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Demographic Structural Theory: 25 years on

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Introduction

I am grateful to *Cliodynamics* for this special issue revisiting the ideas put forth in *Revolution and Rebellion in the Early Modern World* (Goldstone 1991, 2016) a quarter century ago. The two things that one could hope for in advancing any theory are that it proves capable of being advanced and enriched by other scholars, and that it proves capable of being applied to new phenomena that were not anticipated. This issue gives examples of both, and shows how scholars are even now only beginning to tap the possibilities of Demographic Structural Theory (DST) in explaining politics, history, and long-term economic trends.

In this essay, I will tell the story of how demographic structural theory was conceived, relate its early reception among scholars, and comment on the important contributions by other authors in this special issue.

Serendipity and the Origins of DST

I started my career in college hoping to become a physicist. I had won Hertz and PG&E scholarships to study at Caltech, where I was admitted in Fall of 1971 and assigned to the “advanced” group of freshman students (roughly the top 10% of Caltech applicants). However, I quickly found myself at the bottom of that group, and saw fellow-students who I was sure would be the ones to win Nobel Prizes—not me! While I was fortunate to be exposed to some of the greatest scientific minds of our era, including Richard Feynman, Leroy Hood, John Holdren, and Bruce Murray, and received an amazing foundation in mathematics and basic sciences, I began to look at other subjects. It was then again my great fortune to have the opportunity to take social science courses from Charles Plott (one of the true pioneers of experimental economics), and John Ferejohn (one of the most important innovators in mathematical political science). They fired up my curiosity about understanding social systems—their history and dynamics—to such a degree that I soon exhausted what I could do at Caltech. For in the early 1970s, while Caltech’s division of Social Sciences boasted many distinguished scholars, it still functioned mainly as an adjunct to the natural science programs. To study a broader range of history and social theory I would have to go elsewhere.

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I was encouraged to transfer to Harvard, which had recently begun an honors undergraduate program in “Social Studies,” led by Michael Walzer. This program featured a rigorous introduction to social theory, plus options for further study in economics, philosophy, and history. It also offered opportunities for some of Harvard’s best visitors and graduate students to act as Oxbridge-style “tutors” for small seminars. I benefitted enormously from the chance to work with the eminent Spanish social theorist Victor Perez-Diaz, and the social historian Paul Starr (then working in Harvard’s Society of Fellows). I also gained an incredible education from working as a research assistant to Thomas Schelling, Richard Zeckhauser, and Howard Raiffa. While I had some initial missteps in shifting from hard sciences to social sciences (among other things, I had to struggle to learn to write clearly and effectively), I felt I had just scratched the surface. So I continued my studies at Harvard by enrolling in the graduate program in sociology.

When I entered Harvard’s sociology department in the late 1970s, it was a wonder and a joy in the diversity of its faculty and methods. Among the senior faculty were Daniel Bell, the eminent theorist of modern capitalism; George Homans, who blended profound historical and anthropological knowledge with a rigorous methodological individualism; Christopher Jencks, who developed the core data-driven studies of social inequality; Orlando Patterson, who was writing award-winning global histories of slavery and freedom; and Harrison White, an erudite physicist turned sociologist who was using matrix algebra to create new ways of mapping social networks. Among the equally brilliant junior faculty were Theda Skocpol, who was working on the book that would become *States and Social Revolutions*; Ann Swidler, who would become a pre-eminent scholar of the sociology of culture; Ronald Brieger, whose collaboration with Harrison White would produce the “block-model” methods of mapping network structures; and John Padgett, who later produced amazing studies of social hierarchy and network structure in Medici Florence.

I also learned more than I can say from my fellow graduate students, most of whom went on to careers more distinguished than mine: these included the outstanding economic historian Kenneth Sokoloff, whom I met in the mathematical modeling seminar run by Professor White; Thomas Davenport, who became one of the world’s leading consultants on knowledge management; the future Columbia scholars Peter Bearman and David Stark; the leading cultural sociologist Wendy Griswold; the eminent scholar of migration and urbanization Roger Waldinger; the comparative historical sociologists Jeff Goodwin and Richard Lachmann; the distinguished Korean academic leader Hyun-chin Lim; and many more.

I was particularly lucky that during my time at Harvard the eminent Israeli sociologist S.N. Eisenstadt, who rotated his visits at leading American universities,

was spending three years as a Visiting Professor to teach graduate students social theory and the history of civilizations. Eisenstadt was also working on a book on modern revolutions, and became an inspiration and mentor to me.

Early in graduate school, under the influence of both Skocpol and Eisenstadt, I had become interested in explaining revolutions. In particular, I wanted to see if I could use mathematical models to explain when revolutions would occur. Unlike many students whose interest in revolutions in the 1960s and 1970s arose from their desire to utilize and understand revolutions as a means for progressive social change, I was struck more by the devastation and dictatorships that great revolutions produced: France in 1789-1815, Mexico in 1910-1920, Russia in 1917-1940 and China in 1949-1976. I wondered first, how was it possible that governments that controlled armies, vast bureaucracies and great financial resources could nonetheless lose control, leading to a grave collapse of the social order? Second, when the problem of revolution was posed as explaining the collapse of social order rather than progressive change, the question naturally extended beyond the “great revolutions” dealt with by Skocpol, Crane Brinton, and others to ask whether a similar causal model might apply to both major revolutions and to the collapse of states and empires all across history.

From the pioneering insights of Theda Skocpol, complemented by the work of S. N. Eisenstadt, and also of Geoffrey Paige, Rod Aya, John Dunn, and other scholars (for the study of revolutions was still an active and turbulent field in the 1970s), I learned that revolutions were as much about state-elite relations as about popular mobilization and discontent. Indeed, my first major professional publication was a review essay on theories of revolution for *World Politics*, arguing that the state-centered perspective of these scholars constituted a new, “third generation” of comparative revolutionary theory (Goldstone 1980). However, unlike Skocpol, who believed that peasants and workers always had abundant grievances, and waited only upon a crisis provoked by state-elite conflicts and a weakening of local control to rebel, I believed that popular fortunes had risen and fallen over history. Some periods were times of relative plenty and rising real wages; other times were marked by poverty, declining real wages, and growing landlessness, and seemed much more ripe for widespread popular uprisings.

Indeed, what struck me in comparing revolutions with similar but more limited events—peasant rebellions, workers’ strikes, urban riots, coups d’etats, and regional rebellions—was that *only* in revolutions did the social order suffer from such a wide array of conflicts. In revolutions elites fought against states; elites fought against other elites over faction, religion, social rank, or other issues; peasants fought against landlords; workers fought against businesses; and urban craftsmen and professionals fought against local government authorities. It was

obvious that any model that explained revolutions would have to include multiple components at different levels of society, and comprehend state vulnerability, intra-elite conflicts, and popular grievances and mobilization.

But what could trigger *all* of these varied factors to come together at a certain time in a revolutionary conjuncture? If they all moved randomly and independently of one another, then the incidence of revolutions would also be random, arising only when peaks in these varied factors happened to converge in a given country at a given time. Of course, it was possible that revolutions *were* just random conjunctures of state crisis, elite conflicts, and popular uprisings, perhaps brought on by a particularly foolish ruler, or a particularly costly war, or the rise of an unusually potent heterodoxy or movement.

Yet that was not satisfactory because major revolutions and rebellions show a very strong temporal clustering. There had been a series of revolutions and rebellions across Europe, the Ottoman Empire, and China in the mid-17th century, made famous by Geoffrey Parker and Leslie Smith (1978) as “The General Crisis of the Seventeenth Century.” Then another series of revolutions arose in the late 18th and early 19th centuries, made famous by R.R. Palmer (1964) as “The Age of Democratic Revolutions” and by Eric Hobsbawm (1962) as “The Age of Revolution.” So it seemed that some broadly synchronous force was at work. But what hidden force could be strong enough to simultaneously drive state crises, elite divisions, and multiple kinds of popular grievances across many different countries and regions at certain times but not others?

To be honest, I hadn’t a clue. I knew what I was looking for, but not what it was or where to find it. Then fate stepped in.

In Fall 1978, I was eagerly looking forward to being a Teaching Assistant for Daniel Bell in his course on modern social theory. However, two weeks before classes started, Professor Bell suffered a return of his periodic back problems and had to cancel the class. I was stuck—I was depending on the income from the TA position to pay for the coming semester in graduate school. Desperate, I asked if there were *any* sociology classes that still needed a TA. There was just one: American Family Demography, taught by the prominent demographer and housing expert George Masnick. That was discouraging, as up to that point in my studies I had no understanding of, nor interest in, demography. But Professor Masnick was generous, and I owe him everything. We had a meeting to discuss whether I could TA for him. As it turned out, he wanted the course to focus on the public policy implications of the arrival of the “baby boom” generation in America. As an undergraduate and first year graduate student, I had spent two summers on Capitol Hill doing policy research, and I had also been a TA for a Kennedy School of Government course on environmental policy. Professor Masnick told me that *he* would teach me the demography I needed to TA for this

course, if I would teach the undergraduates in the course how to use library and federal depository records and data for policy research. I quickly agreed, and started my crash course in the basics of demography.

I was stunned to discover two things. First, how *pervasive* the effects of demographic changes could be. The “baby boom” affected *everything* in America—the demand for housing, the demand for education, the growth of cities and suburbs, the age structure (creating a surge of youth in the 1960s and 1970s), and the balance of government spending versus revenues. Second, I found that the baby boom in America had created, on a smaller scale, the exact same trends that I had identified as leading to revolutions in pre-industrial societies: a surge of youth open to new ideologies and widely mobilized for protest, a suddenly larger population raising the costs of everything, pressures on the labor market from a sudden increase in workers looking for jobs, and a major increase in higher education enrollments. The U.S. government found itself facing larger deficits as it tried to pay for both “guns” (the Vietnam War) and “butter” (more housing and education to meet the demands of the boomers) without raising taxes. And elites found themselves in conflict with the younger generation over a host of issues, from students’, women’s, and civil rights to nuclear power and Vietnam. Violent conflict rose as well, whether from black riots in northern cities, or civil rights confrontations in the south, or student and anti-war demonstrations and even the terrorism of the Weathermen and other groups.

Excitedly, I thought about how even larger and more sustained population increases might have affected more rigid pre-industrial economies. Armies would certainly grow larger, putting a strain on state finances and propelling a search for additional revenues. Yet ever-larger elite cohorts would be fighting over a shrinking agricultural surplus, as a larger population consumed more of the available agricultural output. Both elites and popular groups would thus likely resist fiercely the state’s additional revenue demands. Rents would go up for peasants as more families sought land, while wages would decline for workers as they flooded limited urban wage markets. Urbanization would increase with the flow of job seekers, which would make mass mobilization easier to organize, and harder for the state to control. Rising rents and expanded cities would offer opportunities for upward mobility and an expansion of those seeking entry to elite positions; but with the government pressed for revenue the number of such positions would probably not keep up with demand, leading to sharpened competition among elite families for places and honors. It seemed that demographic change could be the missing factor I had been searching for, something capable of causing society-wide impacts, simultaneously affecting state finances, elite competition, and popular grievances and opportunities. The only question was—was there any data to support it?

I spent several weeks in the Winter early in 1979 in the stacks of Widener library, burrowing into texts in economic and population history. In another stroke of great good luck, the study of historical demography was just starting to bloom with new data. The Cambridge Group for the History of Population and Social Structure, under the leadership of Sir Tony Wrigley, Roger Schofield, and Peter Laslett, had been gathering and publishing data drawn from a national sample of parish registers, and was just about to publish their monumental *The Population History of England 1541-1871* (1981). In France, Louis Henry had also developed a school of historical demography based on local sources, building on the Annales tradition, which had just led to Jacques Dupâquier's path-breaking thesis *La Population rurale du Bassin parisien a l'epoque de Louis XIV* (1979a) and the first general works on France's population in the two centuries before the French Revolution (Dupâquier 1979b). In part due to the inspiration of the Cambridge Group and Louis Henry, and in part drawing on their own academic traditions, authors working on Latin America (Sanchez-Albornoz 1974), Germany (Lee 1977), Japan (Taeuber 1948), and the Ottoman Empire (Barkan 1970) had all recently produced data on population history for their regions. For China, William Skinner, Ted Telford, William Lavelly, James Lee, and Wang Feng were working on their path-breaking revisions to Chinese population history and starting to publish (e.g. Skinner 1977; Lee 1982).

Although my language skills were feeble (I could only read French and English), enough was published in those languages to provide me entry into the world of demographic history data. Fortunately, I could also read tables in German and Spanish (numbers are numbers and words for population are similar). As I gathered up the data, a clear pattern emerged. Before every major revolution or rebellion between 1500 and 1900, I found that indeed, population had grown substantially in the prior half century. This was true for the European countries involved in the "General Crisis of the 17th Century" (Portugal, Spain, England, Italy, France), the Ottoman Empire during the Celali Rebellions, and China prior to the collapse of the Ming Dynasty. It was also true for the Atlantic Revolutions of the late 18th century (America, France, the Netherlands), the European revolutions of the 19th century (in 1830 and 1848), the Ottoman Empire in the 1830s and 1840s, and prior to the Taiping Rebellion in China. Even more important, during the periods in which revolutions and major rebellions were absent in Europe, the Ottoman Empire and China, roughly from 1450 to 1550 and from 1660 to 1760, population growth was almost nil. In the earlier interval this was due to the slow recovery from the Black Death, and in the second was due to a global reversal and stagnation of population growth stemming from severe weather and a second wave of major diseases, including plague, typhoid, and respiratory illnesses.

I also discovered that many historians had written about the role that demographic change played in specific revolutions. To note just a few: Lawrence Stone (1965) had pointed out the impact of increased numbers of elite aspirants in the intra-elite conflicts that produced England's Puritan Revolution. Philip Kuhn (1978) had noted the impact of rising population in the late Qing on undermining the effectiveness of the imperial state administration. Herbert Moller (1964, 1968) and John Gillis (1974) had written about how rising population and large youth cohorts contributed to the European Revolutions of 1848. In addition, I found that Nazli Choucri and Robert North (1975) had written about how European population growth had contributed to the outbreak of World War I, and that Myron Weiner (1971) had even coined the phrase "political demography" in his analysis of how ethnic immigration could trigger local conflicts with both the native population and with governments. Yet as far as I could tell there was no existing scholarship analyzing the global role of population movements in revolutions and rebellions.

With the data strongly supporting my theory, I wrote up a lengthy, detailed dissertation proposal to research the relationship between population growth and revolutions. It was flatly rejected.

What I had not realized was how much debates on the causes of revolutions had become wrapped up with neo-Marxist vs. cultural approaches to political analysis. Neo-Marxists, such as Skocpol and Immanuel Wallerstein (whose World-System theory was widely embraced in the 1980s), focused their attention on how global economic competition shaped the relations of elites and states. For Skocpol, revolutions were all about international military and political pressures pushing states into conflicts with their elites; if those elites were absentee landlords these conflicts then allowed peasant uprisings to succeed. She had little interest in urban riots, less in the movements of wages or population, and not much in intra-elite conflicts spurred by changes in elite social mobility. On the other hand, scholars of the French Revolution of 1789, which in the 1970s remained the archetype for comparative studies of revolution, had forsaken the once dominant Marxist view for a radically cultural approach, exemplified in the works of Francois Furet (1971) and Keith Baker (1978). For these scholars, talk of population was a throwback to either the now-discarded materialist explanation of Marx or the even more distant and disreputable theories of Thomas Malthus. For them, revolutions had to be explained by a radical shift in political culture.

My proposal to study the relationship between population change and revolutions thus struck my dissertation committee as faintly ridiculous. Professor Skocpol, giving me what I am sure she thought was helpful advice, listed the problems as follows: First, this is not a viable approach to explaining revolutions,

as people are not just passive animals who rebel when subject to population pressure. Second, the data on population history is probably not of sufficient volume or quality to sustain a comparative analysis of population trends in relation to revolution. Third, even if the data were available, you, as a graduate student without specialized training in demography, would not be competent to carry out a convincing analysis. And finally, even you did succeed in completing the analysis, this argument is so out in left field that no one would care.

I was crushed, and retired to my apartment for a few days of soul-searching. Should I continue with this approach? Should I continue to study revolutions, or even sociology? I had, after all, considered going into law and public policy before choosing graduate school in sociology—was it too late to change back?

Still, I believed I had a viable theory, that the data supported it, and that the way science progresses is by developing and testing theories with relevant data. So I resolved to try again. I constituted a new committee, with Professor Skocpol but also Professor George Homans, who I knew believed in empirical science and had deep knowledge of the history of law and social structure in early modern England. I also took the brave step of asking Professor Nathan Keyfitz to join my committee. Professor Keyfitz was one of the world's leading mathematical demographers, and I had hesitated to approach him earlier given my rudimentary training in demography. However, Professor Keyfitz was as generous and open as he was brilliant. It turned out that he had also done extensive work on population and development, and was interested in exploring the links between population change, economic trends, and government responses. Professor Keyfitz not only joined my committee, he gave me financial support for one summer that helped me continue in graduate school and provided wonderful guidance, pointing me to additional work in demography and politics.

For my new committee, I greatly scaled back my initial, overly sweeping proposal. Instead of analyzing population and revolution across history, I proposed to focus on one particular case—the English Revolution of 1640—and explicate in detail every step of the causal argument for that case. I felt that for England, thanks to the Cambridge Group, I was on solid ground with data for population and wages. There was also solid data on elite mobility and elite relationships and on royal finances for the century before and after the Revolution.

The next two years were among my happiest in graduate school, as I now was free to track down all the data I could find on English population, prices, wages, urbanization, age structure, cohort behavior, college enrollments, elite social mobility, royal finances, and study the scholarship on this case. Most scholars relied on religion, or constitutionalism, or battles between the King and Parliament over finances, to explain the revolution. Yet these explanations failed

to make sense of several striking features of the Puritan Revolution. First, as with many revolutions, the divisions between the supporters of the King and Parliament did *not* follow any simple lines of class, or region, or religion. Instead, partisans on both sides came from the same regions, the same economic and legal classes, and on many occasions *even the same family!* This was spurred by competition for preference that penetrated through every level of the elites. Second, the diversity of uprisings confounded any simple religious or constitutional account: Presbyterians in Scotland opposed the King's imposition of the Anglican prayerbook; farmers in the Lincolnshire Fens and the Irish staged mass uprisings; while apprentices, yeoman farmers, and domestic merchants all supported Parliament's revolt against the King. Finally, to the extent that it was conflict over taxation that provoked Parliament, the King's financial difficulties were not due to overspending as much as to the failure of traditional revenues to grow. The traditional Parliamentary and land taxes failed badly to keep up with inflation, forcing the King to add new expedients and to try to squeeze more revenue out of elites by pushing or going around Parliament. During the sixteenth century, much inflation was arguably due to debasing the currency; but prices kept rising and inflation accelerated in the seventeenth century after the silver content of the pound was stabilized. It was difficult to explain this without referencing the pressure of population growth on England's agrarian economy.

In my doctoral thesis, I was able to use econometrics to demonstrate the close relationships among population growth, rising prices, urbanization, falling real wages, rising land rents, declining real royal revenues, and elite social mobility. Bringing together numerical proxies for state fiscal distress, elite competition, and mass mobilization potential (combining real wages, urbanization, and youth bulge) into a function I labeled the "Political Stress Indicator" (PSI) I was able to show that PSI was low and stable prior to the 17th century, rose sharply to a peak in 1640-1660, then declined, giving an excellent account for the timing and magnitude of the English Revolution.

This was sufficient for me to gain approval for my PhD, and even to get my first job, at Northwestern University. But as it turned out it was far from sufficient to gain any attention, much less acceptance, for a demographic-structural theory of revolutions.

When I first attempted to publish an account of my dissertation argument in the journal *Theory and Society* it was summarily rejected. Only when I wrote to the editors explaining that my argument was not simply a Malthusian account, and detailed how it was an *institutional account*, showing how population change affected state-elite and intra-elite conflicts, could I get the paper even sent out for review. After much further review and argument (through which the editors at

Theory and Society were consistently gracious and helpful) it was eventually published two years later (Goldstone 1983).

At Northwestern, I joined the Economic History Seminar led by Jonathan Hughes and Joel Mokyr. Mokyr became a life-long friend who greatly expanded my understanding of economic history. At a meeting of the Cliometrics society that Mokyr had arranged for me to attend, I developed the idea of urban networks driving up the velocity of monetary circulation and thus driving inflation in response to population growth. This idea, later published, helped earn me some credibility among economic historians (Goldstone 1984).

I then returned to my project of showing that the demographic structural theory could be used to explain additional cases of revolution and rebellion. I took a semester leave to fulfill a dream to visit and work with the Cambridge Group for the History of Population and Social Structure. There I was given a warm welcome. To be sure, scholars at the Group often disagreed with me. Roger Schofield, the undoubted expert on Tudor finances, disputed my arguments on inflation and state finances in the sixteenth century. And Sheilagh Ogilvie, who became a distinguished expert on German history, took issue with the application of my model to that country. However, we agreed more often than not, and the Group was wonderfully open in sharing their data and their vigorous research and discussion of all aspects of social, economic, and demographic history. While studying their data, I was able to offer some helpful insights into the shifts between nuptiality and age at first marriage as regulators of pre-industrial English fertility (Goldstone 1986). I cannot say how much the generosity and personal warmth of Professors Laslett, Schofield, and Wrigley, as well as their towering scholarship, as well as that of Richard Wall and Richard Smith, inspired me. Suffice to say that if not for the research produced by the Group, and their generosity in sharing that research, my own work might never have emerged.

By 1986 I was able to take advantage of newly published research on Ottoman demographic, economic, and political history (Karpas 1985; Inalcik 1985; Faroqhi 1979-80, 1984) and on China's Ming-Qing Transition (Huang 1986; Chan 1982; Wakeman 1985, 1986) to extend the work I had done on England. I felt I could now demonstrate the parallels in the underlying dynamics of the English Revolution with the 17th century Ottoman Celali Revolts and the collapse of the Ming Empire. I submitted my comparative analysis to the aptly named journal *Comparative Studies in Society and History*, where it was promptly rejected.

In the mid-1980s, the history profession was still somewhat Eurocentric, and a historian of England who reviewed my paper took great offense at the very idea that the Ottoman capital of Istanbul could have been anything like a rival to London. I had to provide the editors with documented evidence that in 1600 the population of Istanbul was roughly twice that of London. I then was able to obtain

additional reviews, and eventually the paper was published, two years later (Goldstone 1988).

By this time, I had spent nearly a decade doing additional research since my dissertation. I had been stubborn. Once, living in Chicago in the 1980s, I had my car broken into while it was parked. That was unremarkable, except that I had been foolish enough to forget my briefcase in the car, holding within it three months' worth of research recorded on index cards. The next morning when I went to the car, I found only a trail of three or four cards leading to the nearest (now empty) dumpster. Fortunately, I had a separate bibliography of the works I had consulted, but I had to spend several months revisiting every source on that list to duplicate the notes I had taken and that had been lost.

I spent a year reading theses of regional histories of France to learn how various social and political conflicts unfolded in the lead-up to the revolution, and spent additional years researching and consulting with experts on Turkey and China to ensure I had an up-to-date view of the scholarship on their histories. By 1988 I had completed my manuscript for *Revolution and Rebellion in the Early Modern World*, which thoroughly explained the structural demographic theory of social order and instability, and which showed that the PSI function based on the theory accurately identified the timing of the English Revolution, the French Revolution, the English Reform Movement, the Revolutions of 1830 and 1848, and that the theory further explained the onset of the Ming-Qing transition and the Taiping Rebellion, the Celali and Balkan revolts in the Ottoman Empire, and—with a twist—the Meiji Revolution in mid-19th century Japan. I proudly submitted the manuscript to Cambridge University Press, who quickly rejected it.

Again, the European historians who reviewed the manuscript resisted such sweeping comparisons, and recommended against publication. I was fortunate to get another hearing from the University of California Press, as I had just accepted a job at the University of California-Davis. UC Press was willing to take a chance on a highly ambitious manuscript from a still recently tenured scholar. Yet they remained skeptical. They rejected my desired subtitle: *Population Change and State Breakdown in England, France, Turkey and China 1600-1850*. I had acquired a solid reputation as an expert on revolution from my other publications, but they felt linking population change to revolution would be off-putting, especially to historians. They also assigned the completed manuscript to a part-time freelancer to edit, instead of one of their regular editors. The result of that was a disaster: the freelancer felt that the argument *should* be a straight Marxist, class-based theory of revolutions, and edited it to conform to that view. Since the manuscript was in fact a *repudiation* of class-based theories of revolution, that meant the editor had extensively rewritten and revised it, changing negatives and reversing the sense of whole paragraphs.

When I received the copy-edited manuscript, and realized that it was now redlined on every page with extensive modifications that wholly reversed my argument, I nearly despaired. I asked that this version be thrown out and that the copy editing start again on my original manuscript. The press refused (too expensive, they said) and asked me to just go back through the manuscript and change what bothered me. It took me six months to go through the manuscript and correct the copyediting to restore my intended meaning (I still worry that some passages I might have missed remain). Fortunately, UC Press did use an experienced and expert copy-editor for the final version, and the volume went to press at the end of 1991 for a late publication date in that year.

When the book finally appeared, reviews were mixed. It was favorably received by sociologists, getting strong reviews in *Contemporary Sociology* and eventually winning the Distinguished Scholarly Publication Award of the American Sociological Association. However, it was largely rejected by historians, most of whom had been taken hold of by the cultural turn, and was given a damning review in the *New York Review of Books* by Lawrence Stone, denouncing it as a wrong-headed and overly macro approach to history. That was enough for the publisher to bury the book. They never scheduled a paperback edition, and when the book was announced as winner of the American Sociological Association's top award, the Press did not even have a copy available to show at the annual meeting.

I continued to make the case that a demographic approach to political instability had value, arguing that this approach would have helped foretell the collapse of the Soviet Union (Goldstone 1993), and that it explained the continued high levels of political instability in developing countries (Goldstone 1977, 2002). Yet the academic world was simply not very interested, as the study of revolutions was itself going out of fashion. The collapse of communism in Eastern Europe and the U.S.S.R. was considered to be something (anything!) other than a revolution, somewhat oddly given the violence that arose in Chechnya and the former Yugoslavia in the wake of their changes of regime. With the "End of History" it began to be suggested that the era of revolutions was over, as progressive change would now take the form of relatively peaceful transitions to liberal democracy.

In sociology, the study of revolutions was largely superseded by the study of social movements. In political science, more attention was given to civil wars and political forecasting, rather than to comparative and historical studies of revolutions. And in comparative politics, regional specialization flourished, leading to a kind of local myopia; for example, experts on the Middle East focused on explaining the exceptional stability of its authoritarian regimes (Brownlee 2002).

Theda Skocpol thus left the study of revolutions and moved her attention to American politics. I too shifted my research in order to keep publishing, working more on global and economic history, social movements, democracy, and political forecasting.

Comparative historical sociology itself seemed to be moved aside. After the “golden age” of comparative historical sociology in the 1970s and 1980s, my mentor S.N. Eisenstadt told me that while comparative historical research would still be needed, it was not going to flourish in the major centers of sociology. That turned out to be true: No work in that field has won the American Sociological Association’s “Distinguished Scholarly Book Award” since 2005. While from 1993 to 2005 no fewer than seven comparative historical sociologists won the award, (myself, John Markoff, Randall Collins, Charles Tilly, Richard Lachmann, Mounira Charrad, and Beverly Silver), all except Tilly spent their careers outside of the major sociology research departments.¹

Yet while academia can be prone to fashions and fads, history has its own logic that eventually prevails. As it turned out, the age of revolutions was *not* over. This became apparent in 2010-2011, when the supposedly stable autocracies of the Middle East and North Africa started to topple. While the regime change in Tunisia did seem to follow the post-communist script of non-violent transition to democracy, in Libya, Syria, Egypt, and Yemen events unfolded in ways that looked like classic revolutions, with counter-revolutions, civil wars, and revolutionary terror (especially in the form of ISIS, an effort at revolutionary state-building that arose in the aftermath of state breakdown in Syria and Iraq). In addition to the Arab Uprisings, there were also the Kyrgyz Revolution of 2010; the Maidan Revolution in 2014 in Ukraine, which produced a civil war in the eastern portion of the country; and the Burkina Faso Revolution of 2014. Among major revolts there were the rebellion that led to the independence of South Sudan in 2011, violent armed conflicts and power seizures by militias in the Central African Republic in 2013, the Boko Haram insurgency in Nigeria, and a Taureg rebellion in Mali in 2012. Meanwhile, the revolutionary regime of ISIS captured extensive territories in Iraq and Syria in 2014-16; the Taliban rebellion in Afghanistan continued to slowly gain ground; and Kurdish agitation for independence from Iraq grew. Attempted uprisings against the government in both Turkey and Venezuela led to harsh state-led counter-movements that dismantled democratic oppositions and reshaped the government in authoritarian fashion.

All this would have been disturbing enough, and difficult for a prosperous and firmly democratic set of Western nations to cope with in seeking to maintain a

¹ Collins did conclude his career at the University of Pennsylvania, but had spent the great bulk of his teaching career at UC-San Diego and UC-Riverside.

peaceful world order. However, from 2014 onwards, even advanced western democracies were shaken by an unusual degree of political turmoil. The European Union saw the rise of anti-EU and anti-immigration parties in many states, with Britain voting to withdraw from the EU, Catalonia voting to withdraw from Spain, and Scotland nearly doing the same within Britain. Hungary and Poland voted in ethno-nationalist Parties that have weakened judicial independence and opposition parties, while France threw out both of its major political parties in favor of the wholly new *En Marche* movement led by Emmanuel Macron. In the United States, following the rise of the Tea Party movement, radical nationalists elected the political novice Donald Trump as president on a markedly populist platform. The new President has repudiated several long-standing policies of *both* major political parties—previously pro-free trade, engaged in global anti-pollution efforts, protective of human rights, and anti-Russia, the U.S. now seems to have reversed course on all these fronts.

It thus seems that we are again witnessing a global wave of political instability, not greatly different from those of the mid-17th, late 18th/early 19th, and early 20th centuries. This suggests that far from having achieved an “end” of history, we are once again following out a 100 to 150 year cycle of recurrent crises.

The New Relevance of DST

To be sure, even before 2011, there was some notice and elaboration of the demographic structural theory, and a number of scholars made further efforts to develop “political demography,” analyzing how population patterns affect domestic crises and international relations. In 2002, the physicist Bertrand Roehner and economic historian Tony Syme teamed up on a study of *Pattern and Repertoire in History* that took note of DST and developed additional formal models to describe regularities in history (Roehner and Syme 2002). In 2003, Peter Turchin, an ecologist and expert in complex population dynamics, greatly expanded the scope of DST (Turchin 2003). In 2009, working with Sergey A. Nefedov, he made a number of important refinements in the formal modeling of the DST, particularly with regard to elite competition, and demonstrated its applicability to plotting historical cycles (Turchin and Nefedov 2009). There were also efforts in Russia, led by Andrey Koratayev and Leonid Grinin, who created the journal *History and Mathematics*, to bring greater rigor to historical analysis, particularly regarding long-term trends and cycles (Grinin and Koratayev 2009, 2015) that noted and built on elements of the DST.

In the broader field of “political demography,” Richard Jackson and Neil Howe (2008) produced a major study of how aging would affect international relations, while a number of younger scholars, including Jennifer Dabs Sciubba, Eric

Kauffman, Monica Duffy Toft, Mark Haas, Henrik Urdal, Ragnhild Nordas, Elliot Green, Vegard Skirbekk, and Christian Leuprecht made significant contributions linking youth bulges, population aging, ethnic change, and migration to patterns of political conflict (work by these authors is collected in Goldstone, Kaufmann, and Toft 2011). In 2010, the general circulation international policy journal *Foreign Affairs* published an article on “The New Population Bomb” (Goldstone 2010), which argued that while the old fear that growing population would lead to widespread poverty and turmoil in the developing world had largely been overcome, new challenges were being posed by rapid aging in the developed West and East Asia, high rates of urbanization even in relatively poor countries, and the continued expansion of the global Muslim population at a time of widespread conflict between Islamists and the West (Goldstone 2010).

Yet it was only with the onset of the Arab Revolts of 2010-2011, and the need to explain violent events that did not fit the pattern of either Marxist progressive revolution or peaceful democratic reform, that demographic explanations for political conflict gained wide attention. In fact, one demographer, Richard Cincotta, had used a variant of demographic-structural theory, which he has called the “age-structural theory,” to forecast instability and democratic transitions in the Arab World two years prior to their outbreak (Cincotta 2008, 2008-09). Articles were published shortly after the events to show how they fit demographic theories of revolution (Goldstone 2011; Koratayev and Zinkina 2011). In particular, the huge increases in educated but un- and underemployed youth and in urbanization, and the resulting competition for both jobs and elite places, as well as the role of expensive subsidies and grain imports on state finances, received considerable discussion.

Still, the populist surge and overturning of expected patterns of government in the rich countries of the U.S. and Western Europe called for explanation, and—like most revolutions—they seemed to suddenly come out of nowhere. Yet the DST, surprisingly, can claim to both explain, and even have predicted, these events as well.

In the original 1991 edition of *Revolution and Rebellion*, I had included a section in the final chapter on the decline of the United States. For—as I noted in my first introduction to demography in Prof. Masnick’s class—I saw trends in the wake of the U.S. “baby boom” that were similar to those behind major revolutions. I thus expected that in the coming decades, the U.S. would suffer from declining productivity, heightened political polarization, increasingly “selfish elites” focused on their own enrichment at the expense of public goods, and as a result, risked falling for a populist leader promising protection.

Twenty-five years ago, I wrote:

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It is quite astonishing the degree to which the United States today is, in respect of its elites' attitudes and state finances, following the path that led early modern states to crisis. ...America in the wake of the baby boom [is experiencing] polarization of incomes, stagnant real wages, reluctance to pay taxes, and greater struggles for personal advancement. ... We thus find a familiar pattern: [a growing] federal deficit and a public infrastructure increasingly unable able to meet national needs. The long-term result is a loss of faith in the public sector, a greater polarization and fragmentation of society, and a loss of a sense of shared community. ... The specter of being left behind in international competition ... creates emotional needs that are satisfied by aggressive trade policies and protectionism (Goldstone 1991).

I also wrote that, barring a shift in leadership that tamed the deficit, rebuilt public infrastructure, and reversed the enrichment and expansion of elites, these trends would accelerate and America's international influence would decline. While it seemed for a while that the Clinton and Obama administrations might succeed in these goals, the George W. Bush and Donald J. Trump administrations effectively reversed those brief successes, reducing taxation on the rich, failing to rebuild America's infrastructure or return to a more balanced income distribution. As a result, the negative trends, and their consequences, have predominated.

This brings us to the essays in this special issue, and the additional light they shed on these troubling current events.

Richerson on Ages of Discord

Peter Richerson has written a review of Turchin's (2016) *Ages of Discord* that focuses on its methodological merits. As Richerson—an eminent scholar of cultural evolution who pioneered the use of formal models borrowed from evolutionary biology to the development of human culture—laments, historians and most social scientists studying history have eschewed testing of formal models using coupled differential equations, even though that has been demonstrated to be the best way to understand the dynamics of complex systems, and human societies are such systems. (A partial exception should be made for cliometric historians, who use general equilibrium models based on differential equations to explore historical patterns of economic change; but to be sure they commonly use *equilibrium* models rather than dynamic models that give rise to complex behavior).

Richerson applauds *Ages of Discord* for demonstrating how the use of relatively simple equations and carefully gathered empirical data can account for complex societal behavior. Turchin shows how the DST model and data generate long cycles of stability and instability that match up with America's 'era of good feelings' from 1800 to 1830, the descent into Civil War and Reconstruction in 1840-1900, the progressive and New Deal eras of reform, the return to generalized prosperity and stability from the 1940s through the 1970s, then a return to pressures for polarization and conflict from 1980 to the present.

Yet for Richerson, "the correct explanation for ages of discord in the U.S. and elsewhere is *almost* beside the most important point of *AD*. That point is that human societies are dynamic systems interacting with each other and set within dynamic environmental systems." I can understand why Richerson feels that way—after all, despite the successful modeling in *Revolution and Rebellion*, *Historical Dynamics*, *Secular Cycles*, and now *Ages of Discord*, the great mass of the historical profession still sees no value in modeling human societies as dynamic systems. Yet the value of this approach can only be demonstrated by showing that it provides superior explanations of problems of great interest and significance.

In this, I find *Ages of Discord* to be remarkably successful. There are two keys to that success. First, instead of focusing simply on real wages, which generally show a long upward trajectory throughout U.S. history as mechanization from 1800 onwards produced rising productivity, Turchin points our attention to *relative wages*. Relative wages are wages relative to GDP/capita; that is, the share of national output that is returned to workers as wages. The larger that share, the smaller the amount that is left to fuel elite expansion. A high relative wage thus promotes social stability in multiple ways: workers feel they are getting a fair share of economic growth, while growth of inequality due to rising elite incomes is averted. In addition, if elites cannot grow their incomes at the expense of workers, but only by increasing output as a whole, they are motivated to raise and reward worker productivity and invest in public goods that raise overall output. By contrast, if relative wages are low, so that most gains from growing output go directly to elites, then the relative position of workers declines, inequality increases, and more income is available to support rentier elites. If the shift in income in favor of elites produces more elites and elite aspirants, but the same or fewer elite positions (as is inevitable in a world of mainly positional goods for elites as leaders of large organizations or owners of prime properties), then elite competition is likely to increase. As Richerson observes, "the SDT theory pictures a system in which elites are mutualists when comparatively rare but predatory in the elite overproduction phase of the cycle." This shift in the behavior of elites, from productive to predatory, is a key factor driving instability and decline in human societies.

Second, Turchin does a remarkable job in plucking credible data proxies for the variables in SDT from the incomplete historical record. He finds data to track elite production, political polarization, urbanization, age structure, and state finances, enabling him to build the PSI function and track it for two centuries. One of the amazing elements of *Ages of Discord* is its ability to plot this data on graphs from 1800 to 2015, showing exactly how various components change, and how when combined they generate a continuous plot of cycles of stability and discord across American history.

Overall, I think it is an outstanding achievement of Turchin to have produced a quantitative model that explains both the outbreak of the Civil War in the 19th century and the highly polarized and combative anti-establishment populism of our own day.

One may protest that conditions in the United States in 2015-2017 are *not* like those of 1855-1857: there is no stubborn divisive institution with the scope of slavery; there is no vast Western territory to be incorporated into the nation; and the U.S. is the world's leader in wealth, technology, and military power, not a marginal developing country and raw materials provider as it was in the mid-1800s. The U.S. population is also much older, in overall age structure, than it was at the time of the Civil War.

Yet these conditions only change how the crucial conflicts will manifest themselves. The demographic structural theory is not a “dumb” theory, predicting the same result from every turn in the underlying cycles that it generates. Above all, it is a theory of the impact of population change on *institutions*. Therefore the precise response to underlying demographic trends will vary as the institutional context changes. As I wrote in 1991 in *Revolution and Rebellion*, “The United States is faced not with the threat of state breakdown but merely with the loss of relative international economic standing and political influence.”

What I forecast based on the SDT was that the U.S. would suffer from “private individuals among the elite [becoming] enormously richer, while basic public services that support the economy as a whole—primary and secondary education, airports, trains, roads, and bridges—are neglected, overburdened, and deteriorating.” What the SDT forecasts for the United States is increasing elite factionalization and polarization, rising state debts, and falling living standards. While that may not lead to the violence of civil war, it is already producing political turmoil and large human costs, including an unusual and striking *decline* in U.S. life expectancy due to hundreds of thousands of opioid-linked and suicide deaths over the past few years (Case and Deaton 2017). These are arguably casualties of the marked shift of relative income away from wage-earners to the top .1% since the 1980s.

As Cincotta's age-structural theory observes, countries with older age structures are more likely to survive threats to democracy, as are countries with long-established and well-institutionalized democratic procedures. Thus the negative trends in the U.S. are likely to be reversed when the underlying demographic drivers are reversed, as the baby boomers pass from the scene and immigration is more tightly regulated and reduced. Yet in other countries that are younger, or where democratic institutions are less well established, the current secular cycle may produce more violence, as in the Middle East, or turns toward authoritarianism that are harder to reverse, such as in Turkey, Poland, and Hungary.

Attention to how context shapes the operation of DST leads us to the paper by Donagh Davis and Kevin Feeney.

Davis and Feeney on Why 19th Century Britain was Different

Davis and Feeney take issue with my treatment of 19th century Britain in *Revolution and Rebellion*. They are correct. I did not give sufficient weight to the role of outmigration in how England coped with its rising population in the early 19th century. At the same time, there is much on which we agree, and considering how Britain was different adds much insight into the operation of DST.

The conventional history argues that while European countries struggled with a series of liberal revolutions against monarchy and aristocratic dominance in 1821, 1830, and 1848, propelled by emerging capitalism creating new liberal professional and working classes, Britain was different. Having already experienced revolutions in 1640 and 1688 that tamed its monarchy and institutionalized popular representation in Parliament, Britain only needed to upgrade and refine its representational system, which it did through the Reform Act in 1832 and subsequent Acts. And as the famous French historian Elie Halévy argued, British Methodism further acted to tame the working class with its habits of thrift, sobriety, and individualism, blunting any mass movement toward radicalism and social upheaval (Itzkin 1975).

In contrast, I argued that the 19th century revolutions in Europe were not the result of rising capitalism, but of agricultural production continuing to lag behind population growth, producing the characteristic combination of declining conditions for workers and peasants, and heightened elite factionalism and conflict that DST predicts. What made the 19th century conflicts less often fatal to states was the greater fiscal strength that European monarchies had gained compared with their seventeenth and late eighteenth century conditions. As Britain shared in Europe's population growth in this period, indeed led Europe in its rate of population increase, it should have suffered the same pressures and

seen the same combination of popular unrest and elite conflicts leading to political crises. Yet it did not.

My answer was that Britain was roughly 50 years ahead of the rest of Europe in developing an industrial economy that would provide more jobs and rising incomes for workers. However, this positive impact was felt mainly in the north at first where the factory towns of Manchester, Leeds, and Birmingham were growing, while the agricultural south lagged behind. Moreover, this positive effect only shows up in rising wages from the 1830s, so I argued that Britain would look similar to Europe until then, and afterwards diverge. I therefore equated the agitation around the Reform Bill of 1832 with the European Revolutions of 1830, but saw industrial progress as allowing Britain to avoid a reprise in 1848.

In one respect, Davis and Feeney strengthen my argument. When I wrote *Revolution and Rebellion* in 1991, I of course did not have the depth of data on any of my cases that is available today. In some cases, I was arguing from fragmentary evidence hoping that future research would fill in the gaps. Davis' and Feeney's new data set establishes that popular contention in Britain around 1830 was in fact quite substantial, and that radical mobilization was perhaps even greater than we had thought earlier.

Yet that also raises the importance of asking why social conflict declined after 1832. The Reform Act was, after all, only a partial reform of Parliament. And as Davis and Feeney point out, population growth continued and the increase in jobs and elite positions in Britain could not have kept up with the increase in workers and elite aspirants. So something else must have intervened.

Davis and Feeney document how the sheer volume of Britain's outmigration to its current and former colonies was unique. Although other European countries also had empires, none had such extensive overseas territories, and none engaged in such extensive colonization. Even when Spain and Portugal controlled vast American territories, relatively small numbers of Iberians settled there. Most hoped to grow rich then return to Europe, much like British officials in India or French officials in West Africa and Indochina or Dutch officials in Indonesia. Spending their entire lives in relatively uncivilized places surrounded by a vast majority of indigenes did not appeal to capable officers. Britain, however, had acquired vast territories in temperate climates, which by the nineteenth century had been largely cleared of indigenes. For both elites and workers, Canada, the United States, Australia, and New Zealand became reasonable places to settle, as, to a lesser degree, were the Kenyan highlands, Rhodesia, and the Cape Colony.

Most of the debate on the value of Britain's Empire has focused on whether the empire provided material benefits, whether as a source of capital to enable British industrialization or a source of revenue to fuel Britain's naval and military expansion. On these concerns, the judgment seems to be negative: it appears that

the administration and supply of the Empire cost the home country as much as the revenues that it provided (O'Brien 1999). Yet Davis and Feeney argue for an even more important function for the Empire—providing political stability. Without this outlet, the logic of the DST suggests that Britain's extraordinary population growth would have visited further crises on Britain in the mid and later 19th century.

Instead, Britain enjoyed a long period of internal political stability, lasting up to the Irish Republican Revolution of the 1920s. Davis and Feeney thus offer a unique view of the role of the Empire in British history. It was not only overdetermined, but vital to the way that Britain's 19th century politics differed from those of the continent.

In the late 20th century, by contrast, Britain's development looks much more like that of similar capitalist countries, which leads us to the article by Ortman, Mazzeo, Zlodeev, and Korotayev.

Ortmans et al. On Britain in the Long Run

Oscar Ortman and his colleagues have done the valuable service of replicating Turchin's calculations of the DST's PSI function for the U.S. in the parallel case of Great Britain since 1960. As with the U.S., Britain has gone through a period of post-war growth, then stagflation in the 1970s followed by an anti-state pro-market policy that has, since the 1980s, enriched the elites disproportionately to the rest of the country. While rising credit helped sustain an impression of prosperity, the crash in the Great Recession of 2007-9 was even more severe in Britain than in the U.S. The austerity policies implemented in Britain in response to the crash were harsh, and though employment has recovered, productivity and output have not.

Ortmans et al. corroborate two important elements of the DST. First, the major turmoil in British politics—the agitation for Scottish Independence, the Brexit vote, the total collapse of the Liberal Democratic Party (which had only recently been in power in a coalition), and the electoral failures of both Blairite Labour and Teresa May's Tories—only occurred when multiple components of the PSI function reached high values. That is, elite overproduction and competition must coincide with state fiscal difficulties and rising mass mobilization potential for full-blown crises of conventional politics to occur. As their Figure 22 shows, the PSI function computed for Britain shows a relatively flat trajectory until it shot upwards from 2009 to 2016, exactly identifying the timing of today's British political crisis.

Second, the "popular mobilization" component of the PSI function, taken by itself, is a fairly good predictor of popular agitation in the forms of strikes, protests, and riots, as shown in their Figure 11b. Thus, if one is interested more in

the incidence of protest events, rather than systemic crises, one might well want to focus on this component of the PSI function rather than its full expression. Ortmans et al. provide a valuable suggestion that one way to do this is to vary the weights of the components of PSI by adding exponential weights to each component (in the fashion of the Cobb-Douglas function).

Finally, Ortmans et al. point out something I had noted above, namely the global nature of the political crisis that seems to be spreading across many nations of the world since 2007. The global recession of 2007-2009 thus seems not to have been simply a short interruption in the upward trajectory of economic growth in a liberal-democratic world. Quite the opposite. It seems instead to have been an accelerator of long-term trends toward inequality, elite polarization, working-class decline, state indebtedness, and political radicalization against prevailing and establishment authorities that have been developing since the 1980s. In this, the close agreement of results between Turchin in *Ages of Discord* for the United States and Ortmans et al. for Great Britain since 1960 is an impressive confirmation of the DST and its predications.

What is somewhat remarkable to me, and certainly heartening, is that Turchin and Ortmans et al. have demonstrated that the PSI function developed twenty-five years ago to explain revolution and rebellion in pre-industrial monarchies and empires can also be used to explain political dynamics in modern Western countries. Their work, by using an innovative and more rigorous formulation of DST, and by tapping much richer data than was available 25 years ago to plot the PSI function, points toward a remarkable unification of world history, in which the same underlying theory can explain the timing of state crises ranging from the Puritan Revolution, the Fronde, and the Ming-Qing Transition to Brexit and the Trumpian rejection of mainstream politics in the United States.

Turchin, Gavrilets, Goldstone and Paths Toward Even Greater Forecasting Power

In the sciences, one test of truth has always been the ability to turn explanation into prediction and control. Even in the ancient world, the ability to predict eclipses accurately separated real astronomers from charlatans. Thus it has been a major frustration that no theory of revolutions—even those that have seemed to be good at retrospective explanation—has been particularly good at prediction.

Some scholars have argued that revolutions lie in a set of phenomena for which prediction may not be possible, even with a solid theory, because the initial conditions are hard or impossible to detect in advance. For example, Timur Kuran (1995) has argued that even those opposed to the status quo have an incentive to conceal their real attitudes until the weakness of the regime is clear or a

revolution is underway; otherwise they risk being punished for their views. Only once a revolution has begun will more cautious elites and the population as a whole reveal that they are willing to support it.

Kuran may be too pessimistic, however, for while we cannot know people's internal views, we can know the objective conditions that shape those views. That is, we can determine if a regime is financially sound or in debt, is grievously corrupt or holding up an appearance of sound behavior, and is enacting policies that harm or conflict with its own elites. We can determine if income distribution is shifting, if wages are rising or falling, and life expectancy is going up or down. We can find out whether social mobility is rising or falling, and whether the production of elite aspirants is growing or stable. Given this information, we should be able, using the DST and PSI function, to determine if a society is at a relatively high or low level of risk for a major revolt or crisis.

Knowing a country is at high risk, however, is not the same as making a prediction. We know, for example, that earthquakes are most likely on known fault lines, and that the risks of an earthquake grow with the length of time that stress has been building along a particular fault. Nonetheless, the intricacies of fault networks and the details of their geology and morphology are such that one cannot predict the exact moment at which accumulating pressures will cause a rupture, nor whether a rupture will remain focused on a particular location along a fault network or spread along nearby fault lines. And given the complexities of the folds of the earth, some faults may be hidden until a quake occurs. Thus, while we know *where* earthquakes are most likely to arise, we cannot say precisely *when*.

Something similar may hold for revolutions. We may be able to identify where social pressures for instability are accumulating; but we cannot be sure when, or even if, some trigger events will set off a overt regime crisis. What we should be able to do, however, is determine from the historical record the kind of trigger events that have set off crises in vulnerable societies, so that if we see such events in a vulnerable country we will know with high confidence that an instability event is about to unfold.

This is what Turchin, with input from Gavrilets and myself, proposes in the research plan laid out in his paper. That will be a highly valuable refinement of the DST and increase its value for prediction. But there is another route to follow as well, whereby the DST can be used for control of social trajectories even without precise prediction.

Imagine a person wandering on a plain in a random walk. Imagine now that the person is approaching the edge of a cliff that runs along the plain. Can we predict when the person will fall off the edge? No—because it is a random walk, we can compute the likelihood that he will take sufficient cumulative steps

toward the edge to fall off. But we cannot say precisely how many steps or when that will occur. A society in a high vulnerability state of high PSI is, metaphorically, walking along a cliff's edge. We may not know precisely how far from the cliff we are, or where that cliff exactly lies, and so cannot predict exactly when a fall will occur, or what other factors, might push the society over the edge.

Nonetheless, there is a way to exert control in such a situation, *and that is simply to back the society away from the edge*. That is, if PSI has risen and become relatively high (e.g. two or so standard deviations above the long-term mean), a society can avert a crisis by taking steps to lower the value of PSI. Such steps include shifting income from the rich to the middle and working classes; shoring up state finances; and investing in research and promoting investment in infrastructure to provide jobs for workers. Where the number of elite positions is being curbed by low rates of start-ups and consolidation of firms (as has been happening in the U.S.), more enforcement of anti-trust rules could help as well. Where population growth is still strong—not the case in western Europe and the U.S. but certainly in many developing countries—reducing population expansion is also critical. If corruption has become a factor in exaggerating inequality and limiting wage gains and opportunities for the population, then corruption must also be curbed.

These seem like common sense public policies; but they are being abandoned with gusto, and the opposite is being done in many countries today. The problem is that once PSI has reached high levels, a momentum can set in such that the rich see only virtue in using government to make themselves richer, while the rest of the population loses the confidence and influence to change the country's direction. Even in societies where population growth has slowed or ended, this policy momentum can continue to drive several components—state fiscal stress, elite competition, declining wages—still higher, until a crisis occurs.

Toward The Future For DST, and For Us

The growth of scholarship in political demography, the fact of this special issue, and Routledge bringing out a new 25th edition of *Revolution and Rebellion* (Goldstone 2016, this time with the preferred subtitle saying that the book examines population change) are signs that the demographic structural theory is finally gaining respect and attention. There is still a long way to go, as Richerson notes, before historians and social scientists are comfortable with using equations for dynamic systems to explain major historical events. However, the impressive scope and power shown by the papers in this special volume, and by the work of Turchin and his collaborators, showing that DST can explain events far outside its original provenance, from aspects of Britain's imperial policy to the U.S. Civil War and the current political disruptions in both developing and

advanced societies, should provide incentives for scholars and policy-makers to take a closer look.

Perhaps most important, DST provides a powerful handle on our current predicament. It shows that there is strong historical precedent for the combination of rising inequality, growing intra-elite competition, falling relative wages, and rising state debt to create political turmoil. It thus should not surprise us that in societies facing the same combination today, even in developed countries, we find deep political divisions leading to unexpected outcomes. DST also points us toward policies that can walk us away from the edge, and toward a more stable and orderly political future.

In short, the science of understanding social order and social instability across history has advanced. Whether our fellow scholars and our societies will be able to learn from that science, and use it to improve our own futures, is the real challenge that confronts us.

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