Michael D. Ward (1948-2021) and the road to space, networks and geography

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We were very sad to learn of the passing of our mentor, friend, and collaborator Michael D. Ward on 9 July 2021. Mike made important contributions to political geography, and he served on the editorial board of *Political Geography* from 2002 to 2013 as well as the advisory board for the Center for Spatially Integrated Social Sciences at the University of California Santa Barbara. Above all, he played a key role in disseminating insights on the role of geography and spatial concepts and methods to his home discipline, political science.

In this forum, we have gathered scholars who worked with Mike at different times in his career to reflect on themes in his research and the enduring relevance of his contributions. Our introduction gives a brief account of how Mike's interest in geography and space evolved.

Political geography and space did not play a prominent role in Mike's formal training in political science. His doctoral dissertation on the political economy of inequality, later published as Ward (1978), highlighted international factors over and beyond country-specific factors, but not geography or space per se. Unusually for a political scientist, Mike developed an interest in formal dynamic modeling and multilateral interconnectedness, using techniques such as computer simulations and differential equations (e.g., Ward & Guetzkow, 1979; Ward, 1984).

After three years at the Science Center in Berlin, Mike moved to the University of Colorado in 1982, and his first foray into geography arose through the interdisciplinary environment at the Institute of Behavioral Science (IBS). In an autobiographical account of the earliest years of his "space odyssey" (Ward, 1999), Mike attributes his interest "in the importance of spatial analysis" to "having a geographer move in across the hall," a reference to the geographer Andrew Kirby. He credits Kirby with drawing his attention to an article on spatial analysis of interstate conflicts in the *Annals Association of American Geographers*, arguing that spatial proximity appeared to trump all other variables in importance in accounting for outbreaks (published by one of us, O'Loughlin 1986). Ward & Kirby (1987) published a comment, noting limitations in the development of the precise mechanisms underlying spatial patterns, the potential endogenous nature of borders to conflict, as well as the need to rethink connectivity specifications to distinguish borders as conduits for conflict from more general effects at proximity. The comment shows a deep understanding of existing work in geography and spatial statistics, which Mike could relate to his background in dynamic models and formal models of global linkages and relationships that until that point did not consider geographic elements like distance or contiguity.

At a time when the Cold War was coming to an end, yet the discipline of international relations (IR) retained a strong state-centric and bilateral focus, Mike's interest in how geography and spatial approaches could contribute to IR and conflict research was energized. He edited a special issue of *International Interactions* on "The New Geopolitics" (later published as Ward, 1992), and his introduction argued in favor of a less state-focused approach to IR, emphasizing how geography and proximity shapes incentives and constraints.

At a time of great interest in the changes across the world political map consequent to the end of the Soviet Union, political change in Latin America, Africa and Asia, and an increasing ratio of civil to interstate wars, O'Loughlin and Ward won funding in 1995 from the National Science Foundation (NSF) for an interdisciplinary graduate training program on globalization and democracy based in the Institute of Behavioral Science, which emphasized the linkages between economic and political change. The NSF also funded a project on "the diffusion of democracy", highlighting the evident spatial-temporal dynamics. An early descriptive article on spatial clustering and diffusion in democratization was published as O'Loughlin et al. (1998), and motivated subsequent articles on why and how democracy may diffuse, including Gleditsch & Ward (2006). This project also fostered new data development on historically accurate maps, how to conceptualize distance in IR, what constitutes an independent state, as well as the conceptual bases for measures of democracy (e.g., Gleditsch & Ward, 1997, 1999, 2001).

Reveling in the interdisciplinary environment of IBS working with students from all the social sciences, Mike applied his growing interests in geography and spatial analysis to a wide range of topics and issues and made important methodological developments. He revised his earlier work on models of defense expenditures to examine the potential influence and contributions of spatial analysis (see Shin & Ward, 1999). When he moved to the University of Washington in 1997 and became part of another interdisciplinary group in statistics, space remained prominent in his work on dependencies and latent networks in international interactions (e.g., Ward et al., 2007), as well as his effort to develop models for conflict prediction (e.g., Ward et al., 2013). He made contributions to spatial methods for models with binary or categorical outcomes and wrote a popular monograph on spatial regression models that helped popularize the use of spatial

techniques in the social sciences outside geography (e.g., Ward & Gleditsch, 2008, 2018). He continued to work on these topics and methods after moving to Duke University in 2009, where he established the highly productive WardLab.

Before 2000, most political scientists would have assumed that spatial analysis must refer to models of preferences as spatial positions, but in 2022 spatial dependence is widely recognized in the social sciences, and there is increasing interest in identifying how spatial characteristics influence outcomes or can improve inference. Mike's work emerged at the right time, and some of these developments may well have happened without him. Yet his specific contributions leave important legacies and have shaped the specific manner in which research interest in how spatial relationships has evolved and developed.

Mike's interest in geography and space reflects an enduring intellectual curiosity. He loved discussions about new approaches or problems with existing models and conclusions. He was dedicated to the notion that any path worth pursuing could only be evaluated on the basis of evidence. For him, evidence was data, and he pushed students to seek out existing datasets, replicate and extend previous studies, or develop new techniques to show in a graphical, statistical or cartographical way the results of such enquiry. He was always willing to explore new topics and ideas, even if adding marginal extensions to previous work could have more immediate returns. His orientation was highly interdisciplinary, working extensively with colleagues and scholars from outside political science, including fields such as computer science, geography, and statistics. Mike also had a profound focus on collaborative research, and he built strong networks and working groups throughout his career. Mike believed in integrating scholars and students at

many different stages of their careers, and his extensive co-authored scholarship demonstrates how we can learn more as members of a teams than as individuals. He showed an exceptional generosity and dedication to students and collaborators, taking a real interest in their work, and making constructive suggestions for how to improve it.

Among the contributions to this forum, O'Loughlin, Witmer, & Linke (2022) examine how Mike adapted geographical concepts, yet applied these in distinctive ways. Shin (2022) contrasts Mike's work influence in disseminating political geography beyond the subfield and his differences from dominant views in the sub-discipline. Ahlquist, Beramendi, & Wibbels (2022) examine how Mike's work and spatial ideas has influenced research in political economy. Dorff & Minhas (2022) consider Mike's work as a mentor and the resulting collaborative networks. Gleditsch & Weidmann (2022) discuss his work on prediction and his claim that focusing on predictive ability is more useful than investing in theory development.

We remember with heartfelt thanks Mike's impact on our careers as a former student (Gleditsch) and research collaborator (O'Loughlin), but also as a close friend. We have only partly summarized his extensive list of papers and books in political geography in this forum. As emphasized in the Shin essay here and reviewed elsewhere by O'Loughlin (2018), Mike Ward was advocating the traditional approaches of human geographers to understanding political behavior at a time when the sub-discipline of political geography was being exposed to and adapting theories and methods from feminist, anthropological, cultural and psychological studies. Ironically, his corpus of work resonates more in the discipline in which he was promoting traditional geographic

thinking than in the discipline from which it originated as political geography is now dominated by non-quantitative, non- or hard -to- replicate, and more activist work.

Mike humorously disparaged claims without evidence as "arm-waving" and always reminded speakers to leave a clear message for the audience, asking, "What tune will they hum when they leave the room?" His slightly-tilted head with that wry, quizzical hint of a smile when a colleague was trying to convince him about some scholarly point or some student trying to answer a question that seemed easy at first pass but required a much more complex answer remains alive to us even though he is gone. We hope that the readers of this forum will hum a tune that recognizes an individual who left his indelible mark on the quantitative study of conflict through his interdisciplinary commitment to learning unfamiliar methods and pushing new ways of thinking across multiple domains. Few can accomplish what he achieved.

Michael D. Ward - Finding Geography

John O'Loughlin, University of Colorado Boulder

Frank Witmer, University of Alaska, Anchorage

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When Mike Ward became aware of geographic approaches to political phenomena in 1986, he quickly appreciated how the two-dimensional geographic perspective brought both a) "statistical and mathematical" and b) "substantive" insights to his own discipline of political science (Ward 1999). In spreading the word about geographic insights, Mike reviewed descriptive methods for summarizing geographic distributions such as Moran's I and Local Indicators of Spatial Association (LISA) (Anselin, 1995) and demonstrated how the common statistical models, by ignoring dependence in the data structure that is based on geographic units, can lead to erroneous conclusions. He summarized the more "substantive" appeal of a geographic viewpoint as capturing "the extent of similarities and differences among the units of analysis that might be attributed to shared geographic context" (Ward, 1999, p. 8). But what should we do with this insight? Are the dependencies inherent to a "shared geographic context" a statistical nuisance, or can they lead us to important insights into what accounts for the temporal and spatial distributions of the political phenomenon of interest?

Two important considerations in probing the substantive power of geographic relations immediately crop up. As Mike noted in his "odyssey", the first concerns the proper distance metric for the subject of interest. He promoted relative distance considerations such as a metric according

to the strength of trading links. But though able to identify some of their limitations, Mike was quite traditional in his exploration of absolute measures (distance between capitals or centroids, and contiguity). It has been clear for a long time (e.g. Cliff & Ord, 1973) that the choice of weights is absolutely critical since incorrect or biased measures can lead to wrong conclusions about the significance of the spatial dependencies.

The second substantive consideration is more central to the way political geographers and political scientists differ on this question. Peter Gould (1970) was one of the first to get to the nub of the matter. Asking if we need to "throw out the baby with the bath-water", he drew attention to the divide between viewing spatial dependencies as a problem that should be modeled away versus honing in on *how* the dependencies themselves could be used for explanation. The disciplinary divide was first explicitly highlighted by O'Loughlin (2000) but was already becoming evident in the 1996 debate between John Agnew, a geographer, and Gary King, a political scientist, over how to view, measure and incorporate (or not) contextual elements in political behavior (Agnew, 1996; King, 1996).

One kind of "context" central to political geography that Mike wrestled with, but never fully examined, was the subnational one. Perhaps as a result of funding opportunities – countries are easily identifiable on a map to busy policymakers – and also likely influenced by norms in the fields of IR and comparative politics where questions of national-level politics dominate – the majority of Mike's research contributed to conflict and political economy research at the national level. (See below for the actor-network analysis exception.) Mike embraced the value of data and analysis innovations at subnational levels (Weidmann & Ward, 2010), and even analyzed

population survey data in collaborations with geographers (Bakke et al., 2009a; Bakke et al., 2009b; Bakke et al., 2014). For some questions, using country level data is perfectly appropriate, but this is not universally true. Of the country-year "context", Mike once lamented in public company about an International Studies Association 2013 conference presentation; "oh no, not another dreaded country-year study."

Subnational data, which measure the sorts of regional and local contexts geographers prioritize, are ideal in many cases, but scale questions are complicated. If one hopes to understand social forces and political processes behind conflict, for example, the unit of analysis is not necessarily obvious. Does a person act? Does a household act? Does a group act? Does a country act? They all do. Mike absorbed these insights about scale in social science research, adept as he was in embracing perspectives from outside political science.

The definition of "context", as much as how you observe it empirically, has been a point of debate in the arena where political science and geography intersect. In 2015 a special issue of *International Studies Review* was dedicated to "geography ("Spaces and Places: Geopolitics in an Era of Globalization"). Unfortunately, misunderstandings remain, as the disciplines talk past each other, political "geography" (the contextual) contra political "geometry" (the Cartesian) (Johnston 2019)

But this glass is half full if we consider a second type of context, the relational one, that Mike and his colleagues embraced and modeled quite effectively. Mike's prolific network analysis seems influenced - albeit in subtle ways - by priorities held by some geographers. A focus on actors -

countries joined together as alliances with common interests - was a reprieve from the country-year-contiguity dataset. The political science authors in this forum eloquently document Mike's contributions to network analysis in political science broadly and conflict studies specifically.

In political geography, network analysis is the means to an important end, characterizing *relational context* instead of territorial or *locational space* (where Mike's admirable contributions related to distance and contiguity stand out). "Context" or social setting from a perspective appreciating relational context is defined by interactions that may (or may not) be dominated by physical location on a map. His appreciation of this kind of geography was implicit, and often without naming geography as a discipline, his investigations of relational context are found in the top political science journals.

Calling one's research "interdisciplinary" is cheap. Mike appreciated work outside of his discipline in obvious but also subtle ways. There is no quick agreement about the kinds of research questions one should ask, the data one should use, or the models one should run. But Mike served on dissertation committees in geography with spatial-autoregressive terms and standard error adjustments by weights matrix w_{ij}. What interested Mike about such work? It might surprise some, but his general intellectual curiosities and appreciation for geography went beyond *exclusively* contiguity, distance, adjacency and adjusting for assumptions about spatial dependencies (see Bakke, et al., 2009a, b).

One common thread in Mike's work was his emphasis on and skill in visualizing data and results from the earliest years of his career. In his autobiographical essay he lamented the difficulties of

publishing color figures and maps (Ward, 1999). His cost calculations led him to conclude that a single color figure was worth a month's Research Assistant (RA) salary, and that the money was better spent on the RA. Fortunately, researchers no longer have to choose between color figures (at least for online pdfs) and RAs.

Three articles include "visual" (Greenhill et al., 2011, Ward & House, 1985) or "visualization" (Ward & Gleditsch 2005) in their titles, and many more include maps and plots as key communication devices. For Ward & House (1985) he created a 16mm video showing quarterly bilateral political interactions for 54 nation-states over 3 decades. He was clearly disappointed by the limits of the printed format in sharing this visualization, but that did not stop him from including dozens of frames from the video in his chapter.

Mike frequently mapped country-scale data at global scales such as democracy scores and their clustering (O'Loughlin et al., 2008, Ward & Gleditsch, 2005). He enjoyed challenging peoples' assumptions and *modi operandi*; one of the ways he did so was to present data in unexpected ways. An example is his use of cartograms, a well-known device in geography, to show the distribution of democratic and autocratic scores scaling countries by population (Gleditsch & Ward, 2006). In this way, human space, not geographic space, dominates the map

Beyond mapping, Mike also emphasized the importance of graphing statistical data. He contributed to the use of innovative 3D plots to show spatio-temporal relationships in democracy scores (O'Loughlin et al., 1998), and predictive power plots to de-emphasize p-values and evaluate models (Greenhill et al., 2011; Ward et al., 2010).

Though much of Mike's work has a spatial component, what was his influence on political geography? We can explore the influence via the Connected Papers web tool (https://www.connectedpapers.com/about). Connected Papers produces graphs of similar articles based on co-citation and bibliographic coupling, where two papers are more related the more overlapping citations they have. Related papers are then placed on a connected graph where the most similar papers cluster together, and less similar papers are scattered.

His first publication in 1987 with "spatial" in the title was with geographer, Andrew Kirby (Kirby & Ward, 1987). This work is an extension of previous work by Starr & Most (1983) and focuses on the role of spatial autocorrelation in studying peace and war in Africa. In addition to Starr and Most, this work is also connected to IR scholars Siverson and Starr (1990) and Faber et al. (1984), and geographers O'Loughlin (1986) and Anselin & O'Loughlin (1990). A decade later, Mike published a widely cited paper on the diffusion of democracy in the *Annals of the AAG* (O'Loughlin et al. 1998). Even if published in a geography journal and with a geographer as the first author, Figure 1 shows that subsequent (darker green nodes) related work was from IR scholars such as Brinks & Coppedge (2006) and Starr & Lindborg (2003).
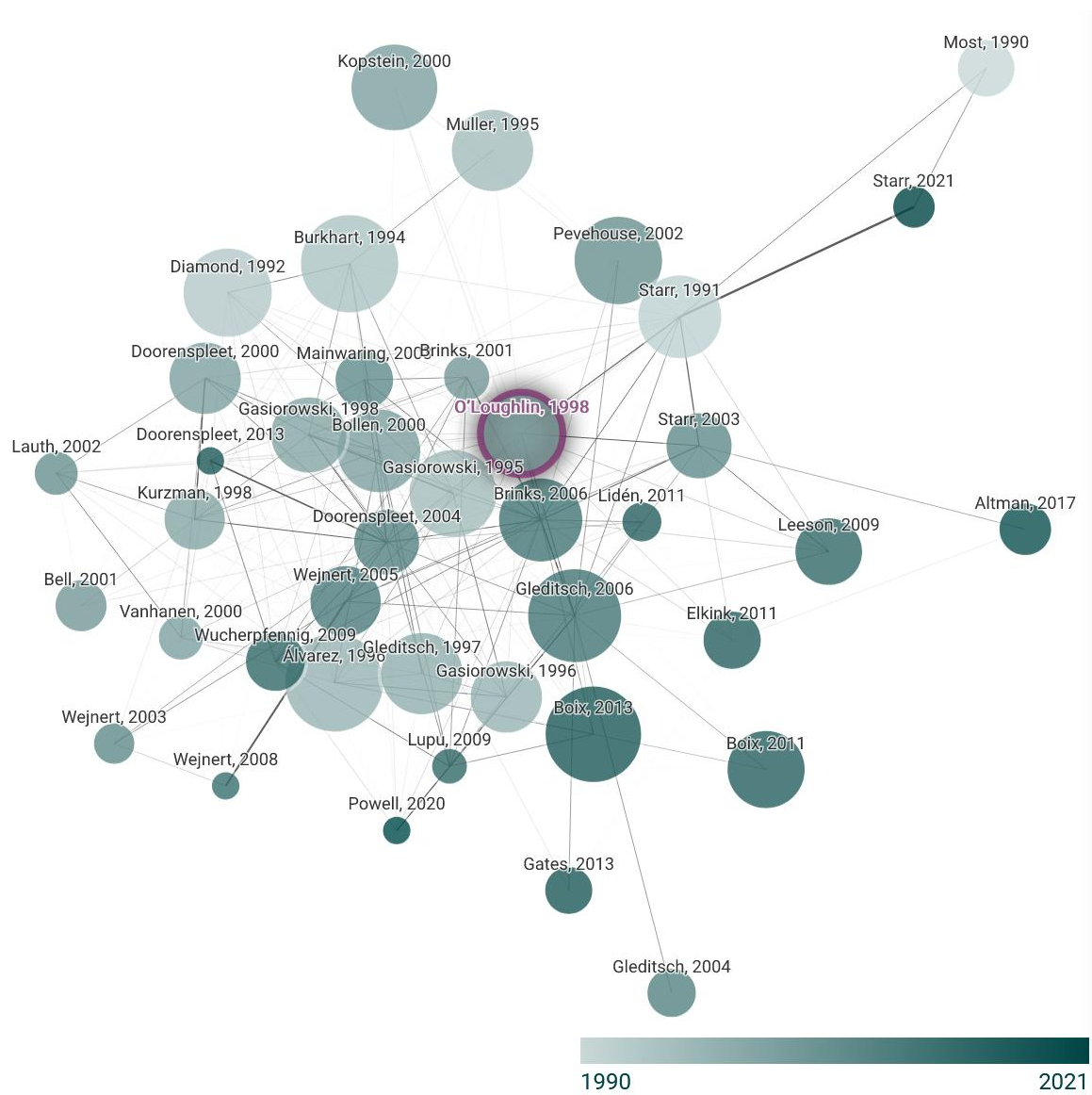
Figure 1. Screen capture from Connected Papers for O'Loughlin et al. (1998). Similar papers have strong connecting lines and cluster together. Color indicates year, size the number of citations.
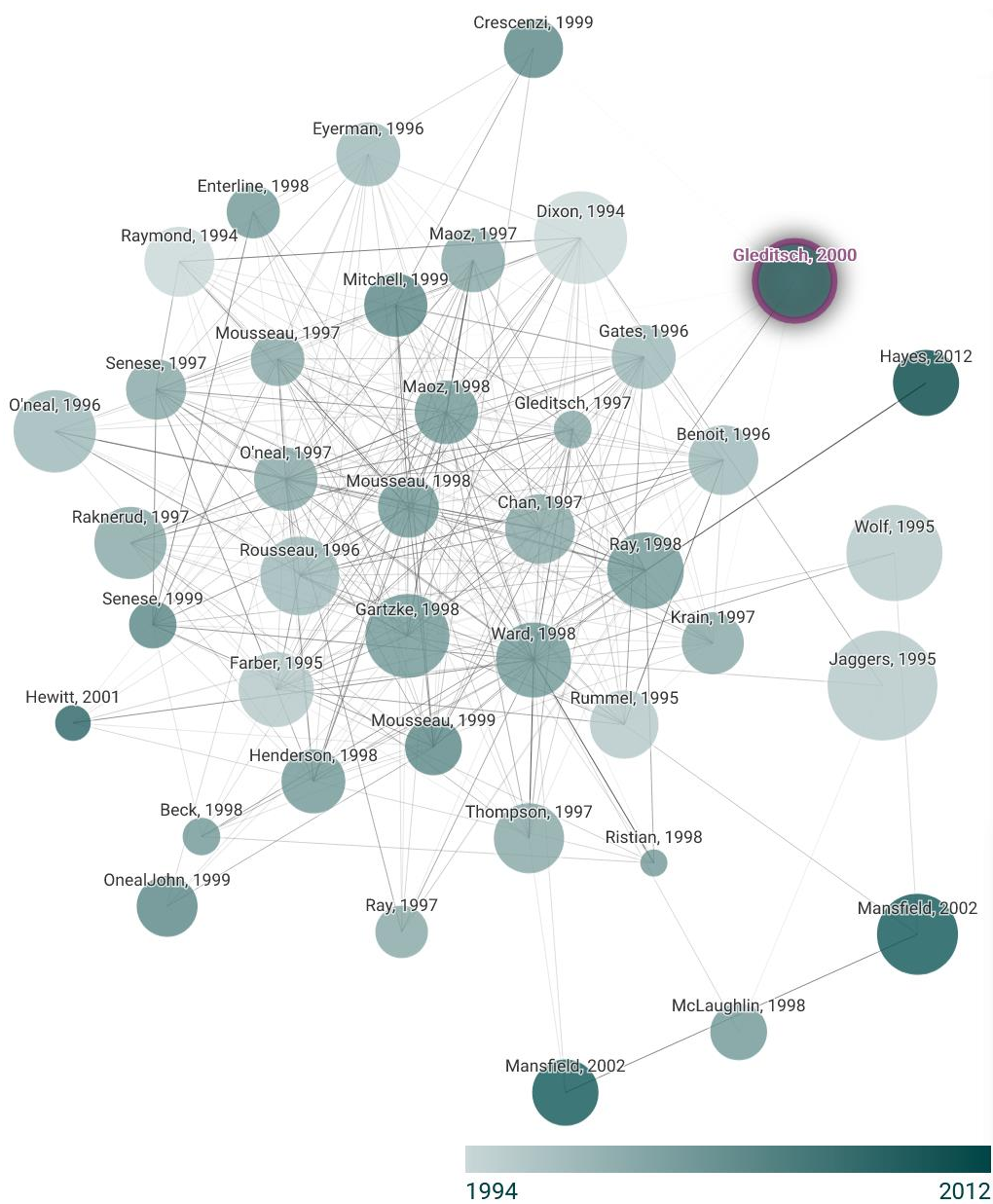
Figure 2. Screen capture from Connected Papers showing Gleditsch & Ward (2000) standing apart from connected articles.

Figure 1 is a typical Connected Papers graphs, with the article of interest is centrally located and a cluster of similar papers surrounding it. Several of Mike's works do not fit this pattern. In Figure

2, Gleditsch & Ward (2000) is pushed to the side and only loosely connected, with Ray (1998) and Hayes (2012) from IR as the most closely connected. This perhaps reflects how his work is generally more closely connected to political science, but set apart by the unique spatial approach.

As others in this forum and beyond have remembered, Mike Ward was a towering presence in their intellectual journeys as a mentor and a colleague. His brilliant analytical mind was matched by a curiosity that knew few limits in either the social science academic world or the scientifically literate one. Though intimidating to some students and colleagues, once past the initial scrutiny, he became a friend for life, a full-on generous supporter and a sender of cryptic messages that required a deep consideration of the topic and demanded an adequate reply.

Spatial outlier: Mike Ward, political geography, and political geography beyond geography

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> *spatial outlier (noun)*: an observation well outside of the expected range of values in a region or study area surrounded by dissimilar values.

To say that Mike Ward was a "numbers guy" is an understatement and mischaracterization. Mike was an expert in political methodology, statistical computing, and spatial econometrics. His methodological expertise and innovations were applied to a range of topics from defense economics to democratization to conflict to prediction. A political methodologist *par excellence,* his greatest talents were his ability to drive research through meaningful collaborations, to connect people together from different disciplines and backgrounds, and to recognize that methodology is an effective means to an end.

Among his noteworthy collaborations, connections and methodological proclivities were those with geography, geographers, and in particular, political geographers. Firmly grounded within the quantitative IR community, Mike liked new technologies, new software, and new approaches to solving questions in IR, and geography offered all the above. Mike's engagement with geography dovetailed with the initial development and diffusion of geographic information systems (GIS), spatial analysis, and statistical computing beginning in the late 1980s throughout the 2000s (see Ward & Kirby, 1987; O'Loughlin et al., 1998; Ward & O'Loughlin, 2002; Gleditsch & Ward, 2008). Mike's intellectual curiosity and methodological expertise really knew no bounds, as shown

in his recent work in multiple imputation (Hollenbach et al., 2018) and conflict prediction (e.g., Beger et al., 2021).

Geography offered Mike novel approaches and new tools that few political scientists were using at the time. As a quantitative researcher studying interstate conflict and interdependence, Mike was easily able to understand how and why the 'spatial is special'. The presumption of statistical independence, and the idea of complete spatial randomness (CSR), is a tacit absurdity to most IR scholars, geographers, and statisticians. Despite widespread recognition of the substantive and statistical significance of geography, many IR scholars remain caught in John Agnew's (1994) 'territorial trap' where geography is reduced, simplified and considered more of a nuisance instead of a meaningful construct.

Mike's own views about whether geography is substantive rather than a methodological conundrum were more muted. At most, he was lukewarmly sympathetic to contextualized geographic explanations. Despite this limited conception of geography, what really distinguished Mike were his efforts and ability to promote, advocate, and evangelize political geography *beyond* geography. These efforts were neither trivial nor unnoticed except (ironically) by political geographers. To shed light on and to recognize Mike's noteworthy geographical contributions, what follows is a brief bibliometric analysis focused on his political geographic research and publication record largely beyond the subfield of political geography.

Political geographer in the closet or political geography in the closet?

Some of Mike Ward's most successful and notable collaborations can be attributed to the fact that he was *not* a geographer. This afforded him certain angles and advantages and professional networks that geographers who seek to engage with other allied fields sometimes lack. A simple citation analysis shows that Mike's impact and influence are largely outside political geography.

Using complementary databases, specifically Web of Science (https://www.clarivate.com) and Google Scholar (https://scholar.google.com), Table 1 reports articles, citations, and h-index with Mike as author.

**Table 1.** Bibliometrics for Mike Ward, 1985 – present (Web of Science and Google Scholar)

Author: Michael D. Ward	Web of Science (since 1985)	Google Scholar (since 1987)
Articles in database	24	238
Citations	1,470	12,035
h index	13	51

Three aspects to Mike's publication record are especially relevant to political geography. Geography, space, and geographic concepts are central to Mike's most cited and recognized works. Of his most cited works, it is easier to identify works not explicitly geographic (e.g., Ward & Davis 1992; Montgomery et al 2012) as most are (e.g., Gleditsch & Ward 2001; 2008). Mike tackled inherently fundamental geographic topics ranging from spatial diffusion (e.g., O'Loughlin et al. 1998; Gleditsch & Ward 2006), spatial dependence (e.g., Gleditsch & Ward 2000; Hoff & Ward 2004), and even the gravity model (e.g, Ward & Hoff 2007).

Some may argue that Mike's use and conception of geography was rather simplistic, but he convinced many that geography matters. For instance, of the 1,470 citations in Web of Science, titles or abstracts contained this number of occurrences for the geographic terms: space (53); distance (34); territory (32); proximity (31); border (29); geography (29); location (24); contiguity (13); international diffusion (11),interdependence (42); and dependency (27).

A second aspect is where Mike published and which journals cite his work. Though he conducted research in political geography and collaborated with geographers, Mike published primarily in political science. Figure 1 shows the relatedness of articles in journals that cite Mike's work based on the number of times they cite each other. Larger circles and more connections (i.e., arcs) indicate more citations and inter-journal relations. Similarly colored connections indicate common topics as detected by the citation cluster algorithm (see: https://vosviewer.com). Most connections and citations are between journals in political science and, in particular, IR. The inset to Figure 1 highlights Mike's citation network for the journal *Political Geography* (in cyan at the top of the inset image).

Figure 1. Journal citation network visualization for Mike Ward (https://vosviewer.com).
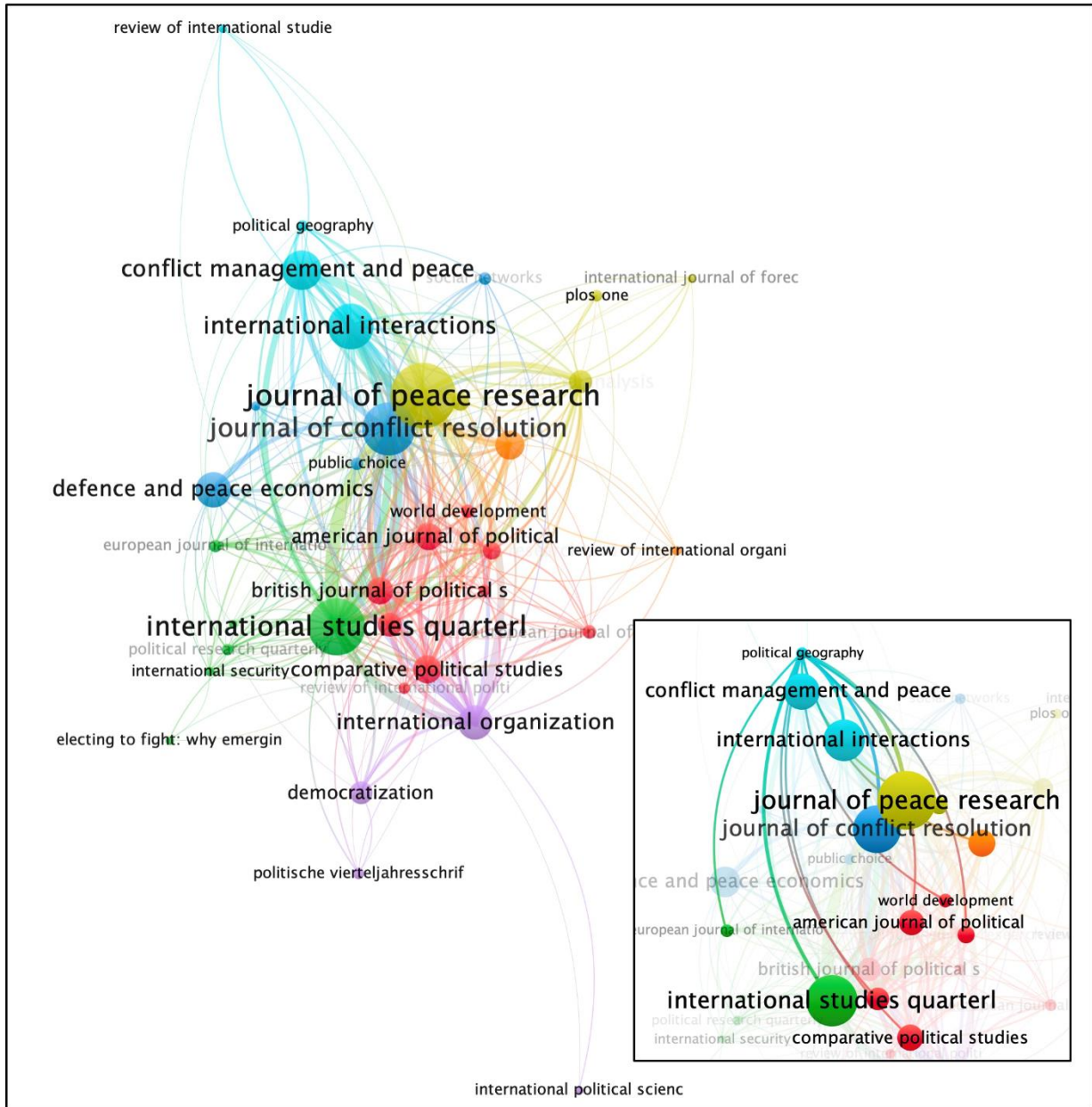
A third quality is that Mike's work was almost exclusively collaborative, with colleagues, postdocs, and students, spanning research centers, universities, departments, disciplines, and countries. Mike was an early adopter of the adage that 'two (or more) heads are better than one', and his publication record is remarkable for the number, variation in size, and different combinations of his collaborations. Mike's collaborations reveal his unlimited generosity with his time, knowledge, and encouragement.

To infinity and beyond

Mike was one of the most gifted, influential, and cited political geographers…beyond geography. Was his use and conception of geography limited? Yes, and I would argue that this was entirely by design. Given his interest in, and engagement with, political geographers over the years, it is more likely that Mike was being strategic with his work and academic output. He knew that more nuanced and thickly descriptive accounts of region and place would not be well received, let alone published, in political science or IR journals. However, a contiguity matrix, or better yet, a choropleth map of conflict clusters reveals a different story!

Despite the inherently geographic aspects of politics, contemporary political geography remains on the margins of social science and largely out of public view. As documented by O'Loughlin (2018), and lamented by Johnston (2019), political geography remains a fragmented affair that is ignored by scholars in allied fields, and its future remains in question, if not in serious doubt. Despite these well-known and persistent challenges, Mike recognized, appreciated, and promoted the potential of political geography and astutely carved out a niche for the subfield beyond geography. His efforts may represent the best way forward for a renewed political geography.

While political geography is largely mired in relativism, non-replicable ethnographies, and hyper-particularism, a renewed political geography that favors maps, data, and geospatial analytics is being created by many beyond geography. As visible after each election, events like Vladimir Putin's invasion of Ukraine, or crises like the COVID-19 pandemic, data mavens like Mike from any number of academic disciplines and non-academic backgrounds are making maps, sharing code, and posting compelling geographical analyses. Such efforts tend to be ignored and disregarded by political geographers who have (in their own minds) moved beyond, for instance, distance, contiguity, and even the map.

Herein lies the dilemma for political geography. There is little evidence to suggest that efforts to inform those beyond political geography about geographic concepts and constructs will be successful (Johnston, 2019). Yet failure to do so is equivalent to surrendering to reductionist tendencies, which reproduces the persistent, simplistic misconceptions about geography and geographers. It is debatable whether this truly matters, but the myopic relativism and pseudo-theory, or what Mike would call the "dog and pony show", running rampant through political geography creates opportunities for those beyond the subfield to redefine and claim it for themselves. In this vein, one of Mike's recent publications provocatively asks and provides thoughtful responses to the question, "Do we have too much theory in international relations, or do we need less?" (Ward, 2017). I cannot answer for IR, but I contend that, yes, there is too much theory in political geography, and we need less. We do however need more Mikes.

Political Geography in Political Economy: Enduring contributions from Michael D. Ward

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Erik Wibbels, University of Pennsylvania

In one of his wonderful books (Ward & Gleditsch, 2008, 2018), Mike Ward describes how German geographers writing before World War II, especially about the Third Reich's obsession with its own *Lebensraum*, all but derailed the study of geopolitics. Beginning in the 1980s, Mike joined emerging attempts to change this. His efforts reflected a deeply held view about the importance of geography in the social sciences, the need to ignore and cross artificial disciplinary boundaries, and a commitment to build and exploit new methodological tools rather than allow his interests to be constrained by existing practice.

For over four decades, Mike carried the banner in the discipline of political science for political geography, a crusade that was sometimes lonely. Fortunately, he persisted. Hidden beneath an unassuming demeanor and a sardonic smile was a titanium skepticism for any intellectual fad. He disliked "theories", especially as the concept bloated with ever-looser assertions about how the world works. And he was particularly critical of empirical research that treated interdependence as a nuisance, a dirty truth about the real world intruding on our pristine statistical models, one that must be confined, exorcized, or simply assumed away. He argued consistently, forcefully, and with methodological rigor that politics *is* spatial. This view led Mike to largely abandon theoretical approaches emphasizing *ceteris paribus* reasoning and statistical techniques that

assumed conditional independence in favor of empirical frameworks capable of incorporating space, distance, and interconnectivity through time.

Mike's long-term research agenda centered on the "Kantian peace." As such, his contributions (and sometimes his ire) were most keenly felt in IR and security studies. But his work continues to have an enduring impact in another wing of the discipline: comparative and international political economy. In this short essay we highlight his contributions in 1) the political economy of democratization, 2) international trade – both natural outgrowths of Ward's interest in the Kantian tripod – and 3) a broader set of methodological concerns for political economists.

Democracy & Democratization

Rewind to the late 1980s. The Eastern Bloc is showing cracks and the Soviet Union is about to crumble. After Samuel Huntington (1991), it becomes fashionable to talk of "waves" of democratization. Then along comes Adam Przeworski and colleagues' (2000) magisterial study of democracy around the world. Exploiting a trove of newly systematized data[1], their work puts a stop to talk of democratic "waves," reorienting the study of democracy and democratization toward largely domestic causes and correlates such as the distribution of income or assets across social groups.

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[1] Their data reflects earlier work from Ted Gurr's Polity project, on which Mike was a young research assistant. Later Mike would both criticize and attempt to partially automate the Polity regime coding process (Minhas, Ulfelder & Ward, 2015).

Mike could never be accused of harboring much love for grand social theories. Characteristically, his pathway into this literature was paved with data. One of Mike's early criticisms of Huntington's notion of "waves" focused on geographic correlation in political systems (O'Loughlin et al., 1998). But the sustained assault had several additional steps. First, he and long-time collaborator Kristian Gleditsch undertook the thankless task of producing a dataset that identified consistently all independent political units from 1816 onward (Gleditsch & Ward, 1999). He then calculated new, politically relevant measures of minimum geographic distance between country borders (Gleditsch & Ward, 2001). Both efforts produced important data repositories that remain widely used today. Finally, Mike turned to the thorny problem of incorporating important geographic and temporal connectivities into statistical tools, exploring a series of approaches (Gleditsch & Ward, 2000; Hoff & Ward, 2004; Ward & Gleditsch, 2002). All this work culminated in Mike's most-cited paper (Gleditsch & Ward, 2006), which shows that there *are* important geographic "neighborhood effects" in *both* democracy and democratization that the new comparative political economy largely elided. But these effects don't look like Huntington's waves. Rather they look like feedback and reciprocity among locally dense networks of polities. Further, geographic relationships contribute more to accurate predictions of democratization than domestic variables, undermining the hypothesis that moments of democratization are basically i.i.d. (independent and identically distributed) through time and across countries.

Mike's impact here is characteristically ambiguous. There is no theory of democracy bearing his name nor did he build *the* canonical empirical model that structured the subsequent literature. Nevertheless, Mike's work has stimulated a flowering of theoretical and empirical research, including some of ours (Ahlquist & Wibbels, 2012), which seeks to account for *both* the spatial

clustering in democracy and its domestic and international correlates. This work is ongoing. But because of Mike's work, naive cross-national regressions on democracy are no longer credible. Simple dummy variables for "region" or continent won't cut it; consideration of international connections is *de rigueur*. Democratization proceeds within and across borders over time. Thanks to Mike, our measurement tools and statistical models are evolving to reflect this.

Globalization: patterns of trade and investment

Before notions of globalization and diffusion became the buzzwords they are now, Mike pioneered the study of spatial dependence and geographic effects in international conflict. As this research program progressed, a significant portion of his work considered the second leg of the Kantian tripod: international economic relations, chiefly trade and direct investment (Ward, Siverson, & Cao, 2007).

In an irony not lost on Mike, the traditional econometric approach to studying trade flows between countries–the so-called "gravity model"–goes to great lengths to try and shoe-horn a model of international *interdependence* into the statistical structure of least-squares regression, which requires the assumption that trading partners are (conditionally) *independent*. In other words, the gravity model assumes that US imports from Japan, US imports from China, and China-Japan trade are all independent of one another so long as we observe and "control for" the right variables that somehow hold the "rest of the world" constant.

Echoing points he had long made in the study of dyadic international conflict, Mike showed that the gravity model's conditional independence assumption is not only untenable (with predictable consequences for model performance) but also that interdependence is the central *substantive* problem, worthy of modeling in its own right (Cao & Ward, 2014; Dorff & Ward, 2013; Hoff & Ward, 2004; Ward, Ahlquist, & Rozenas, 2013). Much of this work builds on a collaboration with statisticians at the University of Washington's Center for Statistics in the Social Sciences, especially Peter Hoff, developing advanced models for network data. The network approach work generalizes notions of geography, distance, and dependence to more abstract, high-dimensional "latent spaces." This Additive and Multiplicative Effects (AME) class of models (Minhas et al., 2021; Minhas, Hoff, & Ward, 2019) has proven enormously flexible and powerful. The approach has demonstrated important "network effects" in international economic and political relations while simultaneously producing far more accurate predictions about these flows compared to traditional modeling approaches.

The full effect of this work is yet to be realized, not least in economics where the gravity approach continues to dominate. But as usual, Mike was leading the social sciences ahead. Several of his students are now extending both the models and their applications in international political economy, notably in the study of economic treaties (Rozenas, Minhas, & Ahlquist, 2019) and sanctions (Dorff & Minhas, 2017).

Broader implications for political economy

After exposure to Mike's way of thinking, one begins to see spillovers and spatial dependence everywhere, something apparent in the number, diversity, and success of Mike's students and collaborators. Spillovers, diffusion, and economic geography crop up in the institutional legacies of colonialism, where we see the imprint of economic and political dependencies of large parts of the world with respect to the great powers (Beramendi & Rogers, 2022). Recent empirical work underscores the importance of measuring key concepts–such as ethnic heterogeneity–at the appropriate scale (Spater, 2022) and the use of GIS data in an enormous range of applications. Spillovers and spatial dependence bear on the substantive study of everything from climate to migration to financial markets to regulation to social media networks. And they bear on the recent methodological work on spatial regression discontinuities (Kelly, 2019), instrumental variables (Betz, Cook, & Hollenbach, 2020), networks (Betz, Cook, & Hollenbach, 2021), and estimating experimental treatment effects in the presence of interference. Awareness of dependencies and progress in understanding them is today part of our normal methodological discussions thanks to Mike's pioneering efforts.

In the spirit of Mike, we conclude on a contrarian note. One of Mike's seminal contributions bears on the perils of *p*-values (Ward, Greenhill, & Bakke, 2010), warning of the dangers inherent in concentrating all substantive and statistical attention on a single championed regressor in the desperate quest for "significant" findings, ignoring predictive failures. We dare say that the field of political economy has been at the forefront of the last decade's push toward more careful research designs. While a lot of good has come from that move, we are also now at a point when the demands of the most vociferous *randomistas* bring Ward et al. (2010)'s warning to mind. The

exact nature of the hack has changed. While Ward et al. (2010) were reacting to inferential weaknesses in (very) large-*n*, highly-parameterized empirical models, today's hacks might involve complex designs intended to precisely estimate a single (carefully measured! pre-registered!) quantity in a way that is completely isolated from the dependencies and spillovers of the real world. This emphasis on design for internal validity often comes at the expense of addressing the kinds of big questions and methodological challenges that attracted Mike's attention. The effects estimated from these careful designs are often supposed to inform actual policy decisions, but, in a development that would surely rankle Mike, they are incapable of generating *prediction*, which is often hard. Especially about the future.

Hidden Networks: Don't Play What's There, Play What's Not There[2]

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Shahryar Minhas, Michigan State University

As students of Mike Ward, we learned a few things very early on: to be creative, invest time in understanding the history of ideas; be rigorous in your empirical approach to social science, never give up on learning new tools; and to be taken seriously, do not have typos in your bibliography. Our mentor was as generous with his time and energy as he was specific about the importance of professional, detailed work that honors the very privileged position of working as an academic researcher.

Mike once began an answer to a question with, "In the early days of the 1900s, there was a shipwreck discovered off the coast of Greece. Divers found that the sunken ship carried a device that was made to predict the future." It seems strange, both that a predictive machine would be aboard a ship full of other valuables, like jewelry and artwork, but also that a mentor might answer a question about a regression model by starting with a story that begins on a stormy day in 1901. While us mentees heard about the Antikythera mechanism from Mike on and off, he eventually published about it in his essay "Can We Predict Politics? Toward What End?" (Ward, 2016). In an apt example of Mike's writing style, he used the story of this early computing machine to outline the deeply entrenched theme of prediction across time and space from the device's

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[2] The title quote is from jazz musician Miles Davis.

inventive origins in 250 BCE through the minds of Comte, Popper, Mills and Richardson, and into present day political science debates about theory and prediction.

Mike was not just interested in prediction for prediction's sake. His intrigue for prediction came from a persistent curiosity about difficult-to-observe complexities and a care for some of the more troubling problems facing our world. From the beginning of his academic career, Mike was motivated to understand how interconnectedness shapes critical outcomes like poverty, foreign policy, and war. In 1978, he wrote about the growing interconnectedness of global actors, predicting that "international events and processes will have greater and increasingly subtle effects upon the events and processes which appear to some to be largely non-international in scope, such as the purchase of gasoline. Thus, it seems likely that international factors will become more important in the affairs of individuals, and conversely domestic factors will become more critical in the affairs of international actors" (Ward, 1978, p. 1).

In this early work, based on his dissertation, he developed a measure of inequality that was novel in its attempt to illuminate how both the international and intrastate distributions of valued goods considerably affect a nation's domestic economic well-being. Mike was, of course, correct: the future would only increase the interdependence of global polities. In step with an evolving world, Mike developed his earlier conceptual ideas of interdependence into a transformative research program on the study of political net- works. Below, we highlight his contributions to the study of space and relationships in the political world—physical space, social space, and the kinds of difficult-to-observe interlinked phenomenon that drive patterns of politics today.

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Spatial Dimensions of Politics

In his most cited article (Gleditsch & Ward, 2006, p. 912), Mike asks scholars to move beyond individual level explanations for the rise of democratic institutions and, instead, argues that democracy evolves from a state's broader environment, from the "diffusion, or enduring, cross-boundary dependencies that influence the development and persistence of political institutions." The relative power between groups—groups that function within a system of relationships—and the changing nature of relationships over time, provides a clearer picture of the conditions under which institutions democratize than any single variable alone.

While many would discard, or simply seek to control for the dependencies that exist between observations beyond what we can specify via a set of exogenous covariates, Mike would consider them central to the work at hand. Like geographers (Gould, 1970; Cliff & Ord, 1973), Mike viewed dependencies as a way to analyze complexities in data beyond what we can directly operationalize. In 2004, Mike and collaborator Peter Hoff published what would become a foundational piece on political networks. In it, the authors identify a glaring problem in decades of previous research: despite the widespread acknowledgement of states' strategic interdependence in the study of International *Relations*, the vast majority of "Quantitative, systematic studies of international relations and national security typically assume that the major events that comprise world politics consist of the independent actions of independent actors" (Hoff and Ward, 2004, p. 161). To advance research, the field needed a new modeling approach.

Mike went on to work with generations of graduate students on the topics of spatial diffusion and networks and always maintained a principled focus on the importance of prediction. With student

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collaborators, Mike showcased the importance of spatial diffusion and network behavior to forecasting political violence (Weidmann & Ward, 2010; Metternich et al., 2013) while also working on projects to improve the tools for generating and evaluating predictions (Greenhill et al., 2011; Montgomery et al., 2012; Minhas et al., 2019). For many of the students working with Mike on these projects, these foci served as an entry point to their own academic careers. The number of publications likely inspired by working with Mike is nearly impossible to inventory.

Networks and the Future of Political Science Research

Today our work, along with a new generation of political scientists, continues to build on many of the core concepts and approaches developed by Mike Ward. The number of scholars whom Mike taught and the subsequent works that he inspired is hard to demonstrate, but we attempt to do so by visualizing Mike's co-authorship network in Figure 1. Edges indicate individuals that co-authored together on a project with Mike and node colors reflecting communities.
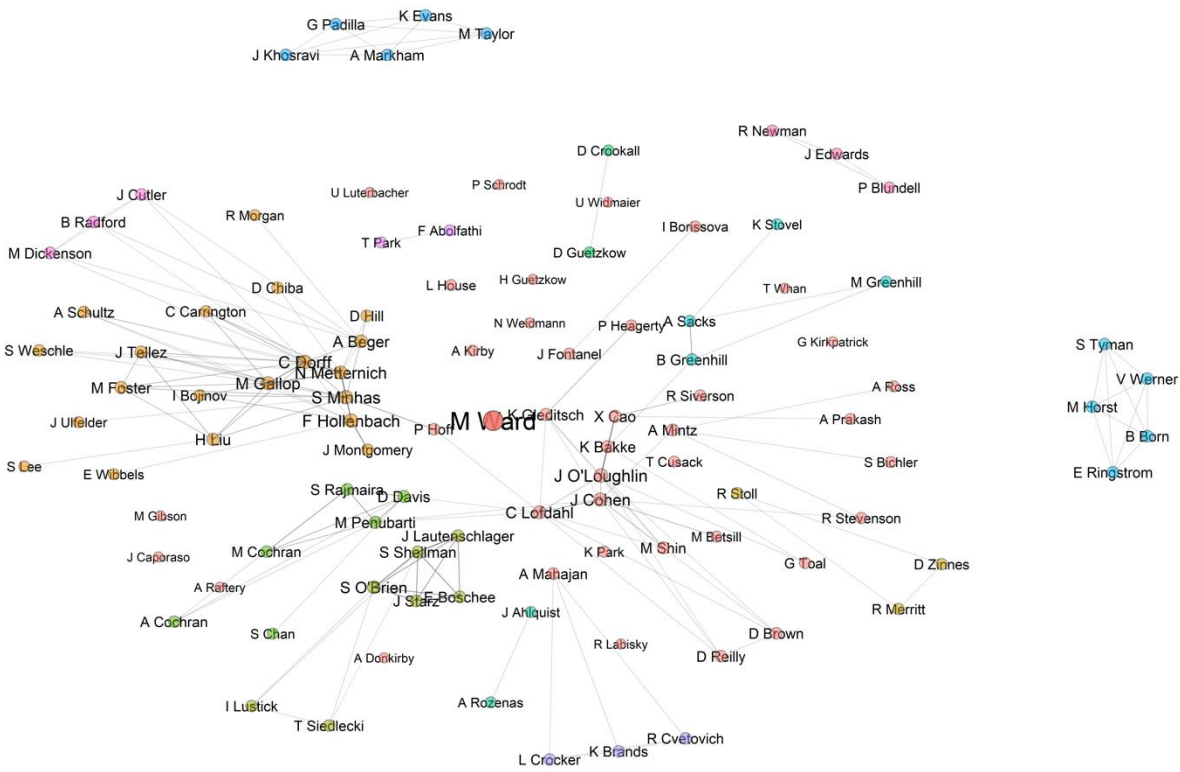
Figure 1: Mike's co-authorship network with edges indicating individual co-authors and node colors determined by a Walktrap community detection algorithm (using random walks to compute distances between nodes, and assigning nodes to communities based on closeness)

One particular feature of the graph is the set of orange nodes seen at the left of the network visualization. These nodes almost all represent students that worked with Mike at Duke University after 2009. In just under a decade, he trained multiple cohorts of scholars who are still practicing in academia today and who have formed deep collaborations with one other.

In our case, Mike's influence has led us towards multiple projects on the role of interdependence in intrastate conflict outcomes. A recent article shows how a network-driven methodology allows us to account for the spread of violence during intrastate conflicts, even where we cannot directly observe the exact conditions that drive diffusion (Dorff et al., 2021). Our approach provides a more accurate prediction of when and where conflict will occur compared to other spatial analyses (Dorff et al., 2021). Other studies also demonstrate interdependence in intrastate conflicts (Phillips, 2014; Gade et al., 2019; Liu, 2022).

Mike's dedication to mentorship and community building is evidenced by his commitment to students, his research lab at Duke, and his involvement in political science societies like the Society for Political Methodology and the American Political Science Association's Political Networks section. Through these connections, Mike facilitated the spread of knowledge about numerous topics, and set the stage for fruitful collaborations among his peers and mentees.

Mike Ward's Contributions to Prediction in the Social Sciences

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Nils B. Weidmann, University of Konstanz

Over the last decades of his career, Mike Ward developed a deep interest in how predictions could be used to evaluate models in the social sciences as well as how to generate better predictions of political outcomes. We detail his interest in prediction, focusing in particular on conflict, and the enduring relevance of his work for current debates on the role of predictive modeling as well as its limitations.

Great Expectations, Modest Returns? Forecasting Conflict and Instability

In the 1990s, there was growing interest in how research insights could help predict conflict and inform policy responses. Following an initiative by former U.S. Vice President Al Gore, in 1994, the Central Intelligence Agency (CIA) convened a task force to identify countries at high risk of state failure within the next two years. Mike's former mentor at Northwestern University and later colleague at the University of Colorado, Ted R. Gurr, played a prominent role in the so-called State Failure Task Force (later renamed the Political Instability Task Force, PITF), which went on to develop a series of models of civil conflict and political stability. They argued that these models could correctly classify failed and stable states two-thirds of the time two years in advance. This applied interest in conflict research extended to international organizations such as the World Bank and helped fund new research on the causes of civil conflict as well as new data sources (e.g., Collier et al., 2003).

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However, the initial enthusiasm over conflict prediction was quickly met by a great deal of skepticism. King & Zeng (2001) noted that the PITF appeared to overstate the predictive ability of their models since they did not adjust the reported predictive accuracy to reflect the case control design, where cases of state failures were compared to a random sample of "non-failures" (i.e., countries without state failure). Many conflict models appeared to display high overall predictive accuracy, but this mainly reflected correctly identifying observations that would *not* see conflict. In one of his most influential articles, Mike and collaborators showed that prominent models of civil war, which identified statistically significant associations and appeared to fit the data well in-sample, had limited ability to predict civil wars out-of-sample (Ward, Greenhill and Bakke, 2010). Moreover, key factors emphasized as important often failed to improve predictive ability, and in many cases led models to perform *worse* than simpler alternatives.

These apparent failures to predict conflict generated considerable interest, and many see this as evidence that prediction about the future is so difficult that we should not waste time on it. For example, a 2012 *New York Times* op-ed piece cited the lack of predictive ability in models of civil war as a "dirty secret," illustrating the wider failure of quantitative predictive political research (Stevens, 2012).

Improving Conflict Prediction

Mike Ward did not see the results of his research as a reason to dismiss prediction. Rather, he took these findings as a call for doing better predictive research and to focus on predictive ability rather than in-sample statistical significance or "policy by p-value." In one of his first explicit efforts at conflict prediction, he demonstrated how attention to geography, space and proximity to conflict

could help improve predictive ability. Ward & Gleditsch (2002) showed how a simple conditional spatial regression model that considers ongoing conflict in neighboring states predicts conflict notably better out-of-sample than a model with only domestic characteristics. An ongoing conflict in one country may increase the risk of violence in neighboring countries due to possible mechanisms such as links between actors across national borders, or spillover effects of ongoing conflict such as increased availability of arms. In practice, conflict - along with many other social and political phenomena - tends to be strongly dependent over time and space. Therefore, incorporating spatial and temporal dependence in prediction models typically adds useful information that improves forecasting. Mike later extended this line of work to the micro-level. For example, Weidmann & Ward (2010) used a spatial prediction model to forecast local violence and demonstrated how incorporating geography can improve the predictive performance of models. This performance can be illustrated through a so-called separation plot, a visualization technique developed by Mike and colleagues to compare the predictive ability of models for binary outcomes (Greenhill, Ward & Sacks, 2011).

Mike's subsequent research on conflict prediction drew extensively on topics he had worked on, including networks and methods for latent imputation of ties. He developed new tools for predictive research, such as event history models for heterogeneous populations (Beger et al., 2017). Mike's interest in predictive conflict research led to an active career in consulting with end users. The external research income allowed Mike to set up the WardLab at Duke University, with many PhD students and postdoctoral associates. Upon his formal retirement from Duke, he set up a consulting company dedicated to predictive modeling (Predictive Heuristics).

Prediction in Contemporary Conflict Research

Many researchers have worked on developing predictive methods and conflict prediction, but there is no question that Mike Ward's contributions have been instrumental in improving conflict research. It is increasingly common for published research on conflict to include predictive assessments of model performance and out-of-sample predictions, using standard metrics to evaluate predictions. There are several efforts to produce regularly updated conflict predictions, for example in the ViEWS Project at Uppsala University (https://www.pcr.uu.se/research/views/). Mike's pioneering research on conflict prediction thus leaves a clear and lasting legacy.

At the same time, prediction has not become an established standard in all conflict research, and testing of evaluate propositions on observed data remains most common. Shmueli (2010) outlines the fundamental distinction between *explanatory* and *predictive* modeling. Predictive modeling aims to optimize fit between model predictions and observed outcomes, based on several features. Variables are included to maximize predictive performance and may not reflect new theoretical insights. By contrast, explanatory modeling aims to estimate causal effects of one or more variables of interest. Other variables are included to improve causal interpretation, but aggregate predictions are not of primary interest. The explanatory and predictive power of a model are different dimensions, and models that perform well on one may often not perform well on the other. It is possible to estimate contributions of individual variables in a predictive model by gauging the drop in predictive performance when removing a variable (e.g., Bätz, Klöckner & Schneider, 2022). But since the predictive contribution of particular variables are contingent on the others in the model, predictive power is highly volatile and model dependent.

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Mike was obviously aware of these issues and concerns, but did not waver in his conviction that prediction was a worthwhile end in its own right. Indeed, he was an outspoken critic of efforts to downplay the importance of prediction relative to theory or the identification of significant associations. Five years before his death, he published a passionate defense of our ability to try to predict political outcomes, with a deliberately provocative call for "less theory" in security studies (Ward, 2016). He would point to how phenomena held to be "unpredictable" in practice have become tractable. Popper (1972) contrasted clouds as unpredictable systems with mechanical clocks. But weather forecasting – involving clouds - is a success story about prediction, where advances in modern computing power allow for estimating Richardson's equations on atmospheric flows from observed data (see, e.g., Lynch, 2006). Similar advances should, in Mike's view, be at least our aspiration for conflict prediction. Mike also argued that much of what passes for theory in the social sciences are not formal analytical accounts, but incomplete verbal accounts pieced together from existing research and empirical observations, which often resist critical examination rather than facilitate new empirical tests. Mike advocated less emphasis on theory and more systematic use of prediction to "winnow the many, many such 'theories' that occupy the world of security studies" (Ward, 2016, p. 84). In one of his last published articles, he similarly criticized efforts to "contrast the roles of theory and 'atheoretical' … predictive modeling" and emphasized how "predictive modeling and machine learning are effective tools for theory testing" (Beger et al. 2021, p. 1405). Since claims about policy consequences are in effect predictions, we should try to think systematically about policy as prediction (see, e.g., Gleditsch, 2022).

The tension between explanation and prediction is a real one, and many will not be comfortable with Mike's emphasis on prediction at the expense of theory-building and identifying causal

effects. In our view, however, both perspectives are valuable. Channeling Mike's position on prediction can help foster dialogue between prediction and explanation in ways that ultimately help us do better research.

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