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NEW WORLDS IN POLITICAL SCIENCE

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Abstract: ‘Political science’ is a ‘vanguard’ field concerned with advancing generic knowledge of political processes, while a wider ‘political scholarship’ utilizing eclectic approaches has more modest or varied ambitions. Political science none the less necessarily depends upon and is epistemologically comparable with political scholarship. I deploy Boyer’s distinctions between discovery, integration, application and renewing the profession to show that these connections are close woven. Two sets of key challenges need to be tackled if contemporary political science is to develop positively. The first is to ditch the current unworkable and restrictive comparative politics approach, in favour of a genuinely global analysis framework. Instead of obsessively looking at data on nation states, we need to seek data completeness on the whole (multi-level) world we have. A second cluster of challenges involves looking far more deeply into political phenomena; reaping the benefits of ‘digital era’ developments; moving from sample methods to online census methods in organizational analysis; analysing massive transactional databases and real-time political processes (again, instead of depending on surveys); and devising new forms of ‘instrumentation’, informed by post-rational choice theoretical perspectives.

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One does not set out in search of new lands without being
willing to be alone on an empty sea.

*Andre Gide*¹

The undeveloped state of political science has been a theme for depressed navel-gazing in the profession over the last two decades. Our collective mood nowadays seems far removed from the confidence that attended the first lectures in Cambridge by Sir John Seeley (1896), the assertive pro-state position of early American political science in the hands of the younger Woodrow Wilson (see Dryzek et al, 1995), the enthusiasm of the post-war ‘behavioural revolution’ or the early hopes of the rational choice pioneers. In the UK’s ‘Political Studies’ or ‘Government’ departments (the continued British naming equivocation says it all) it is not hard to find people who agree heartily with Hal Abelson’s populist judgement: ‘Anything which uses science as part of its name isn’t: political science, creation science, computer science’.²

Yet the mainspring of current pessimism is actually localized chiefly in three areas that should be manageable - the inescapable eclecticism of over-inclusive definitions of ‘political science’; the apparent ‘reset to zero’ character of theory disputes; and the problems of doing cumulative research in a fast-changing area of human behaviour. In Part 1 of the paper I seek to insulate forward thinking against these difficulties by defining an explicitly ‘vanguardist’ (that is, non-inclusive) idea of ‘political science’, but one which none the less rests on a wider sub-structure of ‘political scholarship’ Political scholarship still predominates in our discipline and its role is both essential and fully legitimate. But political science means something more.

In Parts 2 and 3 I briefly explore two ‘new worlds’ for a re-focused political science, whose tackling or neglect will define our discipline for the next half century. One is to begin to build a genuinely global political analysis that can for the first time capture political experiences across the whole world we have and treat them intelligently and equitably. To do so will require that we ‘break the mould’ of comparative politics, whose existing practice fetishizes the nation state as a unit of analysis and focuses single-mindedly on the same stale set of macro-institutional features, when we know that many different micro-institutions matter.

The second set of challenges is to look more deeply and precisely at political behaviours than we have so far attempted. We need to radically modernise and upgrade our ambitions to collect evidence, our analysis of ‘fields’ of possibilities and our standards of proof and analysis. Key pointers to a better future include using non-reactive web census methods, analysing transactional and events data, and embracing ‘post rational choice’ theories and new methods.

1. Political Science and Political Scholarship

Science is built up with facts, as a house is with stones. But a collection of facts is no more a science than a heap of stones is a house.

*Henri Poincaré*³

A lot of learning can be a little thing.

*Spike Milligan*⁴

If our discipline means everything that gets done within departments labelled ‘political science’ or ‘political studies’, its common purposes will seem elusive and its content eclectic. An *APSR* editorial pessimistically observed:

‘Political science is a strange discipline. Indeed, it is hardly a discipline at all... [R]ather than being a distinct branch of learning, political science is a crazy quilt of borrowings from history, philosophy, law, sociology, psychology, economics, public administration, policy studies, area studies, international studies, civics, and a variety of other sources. Any real coherence in political science exists only at the broadest conceptual level...’. (Siegelman, 2002, p. viii).

A second area of concern for pessimists is that political science (like the rest of the social sciences and humanities) will remain a ‘non-paradigm’ field in Kuhn’s terms, where intellectual disputes constantly threaten to ‘reset to zero’ established results by challenging their provenance, methods, empirical validity or theoretical premisses. Because of this ‘pull it up to examine the roots’ approach, political science will not become what Randall Collins (1994) famously called ‘High-Consensus, Rapid-Discovery Science’ as found in the physical sciences. Beginning around 1600 onwards and moving at an accelerating pace over time all the STEM disciplines⁵ were distinguished by ‘high consensus on what counts as secure knowledge and rapid-discovery of a train of new results’. A ‘law of small numbers’ in intellectual disputes still operates in these disciplines (see Collins, 1998), but *only* at the research frontier itself:

‘It is the existence of the rapid discovery research front that makes consensus possible on old results. When scientists have confidence they have a reliable method of discovery, they are attracted by the greater payoff in moving to a new problem than in continuing to expound old positions. The research forefront upstages all older controversies in the struggle for attention. Because the field is moving rapidly, prestige goes to the group associated with a lineage of innovations, which carries the implicit promise of being able to produce still further discoveries in the future. Rapid discovery and consensus are part of the same complex; what makes something regarded as a discovery rather than as a phenomenon subject to multiple interpretations is that it soon passes into the realm of consensus, and that depends upon the social motivation to move onward to fresh phenomena’ (Collins, 1994, pp.160-1).

By contrast, in fields without assured rapid discovery methods, not only is debate between alternative positions pervasive, but academic prestige can often best be built by debating or reinterpreting ‘fundamentals’, ‘the cannon’ or classic texts over (and over) again. In this light, political science certainly has recurring-but-moving-on debates – for instance, modern theories of the state have remained recognizably connected across two decades of modern debates (compare Dryzek and Dunleavy, 2009 with Dunleavy and O’Leary, 1987).

The third source of current pessimism about political science concerns the continuous pressure of hard-to-predict changes in human behaviour and political life. Developments from ‘left field’ continuously catch observers unawares – ranging downwards in scale from the advanced industrial countries’ credit crunch and recession in 2008-10, through the 1989-91 collapse of the Soviet bloc, to the army

coup in Thailand in September 2006. Our inability to anticipate creates a huge demand for forms of study that simply document and update. Pervasive and continuous change allows ‘collecting’ tasks necessarily to absorb many energies of political scholars and it provides essential raw materials for any developed political science. An ‘über-current affairs’ can hence be alluring: ‘A cultivated person’s first duty is to be always prepared to rewrite the encyclopaedia’ (Eco, 1999, p. 21). Yet it also may conduce to the ‘pile of stones’ approach condemned by Poincaré, well captured in the distended ‘factbook’ character of most empirical textbooks, especially in the USA. Students would need the devotion and attention of a saint to distill from them the thinnest possible gruel of theoretical ideas or systematic knowledge of how political institutions and processes work – which must then be re-extracted again in any new sub-field. To make matters worse, the contemporary dethroning of all kinds of meta-narratives (whether liberal progress myths or Marxist dialectics) seems to have been widely interpreted as licensing an increasingly formless empiricism, even in the research literature.

The increasing pace and detailing of the interaction between (especially) empiricist political studies and its subject matter also raises problems in an era of 24 hour news, exhaustive media competition, and the growth of many ‘ideas aggregators’ (including think tanks, ‘public affairs’ specialists, consultancies, specialist media and many professions). Sifted for gold and mediated many times the collective applied interpretations of political scholars may also increasingly *condition* the routine behaviour of the parties, voters, bureaucracies and governments that they claim to simply describe - social science may increasingly create phenomena, as well as study them (Osborne and Rose, 1999). There is also a potential reverse process, where academic research priorities are reset, creating power-suffused discourses that can cripple basic research, and marginalize attention for non-conforming processes and phenomena that cannot be easily packaged as ‘information products’ (Gouldner, 1973, p.79). Yet there are also huge learning potentials in the effort to apply political science knowledge within the state or civil society. It is only within an atheoretical, empiricist version of political studies that the risks of cross-contamination of academic work are likely to become too high.

These problems - eclecticism, professional dissensus, slow discovery and constant social change – are undoubtedly important and difficult. But they do not seem decisive in damning the project of political science. Against the first three I want

to follow up a suggestion of the theologian Bernard Lonergan (1971, pp. 233-4), who wrote:

I wish to propose a convention. Let the term, science, be reserved for knowledge that is contained in principles and laws and either is verified universally or else is revised. Let the term, scholarship, be employed to denote learning that consists in a commonsense grasp of the commonsense thought, speech, action of distant places and/or times. Men [or women] of letters, linguists, exegetes, historians generally would be named, not scientists, but scholars.

To this we can add the explication of Boyer (1997), who distinguished four distinct but linked scholarships, of discovery, integration, application and what he termed 'teaching', but which I have relabelled here as 'renewing the profession' (because teaching aspects are strongly involved also in the other three strands). For Boyer academic discovery reflects 'the commitment to knowledge for its own sake, to freedom of inquiry and to following, in a disciplined fashion, an investigation wherever it may lead' (1997, p. 17). Discovery clearly encompasses the traditional concept of uncovering new scientific knowledge. But it can also involve many other forms of intellectual innovation, including the formulation of new theories, analyses and formulae, methods, philosophies, thematic ideas and memes, and the identification and analysis of unique events, linkages, narratives, interpretations and empirical understandings.

By contrast, integration is concerned with the sorting, sifting, interpretation and understanding of what has been discovered, with the critical academic role of synthesizing, framing and fitting knowledge garnered by discovery and application into a workable framework or paradigm. For Boyer (1997, p. 18) integration is 'serious, disciplined work that seeks to interpret, draw together and bring new insight to bear on original research'.

Boyer's 'scholarship of application' is concerned with the articulation of ideas and knowledge in ways that become useable for human purposes in myriads of different contexts. It raises questions like: 'How can knowledge be responsibly applied to consequential problems? How can it be helpful to individuals as well as institutions?' (Boyer 1997, p. 22). Much of the 'rapid discovery' ethos of the physical sciences has in fact been sustained not by a purist pursuit of 'knowledge for its own sake', but by the industrial and professional translation of knowledge into new technologies for controlling the physical environment, engineering responses to

challenges and managing medical problems. The lines from discovery to application have blurred and shortened, especially in new forms of industrial development and organization. Some quite similar processes have occurred in the social sciences and creative arts/design (CAD) disciplines, albeit involving much tinier levels of resources. Even in the humanities, applications have become important in fuelling new academic thought (e.g. think of the impact of genetic research on medical ethics and related law).

The fourth type of scholarship involves renewing the profession via transmitting and passing on knowledge in carefully designed and accessible forms to students and external audiences. For Boyer this 'is a dynamic endeavor involving all the analogies, metaphors, and images that build bridges between the teacher's understanding and the student's learning. Pedagogical procedures must be carefully planned, continuously examined, and relate directly to the subject taught' (1997, p 24).

This is a large landscape and there is no room for doubting that political scholarship (along with political science) is a legitimate and demanding area of specialization. A very large proportion of 'discovery' work in our discipline involves political scholarship, as does much of the integration involved in building narratives of the unique development of different political situations and settings. This possibly very specialist knowledge is what political scientists (and many others outside the discipline) rely on to make sense of our political world, to draw comparisons and form analogies. And it has important implications too for the scholarship of application, an aspect of political scholarship that has been developed in psephological, electoral and party analysis, and in some 'advice' areas of international relations and of public policy and public management.

By contrast, political science as I have defined it (concerned with formulating law-like propositions and achieving universalizable forms of knowledge) is heavily involved in the scholarship of integration, although some forms of discovery (such as those concerning deductive reasoning, and the development and movements of non-common sense indicators) are also important. This relative distance from discovery and some forms of understanding and application remains controversial with many political scholars, but it is by no means unique to political science. 'The sciences do not try to explain', said John von Neumann (1961, p. 495), 'they hardly even try to interpret, they mainly make models'. Taking it one step further, Samuel Karlin (1983)

argued that: ‘The purpose of models is not to fit the data, but to sharpen the questions’ (quoted Buchanan, 2000, p.85).

Yet the past concentration of political science on the scholarship of integration has undoubtedly been overdrawn in other areas, especially the neglect of applications. In political life application is potentially very important as a kind of ‘discovery learning’, in which stimuli or interventions are made to the political world and results are systematically tracked and traced out. Yet political scholarship remains largely ‘postdictive’ not predictive: ‘Most numbers published in political science are dead on arrival: once printed, they are never used for anything. We can do better than that’ (Taagepera, 2007, p. 114). Of course, the relative importance of theoretically established consequences is always a matter for empirical determination and investigation, as demonstrated in Mackie (2003)’s detailed evaluation of the consequential insignificance of Rikerian cycles and Arrowian impossibility proof for the actually existing operations of liberal democracy.

Looking more fundamentally, Lonergan argued rather controversially (in another work) that ‘Common sense is not concerned with the relations of things to one another.. [It] has no theoretical inclinations. It remains completely in the familiar world of things for us’ (1958, p.175). Since Lonergan’s definition above also links scholarship to ‘a commonsense grasp of commonsense thought’ this kind of characterization may make hackles rise. It may seem to characterize political scholarship as a second class form of knowledge, a set of pursuits that might not make it as front-rank outputs in the UK government’s new Research Excellence Framework.

In fact there is no such implication here. Rather than link it to Lonergan’s ‘common sense’, I see political scholarship as producing a high-end and specialist form of what Lindblom and Cohen (1979, p. 13) call ‘ordinary knowledge’, by which they mean knowledge that has not been formally validated or established through professional social inquiry. ‘Everyone has ordinary knowledge - has it, uses it, offers it. It is not, however, a homogenous commodity. Some ordinary knowledge, most people would say, is more reliable, more probably true, than other’. In no sense am I arguing that ordinary knowledge (or indeed even common sense) in political scholarship is necessarily less complicated, less intensively or systematically acquired, or less difficult to process and organize than is political science knowledge. As Lindblom and Cohen put it the key need is to develop ‘useable knowledge’, and on

these criteria the invaluable ordinary knowledge produced by political scholarship ranks highly.

Lindblom and Cohen also stress that in *understanding* the social world the products of professional social inquiry (including a ‘vanguard’ political science as defined here) are at best scattered pinpricks of knowledge on a large canvass whose meaning must necessarily be constructed holistically. Thus political science outputs must *necessarily* be interpreted and given meaning (by all of us in academe as much as by policy-makers or administrators) within a massively larger fabric of ordinary knowledge, much of which involves complex and esoteric narratives and understandings that are constructed (and continually re-constructed) by political scholarship.

Lastly I agree with the useable knowledge idea that political scholarship and political science also stand on level grounds in deeper philosophical and epistemological terms (Lindblom and Cohen, 1979). There is no special authority claim for professionally validated political science propositions, for several reasons. Not only can political science inherently never be comprehensive or self-contained, but in addition, its methods cannot claim special authenticity. As Paul Valery (1970) said: “‘Science’ means simply the aggregate of all the recipes that are always successful. All the rest is literature’. A political science seeking law-like propositions may well face more difficulties as a result of social changes, in ways that political scholarship’s more resilient and flexible narratives do not. Human beings always retain the reflexive capability to invalidate any ‘law’ governing their behaviour, *especially* when they become aware that such a ‘law’ exists.

The relatively slender fabric of modern political science thus stands alongside and depends upon the older and larger body of political scholarship, with which it almost necessarily remains on level-pegging in intellectual terms. The two forms of activity have different roles but an integral relationship. And given their joint under-development, it is no surprise that political science at least still seems to easily meet the criterion suggested by Max Gluckman (1965, p. 60): ‘A science is any discipline in which a fool of this generation can go beyond the point reached by the genius of the last generation’.

2. Creating a universe of data

We have examined, as it were, a number of specimens, marked the most important differences, arranged them in classes according to these differences and given to each class a name.. Now this seems to me a most important and necessary part of any science of politics...

Sir John Seeley ⁶

If there is a rule that no reflecting mind should ever forget, it is that comparison should be between comparable facts... Purely external analogies are not sufficient for building a positive science.

Jacques Novicow ⁷

It is now more 31 years since William Anders' famous Apollo 8 'earthrise' picture revealed our planet as a galactic presence framed behind the alien landscape of the Moon, and (along with many other factors) began to change human consciousness of global phenomena in many dimensions. In political scholarship a wealth of mostly rather rhetorical books and papers have discussed 'globalization', often narrowly construed in international relations or international organization terms. But when we research the core topics of political science now – such as elections, parties, legislatures, executives, policy-making, public management, sub-national governments, public policy issues, and (yes) foreign policy and international relations - how far can we draw upon well-ordered data sets and literatures that bring together and marshal for us the cumulative fruits of political scholarship's and political science's activities across the whole world? What collective resources and academic endeavours can we point to that are globally scaled, designed and pursued without ethno-centric assumptions, and made widely available to the scholars and political scientists of all countries?⁸ What theories and methods have we developed for encompassing and analysing the whole of the data-universe on political processes in many diverse contexts that is out there and available to us?

Simply framing these 'how far' questions is almost enough in itself to identify the only plausible answer as 'Not much'. There are many small databases of information at the cross-nation state level assembled by particular scholarly research teams at different periods. They are often very variably and inconsistently bounded in

terms of time periods, countries covered, variables included, and analyses undertaken. Some of these stay current for a few years, but many lapse and are discarded after a short time, withering on the vine as their coverage is outclassed or their period of currency recedes into history. A very few international efforts exist to try and standardize data collection, in political or electoral surveys, for instance.⁹ But masses of objective data remain uncollated. Our inherently under-scaled analysis across countries almost never deploys any overall conception of global political analysis. Instead studies are framed within a ‘comparative politics’ (or even more restrictively, a comparative public policy) paradigm that emphasizes all the elements included in column I of Figure 1 below.

Defining what an alternative approach would look like is by no means easy. As Keynes (1936, p. vii) said: ‘The difficulty lies, not in the new ideas, but in escaping from the old ones, which ramify, for those brought up as most of us have been, into every corner of our minds’. However, column II shows a preliminary effort to sketch an alternative paradigm, which I have labelled as ‘global political analysis’. It is no more than a sketch at this stage, and I can cite no supportive sources that make the case for it directly, or indeed advocate anything much like it in the existing literature. Nevertheless, some of the methodological building blocks for this deliberately labelled ‘sketch’ are already available, and it is not simply a utopian design, but draws substance from many different pioneering studies that have explored this or that particular aspect. A critic might well argue that at this stage this is no more than a compendium of specific remedies for the problems with the comparativist paradigm identified in column I. But looking for alternative paradigms must always start with accumulating puzzles and problems of some kind.

Since I hope that Figure 1 is fairly self-explanatory, rather than work through it at more length when space is already short, I hope it may be useful to instead consider two more concrete cases of where standard comparativism is a contemporary dead-end, whatever its past achievements. Because there is no point in attacking straw

Figure 1: The basic pre-suppositions and set up of comparative politics, contrasted with a preliminary specification of global political analysis

	I: Comparative politics approach	II: Sketch for a global political analysis approach
<i>1. Underlying substantive premises</i>	<p>(a) Nation states are (and are likely to remain) core units of organization in all aspects of political life and public policy processes.</p> <p>(b) World-regional and international organizations and institutions are an important overlay upon nation states, but their internal operations also reflect strong nation state and domestic politics influences in mediated or specialized forms.</p>	<p>(a) Nation states are one important level but not necessarily the dominant units of organizing political life.</p> <p>(b) Analyses at national level are often inappropriate for assessing key propositions in political science – e.g. Duverger’s Law cannot be assessed using national-level election data but only in district-level analyses.</p>
<i>2. Characteristic units of analysis</i>	<p>(a) Nation states¹⁰</p> <p>(b) Almost always to the exclusion of any other units of analysis.</p>	<p>(a) Aggregate national data on countries and sub-systems within countries, ideally world wide but in the interim covering many comparable countries</p> <p>(b) Disaggregated sub-national political contests and systems and government/policy systems across many (all) comparable countries</p> <p>(c) Both (a) and (b) are contoured and patterned into multi-level formations reflecting networks, influences flows, and the segmentation of sub-national areas or processes into discrete sub-systems</p> <p>(d) Massed data on sub-national events and transactions of defined kinds, sharing a common meaning across many (all) countries</p> <p>(e) Massed data on individual events, transactions and responses, again defined and collected in common ways across many (all) countries</p>

	I: Comparative politics approach	II: Sketch for a global political analysis approach
<i>3. How units are weighted</i>	<p>(a) In virtually all analyses each country counts for 1, and none for more than 1, despite gross differences in the size, salience and meaning of data points across countries. For example, in analyses of elections or public policy-making micro-states (such as Iceland, Luxembourg and Malta) count for 1 alongside small countries (such as Singapore and Ireland), large nation states like the UK or Germany, and country-world bloc units like the USA with 300 million people and India or China (with over a fifth of the world's population each).</p> <p>(b) Analysis relies only on including 'size' variables in regressions (along with vectors of other nationally averaged country characteristics) to identify influences arising from different country situations</p> <p>(c) For larger states there are no controls on the inescapable sublimation of discrete phenomena inherent in 'averaging' across electoral districts, sub-national policy systems or whatever, when using only national data.</p>	<p>(a) In all analyses separate data points are entered in datasets from the outset to reflect all independent situations. For example, if we judge that India (with 1.1 billion people) includes (say) 400 distinct sub-regional labour markets, then in an analysis of how labour market trends affect political mobilization we include 400 distinct data entries for India, as against 1 for Malta or Iceland. Similarly in the USA there are (at least) 51 different prison systems (one for each state plus the federal system): so in an analysis of prison policy-making we need 51 different American data points, against 1 for Singapore or Luxembourg.</p> <p>(b) All analysis is conditioned from the outset by the need to cover different nation states or sub-national units that have the same substantive meanings or fundamental salience for the political or policy processes being analysed - defined by core comparabilities of population and economic activity, the discrete determination of outcomes or policies, or other objectively determinable criteria.</p> <p>(c) Data are never averaged into or compared across entities that are fundamentally non-comparable in terms of their substantive meanings, fundamental salience or role in global-scale systems.</p>
<i>4. Intended scope of analysis</i>	<p>(a) Varies from a handful of countries in qualitative work to larger numbers of countries (up to 70+) in quantitative analyses.</p> <p>(b) Country selections range from ad hoc justifications to the maximum number of data observations reliably collectable by a single research team, although often drawing on other sources for individual variables such as country scorings.</p>	<p>(a) The interim goal is always to marshal <i>all</i> available and substantially comparable data and information.</p> <p>(b) However, given the scale of these ambitions, sub-global networks or groupings of countries (such as the EU or APEC) may make a start by covering their own world-regions, within agreed global standards..</p>

	I: Comparative politics approach	II: Sketch for a global political analysis approach
<i>5. Typical data set</i>	<p>(a) Maybe contains 10,000 data points and covers perhaps 1,000 events or observation points, usually all at national level.</p> <p>(b) Many datasets comparing sub-national units of government or aggregate patterns of political behaviour exist for single-country studies (and some few-country studies). But they are usually framed well outside any ‘comparativist’ perspective.</p>	<p>(a) Contains from tens of thousands up to millions of data points.</p> <p>(b) Extensively covers the global or comparative analysis of data for sub-national governments or political activities organized at sub-national scale.</p>
<i>6. Dominant methodological approaches</i>	<p>(a) Quantitative analyses use ‘single best algorithm’ competitions, with only rudimentary segmentations of groups of cases in the complete data set – chiefly because of small N problems. ‘Root cause’ and multiple causation tracking analyses are elementary and rely on effects showing up in the single-best algorithm competitions (using inherently pre-averaged national data for larger units).</p> <p>(b) Qualitative work uses primarily literary case studies of small set of countries (2 to 6), often selected on near-random or ad hoc criteria</p>	<p>(a) Quantitative analysis uses diverse approaches, including heavily segmented analyses made possible by much large Ns, and hierarchical, multi-level regressions (Gelman, 2006; Gelman and Hill, 2007, Franzese, 2005). Expanding the sample of units (contests, localities, regions, countries), ideally using the universe of units, means that the ‘context conditionality’ of current theories (their ‘scope conditions’) become much more important. Theories that have worked for EU or OECD countries, for instance, may not be more widely applicable. A key empirical implication for regression analysis is a need to use more multiplicative models (Kam and Franzese, 2007).</p> <p>(b) Qualitative comparative analysis (‘crisp set’) and fuzzy set approaches are key methods for systematically considering necessary and sufficient causes in qualitative analyses of multiple cases for which rich data are available (Ragin, 2000, 2003; Rihoux, 2006).</p>
<i>7. Explicit or hidden methodological</i>	<p>(a) Human societies are easily categorizable (explicit assumption)</p> <p>(b) Macro-institutions and macro-cultural factors</p>	<p>(a) Human societies are just as complex to understand as physical phenomena, hence the most inclusive possible universe of data is always needed (explicit assumption)</p>

<i>assumptions or commitments</i>	determine all major differences between nation states (hidden premise) (c) Useful null hypotheses can be defined using ad hoc or ‘ordinary knowledge’. In particular, differences across country contexts can be compared in ‘face value’ propositions (hidden premise).	(b) Multiple causation is pervasive in social systems and operates on four integrated case levels (the individual, social groups, sub-national communities and national society, see Sober and Wilson 2000) as well as via single-variable level influences./continues overleaf
	I: Comparative politics approach	II: Sketch for a global political analysis approach
<i>7. Explicit or hidden methodological assumptions or commitments ... / continued</i>		(c) Micro-institutions and micro-cultural factors (including near-one-off factors) may extensively condition the influence of macro-institutions and macro-cultures, e.g. causing a general pattern of influence to switch on or off, or even go into reverse, in particular contexts (explicit assumption) (d) Useful null hypotheses can only be constructed via prior theoretical analysis, in which apparently ‘common sense’ phenomena are separated into analytically comparable conditions (explicit assumption)

men, I have focused on two examples of scholarly work that asks excellent questions and uses innovative methods, close to the forefront of standard comparativism and looking across many countries.

A. In their classic 1979 book *Seats and Votes* Rein Taagepera and Mathew Shugart sought to firm up Duverger's Law (roughly that plurality/majority systems inherently tend to produce two party systems) and Duverger's Hypothesis (roughly that proportional representation systems tend towards multi-partism). They showed a regression line based on the average national data from many countries' elections that seemed to integrate the Law and the Hypothesis and to give it a particular empirical specification. Yet their analysis was already out of date in ignoring the point first made by Aaron Wildavsky (1959) that Duverger's Law can only be tested using district-level data. And even in 1979 it was a little crude to use national election data only in a highly averaged form, generating single data points for countries' ENP levels across whole periods – although this procedure has often used by other influential authors (e.g. Lijphart, 1999).

Taagepera's 2006 book, *Predicting Party Sizes* pushes the research forefront a good deal further on. He notes that in every election (whether in a plurality rule or PR system) there is a 'horse-race' tendency for voters and the media to focus on the top two parties P_1 and P_2 , generally boosting their support at the expense of the smaller third and subsequent parties ($P_3...P_N$). He then energetically sets out to determine empirically how strong this 'suppressive' effect is for the $P_3...P_N$ parties, using data on 652 separately entered elections across 46 countries and sophisticated estimation method producing plausible-looking effects (Taagepera, 2006, Ch.8).

Yet the design has a fundamental problem, because of its unanalysed reliance on the comparativist procedures in Figure 1. In many of the 46 countries covered there are parties with strongly regional patterns of support, either because they are explicitly nationalist or regionalist in their ideology, or because their support is de facto limited to particular areas or ethnic groups. Thus in the UK the fourth largest party from the 1970s through to the late 1990s was the Scottish National Party, which stands only in Scotland and hence has a maximum appeal of just 8 per cent of the UK's voters. Yet Taagepera uses only UK data in estimating how far the SNP's support was suppressed by voters' focusing on the Tory/Labour horserace at Westminster – which inherently must mis-state this effect. And what is true for the SNP is also true for all other parties with regionally-limited support in the 46 country data set. By contrast if Taagepera

had instead used data at election district level, his estimation approach would have been entirely appropriate and a much larger dataset would also have greatly increased the accuracy of the estimated suppressive effects.

B. In *The Economic Effects of Constitutions* (2003) the political economists Torsten Persson and Guido Tabellini outclassed previous somewhat ad hoc comparative models by developing deductive economic models and tested (simplified versions of) them in methodologically sophisticated regression models on a large and rich (but essentially standard-form) country dataset (Acemoglu, 2005). They also found a dramatic effect for parliamentary systems to have significantly larger public budgets than presidential systems, after statistically controlling for many other influences. They ‘predict’ this effect by developing a highly simplified (‘toy’) model of the two systems whose essence is as follows. In all presidential systems a single chief executive must submit to at-large election. In pursuit of maximum votes she internalizes a public interest judgement and avoids beggar-my-neighbour distributional policies. By contrast in a parliamentary system with a stylized three districts, there are strong incentives for a majority coalition of representative from two districts to maximize their constituents’ welfare by redistributing resources from the constituents of the third (losing) district. Hence, the argument goes, parliamentary systems engage in more redistributive public spending.

The chief problem here is that the standard comparativist approach has all the problems identified above in column I of Figure 1, so that it is hard indeed to know what meaning to attribute to the single best algorithm outcome of the regression analysis, when the underlying data points cover entities of such substantive difference. More particularly, Persson and Tabellini clearly assume that the macro-institutional distinction between presidential and parliamentary system makes sense in terms of their deductive and empirical models. But does it? There are anomalous classifications (notably Switzerland’s highly collegial executive with an annually rotating PM is classed as ‘presidential’ because the PM cannot be removed by Parliament). There are very few cases of liberal democratic full presidential systems, and the usual problems of categorizing ‘semi-presidential’ but also parliamentary countries.

More seriously it is easy to point to micro-institutional effects not covered in the country data that change the workings of the core ‘toy’ model in statistically

critical cases. For instance, in the UK a tiny and obscure component of the House of Commons standing orders says that ordinary MPs can only propose any legislative change that adds even an extra £1 of new public spending if they have a prior certificate agreeing the proposal from a minister, which is never given. From the UK this provision has made its way into the constitutions of all Westminster system countries, rather neatly negating the premiss of Persson and Tabellini's parliamentary model for an important sub-class of such systems. If these countries indeed spend more than presidential ones (after multiple statistical controls), it is certainly not because of legislative log-rolling, which their set-up makes uniquely hard to achieve.

Meanwhile in the USA the legislature actually dominates all the later stages of budget-setting and there is abundant evidence of pork-barrel and special interest attachments to budget bills (especially 'earmarking'), many of which survive rushed last-minute scrutiny in Congress. Again because of micro-institutions, in a key presidential case (within only a small group of such countries) there is in fact the opposite of budgeting for at large elections. Instead, across all liberal democracies, the USA is the limiting case of legislative dominance leading to common pool resource problems, the polar opposite of Persson and Tabellini's deductive model. The problem here is a lack of commensurability in the level of complexity between the deductive and empirical models and the phenomena to be explained. As Einstein once observed: 'Things should be made as simple as possible, but not simpler' (quoted, Buchanan, 2007, p. 113).

3. Looking deeper (in the digital era)

Ludwig Wittgenstein: 'Why do people say that it was natural to think that the sun went round the Earth, rather than that the Earth turned on its axis?'

Elizabeth Anscombe: 'I suppose, because it looked as if the sun went round the earth'.

Wittgenstein: 'Well, what would it have looked like if it had looked as if the earth turned on its axis?' ¹¹

The essential counterpart of enlarging the universe of data is to remodel political science and political scholarship so as to move out from an era of data poverty and analysis constraint, typified by the toy data sets and ‘falls at the first hurdle’ analysis of conventional comparative politics. We have all been socialized and educated on the methods appropriate to scanty evidence, and much of our training has emphasized data reduction to fit within the constraints of limiting methods and analysis technologies. Five specific shifts are needed, briefly explored below – becoming far more attentive to theoretically informed problems and associated null hypotheses; embracing digital-era data-gathering; moving from sample methods to census methods in analysing organizations; shifting from surveys to analysing transactional data in studying mass behaviour (ideally in real time); and developing new ‘instrumentation’ informed by new-to-political-science theoretical perspectives.

A. Theory-framing and close analysis. We need to look much more closely at taken-for-granted political phenomena, recognizing that in the physical sciences advances have generally not come from grappling with ‘things as they are’. Instead scientific advances have often come from small, marginal-looking or apparently esoteric ‘puzzles’ that only become apparent when common sense perceptions are abandoned for counter-intuitive ways of seeing. Thus it may seem obvious that the sun revolves around the earth; that feathers inherently fall to earth more slowly than lead weights; or that if I stand on a plain and drop a bullet from my hand it will reach the earth sooner than a bullet fired horizontally from a gun in my other hand - but none of these strong ‘appearances’ is correct (Wolpert, 1989. p.3). Yet core propositions in political science, such as Duverger’s Law, continue to be framed in common sense terms that cannot be precisely operationalized (as I show below). Hence they are amenable to multiple redefinitions and a continuously expanded insulation against falsification.¹²

For political science the core of an effort to look deeper must be to produce theory and collect data in ways that continuously sets observation of what actually occurred in a given situation within a theoretical and empirical ‘field’ of what might have happened. The meaning of events can only be determined within an account that takes cognizance of other possible outcomes, using some knowledge of what would/might have happened anyway to track down the impacts of particular institutions, strategies or political interventions. ‘History is the science of things which are never repeated’, said Paul Valery.¹³ But being able to write some elements

of ‘counterfactual history’ and to tell what is or is not repeated (and at what level) is part of what makes political science far more than history. This may seem difficult to do, yet it is one of the main contributions made by (say) game theory applications to history. (For instance, Josep Colomer’s (1995) account of the series of decisions underlying the Spanish transition to democracy demonstrates clearly the contribution that political science can make compared with conventional political scholarship). We need to show, as T.S. Elliot put it in *Four Quartets* (p.14), that:

‘What might have been, and what has been
Point to one end, which is always present’.

To give another apparently simple but actually rather fundamental example, we can only begin to assess the impact of an electoral system on how voters choose between parties if we know something of the preference structures of voters, and hence what they were trying to achieve with their vote, and what they might have done differently in other circumstances (Saari, 1995). Yet Mackie (2003, pp. 86-92) points out that it is in fact rather dubious that we have ever empirically recovered a *complete* set of preference orderings from any group of ‘real life’ (as opposed to hypothetical) voters. In the UK successive electoral surveys funded at public expense for a long period recovered only first preferences, a practice that opinion polls maintain even now – while academic elections surveys still do not go beyond poorly recording first and second preferences (Dunleavy, 2005). Inherently circular concepts like ‘party identification’ also inhibited for decades at a time attempts to map voters’ comparative evaluations and perceptions of parties. As a result we have only fragmentary evidence of UK voters’ preference orderings, a pattern that Mackie finds replicated across many countries.

Pursuing this example a little further, how far can we construe election outcomes in terms of propositions like Duverger’s Law (DL)? A few analysts have abandoned the self-limiting comparativist obsession with national-level election data, and instead examined the evidence at the correct election district level. In a hugely noteworthy study Chhibber and Kolman (1998) compared plurality rule outcomes in India and the USA (later extended to cover the UK and Germany also). Yet in the course of the analysis their key test somehow gets coarsened into determining whether the effective number of parties is below 2.5 in a district (judged DL-consistent) or above it (judged DL-inconsistent) (Chhibber and Kolman, 2004, p. 48). For US Congressional elections though there are often only two candidates and very rarely

more than three candidates, making looking at ENPs only an exercise in book-keeping tautology. If only two candidates contest an election, the ENP must be below 2, since of 50 possible positive integer outcomes only one (50, 50) yields this result. If three candidates contest an election and all parties gain positive integer levels of support, there are 834 possible outcomes when we rank the parties P_1 , P_2 and P_3 in order of the size of their vote. Only one slot (34, 33, 33) can generate a perfect ENP score of 3, while 345 slots generate ENP scores of 2 or less. By failing to set the empirical outcomes recorded within any consideration of what could inherently occur in US Congressional contests, Chhibber and Kolman end up with no null hypothesis at all. In company with many previous researchers into Duverger's Law, they have no way of separating out what is a substantive empirical outcome from what is necessarily bound to happen, given the initial structure of a candidate race (Dunleavy et al, 2008a).

B. Embracing the digital era. The research methods textbooks and most professional practice in political science and political scholarship have so far barely registered the contextual changes in information and analytic technologies now available as a result of social processes moving online. Some of the most consequential political thought of the modern era (by IT gurus and engineers) has also been ignored (see Brate, 2002). Yet the cumulation of digital era changes has dramatic consequences, threatening to create our own 'slow-boiled frog' problem.

In advanced industrial countries every salient political organization is now on the internet in some form, and their web sites, transactional systems, forums, blogs and other elements are largely open for inspection. Their websites at least (but not intranets) can also be systematically crawled for information, although this needs to be done slowly because anti-virus software will repel attempts to fast-crawl a site (see Escher et al, 2006; Petriceck et al, 2006). Sophisticated network techniques can then be used to analyse the 'graph structure' in the web data. Essentially how the organization communicates with citizen, businesses, civil society or other government or political bodies is fully open for analysis by political scientists.

Of course, what the organization *says online it is doing*, and what the organization *actually is doing* may vary, and many political scholars have accordingly dismissed websites as sources of information, as simple public relations 'fronts' for organizations. There is a potential problem here, but in fact it is relatively easily

managed in most areas of political science, as Figure 2 shows. For online methods to work it is important that the vast bulk (say 95 per cent) of all organizational situations will be covered by the shaded cells 1 and 3, where an organization is either doing a lot or a little, and its web presence (when critically assessed using online research approaches) accurately reveals that situation.

Figure 2: Organizations underlying pattern of activities and their online presence

<i>Organization represents itself online as doing</i>	Organization is actually doing	
	A lot	Not much
<i>A lot</i>	1. Web census analysis correctly identifies high activity situation	2. Façade activity
<i>Not much</i>	4. Organizations with 'stealth' activities	3. Web census analysis correctly identifies low activity situation

The two other possibilities would represent problems, if they are widespread and cannot be detected. 'Façade' activity online might mask underlying inactivity (or activity of a different kind) by the organization (cell 2). Yet this situation is actually much harder for an organization to sustain than might appear – because virtually all significant organizations' web operations are now so salient, complex and interlocked with their fundamental transactions systems and ways of working. Essential business processes operate via the internet (not just press releases or creating a public relations gloss); web-sites are too expensive to maintain properly simply for propaganda purposes; and façade content is anyway clearly visible for researchers (see Figure 3 below), and indeed any serious user.¹⁴ The whole concept of 'digital-era governance' stresses that increasingly government bureaucracies are *becoming* their websites, so that the organizational socio-technical system is increasingly manifest on the web. Indeed it has to be completely manifest or modern pared-down, system of risk-adjusted administration will collapse (Dunleavy, et al, 2008b). Increasingly critical government-to-citizen and government-to-business processes are necessarily going to be online – for instance, in the UK for the first time a majority of self-assessment tax returns were submitted online in 2009. And everywhere in the advanced industrial world social security systems are following tax collection online (Dunleavy et al, 2008c, 2009). The same degree of integral linkage between organizational form and

online processes has also developed amongst interest groups, charities and most civil society organizations.

Of course, there are organizations and places that are exceptions (for a time). Online processes have probably had least impact on political parties, because political leaderships are often been non-tech in their approach and the parties still maintain some degree of organizational separateness (i.e. opaqueness) as an (increasingly failing) incentive to membership. Yet even here, blogging makes intra-party linkages and opinion movements clearer and more traceable than ever before, internet fund-raising has transformed political finance and internet presence has transformed leadership selection processes (Margetts, 2006).

Nor is any of this to deny that some culturally-conservative political organizations (especially government bureaucracies) may lag well behind the digital-era curve, in many cases quite dramatically. In 2008, for instance, the UK's Department of Work and Pensions had 195 million customer contacts, of which less than half of one per cent were online contacts at this time. Put another way, it took the DWP's 108,000 civil servants on average four months to process one email or online application (Dunleavy et al, 2009). Yet this conservatism and slowness of response is itself all perfectly trackable using online research methods.

The last problem in Figure 2 concerns cell (4) covering organizations with large-scale 'stealth' activities, who are doing things politically or delivering public services, but not telling citizens or talking about it on the web. Yet who exactly are these bodies? Certainly this is irrational behaviour for any citizen-facing or business-facing public service bureaucracy in an advanced industrial country, and for most interest groups, civil society organizations and parties also – to be implementing activities yet masking this from the public and society. Only a few special purpose agencies (such as intelligence services and defence agencies) and their opponents in terrorist organizations may actually have critically important classes of activity shielded from web revelation. Even police services and foreign affairs ministries must increasingly operate on the Internet, or risk being marginalized from society's key information networks (Hood and Margetts, 2007; Escher et al, 2006). Similarly many modern terrorist movements rely extensively on online sites to raise funds, maintain broadcast communications, distribute ideological memes and provide for decentred patterns of cell organization (Burke, 2004, p. 39).

C. Moving from reactive sample methods to non-reactive web census and other online methods. Samples and sampling systems for social research were developed from the 1940s and substantially refined by the 1970s. In our field they were chiefly designed to allow us to ‘read’ mass behaviour by looking only at the small amounts of research data that we could feasibly collect or analyse. Sampling theory and significance tests were key in this approach, to ensure that the sample was representative and random, and to guard against the potential for mistakes in inferring from the sample to the wider population. Sampling theory remains the basis for almost all our data analysis methods in quantitative research. Yet now digital era changes have undermined many of the assumptions that underlie our previous methodological thinking.

A key way of analysing websites, again as yet completely uncovered in research methods textbooks, is to use web census methods (WCM), which now constitute a credible alternative to survey- or interview-based approaches in organizational research (see Figure 3). Non-reactive approaches can now create complete and rich organizational data about whole classes of organizations via desk research, reliably and quickly. And ‘fuzzy set’ methods can then tackle the analysis of genuinely complex multiple causation (Ragin, 2000, 2003). The combined results normally outclass both reactive sample surveys and in depth case studies. Why sample, when you can conduct a comprehensive census? Why worry about many aspects of conventional statistical significance, if you can include the whole population in your datasets from the outset (also avoiding all missing case problems)? Why base analysis on a handful of cases (left largely un-situated in the wider field of all similar organizations) when you can cover them all, in detail? This basic shift of approach can be easily varied and extended in numerous ways – for example, using external or internal search engines and specialist media tracker sites to track the foci of memes in macro-content through their incidence in discourses; crawling websites for in and out linkages (Escher et al, 2006); and using modern networking analysis to track influences (Fowler and Jean, 2008; Cho and Fowler, 2007; Christakis and Fowler, 2008).

Figure 3: Comparing surveys of organizations and web census methods (WCM)

	Surveys of organizations	Web census methods (WCM)
Coverage	A statistically representative sample, covering a fraction of the whole population of organizations.	The whole population of organizations.
Instrument defined by	Researchers define a strictly limited number of questions. Question wording effects extensively condition subjects' responses. Any incorrect or inappropriate single question wordings contaminate significant sections of analysis and results.	Researchers identify a large number (several dozen to hundreds) of discrete items to be coded as present or not. Items are structured and weighted to tap theoretically relevant dimensions. Any single incorrect item has a tiny impact on overall indices.
Type of methods approach	Reactive methods (surveys, interviews) – those contacted may report erroneously, edit their responses or misrepresent situations.	Non-reactive methods – items are coded as objectively present/ absent in the organizations' websites, using simple dichotomies
Researcher-subject interactions	Obtrusive – respondents know the study is under way and the precise content of its questions	Unobtrusive – organizations need not be alerted that a study is under way
Costs	Substantial	Low
Key 'meaning' problems	Responses may be artefacts of the questions asked. Responses are a poor guide to actual behaviour. The effects of interviewer and coder judgements may be hard to spot or control for.	Organizational behaviours are established, but the salience and meaning of items coded may be disputed (at both an individual and an aggregate level).
Key problems with interpreting the information gathered	Who exactly in the organization completes and returns surveys varies a lot, and may not be known. The 'authority' status of the actual respondents is typically unclear, along with how far they consulted others.	1. Controlling for 'façade' activity – which shows up clearly in well-designed coding frames. 2. 'Stealth' activities that are not detectable on organizations' websites.
Key technical problems with datasets	Small sample sizes. Extensive non-response. Extensive missing data problems in achieved responses. Mistakes cannot be post-corrected by researchers without going back to respondents	Complete returns are always achieved, without missing data or non-response problems. Mistakes and mis-codings can be post-corrected by researchers.

D. Tackling transactions and events data.

Shifting social organization online has also created new datasets of mass transactions and events. In government, enormous transactions databases have also cumulated in social security, tax, immigration, health, educational and other bureaucracies

(Dunleavy et al, 2008, Chs 6-8). More broadly:

‘Digitization and the cheap storage of phenomenal amounts of data (constantly produced by cheaper processing power following Moore’s Law) have greatly changed the economics of analysing large volumes of information. Massive data warehousing operations have become central processes in sectors like the financial industry, stock markets, retailing, the travel industry, telephony, ISPs and increasingly digital commerce and burgeoning digital distribution networks for text, sound, and now video products. Companies have also created so-called ‘super-crunching’ methods for analyzing these huge volumes of data (Ayres, 2008)... In the space of a few years, companies with pervasive information about their operations and markets have moved from the pages of science fiction towards actuality, with the growth of what Thrift (2005) calls ‘knowing capitalism’ – a strong concentration of societal information in the hands of the most advanced businesses’ (LSE Public Policy Group, 2008, p. 28).

Weakly organized and highly siloed on discipline lines as we are, social scientists currently have very little access to these commercial or governmental transactional databases. Yet they are a critical source of far more detailed insights into social processes than we can muster– a development that has thrown some observers into near-despair. Savage and Burrows (2007 and 2009) predict ‘the coming crisis in empirical sociology’ when companies like Amazon, Google, Tesco or Boots in the UK, or in the USA Walmart, dispose of far more information about the cultural tastes, dietary behaviours and health conditions of the population than anything that academic sociology can assemble (Webber, 2009). They see a ‘danger [of sociology] taking refuge in the reassurance of our own internal world, our own assumed abilities to be more “sophisticated”, and thereby we chose to ignore the huge swathes of “social data” that now proliferate’ (2007, p. 887). The same syndrome applies also in political science.

Again, mass surveys are likely to be a key casualty. Figure 4 compares the comparative efficacy of survey-based data-seeking about mass social behaviour with the insights offered by the analysis of transactional and events data that is now achievable in the digital era. The ability to study behaviour directly (instead of just inferred behaviours based on reactive responses) is a key advantage in column 2 here.

In public policy and public management, transactions data show the same event sequences between citizens and government repeated numerous times, giving rise to very many but still finite patterns, and allowing us to segment interactions into multiple different categories and sub-categories. ‘The sample survey is not a tool that stands “outside history”. Its glory years, we contend, are in the past’ Savage and Burrows (2007, p. 890). (For related debates see Crompton, 2008: Stanley, 2008).

So the challenge for all the social sciences is to act together (or fail apart) so as to secure access to government-held or corporate-held transactional databases, and to take forward the study of bureaucracy and governance in new ways. Unless we can succeed there will be a real threat of obsolescence to older and more conventional forms of political scholarship, of which some earlier signs can already be detected.¹⁵ However, even while we must address these thorny problems of professional collective action, a vanguard political science (as I have sketched it here) can also make major advances in framing research questions in the digital era by defining innovative methods and achieving new applications.

For example, the difficulty and time lags involved in accumulating over-time memberships or activity levels in interest groups mean that we have only isolated studies of their dynamics (see Hansen 1985, 1991). Yet now these dynamics more public and measurable on the internet. We also have the political equivalent of fruit flies in biology – short-lived collective mobilizations online, which allow us to study many iterations of different group dynamics. Thus Margetts and colleagues (2009) analysed the jump points when petitions on the 10 Downing Street website take off and reach critical mass or not, and the factors that are associated with them. Many repetitions of such real-time dynamics offers us a window into processes where previous research has generated few insights.

Figure 4: Comparing mass surveys and the analysis of transactional data

	Surveys of mass behaviour	Transactional data
Coverage	A statistically representative sample, covering a tiny fraction of the relevant population.	All relevant transactions.
Instrument defined by	As in Figure 3. Question wordings with unambiguous single meanings are rarely or never achieved for heterogenous populations.	Researchers monitor actual behaviours, but within limits fixed by the organization originally collecting data.
Type of methods approach	Reactive methods (surveys, focus groups, interviews) as in Figure 3	Non-reactive methods – using data-warehousing techniques
Researcher-subject interactions	Obtrusive – as in Figure 3, but perhaps with less impact since samples are larger and some response biases may offset each other.	Unobtrusive – the behaviours being covered are (mostly) unaffected by the researchers. But people knew that their transactions were being tracked by the original data-gatherer.
Limits on analysis	Small sample sizes inherently limit analysis. Little hierarchical modelling.	Analysis is feasible down to small area levels and hierarchical modelling is fully achievable.
Costs	Very substantial	Low – large volumes of transactional data are already gathered by governments or corporations
Timing	Inherently episodic and usually cross-sectional. However, internet surveys are improving on previous long time lags.	Real-time, with strong across period coverage.
Key ‘meaning’ problems	Responses may be artefacts of the questions asked. Responses are a poor guide to actual behaviour. The effects of interviewer and coder judgements may be hard to spot or control for.	Behaviours have real costs and benefits for subjects, so that ‘revealed preferences’ can be extensively decoded by seeing what people do – especially where they have many real choices open to them.
Key problems with interpreting information gathered	Researchers often reconstruct complex respondent motivations from scanty data using post-hoc rationalizations.	Key to ask: Is the behaviour required (e.g. a tax return) or voluntary (e.g. buying products in competitive markets)? Can subjects control how they transact?
Key technical problems with datasets	As in Figure 3, but more so.	Managing very large datasets.

E. New instrumentation informed by new theory. Specifying problems theoretically and placing a premium on close analysis are important because they hang together as a syndrome (just as much broad brush political scholarship ‘hangs together’ with common sense or ad hoc framings of problems – see Morton, 1999). The physical sciences made much of their early progress through the accurate classification and measurement of phenomena, the analysis of apparently small anomalies and ‘weak signals’, and the interaction of many different (‘swarms of’) methods, practices, and ways of seeing (Gribbin, 2003). Although most people think of Thomas Kuhn’s scientific paradigms approach as concerning only macro-scale, worldview changes, in fact he stressed that changes take place on *many* levels and often in small sub-sets or communities of researchers: ‘Paradigms are accepted examples of actual scientific practice – examples which include law, theory, application, and instrumentation together – providing models from which spring particular coherent traditions of scientific research’ (Kuhn, 1996, p.10).

Our basic data in political science remain widely inadequate partly because there is almost a disdain in the discipline for instrumentation or theory without immediate accompanying evidence of its utility in addressing problems at the common-sense level. The reaction against public choice theory’s heyday period of over-modelling and its close analogies to ‘autistic economics’ has been for contemporary journal editors to demand of any innovation an immediate empirical application. Some of the current malaise in political scholarship and political science reflects a hard-to-avoid scissors tackle. On the one hand, large areas of political scholarship appear to display a near-fatalistic methods complacency, especially on indices. The implied stance seems to be: ‘What’s wrong with a “bog standard” approach’? Consider, for instance, the effective number of parties: the index has some key and known deficiencies (Dunleavy and Boucek, 2003) but it continues to be very widely used as almost the only index measure of party systems in comparative research. On the other hand, many scholars who think of themselves as being at the ‘hard’ or ‘techno’ end of the discipline apparently believe that progress can be best achieved by a premature push for ‘normal science’ closure to new theories, methods and critiques. This is accompanied by a kind of misplaced ‘physics envy’ that stresses accuracy in the analysis of data that yet remain fundamentally flawed (as in newer political economy approaches to comparative politics).

By contrast it seems to me that there are many areas of political science where new theories and associated new methods and analysis techniques are badly needed and where a wider range of fruitful, ‘post-rational choice’ building blocks are already in place, including agent-based models (Laver and Sergenti, no date; Laver and Schilperoord, 2007), network analysis (Cho and Fowler, 2007), biological models (Kremer and Olken, 2009), behavioural economics and more. Take, for instance, the analysis of collective action problems in the light of modern advances in theory. A long-standing strand in political science and scholarship discusses the ‘democratic myth’ that leads big majorities of ordinary voters in surveys to declare that their votes are important and that they personally can influence the results of large elections (Dunleavy and Margetts, 1995). Of course, as professionals we know better, whether we are teaching rational choice 101 or psephologically separating out marginal from safe seats. Individual voting may be normatively desirable and collectively influential, our professional consensus goes, but it is almost always individually negligible in its impacts.

Yet the classical models of probability on which such views depend are now somewhat under siege by newer approaches. Network analysis and ‘small worlds’ theories predict that individual voters have more extensive influence (Watts and Strogatz, 1998). And some empirical evidence supports the claim in US elections (Fowler, 2005). It is clearly much more complex to chart and assess influence flows than statistically calculating objective probabilities (or than just guessing at 1/N impacts). Suppose we have a grid of small and local influence flows, such as voters discussing politics with their nearest and dearest, plus a few work colleagues and pub friends. It might seem cripplingly unlikely that the micro-stimulus of one individual’s decisions or opinions could spread very far in such a ‘small worlds’ set-up. And so wherever we see a large-scale change conventional analysts are inclined to reach for macro-level stimuli (such as an initiative by parties or politicians or coverage by widely-seen media) as the cause. But in fact it may be perfectly feasible to achieve pervasive coverage or knowledge of particular micro-stimuli across a ‘small worlds’ electorate, so long as a minimum number of random (or near-random) links exist that are non-local and that communicate information between otherwise unconnected parts of the grid. Depending on the particular set-up of a situation, in simulations the density of non-local links needed can be very low while the system coverage achieved can be very high. In business the concept of ‘viral marketing’ exploits this

phenomenon, and in the digital era the commercial viability of such approaches and the importance of tipping points are obvious to the mass readership of ‘pop science’ books (Watts, 2003; Gribbin, 2009; Buchanan, 2000).

Similarly the concept of ‘self-organized’ criticality argues that physical and natural systems may often exist in a condition where they are continuously on the edge of dramatic changes in response to small stimuli, even though most of the time small stimuli only have small effects. For instance, suppose we set up an experiment that randomly drops rice-grains across a surface to create a variably shaped pile of grains.¹⁶ The pile will get steeper until a slope is reached that is not sustainable and a slippage of grains will occur, most often only a small adjustment but occasionally a major landslide that radically reshapes the topography of the pile. The system will recurringly move back into the same condition of self-organized criticality in which it is not possible to anticipate or predict what kind or order of magnitude of slippage will occur next (Brunk, 2000 and 2001).

Now just as rice grains will pile into slopes systems that are susceptible to big adjustments unpredictably, the same may often be true also of human social organization. In economics the implications may be that market models based around Gaussian distributions need to be radically rethought, and that major crises such as the credit crunch near-collapse of the world’s financial systems in autumn 2008 need to be re-factored into all our thinking (Mandelbrot and Taleb, 2006; Taleb, 2007). Statistical models better adjusted to ‘power law’ phenomena and ‘thick tail’ distributions may be far more applicable in social systems and political processes than we have thought.¹⁷

Flip back to the collective action problem though and perhaps what voters espousing the ‘democratic myth’ have been telling us all these years in survey responses is that the electoral competition between parties (both in close-fought and in potential ‘landslide’ elections) is a finely poised system in a self-organized critical state, where any small input can potentially make a big difference. Indeed, many such cases may be happening on many different scales already, but being mutually offset or bounded in most cases. (How would we know, given the current quality of our data and methods?) So voters might need very low levels of influence in networks to make it individually worthwhile for them to vote, effectively taking out insurance against missing their chance to help forward a large change that they favour, or to bound or offset a big change from elsewhere that would be adverse to their interests. This is

especially true when we remember that at the group level those who systematically do not vote will almost certainly see state power wielded by others, perhaps often against their interests.

Conclusions

A political science orientated to developing general laws and generic knowledge depends on and may well always rest upon a broader foundation of political scholarship that builds up ‘ordinary knowledge’ of ‘things for us’. Political science is an integrated body of thought concerned primarily with Boyer’s scholarship of integration, and to a lesser degree with his scholarships of discovery and application. That does not mean that political science can or should be a high consensus area on the physical science model – for as William Blake noted ‘without contraries, there is no progress’ (quoted, Paulin, 2007). However, a political science conceived in this way has a good chance of developing a rapidly moving knowledge frontier, even while political scholarship more broadly continues to absorb ever-changing political developments, to regenerate and innovate in the light of them, and to reinterpret the canon of conventional political thought.

Two major areas of change seem to be needed. First, we need to ditch comparative politics as a time-limited conception – one that contentedly accepts a habitat of toy models analysing toy data sets with known and evident (‘Emperor’s new clothes’) defects, which cannot be corrected or compensated even by the most sophisticated statistical methods. We need to form a clear ambition to pull together and analyse political changes at the most inclusive (ideally global) level, creating large-scale data sets that escape from the prison of ‘nation state primacy’ assumptions, and can be analysed at highly disaggregated levels and as a product of multi-level causation flows.

Allied with this must be a broadening and sharpening of political science to focus on problems that are deep-constituted by theory, framed in more carefully-defined operational terms, situated within appropriate theoretical and empirical ‘fields’ of possibilities, addressed with new instrumentation and open to being influenced by new-to-political-science theories. Transitioning into digital-era methods also means reducing our dependence on reactive methods, especially sample surveys, in favour of non-reactive methods, especially web-census methods in organizational

analysis and a focus on large transactional databases for studying mass behaviours and event processes.

This is a big agenda to tackle and it will require concerted collective action amongst political scientists and scholars, and often across the social sciences disciplines, to accomplish. But some key foundations for change are already in place. And with many established areas of our discipline already operating under the looming threat of intellectual or methodological obsolescence, the challenge to raise our game by innovating is unavoidable.

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Endnotes

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1 Quoted in Minkin, 1997, p. iv. Minkin's book has much to offer any serious political scientist.

2 http://en.wikiquote.org/wiki/Hal_Abelson Abelson is a well-known computer scientist at MIT.

3 Henri Poincaré (1901), Ch. 9: 'Hypotheses in Nature'.

4 Miligan (2002), p. 294. This quote is the Envoi of a poem (called 'England Home and Beauty for Sale') about UCL architects putting up low-grade buildings in Bloomsbury.

5 STEM means science (including medicine), technology, engineering and mathematics.

6 Seeley, 1896, p. 361.

7 Novicow (1910) quoted in Pichot (2009), p. 31.

8 Ethno-centrism in political science is hard to combat, especially in the dominant US profession where an almost unchanging, single-country network of PhD institutions

dominates recruitment (Fowler et al, 2007) and a ‘science of American politics’ can still be taken seriously. Senior US figures still often talk and write unreflectively, as in: ‘The cold war’s end has been followed by an unprecedented wave of democratization and marketization, and the extension of *American* political, cultural, and economic practices around the globe’ (Schapiro and Deo, 2008, p.1, my italics).

9 One of the best-organized and most successful co-operative groups creating disaggregated many-country datasets is the Comparative Study of Electoral Systems (CSES), on which see Vowles (2009). By contrast, the International Political Science Association, the American Political Science Association and the European Consortium of Political Research, and other powerful country-level political science associations in the UK, Japan and elsewhere have done nothing effective in this key area.

10 Landman and Robinson (2009)’s *Sage Handbook of Comparative Politics* notes that:

‘Today comparativists engage in the quantitative comparison of many countries, qualitative and quantitative comparison of few countries and quantitative analysis conducted in single countries. These comparisons typically use the nation state and annual observation as the basic unit of analysis...’ (p.3).

Google Books’ analysis of this text shows that across its 509 pages (at least 350,000 words), the many authors here mention ‘local’ anything 37 times, ‘sub-national’ anything 10 times, and ‘multi-level’ anything just three times.

11 Anscombe, 1959, p. 52. I have put her account of the conversation into a dialogue form here and omitted the commentary on both participants’ non-verbal behaviour. I am very grateful to Keith Dowding for drawing this quotation to my attention.

12 Looking across modern physics and chemistry journals Colomer (2007, p. 134) highlights three parallel lessons for political science: ‘Variables should be well defined and measurable; the relationships between variables may be non-linear; the direction of causality should be clearly identified and not assumed on *a priori* grounds’.

13 Paul Valery, *Variété IV*, quoted in (Buchanan, 2000), p. 1.

14 Suppose, for instance, that a city local government wants to represent itself as having a joined-up approach to services for the elderly, when in fact its provision remains resolutely siloed, limited and under-funded. It can certainly draft up some nice pages of plausible pictures and high-sounding goals, but it cannot then back up this misleading gloss with all the necessary in-depth website provision needed, nor keep it up to date. If in fact the city has only weak links (or no links) with local health services, the social security department or the housing and transport sections, this will quickly become apparent to anyone trying to secure some help for themselves or an aged relative. Contradictions and loose connections that may not be obvious to a causal website visitor, clicking through in two or three minutes, are immediately visible on closer inspection – especially by a trained researcher spending an hour on the website. Figure 3 also shows how the actual integration of services can be mapped

by evaluating and coding dozens of objective indicators.

15 Cohen (2009) explains a legislative move by an Oklahoma senator to ban US political science from receiving National Science Foundation grants.

16 Per Bak (1997) famously argued in terms of a theoretically-specified hypothetical sand pile. But empirically it turns out that sand piles are more Gaussian and do not in fact develop self-organized criticality - whereas rice piles do.

17 Some early applications have been made in political science, such as Baumgartner and Jones (2009); and John and Margetts, (2003).