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Do Physicists Have 'Geography Envy'? And What Can Geographers Learn From It?

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Abstract: Recent years have seen an increasing amount of work by physicists on topics outside their traditional research domain, including in geography. We explore the scope of this development, place it in a historical context dating back at least to statistical physics in the nineteenth century, and trace the origins of more recent developments to the roots of computational science after the Second World War. Our primary purpose is not historical, however. Instead, we are concerned with understanding what geographers can learn from the many recent contributions by physicists to understanding spatiotemporal systems. Drawing on examples of work in this tradition by physicists, we argue that two apparently different modes of investigation are common: model-driven and data-driven approaches. The former is associated with complexity science, while the latter is more commonly associated with the 'fourth paradigm', more recently known as 'big data'. Both modes share technical strengths, and more importantly a capacity for generalization, which is absent from much work in geography. We argue that although some of this research lacks an appreciation of previous geographical contributions, when assessed critically, it nevertheless brings useful new perspectives, new methods and new ideas to bear on topics central to geography, yet neglected in the discipline. We conclude with some suggestions for how geographers can build on these new approaches, both inside and outside the discipline.

Keywords: social physics, computational science, complexity, cybernetics, systems theory, fourth paradigm, big data, quantification, geographical thought

1 Introduction

In 2010, the New York Times magazine featured a headline story "A Physicist Solves the City" describing how physicists developed new understanding, via a set of 'hidden laws', of urban form and function using sophisticated mathematics and large datasets (Lehrer 2010; see Bettencourt and West 2010). On closer inspection, the 'solution' is in many ways not new at all because it revolves around well-known advantages that larger cities yield in economic specialization, the generation of ideas, and so on.[1] While the physicists featured in the article, Bettencourt and West, give credit to Jane Jacobs (1961; but not 1969) for presciently pointing a few of these things out, the reader is left with the impression that this work generated insights unavailable until the attention of physicists was trained on the city. At the same time, this research and follow-on efforts offer new directions in our understanding of cities (Batty 2013; Bettencourt 2013).

This is an instance of a growing number of studies by physicists addressing topics long studied by geographers and others that demonstrate potential advances while ignoring earlier research. We might term such 'intrusions' into geography by physicists, tongue-in-cheek, *geography envy*, the obverse of the more familiar *physics envy.* Physics envy is a (generally) derogatory term aimed at advocates for the adoption in other fields of methods and concepts from physics, particularly an emphasis on parsimony and elegance in explanation. The notion of 'envy' arises from the perceived desire of such advocates to move up an academic scientific hierarchy that has physics at the top, followed by other natural sciences, then the lowly historical and social sciences, and finally the humanities (Massey 1999).

Such physics intrusions are recognized across many academic disciplines, to the point of caricature (Figure 1). A standard template emerges when reading such work. A cursory review of existing research is reeled off then set aside. Central questions are reframed in terms of reproducing statistical features of the phenomenon, such as the highly skewed distribution of earthquake magnitudes or city populations. A highly stylized computational, statistical or mathematical model is demonstrated to exhibit the required statistical features, implying that the model explains the real-world phenomenon. Filling in the details of real world mechanisms that explain why the model works, can be done later by domain-specific experts. The implication is that a general explanatory mechanism is more significant than incidental details of any specific field.

While this is a caricature, it has some basis in reality. What sets at least *some* physicists apart is a conviction they have the necessary analytical rigor and skills to identify foundational rules of all kinds of systems without needing to substantively engage existing theories, data, or researchers. The physicist Dietrich Stauffer puts it like this: "physicists not only know everything; they know everything better" (2004, 1). While a physicist 'solving the city' may seem extreme, consider also Per Bak's *How Nature Works* (1997). In one slim volume numerous phenomena, including earthquakes, forest fires, economies, traffic jams, even the evolution of life itself, are explained by a single mechanism, self-organized criticality. A much less svelte volume, Wolfram's *A New Kind of Science* (2002) makes even more ambitious claims.[2] The mindset underlying these examples extends to other domains, including computer science and mathematics, although physicists are the most notable practitioners. Geographers have been slow to recognize these incursions compared to other disciplines where similar patterns have been noted (Parsegian 1997; Ball 2006; Egler 1986; Gallegati et al. 2006).

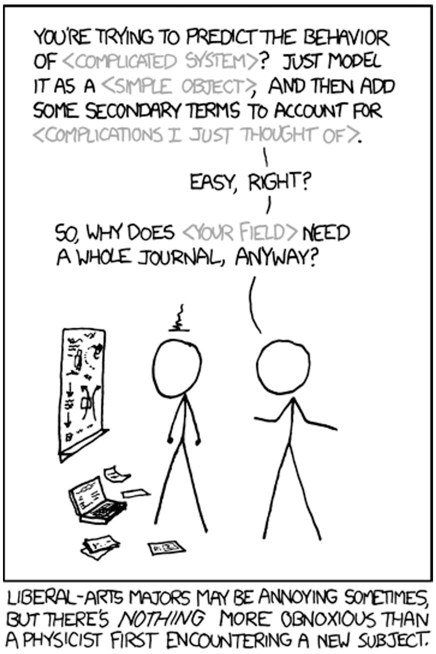

Figure 1 The trouble with physics as illustrated by XKCD 'A webcomic of sarcasm, romance, math, and language'. This cartoon is entitled 'Physicists' and also featured pop-up 'alt-text' when a mouse is hovered ovwaer the image, "If you need some help with the math, let me know, but that should be enough to get you started! Huh? No, I don't need to read your thesis, I can imagine roughly what it says." *Source*: http://xkcd.com/793/

While notions of physics envy and geography envy[3] are our point of departure, we have no interest in defending the intellectual 'turf' of geography, nor in putting physicists in their place. Instead, we want to encourage greater engagement by learning from work in this vein of physics, while also better understanding its shortcomings. Aggressively defending the intellectual 'territory' of geography would be delusional given the breadth of phenomena geography itself claims to address, and its array of underlying assumptions, theories, and epistemologies.[4] Instead, we want to walk a fine line, assessing how geographers can engage research that ignores the lessons of geography and understand what the 'geography envy' of physicists might have to offer, all without succumbing to a latter-day physics envy. Physics envy and geography envy are therefore entry points for our aim of stimulating debate about how geography should engage with knowledge production in which physicists are prominent, particularly complexity science and big data, where scientific discovery is defined by data- and computer-intensive exploration (Hey et al. 2009; Bell, Hey and Szalay 2009).

Our argument has three parts. First, we provide examples of the work that interest us, examining the recent rapid growth in physics research on geographical topics, while placing it in the broader context of engagement among social sciences and the natural sciences. We emphasize that the changing nature of data and computation has altered the context, but suggest that these issues are not new, and that we can learn from the past while interrogating recent developments. Such disciplinary exchange dates back at least as far as the origins of statistical physics in the social physics and statistics of the nineteenth century (see Ball 2002) when the trade was in the other direction, and has been revisited in geography many times, in the guise of tensions between quantitative and qualitative approaches. We examine some of these past moments without pretending to provide a canonical history, much of which the reader can find elsewhere.[5]

Second, we distill key lessons from past and current engagements to guide future ones. These lessons focus on two modes of research in physics, namely model-led and data-led exploration. These are two sides of the same coin, which is the development of parsimonious, general explanations for complicated behavior in many systems, without recourse to historical or geographical specifics. *Model-driven* work seeks to develop simple mathematical or computational models that produce complex dynamics comparable to real-world behaviors described by a range of data. The *data-driven* approach is focused on inferential data-mining and pattern matching across (ideally) large data sets. Both approaches rely on the assumption that apparently complicated phenomena are, at their heart, driven by comparatively simple and generalizable features and behaviors.

Third, we look more closely at examples of this work and use these as a jumping-off point to discuss what geographers can learn from these developments, and the obstacles to engagement with work of this kind. We point to three ways forward. One, engaging with other fields while drawing on core geographical traditions of spatial-chorological and human-environmental research. Two, rediscovering quantitative analysis in partnership with qualitative approaches. Three, coupling our strengths in abstraction with a renewed focus on generalization via model-driven and data-driven approaches among others. We conclude with suggestions for future engagement with other fields, such as physics, that increasingly address geographical subjects.

2 Physics branching out

Work on geographical topics by physicists is not novel, even if it is increasing. Connections between physics and the social sciences are of such long standing that the origins of statistical physics lie in social science, inspired by the development of statistics in actuarial life tables (see Ball 2002, 2004). Since that 'origin moment' the traffic in ideas has more often been in the opposite direction from physics into the social sciences. As we show below, the engagement between physics and other domains has grown rapidly in recent years, but builds on a long history of interaction.

To begin, we consider recent trends, to establish that our account is not 'physics paranoia'. Figure 2 shows results from Web of Science for ten search terms in the category 'physics mathematical', for the two decades from 1993 to 2012.[6] While there is a general increase in the numbers of research articles across all topics over this period, areas that are not obviously traditional physics, such as 'social OR urban OR city' and 'game' (as in game theory) have grown rapidly, while more obvious terms such as 'laser', 'neutron' and 'quark' have grown more slowly. Of course, when a topic area starts from a low base, rapid relative growth is almost inevitable, but it is not obvious that topic areas such as 'urban' are part of physics at all, still less that research in mathematical physics in such topics would be comparable in scale to research on 'superconduct*'.

As another indicator of the growth of physicists doing geographical research, the American Physical Society journal *Physical Review E*, reports for the five year period from 2008 to 2012 (inclusive) 48 articles with the term 'social' in the title. In almost all cases, 'network' is the next word in the title, and over the same period there are 1415 articles 'network' in the title. There can be little doubt that an important driver of these developments is an interest (shared across all disciplines) in networks, and that social (and spatial) aspects of networks loom large (Barthélemy 2011), especially opinion-formation in networks, an example of which we consider in the next section. A wide-ranging review, covering well over 500 papers, highlights the major social topics of interest to physicists (Castellano, Fortunato, and Loreto 2009). Around 370 of these were published in the 2000s and only around 100 in the decade before. Even allowing for a bias towards more recent work, it seems clear that the volume of work in the general area of the 'statistical physics of social dynamics' is significant, and has increased in recent years. As many as a third of the articles cited cover opinion formation, a field closely linked to geography's interest in innovation diffusion (Hägerstrand 1968).

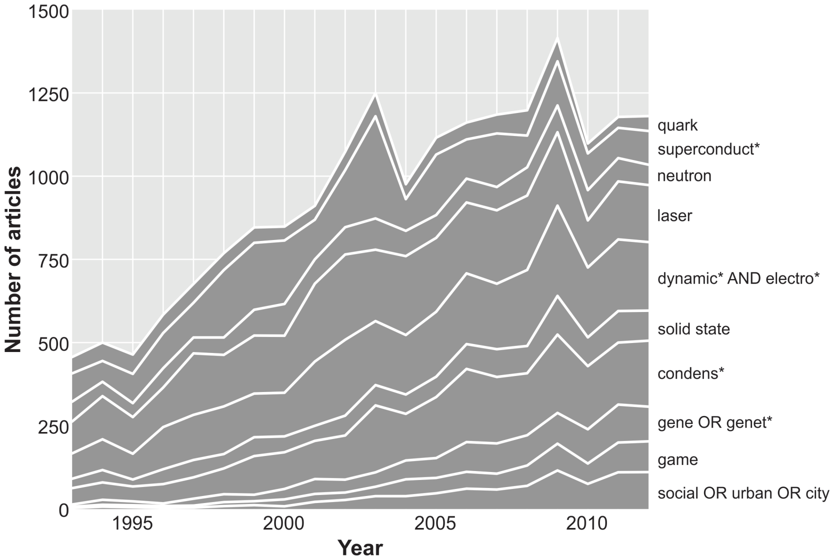

Figure 2 Articles classified by Web of Science as 'PHYSICS MATHEMATICAL' for years 1993 to 2012, for a variety of topic search terms.

This growth in physics-inflected work on society is the latest stage in a long history of traffic in ideas between physics and the social sciences. Early work in demography, for example, extends back to the 17th century when the natural philosophers Edmond Halley and Leibniz promulgated and extended the work of John Graunt and Caspar Neuman respectively, who had developed some of the first statistical population studies (Weeks 2011, see Wyly 2014b for an account of another early advocate of social physics, Auguste Comte). A particularly prominent example is the intimate connection between neoclassical economic theory and nineteenth century physics meticulously document by Mirowski (1989). Mirowski argues in later work (2002) that the strong influence of physics on economics continues up to the present via information theory, cybernetics and computational science. Early contributions of mathematical physicists such as John von Neumann are important to this history, although mainstream economics has resisted updating its physical influences to these newer models (Mirowski 2007).

One pathway in this traffic has been significant to the development of geography via the quantitative revolution (see Barnes 2001; Warntz 1984). A prolific contributor was the astrophysicist John Q. Stewart (1941, 1947a, 1947b, 1948, 1950) encouraged by William Warntz (see Warntz and Wolff 1971). Stewart suggests that many phenomena can be related to the "potential of population" or "demographic energy" (1950, 248) of a city or region, an early incarnation of the gravity model. Linear log-log plots demonstrating these relationships (1950, 248-51) would not appear out of place in contemporary general science journals nor would they be alien to Bettencourt and West (2010; see also Olsson 1965). Presenting his ideas, Stewart (1950) recounted much of a history retold by Ball (2002, 2004), identifying contributions from unexpected quarters, such as retired publisher Carey's three volume *Principles of Social Science* (1858), which includes what might be considered an 'atomic' explanation of the gravity model: "Man tends of necessity to gravitate towards his fellow man. Of all animals, he is the most gregarious…" (42).

There is a palpable optimism in Stewart's presentation of social (and spatial) physics, mixed with perplexity at its failure to take hold in the social realm, given such promise: "There is a marked difference of opinion with respect to the usefulness of empirical regularity […] in progressing from observation to hypothesis or theory" (1950, 240), and later: "What conclusion is suggested by the fact that seventeenth-century activity in applying mathematics in social science was not followed through with enthusiasm in later years?" (1950, 245) Stewart blames academic specialization, and suggests that many significant contributions (such as Carey's) come from outside the academy, apparently not stopping to consider that social scientists might find mathematical and physical tools unsuited to understanding societies in detail.[7] Notwithstanding Stewart's pessimism, we assume he would have been pleased at the rapid development of gravity models (particularly by Wilson 1971, 1974). Descendants of the geographical gravity models that Stewart proposed, are by now widely used in regional science, trade theory and urban modeling (Van Bergeijk and Brakman 2010). They are also a cornerstone of work in fields from international economics to public health (Bergstrand and Egger 2011; Keeling and Rohani 2008).

Among his contributors from outside the ivory tower Stewart includes Warren Weaver, who is also enthusiastic at the prospect of applying computers to problems of "organized complexity" (1948, 539ff.). Weaver envisaged developments in line with what has since become 'complexity science',[8] where computer modeling is a critical aspect of how "middle-sized" systems are studied (for accounts of complexity science see Waldrop 1992; Coveney and Highfield 1995; Mitchell 2008; or, more specific to geography, Thrift 1999; Manson 2001; O'Sullivan 2004).[9] Even so, Weaver warns:

> "the humble and wise scientist does not expect or hope that science can do everything. He [*sic*] remembers that science teaches respect for special competence, and he does not believe that every social, economic, or political emergency would be automatically dissolved if the scientists were only put into control" (1948, 543-44).

Many of Weaver's successors are rarely so humble. Complexity science deploys computational models to explore real world systems and their properties, and this is where many of the more grandiose claims for the wide-ranging applicability of simple models are made. Excitement around complexity science has lessened in recent years, but the 'style' of doing science it pioneered is well established, finding echoes in the science of networks (Newman 2010). There are other strands in the post-war history of physicists working beyond the traditional boundaries of their discipline. The development of complexity as a field closely tied to computational models (see Dyson 2012) is paralleled by an approach from thermodynamics (Prigogine and Stengers 1984), while Haken's synergetics (1984) presents a related, alternative approach. While both have been taken up outside physics (see Allen 1997; Portugali 2000, De Landa 1997), neither has seen similar widespread adoption as a unifying 'paradigm' of complexity science.

The most recent development in physicists engaging with other disciplines is also closely tied to computational technologies in the form of the 'fourth paradigm' latterly rebranded 'big data' (Mayer-Schönberger and Cukier 2013, Kitchin 2014). The fourth paradigm refers to scientific inquiry that couples large data sets with interactive visualization and analysis, much of it in physics, mathematics and computer science (Hey et al 2009). This 'fourth' paradigm follows three posited earlier paradigms of empiricism, mathematical analysis, and computer simulation. The phrase fourth paradigm has been supplanted by 'big data', a term still being debated, but which at its core refers to analysis designed to deal with large amounts of data created by a growing number of sensors and systems (Hilbert and López 2011). In geography, big data are coupled to the emergence of spatial cyberinfrastructure designed to apply geographical methods to massive datasets (Wright and Wang 2011) collected from sensors ranging from government-launched satellites to individuals uploading observations as volunteered geographic information (Haklay, Singleton and Parker 2008).

3 A physics of social phenomena: motivating examples

In spite of the increasing popularity of non-physics work in physics, it is important to recognize that it remains a small fraction of the discipline's output. Even so, physics has made inroads in venues from traditional journals to new media. A recurrent feature of this work is the search for parsimonious and elegant explanations that account for the complicated behavior of systems, without invoking historical or geographical details. The pursuit of parsimony leads to the apparent blithe unconcern for earlier, more discipline-specific work. Before considering the potential advantages of these approaches, it is worth considering specific examples in more detail. We suggest that this work falls into two broad classes. First, a complexity-oriented computational modeling approach, which we term *model-driven*; second, a top-down, data-mining, inferential or *data-driven* approach. While, as we will see, these appear different, they share the conviction that apparently complicated phenomena can be expected to exhibit simple, generalizable features and behaviors.

Model-driven: exploring complex outcomes of simple dynamic systems

The *model-driven* strategy involves developing abstract mathematical or computational models of social phenomena in an exploratory mode. Our exemplar is a paper by Holme and Newman (2006), who present a model of the formation of opinions in social networks. This paper is interesting because it exhibits many features typical of work in this vein, speaks to a common social topic in physics, and relates to geographical work on innovation diffusion ranging from Hägerstrand (1968) onward (Polhill et al., 2008, Evans et al., 2011). Holme and Newman present their model with only cursory reference to empirical data or sociological details of the topic. By the second paragraph, the paper is concerned with a *model* of opinion formation not the phenomenon itself. Individuals have become nodes in a network, social interactions links in that network, and opinions an attribute of nodes. Events are limited to nodes breaking an existing link at random and making a new link to another node with the same opinion, or changing their opinion to match a randomly chosen neighbor. The model is evaluated against criteria that relate to interesting general dynamical phenomena, such as phase transitions, times to equilibrium, and how those times scale relative to model parameters. The only model characteristic related to directly observable social data is the frequency distribution of community sizes, where a community is a set of nodes holding the same opinion. This leads the authors to make only a cursory attempt to match findings to the social phenomena that inspired the model:

> "it is known that the sizes of the communities of adherents of religious beliefs are in fact distributed, roughly speaking, according to a power law.[10] This may be a signature of critical behavior in opinion formation, as displayed by the model described here, although other explanations, […] are also possible." (Holme and Newman 2006, page 056108-4)

The overall sense is that 'opinion formation' is a loose motivation for building an interesting model, and that the authors are not especially concerned with the sociological, political, or cultural questions surrounding opinion formation. Such ambivalence about how well the model represents the phenomenon of interest is emblematic of a broader practice of limited statistical testing and reliance on matching a static frequency distribution.

Data-driven: the search for empirical regularities in large data sets

For our data-driven example, we return to Bettencourt and West's (2010) research on urban economics, particularly as it relates to innovation and urban success or failure. This research deploys data analysis techniques to identify broad patterns of interest in extensive datasets in an inferential statistical mode. The authors compiled a large dataset on myriad features of cities (e.g., walking speeds, miles of electrical grids, patents filed) to develop insights into known aspects of growing cities, namely that they become denser in population and infrastructure and home to more complicated economies (Bettencourt et al. 2007). Matters become more interesting when these data are expressed as power law scaling equations (Bettencourt 2013) that point to a balance between positive effects of social interactions (e.g., economic activity, creativity as measured by patents) that support city vitality and negative impacts caused by increasing populations, such as congestion. In general, positive impacts outweigh negative ones because the latter grow more slowly, and these interactions map onto biological allometry, where animal metabolic rates and life spans scale differently with physical size.

This research exemplifies the tensions, challenges, and excitement inherent in data-driven work on social dynamics by physicists. The authors see it as the beginning of a "unified theory of urban living" defined by "integrated, quantitative, predictive, science-based understanding of the dynamics, growth and organization of cities" (Bettencourt and West 2010, 912). Arguably, the research is not particularly integrated, in that it ignores previous urban scholarship. But from a perspective where researchers find "urban theory to be a field without principles, comparing it to physics before Kepler pioneered the laws of planetary motion in the 17th century." (Lehrer 2010, 49), previous urban studies is *ad hoc*, ungoverned by generalizable theory, and dominated by a diversity of unsynthesized, observational studies. The argument is that Bettencourt and West's data and equations show that cities operate according to hidden laws only discernible this way. Bettencourt and West acknowledge that the specifics of any given city will vary from their lawful expected values due to "intangible qualities of social dynamics" (Bettencourt and West 2011, 52) and that geography, history, and context matter (Bettencourt 2013). Even so, the overall message is that their work is new and important because it introduces fundamental 'laws' of cities to which other considerations become secondary; we examine this argument more closely below with reference to work in geography (Batty 2013).

Commonalities

Both model-driven and data-driven work match a template introduced above, reframing 'big picture' questions in terms of replicating statistical features of the phenomena to either create, or support, a simple model that sidelines existing domain knowledge. While different in execution, model-driven and data-driven approaches to geographical research have much in common. Both rely on deployment of substantial computational resources and skills. More significantly, while each pursues understanding differently, they imply a similar perspective on the nature of reality: that it can be expected to be lawful. One way or another, the world can be expected to exhibit behavioral regularities; that these regularities can be uncovered is a foundational principle for much of science, but absolutely central to physics.[11] Model-driven inquiry considers how the world is structured, in terms of the interactions of its constituent parts and mechanisms, and investigates the relationship between those structures and possible outcomes. Data-driven inquiry is agnostic about details of system structures, but nevertheless assumes that high-level regularities can be expected and can be identified in data. Importantly, the two approaches often appear in tandem as in the first example, when an aggregate regularity is shown to be consistent with a simple model. Data-driven work, moving in the other direction, appeals to simple mechanisms (scaling laws) as strong candidate explanations for observed regularities. Armed with such high-level, general assumptions, the systems of interest to physics might as well be social as natural or physical, and the approach can be extended to any topic. While the apparently willful ignorance of physicists is galling for practitioners in the fields in question, for physicists it is *purposeful*: paying too much attention to the particulars is a distraction from whatever universal features might be uncovered, and it is universal, general features that are interesting, *not* the details.

In sum, both model-driven and data-driven work are potentially exciting, because they allow free trafficking of ideas between domains. While apparently extra-disciplinary research by physicists certainly has shortcomings it also has the potential to be innovative, perhaps particularly in the social sciences. Ignoring existing research and adopting a 'back to basics' approach may yield new insights, even as it reinvents old disciplinary wheels. Domain-free inquiry encapsulates what is good and bad about this work. Its strength lies in generalization drawing on methodologically sophisticated models and data sets. Its weakness is (an often deliberate) lack of attention to previous research that makes it vulnerable to overblown assertions of novelty, or even to claims at odds with existing knowledge. Importantly, physicists focus on *generalization* to a degree unusual among social scientists, perhaps especially among geographers concerned as they are with the local and particular.

4 Learning from physics

Whether such work in physics is 'good' or 'bad' is not our concern and we are not qualified to evaluate it *as physics*. We are more concerned with what geographers can learn from it, and how geography as a discipline can better place itself to respond. We believe these developments raise questions for geographers. What aspects of geography lend themselves to investigation with methods and concepts adapted from physics? How can geographers engage with such research: to limit its excesses, while learning from it, and also to demonstrate the value of geographical perspectives? Why are more geographers not doing this work, and how would geography be different if they did? Answering these questions requires considering what we can learn from physics in its approach to the social world, and assessing how geographers can engage with geographical work by non-geographers.

To address these questions, we first consider an example that serves as a warning of how uncritically received innovations from physics can be problematic. We then consider more promising examples where a willingness to engage with work by physicists has been productive. Drawing on this we then consider obstacles to more work of this kind, and propose ways to build on the successful examples.

What not to do: a cautionary tale (of the tails)

A feature of much of this work is the importance attached to heavy-tailed, right-skewed size distributions, power law distributions in particular. Such distributions are characterized by extremely large numbers of small instances of the phenomena of interest and very few larger instances. Heavily right-skewed distributions are commonplace. City sizes are a classic, well-known geographical example (Zipf 1949, Auerbach 1913), illustrated in Figure 3. Newman

(2005) notes that such distributions are to be expected when a limited resource (for cities, human population) is distributed among a collection of 'bins' (the cities) given that the bins range widely in size, from a few thousand to several million people (the data in Figure 3 are only for cities of one million or more). If 4 billion people worldwide live in cities, and the largest have populations around 20 million, while the smallest contain only (say) 10,000 people, then it is unsurprising that the distribution of city sizes is highly skewed, with a few very large cities and many small ones. More unexpected is that such distributions follow a roughly straight line when plotted on log-log axes, so that the probability of occurrence of cases of size x is approximated by a power law, $p(x) = Cx^{-a}$, where C and a are constants. Newman (2005, page 325) points to research on power law size distributions of earthquakes, solar flares, and wars, through to more cultural examples such as citation counts and music sales. Interest in statistical physics resides in the *scale-free* nature of power law distributions, which makes it meaningless to talk of a typical-sized individual, and in the fact that such distributions are often observed at *phase transitions*, when change occurs across a whole system, at all scales simultaneously. Thus, power law distributions have theoretical significance *in some branches of physics*, which helps explain why physicists seek power law relationships in data and in their models.

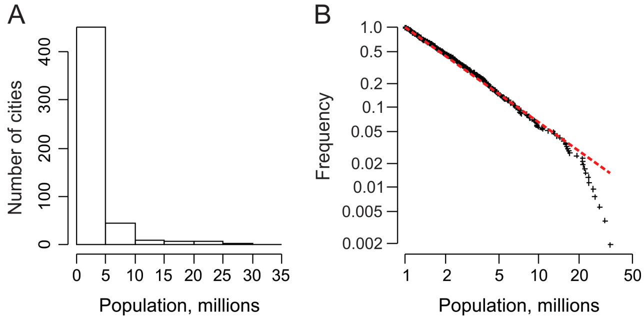

Figure 3 The distribution of city sizes. In A, a conventional histogram does not reveal much information because of the large numbers of relatively small cases (population below 5,000,000) and the small numbers of large cases. A power law fitted to these data is shown in B. Regardless of the validity of a power low distribution as a model for these data, structure in the data is more apparent on the log-log plot as a result of the nature of the distribution. Population data from Brinkhoff (2013)

Nevertheless, there are reasons to be cautious about recent work identifying power law distributions apparently everywhere. The first is practical: heavy-tailed distributions are difficult to tell apart. Clauset and others (2009) present a method for determining if a power law is a reasonable model for right-skewed data, and on re-analyzing Newman's (2005) examples and others, find the evidence for power laws to be inconclusive in most cases. Second, many mechanisms produce heavy-tailed distributions. Carroll (1982) provides a good overview in the context of city sizes, while Mitzenmacher (2004) adds yet more potential mechanisms. This suggests that heavy-tailed distributions may not be interesting after all—perhaps only a little more so than normal distributions which scientists have long since taken in their stride (see Stumpf and Porter 2012). Even so, the search for power laws continues, evidenced by dozens of papers in journals such as *Science* and *Nature* in recent years.[12] Third, aside from methodological issues, even when a power law has been identified, the theoretical implications are unclear in many research contexts, unlike in physics where, as we have seen, power law distributions may be diagnostic of systems at phase transitions.

Outside physics, it often appears that researchers look for power laws (and find them), because they have significance *for theoretical physics*, not from the perspective of the discipline in question. Often, it would make more sense simply to note that the distribution is right-skewed and consider what that means substantively in the specific research context. It is hard to avoid the conclusion that a fixation on power laws derives from contexts in physics where power laws *do* have meaning, and that the reasons to search for power law distributions in other fields are poorly understood, if they are considered at all.

Looking more closely at an example where power law distributions dominated research efforts for a period demonstrates why caution is called for. This is an unusual example, in that power law distributions were not only putatively identified, but a theoretical explanation was posited, that went beyond ill-defined analogies with physics. The field in question is movement ecology, closely allied to biogeography, where a substantial literature on the movement of animals in their environments has recently emerged (Codling, Plank and Benhamou 2008; and Nathan et al. 2008 provide overviews). Early work focused on simple random walk models of animal movement (Pearson 1906; Wilkinson 1952; Morrison 1978), although over time more sophisticated models developed to account for directional persistence in empirical data (Patlak 1953; Kareiva and Shigesada 1983; Bovet and Benhamou 1988). Regardless of how complicated the mechanisms in such models become, they exhibit characteristic length scales (such as distance traveled during an activity) and are only suitable for understanding movement due to one behavioral process at a time, such as foraging on scales of (say) hundreds of meters or migration at larger scales. Distances between locations at regular time intervals ('step lengths') have probability distributions with well-defined mean values and the movement has a characteristic scale.

This changes dramatically if step lengths are distributed according to a power law, when random walks becomes 'Lévy flights'[13], where most steps are short range and localized, but very rare, much longer steps dominate movement. As a result movement no longer has a characteristic scale, but exhibits structure at all scales. This is seen in Figure 4. The walk in Figure 4A has equal-sized steps of 1.295 units, while Figure 4B has power law distributed steps with a mean length of 1.295 units. Both walks consist of 1000 steps, but the power law step length distribution makes much more rapid overall progress, due to rare long distance moves.

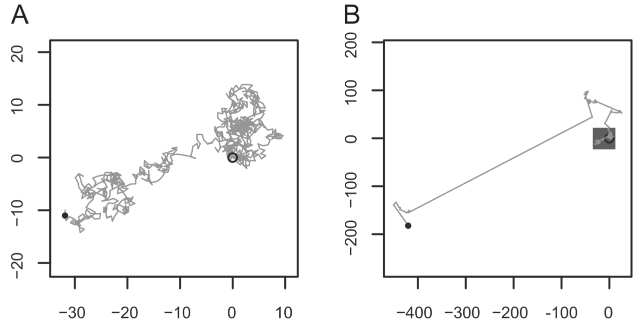

Figure 4 A, a simple random walk and B, a Lévy flight with power law distributed step lengths of the same mean length (1.295 units). The overall progress of the Lévy flight is much more rapid, and the path has structure at all scales. The small rectangle in B shows the simple random walk extent for direct comparison.

The suggestion that animals' movement patterns might be Lévy flights appears first in connection with the wandering albatross (Viswanathan et al. 1996). This was lent theoretical significance when it was shown that, depending on the distribution of food resources such movement patterns may represent an optimal foraging strategy (Viswanathan et al. 1999; Bartumeus et al. 2005). The statistical model might thus have theoretical meaning, and subsequently, many studies reported on Lévy flight movement in various species (see James, Plank, and Edwards 2011), notwithstanding the difficulty of explaining how such movement could work in practice. Much of this work was thrown into serious question when reanalysis of the albatross data show that step lengths were *not* power law distributed (Edwards et al. 2007). The relevance of Lévy flights to movement ecology remains in debate (James, Plank, and Edwards 2011; Viswanathan et al. 2011); as Buchanan (2008) notes the term 'Lévy-like movement' is often used to signal that while pure Lévy flights may be irrelevant, animal movement exhibits some characteristics of Lévy flights.

Three points are worth emphasizing. First, this 'misadventure' in power laws shows how research in an unrelated field such as animal ecology can be influenced by a statistical relationship whose significance derives from physics, even if there is no obvious basis for such a relationship in terrestrial scale biology. This is not to argue that Lévy flights are irrelevant to movement ecology, simply to note how strong the appeal of ideas borrowed from theoretical physics can be. Second, while some work may have given too-ready credence to the Lévy flight model, there have been benefits to ecology from this intervention, if only the considerable interest in movement ecology that it sparked, although it took physicists' attention for this elevation to high prominence in the general science literature. Third, ecology and biology are disciplines with stronger mathematical traditions than geography that have contributed in their own right to random walk theories. This meant that ecologists were able to engage with an idea from physics, and advance the field, even when initial excitement around a simple all-encompassing explanation for animal movement patterns proved ill-founded. No similar disciplinary tradition exists in geography to adapt the same or similar ideas to theories of human movement, which are already showing interest in the 'scale free' movement of Lévy walks (Brockmann, Hufnagel, and Geisel 2006), pollution exposure (Schlink and Ragas 2011), and hunting behavior (Raichlen et al. 2013).

What to do: seeing the forest and the trees

If unquestioningly importing methods from physics is problematic, how can geographers successfully engage with the enthusiasm of physicists for geographical questions? Engagement is important for several reasons. Geographers should be part of conversations about geographical topics among those outside the field (i.e., public, funding agencies, other scholars). There is clear cause for disciplinary angst if physicists are taken more seriously as geographical researchers than geographers themselves, even when insights from their research are underwhelming. Also, it is incumbent on geographers not merely to complain about physicists encroaching on 'our' territory, but to improve research through scientific discourse, especially where geographers can point to existing research. Finally, it is simply a good idea for geographers to continue to embrace a diversity of approaches.

We see three ways to engage with physics-oriented work. One is to connect with other fields while drawing on our strengths in spatial-chorological and human-environmental research. While many geographers are apt to argue about the definition of geography, these traditions are clearly within the remit of the field. Second, placing greater emphasis on quantitative analysis as a partner to qualitative approaches. This is particularly relevant for human geography, given its decreasing interest in quantitative work at a time when other disciplines are discovering geographical topics via these approaches. And third, is to become more comfortable with generalization as opposed to abstraction, particularly with respect to using data-driven and model-driven approaches to understand a host of systems.

First, we must engage with other fields and their approaches while drawing on our traditional strengths in spatial-chorological and human-environmental research. As Thrift (2002) notes of geography, there "are just too many other disciplines interested in its domain and they cannot be kept out [...] we have to look for a model based on respect for the quality of the work that a discipline produces" (page 295). This respect stems from hard-won geographic insight about space and the human-environment subject. Models from physics fall down in their overly abstract conceptualization of space. The earth surface as understood by geographers is a very different space from the spaces with which many other scientists work. Integrated modeling efforts are one way of constructing knowledge of global change, for example, but often reduce human dimensions to caricatures of socioeconomic and demographic driving forces or institutional frameworks for policy, and therefore represent a prime opportunity for geographers (Clifford and Richards 2005). Many facets of physical geography already interface with physics in understanding earth systems, to the point of contesting notions such as equilibrium vs. nonequilibrium imported from physics and mathematics (Phillips 2004). There are growing connections to climate change modeling, long home to physicists, via integrated assessment models and other approaches designed to incorporate knowledge of biophysical and social systems (Evans et al. 2005, Malanson et al. 2014). Another example of how engaging work from physics can yield returns is in the rapidly growing field of spatial networks (see Barthélemy 2011 for a review of work largely in physics; and Levinson 2009 for examples in geography, transportation and regional science). The overlap with important themes in economic geography is apparent even if the latter regards the networks analyzed in quantitative work as too shallow in structure, again providing an opportunity for engagement (see Urry 2004). There is a clear need for more sophisticated analysis of the human world in these models and geographers should be playing their part, especially because many of the underlying ideas and metaphors are of interest to a wide spectrum of geographers. As a result, we are better positioned today to engage with physics than we were fifty years ago, because both geography and physics have changed. Geography's engagement with physics in the mid-twentieth century was focused on essentially Newtonian concepts – entities arrayed in space defined by social physics, gravity models, and laws of attraction and location. More recent topics and points of engagement are broader and focused on concepts that see entities and interrelationships via big data and complexity ideas ranging from chaos and fractals to networks and connectivity (Massey 1999; Batty and Longley 1994).

An in-depth example of a sustained engagement with physics that simultaneously draws on geographical strengths is the work of Michael Batty and collaborators (Batty and Longley 1994; Batty 2005a, 2008). It is telling that Batty is one of the few geographers whose work speaks to that of Bettencourt and West (Batty 2013). This work explores the application of ideas from the physical sciences and mathematics, like chaos, fractals, and complexity using tools and methods from the same sources, such as cellular automata and agent-based models. The strength of this research is that it does not lose sight of the forest for the trees: it uses a range of approaches to search for substantive issues genuinely interesting in the research domain, while drawing on the traditional geographical strengths of spatial-chorological and human-environmental research.

Batty's work is attuned to the literature in urban geography and planning while borrowing from physics. The only points of reference for Batty that are regularly cited by physicists in this area are Jacobs (1961) and Zipf (1949). Batty sees the former as a point of departure for a reorientation of work on urban modeling from top-down to bottom-up approaches. The latter provides a connection between bottom-up models of urban formation and regional and global patterns of city size. Batty shows how incorporating simple spatial effects into abstract growth models changes the size distributions and produces hierarchically organized settlement systems (Batty 2006a). Another example of how these ideas can be productively explored in the context of longstanding theories in urban studies is provided by Batty's 'rank clock' (2006b) a visualization tool emphasizing instability in rank-size urban hierarchies in historical data. This contrasts with much of the physics on rank-size (i.e., power law) distributions, which (perhaps unintentionally) tends to emphasize stability in outcomes. This work argues against mindless adoption of power laws because they are a totem of physics; rather, Batty is trying to understand how power laws arise in more plausible models of urban development.

Importantly, this work recognizes its own limitations in a way that work by physicists and others does not: "not one but many different approaches will always be required." (Batty 2005b, page 1393) While this comment refers particularly to different types of model representation, the argument can be extended to recognize that many different approaches to understanding the world are justified once we accept its complexity (Manson and O'Sullivan 2006). This suggests that while urban work may engage directly with approaches coming out of physics it can also accommodate a broader range of approaches from across geography.

Second, we need a renewed emphasis on quantitative analysis both in and of itself and as a way of reclaiming the human-environment subject and spatial-chorological approach. Writing of global change modeling, Pitman (2005) argues that since models often use descriptive storylines to ground models, human geographers should use simple narratives to contribute their understanding of the messy human side of things. However, he notes that the work of modeling falls to social scientists who use numbers, and engagement entails learning the languages of modeling, namely mathematics, statistics, probability, and programming. Perrings and others note that the biggest need in understanding climate impacts is a leap "in our capacity to model interactions between the socioeconomic system and the biophysical environment" (2011, page 1139). This would ensure that geographical knowledge is translated into models while dovetailing into the related goal of reinvigorating the quantitative side of the discipline.

In the context of big data, there are challenges in developing the training and institutions necessary to integrate across sciences and engineering to make sense of the data deluge. One bright spot is that geographers are embracing big data and spatial cyberinfrastructure (Wright and Wang 2011) that drive so much work in physics, while critically engaging with underlying precepts of this 'new quantitative revolution' (Wyly 2014a). Even so, many geographers continue to reject quantitative spatial analysis when other disciplines are advancing quantitative spatial research. Geography has had long-running conversations and arguments about the role of quantitative methods. While these debates have resulted in a richer, more nuanced understanding of the role of quantitative research, they have also left quantitative methods in geography somewhat embattled. Some foresee the death of quantitative geography, especially for human systems (Hamnett 2003; Shearmur 2010), while others point to an ongoing, if reduced, body of quantitative work with pockets of vitality (Johnston et al. 2003; Plummer and Sheppard 2001). To these instances we can add various détentes where self-identified quantifiers incorporate critiques of positivism and modernism (Poon 2005) and allow for a plurality of explanations grounded in different epistemologies (Sheppard 2004; Chrisman 2005; O'Sullivan 2006). We can point to the geographic information science enterprise as evidence that quantitative geography is alive and well in that people use geographic information systems to understand myriad issues, but many researchers only use GIS software in the simplest of ways (Longley 2000). A realistic assessment of where spatial analysis and advanced GIS are used must recognize the role of other fields, particularly ecology, economics, biostatistics, and computer science, and the perhaps uncomfortable truth that geography has "never been central to the development of GIS" (Longley 2000, page 39).

Overall, while the state of quantitative methods in geography remains the subject of debate (see Johnston et al. 2014), it is clear that many geographers have moved away from conducting quantitative research precisely when other fields are using quantitative approaches to move toward geographical topics. As Poon argues, "…paradoxically, just as quantitative geography is coming under increased fire, other disciplines have begun quantifying geographic phenomena, and stepping up their production and use of quantitative methods in areas where some geographers have abdicated in favor of more qualitative approaches." (2003, page 753) Kitchin (2013) similarly argues that, "…it is fair to say we are largely underprepared for the era of big data beyond a handful of scholars and centres" (page 264). A growing number of researchers across disciplines are calling for more use of such spatial concepts as physical location, varying definitions of neighborhood, and place-based social networks (Sampson, Morenoff, and Gannon-Rowley 2002; Entwisle 2007; Matthews 2008). This trend reflects broader scientific interest in spatial data provided by technologies such as GPS or satellite-based imagery, even where these spatial data are treated as just another form of data that can be wedded to generic computational tools for analysis of social systems (Watts 2007; Shneiderman 2008; Lazer et al. 2009). Incorporating the effects of location, distance, and spatial relationships is thus remaking wider understandings of human and environmental processes (Unwin 2005).

To be clear, we are not arguing for quantitative research *over* other approaches, but that many geographers may have abandoned or de-emphasized quantitative work at the wrong moment, leaving geographers less well placed to communicate with other disciplines. Too many geographic researchers incorrectly conflate computational methods with narrow epistemologies or see them as antithetical to critical research (Sheppard 2001b) when quantitative work has a long-standing role in emancipatory research and practice (Plummer and Sheppard 2001; Johnston 2006; Barnes 2009). Qualitative approaches are and will remain important as a free-standing mode of inquiry as well as a vital companion to quantitative methods. Critiques of quantitative approaches such as spatial statistics and GIS have had a positive impact on their application and have opened up many new and constructive forms of engagement and research. Ultimately, a quantitative resurgence is not incompatible with qualitative work, and the time may be right for "hybrid geographies" that combine powerful approaches and may be one of the few ways for geography to retain its coherence in the face of ongoing rifts among subfields (Kwan 2004). Thrift (2002) suggests that large-scale computing holds much promise for human geography, such as in the analysis of video or its deployment in ethnographic fieldwork, and we can also point to work in land change science that adopts a fruitful mixed-method approach (Lambin et al. 2003; Turner et al. 2007). More broadly, a growing number of geographers see that it is necessary to move beyond facile dualisms such as quantitative vs. qualitative and positivist vs. post-positivist to recognize how statistical and mathematical approaches can be used for critical inquiry and practice (Sheppard 2001b; Barnes 2009; Wyly 2009). Indeed, it seems ever more apparent that numeracy is essential to fostering social change and "deciphering and challenging regressive political agendas, now often supported by numbers and quantitative analysis" (Kwan and Schwanen 2009).

Third, we need to become comfortable with generalization while preserving hard-won epistemological lessons on the nature and value of theory and embracing our expertise in understanding particular places. We are not calling for a renewed focus on simple models and on quantification and large datasets as a mere aping of methods from other fields. That would be to advocate a retrograde physics envy, a 'theory-lite' approach to geography. Hart (1982) cautioned against the siren call of scientism, the trappings of science without the practice of science, a message echoed by Massey (1999, page 264), who warns against a "reverential reverence" for physics. Much big data-driven work in physics and allied fields is the vanguard of a new iteration of purely inductive work embracing the idea that hypotheses and theories are unnecessary and implying that much existing research is unnecessary. Readily available data, it is suggested, allow researchers outside the social sciences to predict human behavior without reference to personal and social characteristics foundational to explanation in the social realm (Anderson 2008). The corollary is that this work needs no input from social science theory and methods (Savage and Burrows 2007).

Renewing an interest in meaningful generalization is not to deify supercomputers and data mining algorithms and an illusory 'theory-free' development of insights. Instead, geographers must appreciate how other fields go about seeking generalities via statistical and mathematical approaches applied to large quantitative datasets. Here it is helpful to distinguish between generalization and abstraction. Geography has no difficulty with conceptual abstraction *per se* or so it would seem from the rapid turnover in geographical theory—Marxist, post-Fordist, postmodern, post-structuralist, phenomenological, non-representational, post-humanist — and a range of attendant abstractions such as site, scale, intersectionality, and performativity (Gregson and Rose 2000; Nash 2000; Thrift 2007). This work is important, but much of it validates abstract concepts via highly specific accounts of near-atomic social events (see, for example, Laurier and Philo 2006). Further, the tools of generalization used in many fields – statistics and mathematics as applied to large empirical datasets – are minimized to the point of being denigrated and dismissed as unsuited to theory-building (Hamnett 2003; Kwan 2002). As a result, geographers struggle in the conceptual domain not with abstraction but with generalization defined as identifying *based on broad-scale observations* phenomena that are interesting enough to both merit investigation and lead to interesting broad conclusions. As Turner (2002) laments, our disciplinary disposition towards sweeping generalization is increasingly one of distrust. While such work necessarily invites criticism, as must any idea that emphasizes broad brush strokes over finer details, the tenor of critique is too often dismissal of ideas that do not privilege singular moments in space or time. From this follows the question:

> "Why does geography repeatedly abdicate powerful ideas developed or nurtured within its ranks, abandoning them for rediscovery and reinvention by other fields of inquiry? Why are these reinventions, despite our labeling them simplistic and even erroneous, taken seriously by the academy and public at large, reifying the importance of the powerful disciplines of rediscovery?" (Turner 2002, page 428)

There is a heady promise in work by physicists, an audacity in the sweeping formulations of Bettencourt and West, for example, that is at odds with the careful, nuanced and specific focus of much urban work in geography, despite calls for better understanding and models of the city (Batty 2008, 2013). Geography does not lack for big ideas, although it is unusual for them to be expressed in general terms drawing on substantial empirical data, rather than on theory. A notion such as the 'world city' (Sassen 1991; Taylor 2004) is a concept whose origins lies in characterizing a general global urban 'landscape' and pointing to interesting features that demand explanation. A more localized example in the same vein is Li's ethnoburb (1998). Another, more abstract, theoretical example is Harvey's 'spatial fix' (1978, 1982) a concept with considerable purchase that can only be made more concrete by empirically testing its validity, notwithstanding the difficulties (see Christophers 2011, who notes the paucity of attempts at empirically validation). Any of these big ideas would be strengthened by more extensive quantitative empirical evidence. Finally, we can look to the success of land-change science in harnessing multiple approaches to create generalized knowledge, ranging from cross-site comparisons (Rindfuss et al. 2004), using quantitative approaches such as remotes sensing (DeFries et al. 1999), and meta-analysis (Lambin et al. 2003), all within an international scientific framework designed to produce generalizable knowledge (Turner et al. 2007), and drawing on complexity theories that motivate so much research in physics and other natural sciences (Crawford et al. 2007). These examples also demonstrate the importance of such general concepts to the direction of a discipline. Ultimately, such keystone concepts *must* emerge from general models—we can only 'see' such concepts in the context of a general landscape whose overall shape we understand.

5 Conclusion

While there has demonstrably been more work in recent years by physicists and other generalist mathematical scientists on geographical topics, it is unlikely that it stems from 'geography envy' as our title suggests. A more likely explanation is the increasing datafication of social life (see Wyly 2014a), a development that renders geographical topics more amenable to investigation using methods from other domains (Lazer et al. 2009). Whatever the reason, it is incumbent on geographers to consider how to respond to these developments, and in doing so to avoid narrow-minded, or worse, high-minded defensiveness. Geography can draw on a rich tradition in understanding how place and space matter in understanding human-environment systems. That tradition encompasses not only a contemporary diversity of approaches from ethnography to large-scale geographic data analysis, but the discipline's earlier experiences with optimistic, highly generalized statistics and abstract models. While that salutary experience may prompt frustrated annoyance with similar exercises by hubristic 'outsiders', a more reflective reaction is to revisit what we have lost in our discipline in moving 'beyond' such approaches. Looking back to our example of physicists 'solving' the city, we must *be able* to do more than roll our eyes in exasperation and argue that the findings reflect accepted ideas built over decades of geographical research. Geography needs more researchers willing to engage work of this kind, to demonstrate what that work lacks even in its own terms, and (crucially) capable of taking it further in an interdisciplinary manner. Such a forward-looking response requires that the discipline foster stronger technical and analytical skills in its research training. Suitably equipped scholars might then set about demonstrating how fully attending to geography enhances our understanding of a world much more complex than even physicists can imagine. Perhaps then they *really* would be envious.

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Notes

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[1] The analysis is also weakened by its reliance on extensive urban totals, rather than per capita measures (Shalizi 2011).

[2] To be fair, it is not clear that Stephen Wolfram is a physicist or even a scientist.

[3] One anonymous reviewer suggested that 'disciplinary imperialism' is a more appropriate term than our geography envy coinage, and that physicists are not the first nor likely to be the last to act this way. We agree that disciplinary imperialism is something all fields engage in from time to time, but prefer to stick with our term, for its symmetry and focus on geography, which leads to our questions about how geographers might respond, rather than to a less useful 'pesky physicists' reaction.

[4] One reviewer takes us to task for not offering a more extensive discussion of the different assumptions that physics and geography bring to bear on the materials they examine. A systematic consideration of that contrast would greatly extend this article. Our argument is that while the nomothetic law-seeking model of explanation attributed to classical physics is clearly inadequate as an approach to geography, there are aspects of it from which geographers can surely learn, without abandoning a commitment to diverse approaches. At the same time physicists could surely learn something from idiographic, narrative modes of explanation in geography, and it is only constructive conversation that offers any chance of that outcome.

[5] While we doubt he would make any claim to writing canonical history, Trevor Barnes's work over the last decade or so provides much of the back story to the most familiar geographical encounter with physics during the quantitative revolution. See, for example, Barnes (2003, 2004, 2008).

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[6] While anecdotal evidence only, we have also found considerable agreement among colleagues in informal discussions of recent trends in the literature that these kinds of interventions by physicists in geography (and other) literatures are increasing.

[7] It is difficult not to see a parallel between Stewart looking beyond the academy and current enthusiasm for big data, much of it driven by business and the media (see Wilson and Barnes 2014).

[8] Weaver, as a leading player in the post-war science funding system in the United States was well-placed to make prophetic predictions: he was at the same time making funding decisions critical to those predictions!

[9] All this work is an extension, elaboration, and combination of cybernetics (Wiener 1948) and general systems theory (Von Bertalanffy 1950), long of interest to physical geographers (Chorley and Kennedy 1971).

[10] This claim is made with reference to work by Zanette and Manrubia (2001) which relies on rather limited internet databases.

[11] It is striking that this expectation remains immune to insights from complexity science—from within physics itself!—that suggest the importance of contingency, history and geography, even when the research tools are 'bottom-up' models, the very tools that yielded those insights. Understanding why physics in particular and science more generally is so resistant to contextual, historical, and geographical modes of explanation, is a much larger challenge than we can tackle here.

[12] For example, there were over one hundred papers in *Science* and *Nature* in 2010 and 2011 that invoke power laws.

[13] There is a fine distinction to be made between Lévy flights and Lévy walks (see Viswanathan et al. 2011) but it is unimportant for our present purpose, and the terms are frequently used interchangeably. The term 'flight' appears more common in the literature but it is unclear that it is used consistently to refer to models that would be considered flights by mathematical physicists. Intriguingly, Lévy flights found renewed prominence in physics in part via work on price change dynamics in finance by Benoit Mandelbrot (1977), according to Philip Mirowski (1989, 386-87)—a recent example of how concepts *can* move in both directions between the social and natural sciences.