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A Loss of Purpose? Sartori and the Current State of Political Science

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Abstract

This article revisits Giovanni Sartori's seminal critique of political science, examining its relevance in the contemporary context. It acknowledges the significant advancements in political science since the early 1990s, particularly in the sophistication of concepts, methods, and data and questions the idea that social sciences can match the 'hard' sciences. Sartori's four identified errors — parochialism, misclassification, degreeism, and conceptual stretching — are critically engaged with, providing a nuanced assessment of their persistence and evolution over time. The article, originally conceived as a lecture for the Annual Congress of the Società Italiana di Scienza Politica, adopts an autobiographical perspective to extend Sartori's critique to broader contemporary issues in political science, advocating for a more constructive approach in addressing these enduring challenges.

1. Introduction

Let me begin by just stating how extremely honoured I am to be here today with you on the occasion of your conference. I do not mean this as merely a formal expression of thanks, but as something that reflects deeper feelings of gratitude. The reason is quite straightforward: from early on in my intellectual life, I have been highly influenced by foundational works of political science authored by Italian scholars; I still believe in the lasting impact of these works on my thinking, but also more broadly on my understanding of what it is to be a social scientist today. Which is why I thought it would be appropriate for me today to use this talk as an opportunity to reflect on our discipline, political science, and its evolution over the past thirty years. I will do so taking as my starting point the work of the most influential Italian political scientist of our time, Giovanni Sartori.

The title of my talk, *A loss of purpose*, lifts a sentence from a well-known and oft-cited article of his, "Comparing and Miscomparing" (Sartori, 1991).[1] It is effectively both a sequel and an update of an earlier article, his famous piece on "Concept Misformation in Comparative Politics" published twenty years earlier, in 1970, still taught in comparative politics seminars across the world and very widely cited (Sartori, 1970).[2] It is indeed, one of these rare articles that have defied the test of time.

[1] This article has garnered 1,211 citations according to Google Scholar, as of January 2024.
[2] It has 5,262 citations on Google Scholar, as of January 2024.

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In those two articles, separated by more than twenty years, Sartori articulates a pointed critique about the way most political scientists use concepts; although his focus was on comparative politics, his points apply broadly to political science as a whole and even beyond, including political sociology and political economy. He argues that we use concepts in a way that is often inappropriate and ultimately misleading, effectively tainting our analysis and undermining our findings. In his words, "a growing cause of frustration and failure is the undetected proliferation of … nonexistent aggregates, which are bound to defy … any and all attempts at law-like generalizations." He concludes that "vis-à-vis the high hopes of three decades ago, comparative politics is, to say the least, a disappointment" Sartori (1970: 255).

Like many political scientists, Sartori shared the idea that the study of politics should aspire to the scientific ideal; that is, it should produce law-like generalizations. Despite some contestation, mostly from the field of political philosophy, this objective remains the driving force behind the enterprise of political science. Yet, despite the considerable expansion of political science that had taken place between the 1970s and the 1990s, Sartori remained pessimistic. He did not see political science progressing in the right direction, and he attempted to explain this failure by identifying fundamental errors with the use of concepts.

Hence my question: is Sartori's critique valid today? In many ways it is not. Political science is much more sophisticated in its use of concepts, methods, and data compared to where it was in the early 1990s, let alone the 1970s. In fact, reading Sartori's piece, it is easy to detect a tone that we often associate with an aging man's rant, what we would nowadays describe as an "OK Boomer" type of tone. As far as I am able recall at least, this must have been my reaction when I first began to read this article back in 1991. I was then a graduate student in my twenties, beginning to research my dissertation. All too naturally perhaps, I was disinclined to pay too much attention to this kind of complaint.

But though Sartori's style might be far from ideal, it would be wrong to dismiss his critique. If anything, he sought to make sense of our inability to achieve the standards of the so-called hard sciences. And who would disagree that this is a goal that we have yet to achieve? In fact, it would be hard to dismiss the obvious fact that despite our growing sophistication, social scientists have still not achieved the kind of quantum leap that, say, the either the life sciences or the study of the physical sciences have achieved in the past. We do know more than we knew about the world of politics in the 1990s or the 1970s and yet, our progress appears to be tiny in comparison with these areas of research.

Now, my goal today is not to discuss why the social sciences are not scientific in the same way that the life or physical sciences are. Despite the rising tide of attacks against science, the social legitimacy it confers, let alone its overall contribution to human development, remains, thankfully, evident. It is thus completely understandable to try to emulate its ways when studying politics and society. At the same time, however, we have placed ourselves in a kind of trap by setting standards that might well prove impossible to achieve. But exploring these critical issues is a different talk from what I have prepared today.

Instead, I would like to focus on Sartori's diagnosis, which strikes me as both relevant and incomplete. Sartori focused on concepts, and it is true that concepts are the building blocks of any analytical enterprise. Unlike "hard" sciences, where concepts are generally precise, consensus-laden contraptions produced by scientists in the glorious isolation of their labs, the social sciences suffer from the problem of natural categories, a term popularized by Emile Durkheim (Schmaus, 1998). We inherit most of our concepts from society and we thus rely on terms that are often imprecise and contested, such as democracy, populism, or civil war. It is just very hard to replace these concepts with more appropriate ones because the way we think about the political world is permeated by the political world. We are in a loop.

But that is not Sartori's point. So let me take up his critique. He reminds us that the point of comparing is to control; it is, in other words, an approximative adaptation of the experimental logic on questions of political and social significance. In other words, he saw the qualitative, small-N comparative politics of his time as a substitute for the experimental method which he thought was out of reach. Given this condition, his critique focused on four errors: parochialism, misclassification, degreeism, and conceptual stretching.

What did Sartori mean by those terms?

By **parochialism** he referred to single country case studies done in a theoretical vacuum, using local terms that were arbitrary and meaningless from a broader, theoretical and comparative perspective. The example he used was of a study of coalition government in a non-parliamentary system, then generalized to include all political systems.

By **conceptual stretching**, he pointed to the creation of artificial categories through the broadening of concepts meant to increase their empirical capacity. His examples were concepts such as constitution, pluralism, or mobilization that were used so broadly as to contain within them a host of heterogenous, even contradictory phenomena.

By **misclassification**, he meant the misallocation of empirical cases to existing categories, real or artificial—what we could describe today as miscoding. He supplied the example of single-party systems that contained both dominant party systems in western democracies and authoritarian single-party systems, an example suggesting that misclassification is not a mere error of coding, but the flip side of conceptual stretching.

Lastly, by **degreeism**, he castigated the replacement of binary concepts by categorical ones and provided the example of coding democracy on a continuum a practice that, in his mind, caused concepts to lose their substance.

Is Sartori's critique still valid—or to put it otherwise, are the problems he identified still with us? The answer is both No and Yes.

On one side, some of these problems, like parochialism, appear less acute today. Parochial case studies do not get much traction nowadays and there is much more awareness of how concepts that we might take for granted in specific contexts are just products of our own societies and do not apply everywhere. In contrast, some other problems are not thought as problems at all. Most political scientists consider that democracy is best approached on a continuum rather than being a binary concept: witness V-Dem.

Yet, in between these two extremes, I would argue that we still suffer from many of the afflictions highlighted by Sartori. Many of our concepts have an artificial flavour to them and we often rely on problematic contraptions so that we can conduct certain types of empirical analysis, resulting in considerable measurement bias. If anything, the problem is amplified given the massive use of datasets. I could give you many examples, but I will spare you.

I would argue, in short, the evolution of political science since the early nineties has been quite spectacular, and yet we have failed to solve some of the problems identified by Sartori.

Normally I would provide a few examples and stop here, but the organizers asked me to speak for 40 to 45 minutes and on top of it, I would like to be constructive rather than just critical! So, what I want to do is to take some liberties with Sartori's critique and broaden his four problems to capture some larger issues. In other words, I propose to conceptually stretch Sartori.

I would like to argue that his critique of parochialism can be broadened to apply to the uses of theory, conceptual stretching to the construction of concepts, and misclassification and degreeism to measurement and operationalization. By doing this, I will try to discuss current practices in a broader and perhaps more meaningful and constructive way (Table 1).

However, and here is the catch, I thought that rather discuss our current practices in an impersonal, dry, and let's recognize it probably dull way, I should take advantage of this occasion to rely on my own experience and professional trajectory as a source for examples. This way, I could hope to make my talk if not more interesting, at least more entertaining.

Depending on how I count it, I have been actively involved in the study of politics either for over forty years, since I began my undergraduate studies at the University of Athens in 1981 or exactly thirty years since I began my professional career as an assistant professor at Ohio State University in September 1993. These are significant numbers, and they ought to bestow, at least in theory, the gift, if not of wisdom, at least of experience--an additional reason being that in those forty years I have moved between several cities, countries, and continents: from Athens to Chicago, to Columbus, Ohio, to New York, back to Chicago, and then on to New Haven, Connecticut and since five years ago to Oxford, in the United Kingdom. So let me begin, by adopting the format of this life journey.

Table 1. Stretching Sartori's Concepts

Sartori's categories	Broader category
Parochialism	Theory
Conceptual stretching	Concepts
Misclassification & Degreeism	Measurement & Operationalization

Source: own elaboration

2. Athens

As a high school student in Athens, I did not even suspect the existence of a political science discipline, let alone professional occupation. On the one hand, politics seemed to be both highly partisan and ideological, hence not amenable to a cool-headed, even less scientific approach. Because our memory is so short, we tend to believe presently that we live in an era of unprecedented polarization. Yet this is hardly the case. When I was growing up, in Greece during the 1980s, I went through a time of extreme polarization. For example, it was quite common for people to place a party flag on their window or balcony in order to publicly declare their partisan affiliation. Parties were able to mobilize tens of thousands of people and organize humongous rallies. I still remember a question I got from a classmate when I was 13 years old: “What are you?” What he meant by this question was which party I identified with and support. It was almost unthinkable not to be partisan.

Not surprisingly, such a high level of politicization made partisan bias a universal affliction: how could one seriously claim to be an unbiased and objective student of politics? Furthermore, with such a high degree of interest in politics and constant partisan mobilization, everyone had come to believe that they had become experts in politics. But when everyone is an expert, there really is no room for real expertise. Political science, at least for those who had heard of it, was thought to be either a stupid pursuit or an outright fraud, either propaganda or opportunism, a way to get into politics and gain an office or a job. In short, the idea that a person would get paid to study politics was considered either hilarious or suspect. No wonder, no one claimed to be a political scientist.

So, I did not know of political science’s existence, but being nevertheless fascinated by politics, I thought that a good compromise would be to study history. History was much more legitimate than political science, both because this was discipline with a long pedigree and because to study the past (the more remote the better) was seen as somehow safer from the ravages of partisan bias. Unfortunately for me, the discipline of history in Greece was positioned in the Faculty of Philosophy, which was also a misnomer. In fact, what the Faculty of Philosophy did was train philologists. And because philologists were at the time assured of a public job as high school teachers of ancient Greek, getting into the Faculty of Philosophy required a stellar performance at the university entrance exams, which in turn entailed an extremely high capacity for memorization of ancient Greek that I was simply unwilling to contemplate or incapable of achieving. As a result, I failed in the highly competitive exams and through the system’s bizarre allocation process I ended up being admitted to the department of Public Law in the Law Faculty, effectively a sort of second-rate Law School.

As I was going to find out throughout in my life, there is almost always fortune hiding in misfortune. It turned out that the department of Public Law was being transformed at that exact time into a department of “Political Science and Public Administration.” In this context, it had just hired for that purpose two young “modern political scientists” fresh off the boat from the University of California, Berkeley and Harvard, respectively. They were smart, young, enthusiastic, and up to date in political science. I was, therefore, able to receive a high-quality introduction to American-style political science—with a welcome twist to boot. Because one of these political scientists was a historian of political ideas and the other an empirical political scientist using data analysis to make sense of modern Greek history, I learned that political science could combine (a) ideas with data, (b) data analysis with qualitative and historical approaches, and (c) an abstract scientific approach with a passion for real politics. To put it in different words, I realized that one could be at once parochial (in the sense of being motivated by the politics of a specific place and grounded in its messy reality) and theoretically motivated and scientific. Of course, I would only be able put this insight into words much later. But there is no doubt in my mind that I absorbed these lessons during my undergraduate studies in an indirect but deep way, because of this unexpected education for which I remain extremely grateful.[3]

To put in it Sartori's terms then, what I took out from Athens was a positive version of parochialism: one that could be both theoretically motivated and passion-driven.

3. Chicago

Needless to say, I found my undergraduate experience to be totally eye-opening. Naturally, I wanted more, and my professors were happy to help, by encouraging me to apply to the top departments in the United States. In those pre-internet times, figuring out this process was almost impossible; the United States felt less like a different country and more like a different planet. The result was that, outside sciences and engineering, very few people applied from abroad and, when they did, the outcome was usually negative; it was just terribly hard to crack the social "code" of the application process. That turned out to be almost my own experience. I applied to a dozen great departments only to be soundly rejected by all. I then took two years off, served my compulsory military service in the Greek Navy, improved my English, studied harder for the GRE exams, and applied again. Again, I was rejected by everyone, but two departments. One of those was the department of Political Science at the University of Chicago. With the help of a Fulbright fellowship, I was able to pack my bags and fly to the United States for the first time of my life. That was in August 1988.

The University of Chicago was perhaps the hardest but also the best experience of my life. It felt like a boot camp that made the Greek Navy pale by comparison. There was no room at the time for failure and failure could easily result from a middling performance in a single class. At the same time, this was also a place that had assembled some of the most creative minds of the time in political science, and where the dominant ethos was that of ambitious, almost unrestricted, open-ended exploration. The department encouraged us students to explore our interests with rigor but with no concern whatsoever for professional etiquette or hierarchy. The goal was to come up with the best possible ideas rather than merely get a job. Indeed, like many of my classmates, I did not expect to find an academic job at the end of my studies: the conventional wisdom at the time was that there were very few academic jobs available anyway; I thought that I would use my graduate studies to write a thesis (i.e. a book) and eventually find an interesting non-academic job. As a result, I felt free to be as creative as I wished to be. I was inspired by my professors, people like, Adam Przeworski whose books *Paper Stones* and *Capitalism and Social Democracy* merged history, mathematical models, and empirical political science; David Laitin who used ethnographic fieldwork with an experimental bent in places like Somalia, Nigeria, and Catalonia to study political culture; Jon Elster whose book *Making Sense of Marx* used rational choice theory to reformulate Marx's theories; Bernard Manin who explored the evolution of our understandings of key political concepts like representation; and Mark Hansen who applied hypothesis testing in a way that was intuitive and stimulating—among many others.

[3] This is a great opportunity to thank here my two teachers: George Th. Mavrogordatos and Paschalis Kitromilidis.

I thus ended up with a thesis that felt completely idiosyncratic and outside the main trends of the time, a study of how Catholic parties emerged in 19th century Europe. I did so using a combination of historical research with rational choice theory. I later realized that in most other departments at the time I would have been discouraged from blending two approaches that were widely perceived to be contradictory to each other and, what is more, on a topic that struck most people as unusual or plain eccentric. But rational choice theory, like many of the approaches we use, is open-ended. When done right, it boils down to a set of insights that helps you decide how to ask your questions, what kind of data you need, and how to organize it and use it to answer your questions; it does not tell you which questions to ask nor does it suggest what the answers are before you do the research. Rational choice theory told me that political actors tend to maximize their preferred goals, but it did not tell me who those political actors were in the first place and what their preferred goals actually were. This had to be ascertained by historical research. In contrast, existing accounts of Catholic parties tended to assume who the actors and their preferences were. These assumptions turned out to be incorrect for reasons that are too lengthy to explain here—a classic case of Sartori's misclassification.

In short, what I learned in Chicago was to question existing accounts and classifications by plunging deeper into context and data—that is, to recognize and correct misclassification. And a way to do this, was to imaginatively stretch concepts—in my case to broaden the concept of political entrepreneurs to include social actors that had been marginalized in existing accounts, such as the low clergy and the lay Catholic people. What I also took away from Chicago was the willingness to be bold and take risks, to prioritize the question over the method and the data, and to come up with new concepts even if that meant raising the bar of empirical validation. Lastly, I learned how to combine new and old methods in creative new ways.

4. Columbus

Contrary to my initial expectations, I was able to land an academic job, albeit in a rather unlikely place, at Ohio State University. I felt very fortunate. This was back in 1993 and it was hard to think of a department that was as "square" as this one, and a university more unlike Chicago. Yet, the fact that this department was willing to take a chance on me even though my work must have looked so different from what they were used to, meant that they were eager to diversify. Given that we inhabit a discipline where our approaches are imperfect, there is always something to gain by bringing together people who deploy different methods.

The political science department at OSU was a place which at the time prioritized the statistically sophisticated study of American politics much more than Comparative politics; many there believed at the time that political philosophy was not a necessary subfield. The department was very good at producing large quantities of well-placed "meat-and-potatoes" type of work, mostly centred around US electoral behaviour; work that was solid but, with the hindsight of time, rather forgettable. I am no longer sure if this paper came from OSU: it showed that the best predictor of turning out to vote in elections was the intention to turn out and vote a few days before. Which is to say that, although the department offered excellent conditions of work, it felt a bit uninspiring.

All in all, OSU awakened me to the importance of a curated methodological pluralism, but also to the professional dimension of academics that I had missed at Chicago (which tells you again the kind of place Chicago was), i.e. the idea that one was expected to specialize narrowly and publish extensively. Originality and big ideas were frowned upon as a mark of unprofessional dilettantism. "Here, we are academics, we are not intellectuals," I was told with authority. Sartori would have probably looked at this attitude as a factor likely to sustain opportunistic conceptual stretching through the reproduction and proliferation of poor concepts with the aim of maximizing publications.

5. New York

A year later, in 1994, I unexpectedly moved to the department of Politics at New York University which at that time was an unremarkable department, albeit one located in New York City, a city at the time perceived as in the throes of decline. In fact, I had never been to New York before and having spent all my time in the US in the Midwest, I had internalized the perception that it was a dystopic place. Movies like *Taxi Driver* and *Escape from New York* reinforced this view. I almost didn't show up for the interview, but ultimately, I changed my mind and realized how wrong I had been! I ended up going and spent six years there which were happy and productive. My Chicago-induced worldview was strengthened because almost half the Chicago department moved to NYU around that time, including Russell Harding, Adam Przeworski, and Bernard Manin, while Jon Elster moved to Columbia. Mobility, as I was about to learn, was a key facet of both American life and American academia—and despite the occasional disruption it caused it was a source of endless stimulation.

NYU had poached all these people away from Chicago acting like a football team owned by a Russian oligarch or Saudi Sheik. As New York was staging a comeback, it found itself with loads of cash which it could spend to improve its ranking and reputation. And after bringing in all these stars, it decided that it had to become a "high-tech" powerhouse—which it eventually did. As a result, this led to a situation where methods began to drive questions and technical proficiency took precedence over substantive creativity. To go back to Sartori's terms, privileging certain methods and techniques following a narrow technical logic worsened conceptual problems and added "opportunistic measurement" to "opportunistic stretching" in the sense that technique dictated what data to use, rather than the data leading the techniques. In Sartori's term, this was breeding ground for misclassification and degreeism.

6. Greece

At the same time that NYU was transforming itself, I was facing an important personal problem: my past came back to haunt me. Recall that I had attended the University of Chicago on a Fulbright grant; it turned out that this grant required me to go back to Greece for a period of two years, a measure meant to stem the brain drain for countries whose citizens received Fulbright grants or, alternatively, to limit immigration in the US. There was nothing I could do. Suddenly I was forced to return to Greece with no research plans whatsoever.

Again, disaster bred opportunity. My forced exile helped me develop a new research project on civil wars which evolved into the research agenda that I am still working on. My idea was to take advantage of my presence in Greece to do exploratory field research on how people behave amid a civil war—how they decide whom to support, whether to join an armed faction or to commit violence against their neighbours. The standard account was that individual behaviour was an expression of pre-existing social and political cleavages, but there was no research on this topic. My foray uncovered a different, and puzzling, mechanism: rather than just political preferences leading to violence, violence often was critical in shaping peoples' allegiances; furthermore, violence was often the result of military rather than political considerations. This realization led me to completely reframe my question and therefore my research project and focus on understanding the production of violence. This was an instance of "good parochialism" at work, whereby the context suggested and forced me reframe the question. It also forced me to develop new concepts, like territorial control, and come up with appropriate empirical measures that would have been impossible to even imagine in the absence of this type of deep engagement with the context. This research would eventually become *The Logic of Violence in Civil War (*Kalyvas, 2006).

7. Chicago

With my exile over, I left NYU and moved back to Chicago, this time as an associate professor. It is, I think, a recurring academic fantasy to become a professor in the department one was a student, and I couldn't resist the temptation to fulfil it. While there, I worked on my civil war project and as is often the case, I began to test the waters by submitting my first papers to various journals. They kept being rejected. I quickly realized that it was just impossible to publish them. On the one hand, the emerging field of civil war studies was at the time overwhelmingly macro-oriented, using country-years as units of analysis and focused on the causes of civil war rather than the causes of violence in civil war. My work which focused on individual behaviour and local dynamics did not fit in at all with this agenda and was, therefore, bypassed. On the other hand, I argued in favour of decoupling war and violence, arguing that these two processes were analytically distinct. Most people then assumed that war and violence were the same: war was violence and violence was war. They, thus, had very little patience for my approach. Perhaps they saw it as a form of conceptual stretching. I had two options. The first was to abandon this project because this type of rejection meant that I was wrong—and if not wrong, certainly about to commit professional suicide. The second was to follow my intuition, for better or worse, and persist.

I decided to persist for two reasons. First, I trusted my intuition. Obviously, we often think we are right when we are, in fact, wrong. In my case, however, the strength of my intuition came from the research I had conducted: when you walk where civil wars had been fought and you talk to those who survived to tell the tale, you develop a very different sense of the phenomenon compared to when you just read about it or when you interview high-placed actors. Of course, I tried to counteract my confirmation bias tendencies by using my best professional judgment which, I thought, could not have been totally arbitrary, as it had been shaped in some of the best universities in the world. The encouragement of my former professors and my peers was also key at this stage. Second, I was part of an institution (the University of Chicago in particular, but most excellent research universities in the US followed the same principle) which encouraged risk-taking and the production of work that had a shot in being long-term impactful over those leading to publications with limited shelf life. Departments, in other words, that did not treat their faculty as line workers who had to fulfil yearly productivity norms. That's the cloth great universities are made of.

What I learned in Chicago, then, was how to pursue my intellectual vision even in the face of initial rejection. In my case, I was eventually proven right. However, this vision would still have been worth pursuing even if I had been wrong because this type of failure can be productive. What I very strongly believe I should have avoided instead is the obliteration of intellectual vision and ambition to satisfy intellectual conformism and the prioritization of quick publications over longer-term contributions. This is not part of the four problems Sartori identifies but it is connected to the deeper logic driving his critique, namely his injunction to question current practice even (or perhaps especially) when it is both popular and dominant.

8. New Haven

In 2003, I moved to Yale. *The Logic of Violence* came out in 2006, but most of the work was done in Chicago (Kalyvas, 2006). At Yale, I focused on creating an intellectual community which took the form of a research program, the Program on Order, Conflict, and Violence; its goal was to help transcend existing boundaries between subfields (comparative politics and International Relations) or even disciplines--something nicely reflected, I think, in an edited book we published in 2008, *Order, Conflict and Violence* (Kalyvas, Shapiro and Masoud, 2008). I had a strong intuition from my days at Chicago, that good research requires a community rather than individuals working in isolation from each other. It is daily, face-to-face community that helps generate the kinds of interactions, ideas, good judgment and ultimately confidence that leads to risk taking and important breakthroughs. I was fortunate to work there with many colleagues, graduate students and postdoctoral researchers and I can see today how many works bear the stamp of this environment. Again, this does not boil down to any of Sartori's four points but rather fits his overall perspective.

While I was at Yale, I witnessed the eruption of the so-called "credibility revolution". As a result of the problems encountered by both traditional statistical analysis and game theory, a new research school emerged, heavily influenced by both economics and psychology; it advocated a tighter correspondence of social science with the standards of experimental science. Initially this was to be achieved with field experiments, but eventually new statistical methods emerged that allowed the analysis of observational data in ways that closely imitated the experimental approach. Today these methods tend to be described under the label of "causal inference" and they have become as dominant if not more as game theory or "naïve" OLS regression analysis used to be in the past. The ability to infer a causal relationship between two variables is obviously extremely important and an important component in the evolution of the social sciences. At the same time, however, it is important to keep in mind that no technique is substitute for a deep understanding of the data-generating context and a capacious theoretical imagination. Nevertheless, as much as I welcomed their arrival, I also noticed that these techniques were often used to produce "findings" bordering on the artificial that I sometimes found problematic; these findings required considerable conceptual stretching to materialize.[4] More specifically, and rather surprisingly, the "credibility revolution" appears to have led us back into Sartori's world of parochialism with work that is at once theoretically very broad and technically extremely ambitious yet empirically very parochial. This work typically juxtaposes a highly ambitious title with a very narrow empirical subtitle, something like "The Effect of Democracy on Development: Evidence from South-Central Guinee." Because this work suffers from problems of external validity it requires considerable conceptual stretching to overcome it. Overall, then, it is possible to argue that Sartori's comment about "a loss of purpose" of the discipline applies to the world of the credibility revolution. To quote him directly from thirty years ago: "Let us squarely face it: normal science is not doing well" (Sartori, 1970: 255). Table 2 summarizes this discussion.

Table 2. Sartori's Categories and the Evolution of Political Science

Sartori's categories	Broader category	Negative	Positive
Parochialism	Theory	Causal inference parochialism	Theoretically motivated parochialism
Conceptual stretching	Concepts	Opportunistic stretching	Theory driven imaginative conceptual-stretching Subfield-transcending concepts
Misclassification and Degreeism	Measurement and Operationalization	Opportunistic measurement	Measurement with deep understanding of question and context Methodological pluralism

Source: own elaboration

9. Conclusion

Let me come to my conclusion.

As a discipline, political science has made enormous strides in terms of concepts, methods, and data during the past thirty years. It is a fact that political science has never been as big, as diverse, and as international as it is now--and this conference is a testament to this positive evolution.

Nevertheless, I would argue that we have not succeeded in escaping from Sartori's critique. We still face many of the problems he identified thirty years ago. We still suffer, albeit to a different degree and in different forms, from the problems he identified, from the incoherent use of political concepts to the paucity of theoretical imagination and the proliferation of trivial "findings." It is as if every new development carries with it the afflictions identified by Sartori. This is not to say that we are in limbo; we are sitting on top of more data about politics than we ever imagined, and we have the tools that allow us to analyse them. Yet, we somehow can't turn our findings into cumulative, general, law-like propositions and, thus, predictions.

What to do? I can see three ways to go. One is to ignore these problems and pretend that we are becoming a real science, that this goal is just behind the corner. The advent

[4] For a discussion, see Kalyvas and Fedorowicz, 2022.

of Artificial Intelligence, for instance, might be the last boost that we need. I personally think that this is an illusion, but I also recognize that sometimes illusion is what drives progress.

The second one is to become deeply pessimistic about the current state of affairs and altogether reject the positivist drive toward a more scientific political science. This is the position adopted by the post-positivists. I think it is wrong, perhaps even dangerous. Questioning the value of science undermines it and opens the door to arbitrariness and ultimately autocracy.

Sartori would have rejected both these options: "It is infinitely easier to behead problems by invoking incommensurability or by letting computers do our work while we relax" (Sartori, 1970: 254).

There is a third way, however, which I think would be fully in line with Sartori's spirit. Perhaps instead of only pushing, headfirst, into the same direction of more data and more computer power, in the hope that we would achieve the breakthrough that has eluded us so far, we could instead process the data we have differently and better. We could still aspire to be as scientific as we can realistically be while at the same time recognizing that this might be an unattainable target. And we could try to fill the gaps in our understanding with more care: deeper contextual knowledge, better theoretical imagination, more creativity and, yes, careful consideration to concepts.

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