Great Punctuations: Prediction, Randomness, and the Evolution of Comparative Political Science

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I argue that comparative politics has been shaped by two "Great Punctuations" that, on each occasion, transformed our conceptions of what the subfield is and what we do. Just before a punctuation occurs, the subfield seems especially coherent, united by a set of common assumptions, methods, theories, and so on, which are then punctuated by a series of events that destroys faith in them. The subfield then reconstitutes itself around new assumptions, until, just as coherence is achieved, the next punctuation occurs. To demonstrate why the subfield has evolved in this way, I draw on probability theory to argue that the desire to be a predictive science causes us to imagine the world to be far more predictable than it actually is. This results in the development of theories that are surprised by events; hence the peculiar trajectory of the subfield.

The history of comparative politics reveals periods of intellectual stability punctuated by moments of theoretical collapse. Going back to its origins, Ido Oren has shown how the old "institutionalism" of Woodrow Wilson and others, which dominated the field from its inception to World War I, took much of its inspiration from the Prussian state as *the* model of good governance and proper public administration (Oren 2003). Political scientists of the day sought to draw general lessons from this single case in order to develop better models of governance. Sampling on this particular datum proved costly, however, when Germany became the enemy during World War I. Caught in the backlash against all things German, the guiding models of the subfield, and thus the field's research focus, collapsed.

Following World War I came the turbulence of the 1920s and 1930s. Communism and fascism rose to prominence as the world's great powers fell to deflation and imperialism. Yet during this time of great political upheaval, political science became a study in irrelevance. Perhaps as a result of no longer sharing common theories and assumptions, the discipline fragmented and retreated inwards. Scanning the *American Political Science Review* from 1923 to 1936 for any sustained analysis of the great events of the day such as Mussolini's march on Rome, Japan's occupation of Manchuria, or even the Great Depression, one will come up empty. What one does find are, for example, reports of constitutional change in Estonia (Roucek 1936), predictions that the German administrative structure would stop Hitler becoming a dictator (Friedrich 1933), and analysis of the legal monism of Alfred Verdoross (Janzen 1935). Although such examples may seem evidence of misplaced foci as much as predictive failures, the fact that the scholarship of the 1930s systematically missed what was going on by such a wide margin paved the way for those who followed in the 1940s, the behavioralists, and their attempt to rebuild political science along explicitly predictive lines (Dahl 1961[1]).

BUILDING A PREDICTIVE SCIENCE

Political science's inability to predict any of the great events of the previous decade had proven a serious embarrassment. Eager to make up for their prewar irrelevance, post-war political scientists sought to provide policymakers with predictions regarding, as Gabriel Almond put it, "exotic and uncouth" parts of the world (Almond and Coleman 1960, 10). As Karl Lowenstein (1944) wrote, to overcome past errors comparative politics would have to become "a conscious instrument of social engineering" (541) because "the discipline ha[d] a mission to fulfill in imparting our experience to other nations . . . integrating scientifically their institutions into a universal pattern of government" (547). Political science therefore had to become positive and predictive, and the discipline rebuilt itself around the latest theories of the day (functionalism, modernization theory, and political culture) to meet these new expectations.

This new version of political science posited that societies were self-equilibrating entities that shared common functionally related subsystems for integration, adaptation, and goal attainment. Actually existing societies were then arrayed along a developmental continuum with the United States posited as the world's historical end. Where states actually sat on this telos was determined by some combination of their functional fit (Huntington 1968) and/or political culture (Almond and Verba 1966). Some political cultures were seen as better or worse at adapting to the dictates of modernity, but overall the path to a stable capitalist democracy was pretty much set. At least this is what members of the discipline imagined into the 1960s, a decade that proved

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[1] Much of what political science does is more accurately described as "retrodiction" to a line of best-fit. However, when seen from a deductivist/positivist standpoint, such exercises take prediction and explanation to be equivalent. This position has been dominant in political science for over 50 years. See Friedman 1953 for the canonical statement.

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to be, just like the 1920s and 1930s, a watershed for political science. As occurred in the 1920s and 1930s, these new and scientifically rigorous theories were about to be punctuated (and thereby invalidated) by the politics of the day.

THE SECOND GREAT PUNCTUATION (AND RECONSTRUCTION)

At the height of modernization theory's popularity, the United States, the home of comparative politics, was caught in the midst of a civil rights struggle between a disenfranchised and functionally unintegrated racial minority and an integrated "modern" public, where the "unintegrated" confronted the "integrated" in episodes of violence. Contrary to theory, however, it was the integrated/modern white majority that surrendered rationality, whereas the practices of the unintegrated black minority were marked by rational action and moral argument. Despite these events being perhaps history's first televised falsification of theory, their significance as real-world disconfirmation of the dominant theories of the day was more or less completely ignored by the profession (Easton 1969).

Similarly, and equally contrary to theory, the new developing democracies were falling to dictatorships. Coups d'etat in Latin America and Africa were becoming commonplace while modernization was grinding to a halt throughout the world. Finally, with the rise of Soviet industrial power (an odd thought today but true for the time), the institutional realization of modernity became plural. There could be more than one version of the modern, and consequently, the telos that informed the theory behind practically all comparative political science became unstable and once again collapsed.

In this unsettling context, comparative politics underwent yet another rebuilding period. During the early 1970s, however, with the modernization–pluralism–political culture model of political science in tatters, the intellectual center of gravity of the social sciences shifted from the United States to Europe. Sociology abandoned Talcott Parsons to embrace Nicos Poulantzas and class analysis, while economics became embroiled in the "capital controversy" and the "microfoundations" debates. Political science similarly looked around for new anchors, and in comparative politics two were identified in the ferment found elsewhere.

Comparative politics scholars in the United States were, by and large, unwilling to sign on wholesale with the new Marxism popular in Europe (Poulanzas 1973). They were nonetheless affected by it. Some comparative politics scholars found refuge in a sanitized Poulanzian analysis of late capitalism called "state-theory" (Evans, Rueschemeyer, and Skocpol 1985). After developing a viable research program based upon the notion of the "state as actor" with varying degrees of autonomy and capacity, this part of the subfield disaggregated the state into a series of institutions and looked for the effect of these institutions historically; hence historical institutionalism (Steinmo, Thelen, and Longstreth 1992). Building on this legacy, a significant part of the subfield has developed ever more rigorous models of institutional stasis and change from these common foundations (Thelen 2004).

At the other end of the spectrum, those suspicious of such structural and supraindividualist arguments flocked to Mancur Olson rather than Nicos Poulantzas, thus igniting the Rochester Revolution (Amadae 2003). Beginning with simple static models borrowed from public choice research, this methodologically individualist research program became increasingly sophisticated, developing ever more encompassing theories and cutting-edge technologies. Together, these two camps have, for the past 20 years, dominated comparative politics.

TOWARD A THIRD GREAT PUNCTUATION?

We may well have rebuilt comparative politics—in two flavors—but what of it? Beyond engaging in a more or less constant *Methodenstreit* between rationalists and historicists that has become a research program in and of itself (Katznelson and Weingast 2005), are we any less likely than our intellectual forbears to be blindsided by the next Great Punctuation and find our theories suddenly redundant?

Consider, for example, the already influential literature on the varieties of capitalism (Hall and Soskice 2001). This body of work argues that under conditions of globalization two particular forms of state, a liberal market economy (LME) and a coordinated market economy (CME), have become the optimal forms of organization for advanced capitalist economies. Although this analysis is indeed informative of what types of state seem to do well in the global economy *at the moment*, recall that according to similar analyses a decade ago, the Scandinavian welfare state would to wither and die under globalization (Kurzer 1993). Instead, the Scandinavian economies today rank as Europe's most efficient.

Similarly, two decades ago practically all IPE scholars agreed that the United States was in terminal economic decline and that Japan would soon take over as the global number one (Keohane 1984). Instead, the United States became a hyper-power and Japan became mired in a decade-long recession. Finally, consider that the place of religion in politics, so obviously important post 9/11, was given practically no attention in graduate curriculum in comparative politics in the U.S. until 9/11 put it on the agenda. After all, we had no theoretical reason prior to 9/11 to predict that it might become important.

Given the rapidity with which our dominant theories turn out wrong, can we be so sure that, for example, either the LME or the CME will still be most viable form of state a decade from now? Or will we be talking about, perhaps, the surprising success of something called the "Mediterranean Model" 10 years hence? Can we say with certainty that the United States' current unipolar moment will last, or will China, in

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a rerun of the arguments of the 1980s, overtake the United States? Will religion continue to be a crucial mobilizing force in politics throughout the world, as seems to be the case at the moment, or will it give way once again to the forces of secularization? In short, do our current theories allow us to see the next Great Punctuation coming any better than those of the 1960s and 1970s?

RISK, ESTIMATION, AND POLITICAL SCIENCE

If one views such punctuations as wholly exogenous events, then the answer must be no. But to stop here would be a mistake, for an arguably larger part of the reason our theories are surprised by events is endogenous to our theories. That is, our theories assume the world to be far more stable (and hence predictable) than it actually is. Specifically, we assume that agents in the social world face tractable and normal probability distributions, that they face a world of risk, when in fact they face a world of uncertainty. This, at base, is what causes the trouble.

Unfortunately, political scientists routinely confuse risk and uncertainty although they are quite distinct states (Burden 2003, North 1990). Theorists typically assume that agents' cognitive limits, informational restrictions, and the environmental complexities they face lead them to be uncertain about likely future states of the world. Note that in this rendition "uncertainty" is probabilistic. As such, possible outcomes (given an adequate sample of past events) are predictable and surprises are rare. Given prior experiences and institutions to simplify the environment, life is uncertain in the sense that rolling a dice is uncertain, and this is why it is not uncertain at all.

Two theorists who realized that uncertainty was not reducible to risk were John Maynard Keynes (1936) and Frank Knight (1921). Both saw uncertainty and risk as ontologically distinct states of the world. For Keynes and Knight, true uncertainty was found in moments that agents subjectively defined as unique events where there were no priors to rank, and thus no basis for probabilistic calculation. In such environments the equilibrium outcome is a moving target that is *ex ante* unpredictable because the parameters of the system cannot be *a priori* estimated. To see the importance of this distinction and why it tells us much about doing comparative politics, we need to play a game of Russian roulette.

UNCERTAINTY, PREDICTION, AND THE BARREL OF REALITY

Is it rational to play Russian roulette? No, because the risk–reward ratio is too high; that is right, but it presumes that one knows how many chambers the gun has. Assume for a moment that I have a gun and that I suspect, but am not sure, that it has over a billion chambers and only one bullet. I know for sure, however, that each time I pull the trigger I will receive $10,000. Given this estimate of the odds (and the definite rewards), I decide to play. I click once and am $10,000 richer, so I click more. By lunchtime I am a millionaire and grow confident. Technically, each trigger pull is equivalent to a piece of information about the likely probability distribution I face. As I sample more and grow richer (and more confident), assuming that more information is better than less, I should be able to get a more accurate prediction of where the bullet is (assuming the probability distribution is normal). Unfortunately, with the next click, I die.

The problem here, and in the social world in general, is that we cannot see the generator of reality (the number of chambers of the gun), only its outcomes (the clicks of the trigger). Sampling on outcomes suggests that an agent can compute risk insofar as sampling the past tells us something meaningful about the likely probability distribution that we face. In some environments this assumption may be justified (we may live in a risky world of normal distributions some of the time), but in many situations of political interest, this assumption, as this example suggests, may be deadly.

Two probabilists who have thought a great deal about this question are Nassim Taleb and Avital Pilpel (Taleb and Pilpel 2004). They argue that for an agent to find out whether the world is risky or uncertain, one needs to generate a probability distribution of likely events. Unfortunately, the generator of probabilities in the social world (unlike a six-chamber revolver) is not directly observable; only its results are.[2] As a consequence, the future may be risky in that priors can be ranked, but the future may also be deeply uncertain in that there are may be no priors to rank. How would an agent know the difference? Typically, in the absence of seeing the generator of reality, we sample from past events with the notion that more information is better than less. In doing so we try to figure out the probability distribution of future events; and here lies the first problem.

As Taleb and Pilpel (2004) put it, "one needs a probability distribution to gauge knowledge about the future behavior of the distribution from its past results, and . . . at the same time, one needs the past to derive a probability distribution in the first place" (2). In other words, to estimate risk one has to assume an adequate sample of past events; but how much is enough? And perhaps even more disturbing, can more information be worse rather than better? In order to answer this question, I ask the reader to follow me through three possible worlds that have different probability distributions attached to them, and then decide which world political scientists actually study. The answer to this question, I argue, explains why our discipline's evolution is characterized by these Great Punctuations.

[2] For example, when examining the stock market one sees the results of market actors' decisions in the form of price movements (outcomes), not the decisions themselves, nor what is driving them (the market as a whole and its prevailing "sentiment").

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THE THREE POSSIBLE WORLDS OF POLITICAL SCIENCE

The first (type-one) world is the familiar one of games of chance where the generator is directly observable. Here we live in a world of risk. To use a die as our example, we can see that the generator has six possible outcomes. Given numbers one to six and a few dozen throws of the die, the expected and actual means converge rapidly, and this is sufficient to derive the higher moments of the distribution. This distribution, given the fixed and known values of its generator, is reliably normal and sampling the past is a good guide to the future. One cannot throw a "20" and skew the distribution with a six-sided die. This world is Gaussian and is, within a few standard deviations, predictable. Great punctuations do not occur in such a world.

In terms of the objects of study of comparative politics, type-one assumptions correspond to particularly well defined and well behaved environments that are characterized by clear rules and observable generators. The study of courts and legislative politics may be good examples of such a world. Generators can be directly observed (votes can be counted, preferences can be revealed, rules are followed) and large deviations are rare. Given such dynamics the relative success of quantitative and game theoretical techniques within this area of comparative politics becomes understandable.[3]

The second world (type-two) is a "fat tails" scenario (a Gaussian plus Poisson distribution) where uncertainty rather than risk prevails—at least in the tails. In this world, examples of generators would be stock markets, national elections, and the causes of wars. In each of these cases one can sample past data exhaustively, but one does not directly observe the generator of reality (what drives the markets, turns the election, or causes the war). As the Russian roulette example demonstrates, there is the possibility that large events not seen in the sample (the bullet) may skew the results and become known (rather painfully) only after the fact. None of the trigger pulls before the bullet (or statements by politicians before the collapse, or polls before the election, or data-mining of battle deaths over time) will tell us that the punctuation is coming.

For example, stock market returns may seem normal by extensive sampling, but a Russian default or a "Tequila Crisis" lurking in the tail will radically alter the distribution in ways that agents cannot calculate before the fact, irrespective of how much information they have. Similarly, in the case of the electoral politics, the reelection of the German SPD in 2002 offers an interesting case in point. The SPD was widely predicted to be in an impossible position until Gerhard Schröder's opportunistic opposition to the Iraq war at a stroke invalidated months of accurate polling. Finally, no amount of foreign policy analysis and data mining would have predicted a second Israeli invasion of Lebanon in 2006. In these cases we seem to live in a world of uncertainty more than risk. Agents simply cannot know what may hit them, though they may be (over)confident that the probability of being hit is small.[4]

The third possible world (type-three) is even more unsettling. So far we have assumed some form of normality in the distribution, even allowing for fat tails. However, imagine a generator such as the global economy. In this case, not only can one not see the generator directly, but also agents can sample the past until doomsday and become steadily more wrong about the future in doing so. We could be facing a Pareto–Levy–Mandlebrot distribution where no amount of sampling will work because these distributions offer no tractable solutions.[5] In such distributions there is no mean to sample for, because there is, as Keynes observed, no limiting point of equilibrium (1936, 3). With such complex generators "no amount of observation whatsoever will give us E(Xn) [expected mean], Var(Xn) [expected variance], or higher-level moments that are close to the "true" values . . . since no true values exist" (Taleb and Pilpel 2004, 14).[6] To see what "there are no true values" means in practice, consider the following two examples.

The comparative politics of the welfare state has, over the past 50 years, given rise to at least five different general theories of welfare state growth and development. First of all, modernization was seen to be *causa prima* (Briggs 1961). This was followed by labor power as the prime mover (Korpi 1978). Next, the state's position in the international economy was seen as critical (Cameron 1978, Garrett 1998), as was the autonomy and capacity of the state in question (Skocpol and Weir 1985). According to the latest thinking, it was employers who both desired and designed the welfare state all along (Mares 2003). A similar phenomenon seems to have occurred in macroeconomics, which too has had five general theories over the past 50 or so years. In this case, however, the *explanandum* was inflation rather than the origins of the welfare state, with culprits as varied as exchange rate fluctuations, money supply problems, and labor market rigidities being identified (Watson 2002).

Both these examples suggests first, that such theories cannot be general because they change every decade or so, or, second, that they might be thought of as general at the time they were constructed given the sample that they were derived from; but, to highlight the case of macroeconomics, such theories must become redundant because the sources of inflation change over

[3] Although I am open to the possibility of such environments exhibiting type-one dynamics, I am skeptical that such environments are as purely type-one as is typically assumed by such scholarship. For an analysis of Congress that makes this point, see Sheingate (2006).

[4] Moreover, the situation may be nonergodic because in the social world the statistical assumption of stationarity may not apply, so the sample may be misleading.

[5] Such distributions look normal (and indeed they look more normal the more one samples the past) until something profoundly unexpected happens that invalidates the theories developed from sampling past data.

[6] As Taleb and Pilpel (2003, 22) put it, "many economists dismiss the possibility of . . . [our third world]. We claim that, unfortunately, in economic situations, generators of this type can occur."

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time.[7] For example, if the causes of inflation in one period (monetary expansion) are dealt with by building institutions to cope with such causes (independent central banks), this does not mean that inflation becomes impossible. Rather, it means that the conditions of possibility change such that the theory itself becomes redundant. In such a world, outcomes are fundamentally uncertain because the causes of phenomena in one period are not the same causes in a later period (Shapiro 2005, 195).

Given this, when we assume that a generator produces outcomes that conform to a normal distribution, we assume that we live in a type-one world. This may, however, be too strong a claim. Any large sample of past events can confirm the past, but cannot be projected into the future with the confidence we usually assume. Take away that prior assumption of normality in the distribution and standard expectations (e.g., regarding stationarity or the confidence level) fall apart. Sampling the past to predict the future in such environments is not merely uncertain, it becomes a dangerous exercise because such dynamics "invalidate our ability to conclude much from . . . past behavior to . . . future behavior; in particular, it makes it impossible for us to assign any specific probability to future outcomes, which makes the situation one of uncertainty" (Taleb and Pilpel 2004, 16).

WHAT WORLD DO WE LIVE IN?

So which world is the one most likely faced by agents and studied by political scientists? The type-one world can be ruled out because if the world were so predictable, risk would be the only issue. Our theories would predict accurately and the discipline would not be shaped by random punctuations. Given the evidence to date, we must conclude that in many cases we do not inhabit such a world. Although it is conceivable that some environments are more type-one than others, as intimated above, the fact that the discipline has evolved as it has suggests that such environments may be a small subset of our concerns.

The type-two world seems suspiciously normal most of the time, but our predictions are disconfirmed more than they should be through standard error estimates because most of the action occurs in the tails and we cannot see the generator of outcomes. Events such as financial meltdowns, "unexpected" election results, and wars occur far more often than our theories, based on type-one assumptions, would predict. Consequently, the more we focus on the assumed middle of the distribution, the more we are sure of what is going on, the more often we are actually surprised.

A type-three world is even more worrying because it implies that the more one samples the past, the less one knows. In a type-three world, uncertainty becomes so extreme that agents would have a tough time living in it. Life would be a series of constant surprises and nothing would be predictable. In such a world, society, and hence government, would be impossible, which suggests that we do not live in such a world. But perhaps thinking of the world in this way allows us to figure out why the world that we occupy might be type-two, and why the discipline models it as type-one.

CONCLUSION: WHY THE EVOLUTION OF COMPARATIVE POLITICS IS ONE OF GREAT PUNCTUATIONS

Assume for a moment that the default condition of human societies is our very uncertain type-three world. Humans do not, however, deal particularly well with such uncertainty and try to insulate themselves from it; they try to make their world risky rather than uncertain. On this point contemporary rationalists, historical institutionalists, and constructivists all agree. What holds the social world together is not given by the material environment. Rather, it is human agency that produces stability. Whether through the promulgation of norms, the instrumental design and construction of institutions, or the evolution of governing schemas and ideologies, the result is the same. Human agents create the stability that they take for granted.[8] In taking it for granted, however, we assume the world to be much more stable than it actually is. Consequently, the theories we develop about our world tend to assume much more stability, and thus predictability, than is warranted.[9]

Because we cannot live in a type-three world, we build institutions, cultures, organizations, and so on, to cope with uncertainty. When we are successful at doing so, we assume that we live in a type-one world of estimable risk and we develop theories to help us predict such a world. Unfortunately, we actually have succeeded only in constructing a sometimes-stable type-two world of fat tails, and this is why we are consistently surprised. We think and model type-one while living type-two. Meanwhile, as a discipline, we refuse to admit the possibility of a type-three world generating the two other possible worlds.[10]

The result is that the action is in the tails, and like the proverbial drunk under the lamp-post we focus on the middle of the (normal) distribution, because that is where the (theoretical) light is. In this case, however, we are blinded rather than illuminated by our theories, because the more sure we are about them,

[7] Clearly it is not the case that the causes of welfare state emergence change over time because there is only one point of origin for each state. This suggests that political science theories change in response to what Giovanni Sartori called "novotism"—the desire to say something new—as much as they change due to need for another theory.

[8] When I leave the house in the morning I do not say to my wife "see you later honey, I'm off to replicate the structures of late capitalism" even if my going to work does precisely that.

[9] We also like to assume that the world conforms to a normal distribution because it makes the statistical treatment of the world possible.

[10] The similarities of this line of argument to those of scientific realists are not accidental. The analysis here is broadly consonant with that taken by scholars such as Roy Bashkar and Richard Miller.

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the less able we are to see the next Great Punctuation coming round the corner. This is why comparative politics (and arguably other subfields in political science) have evolved in such an irregular manner. Rather than cumulative and linear accumulation of knowledge characterizing the field, we live in a world of Great Punctuations.[11]

Whether comparative politics can overcome these problems is an open question, but a first step is to acknowledge that such problems exist. Its always a good idea to establish what one does not know before saying with any degree of confidence what one does know, and the *a priori* specification of probability distributions is something we do not, as yet, know very much about. These problems are difficult, and the consequences of admitting them are far-reaching, but if we do not face up to them we risk becoming the modern equivalent of Keynes' famous Euclidian geometers (1936, 16–17). We can go around rebuking (or ignoring) the world for not behaving as it should and insisting on the predictive value of our theories, only to have them wiped out by the next Great Punctuation. If we do this, we risk becoming an odd anthropological sect that imagines, theorizes, and measures a world that is not there, and spends its time predicting the unpredictable, rather than being a progressive intellectual discipline.

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[11] This is not to claim there has been no progress in political science. There has been, but its just not linear or cumulative. See Blyth 2003.

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