in Haggett’s (1965) book.
 13. Haggett (1965: 186) made the same point in his pioneering spatial analysis text.
 14. Barber (1988) refers to MAUP in three sections of his book, but with no references! Norcliffe’s (1982) is ostensibly about MAUP but focuses almost entirely on ecological inference; Bennett (1984) has a substantial discussion in his chapter on space-time modelling.
 15. Peter Haggett (pers. comm.) reports that his copy of *DCD* is heavily annotated, indicative of its impact on his early work.
 16. Duncan (1957: 31) had first raised this argument a few years earlier: its 121 Google Scholar[®] citations include very few by geographers, certainly not in the years immediately after its publication.
 17. For a contemporary discussion of that issue, see Hand (2018).
 18. There have been studies – e.g. Haggett, 1964a; Moellerling and Tobler, 1972; Voas and Williamson, 2000 – that have sought to decompose spatial variation into its various scale components, but these have not addressed *DCD*’s argument regarding cross-scale dependence.
 19. Interestingly, Chisholm (1960) made a similar point, quoting Choynowski (1959), that observed regional differences in ratio measures based on small samples may not be statistically significant (see Jones et al., 2016); the issue is not sampling variation but natural variation when dealing with *all* observed outcomes (not a sample) with a small absolute number of observations per areal unit.
 20. *DCD* also cites a University of Michigan 1956 PhD thesis – ‘A Quantitative Analysis of Regionalism in the United States, 1940’ – by David W. Varley, who died in 1970.
 21. Jim Simmons was a graduate student working with Brian Berry at Chicago in the early 1960s.
 22. On Dudley Duncan (1921–2004) see the In Memoriam at https://senate.universityofcalifornia.edu/_files/inmemoriam/html/otisdduncan.html (accessed 25 May 2018) and Goodman (2007); on Beverley Duncan see <http://texts.cdlib.org/view?docId=hb967nb5k3&doc.view=frames&chunk.id=div00018&toc>.

- depth=1&toc.id= (accessed 25 May 2018); and on Raymond Cuzzort see King (2014).
23. That it failed to attract attention is particularly problematic given the situation in the 1960s. Peter Haggett (pers. comm.) notes that Torsten Hägerstrand likened the accumulation of knowledge to a bathtub with two taps, one controlling the inflow of material and the other the outflow: that remaining in the bath at any time comprises the currently influential material. In the early 1960s the inflow of quantitative work would have been little more than a trickle, and most material should have been retained in the tub; 60 years later the inflow is many times larger, and much of it quickly disappears through the outflow – and the half-life of papers is declining (Stoddart, 1967). In contemporary spatial science, therefore, individual items may have little or no impact because they are competing with many others for attention; in the 1960s that was not so.
 24. It is almost certainly the case that human geographers trawl more widely through the literatures of the other social sciences as well as the humanities in the search for weak links – and, to a lesser extent, vice versa – than was the case in the 1960s, a sign of human geography’s growing maturity?! Perhaps – as was the case in the past – *Progress in Human Geography* should solicit more essays that report on such trawls and suggest potential links that might be followed up.
 25. In that case, the original development (Jones et al., 2015) took place without any reference to *DCD*: the latter’s relevance only became apparent to the authors as they extended the work.
 26. There is a later tradition of analysing quantitative data on individuals within geography, although the initial stimulus came from transportation studies (Wrigley, 1985).
 27. One difficulty that geographers experienced in the application of methods that recognised the spatial autocorrelation and modifiable units issues was the availability – or lack of it – of computing resources. In some places (as at Chicago in the late 1960s, as Brian Berry – pers. comm. – points out) computing time was not only rationed but also expensive, and while some standard packages became available their implementation was not rapid.: SPSS, not the first but one of the most accessible early packages, became available in 1968 (Nie et al., 1970: Nie was a political scientist who started the work at Stanford and continued it at Chicago). However, bespoke software for spatial analysis only became available much later – notably GeoDa (Anselin et al., 2006; see also <http://geodacenter.github.io/>).

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