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Why We Can't Help but Call Ourselves (All) 'Sartorians'

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Abstract

This article, presented at the Annual Conference of the Italian Society of Political Science in 2024, explores the intellectual legacy of Giovanni Sartori and his enduring influence on political science. The author reflects on Sartori's contributions to three key domains: political theory, methodology, and comparative politics. The text highlights Sartori's insistence on theory-driven approaches over purely data-centred methods, as well as his critical stance on the behavioural drift of social sciences. The article also underscores Sartori's dedication to bridging theory and practice, advocating for political science as a tool for addressing real-world challenges. In conclusion, it calls for greater public engagement within the discipline, proposing initiatives like a political science festival to further Sartori's vision of a socially impactful academic field.

The title of my speech "Why We Can't Help but Call Ourselves 'Sartorians'" paraphrases the title of an essay by one of the authors that Sartori reported he had studied during his Florentine refuge after September 8, 1943, while he was trying to avoid being enlisted in the fascist troops of the Italian Social Republic of Salò (Croce 1942).

On the one hand, this title recognizes the pervasiveness of Sartori's influence in political science, even among those considered his opposites, while on the other, it serves as a sort of personal disclaimer.

All the participants in this round table are either members of the original 'Florentine school' (as Sartori himself termed it, listing its members by name) or direct students of that school – second-generation Sartorians, so to speak. I am the only participant who, despite having completed my doctorate in Florence at the Cesare Alfieri Faculty, is neither one nor the other. By saying that, in the end, we are all Sartorians, it's like relieving myself of the embarrassment of not being one 100%, and trying to avoid the feeling of being in the wrong roundtable.

In fact, if this were a strictly scientific conference on Sartori's thought, my speech would probably be out of place. But in this professional setting, on an occasion intended primarily to remember Sartori for the role he played in developing our community and in training all of us, I can perhaps allow myself a slightly different approach, especially since there have been and will be other institutional opportunities when his scientific thought and contributions are remembered by 'certified Sartorians'.

From this perspective, there is even room for some generational, if not personal, memories.

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My doctoral cohort, the third one, was the only one in Italy that had Sartori as a regular professor for more than a month. It was the autumn of 1987, and I remember our first meeting with him in the turret of Villa Fabbricotti in Florence. I recall Sartori climbing the spiral staircase and entering the small classroom with his somewhat gruff demeanour. He was holding a slip of paper that looked like a short shopping list and on which, upon closer inspection, were written obscure sequences of letters and numbers, like TDR 8 or ETP 14.

In fact, these were references to selected chapters of *Elementi di Teoria Politica* (ETP) and *Theory of Democracy Revisited* (TDR), both published in that year (Sartori 1987a, 1987b), and which we doctoral students would have had to present during the lectures.[1] Sartori's classes went in fact like this: it was up to us to introduce the lesson by presenting one of his chapters, then broadening the discussion to other authors and, if possible, criticizing him. Thereafter, he would take charge. I recall we had a couple of lessons per week and were so anxious about having to present Sartori to Sartori that we started preparing our presentations at least a week in advance, also because he was ready to admonish us for every minor logical fallacy or terminological imprecision.

I remember that, in urging our criticisms, he claimed to have no reverential fear of anyone except Karl Deutsch and perhaps Raymond Aron. When he received the IPSA award named after Deutsch in 2009, Sartori recounted an anecdote that seemed to confirm this memory. During a seminar, Sartori had voiced an objection to Deutsch, who responded with a reply full of numbers and statistics, like "a computer, a human computer…", Sartori said. Indeed, after that first occasion, he confessed he never had the courage to argue with him again.[2]

It's something quite difficult to believe, and certainly it was hard for us to believe at the time… so much so that, struck by that memory, as soon as I could I bought Deutsch's *The Nerves of Government* (1963) at a used book stall.

As said, none of us doctoral students who had him as professor almost forty years ago can be called an acolyte of Sartori because of that experience. We have always been very different from each other in terms of interests, approaches and methodologies, and yet I believe that none of us has forgotten those lessons.

And it is not just a matter of personal or generational biography. It is something that has to do with the contribution Sartori made to all of political science, not only to those who had him as a master (the first generation) or as a teacher (my generation).

Sartori described his studies as follows: "My work can be divided into three parts: *(a)* pure political theory; *(b)* methodological studies where methodology is understood as the method of *logos*, of reasoning; and *(c)* comparative politics in the strict sense" (Sartori 2011: 252).

Besides his contributions in the first and third fields (Pasquino 2009), which concern more the specialists in those respective sectors and on which one may agree or disagree, I believe that Sartori's contribution in the second sphere –his teachings on the

[1] ETP 14 was the chapter in Elementi di teoria politica entitled "Tecniche decisionali", and TDR 8 was the one in *Theory of Democracy* dedicated to "A Decision-making *Theory of Democracy*". I remember that well, since I had to present both of them, hoping to take advantage of some familiarity with public choice theories, and of my fortuitous recent reading of the book written by Buchanan and Tullock (1962).

[2] See http://videolectures.net/ipsa09_sartori_kdlecture/

method and logic of social sciences – is even more undeniable and, in a certain sense, universal (Collier and Gerring 2009; Sartori 2012).

In this regard, I would like to briefly discuss three main aspects to show how they have travelled far beyond Sartori's students, and are shared even by scholars who certainly cannot be called Sartorians, and who, in some respects, might even be considered diametrically opposed to his thought.

The lessons to which I refer concerned:

- The role of theory
- Comparison as a control method
- The importance of applied knowledge

Let us begin with the importance of theory.

Political science is an empirical science, but this does not mean that it is data-centred. On the contrary, in his well-known autobiographical essay, first published in the volume edited by Hans Daalder (1997), Sartori wrote, "I have always insisted on a 'theory-rich' discipline monitored by a sound training in logic and method ('methodology'); I never believed in a 'superior' quantified science" (Sartori 1997: 96).

Let us now consider the following quote:

"The ruling emperor of social sciences has no clothes. His quantitative garb is largely make-believe (...) 'a form of mass deception'. Our qualitative understanding of social phenomena has expanded beyond recognition, during the last 100 years. It has produced durable results. Yet, social sciences have not become as scientific as this basis would allow them to be, because they have overemphasized descriptive statistical analysis to the detriment of conceptual model building" (236).

This almost seems like a passage taken directly from Sartori himself. It is, in any case, the same critical objective that Sartori pursued when he discussed the behaviourist drift of American political science.

The same author also writes: "In social sciences, (...) unambiguous prediction – that could prove right or wrong [*ed.* here echoing the idea of comparison as a form of control (Sartori 1991: 244)] – was discounted in favor of statistical 'models' that could go this way or that way, depending on what factors one included and which statistical approach one used" (vii).

I'm not sure how many readers have recognized these quotations. Some years ago, the *Rivista Italiana di Scienza Politica* published an insightful debate on their author (De Sio and Pisati 2010). The extracts are taken from *Making Social Science More Scientific* by Rein Taagepera (2007), who incidentally was awarded by IPSA with the same Karl Deutsch prize that Sartori received only a few years later.

It is hard to imagine anyone more distant from Sartori than Taagepera, ranging from the model of science they had in mind to the type of predictions they sought to make. Sartori favoured a 'science of man', while Taagepera endeavoured to align with the natural sciences. Sartori advocated circumscribing the conditions under which his generalizations applied, whereas Taagepera sought supposedly 'universal' laws. Sartori adhered to the tenets of John Stuart Mill, while Taagepera drew on the toolkit of mathematics and on his past training in physics.

And yet, in some respects, they are precisely what are sometimes called 'strange bedfellows'.

An idea that they shared, and which is apparent in the above quotations, is that of a scientific knowledge that 'walks on two legs', so to speak: a theoretical 'leg', founded on logic and independent of data — 'ignorant,' as Taagepera (2016) sometimes called it — and an empirical 'leg', which serves as a control but is capable of neither generating hypotheses nor providing support for generalizations on its own (Taagepera 2017). It was not by collecting an infinite amount of data on falling bodies that Newton managed to formulate the law of universal gravitation; nor is it by assembling an endless array of values for the fragmentation of party systems that we can better understand their link to electoral systems.

We are speaking not only of the fact that theory is important, but also of the fact that it is considered a priority and a discriminating element in the scientific work of scholars with completely different research characteristics and approaches.

Thus, the divide is not the one that separates qualitative and quantitative approaches; if it were, one would not understand the respect Sartori pays to Deutsch. Rather, it concerns the space and priority given to theory as opposed to empirical data. A qualitative case study does not necessarily leave more room for theory than a quantitative study does; nor, of course, is the opposite true.

A second aspect in terms of which, *mutatis mutandis,* Sartori and Taagepera could paradoxically be grouped together concerns the type of explanation.

Indeed, one might argue that they are both suspicious of additive explanations, preferring combinatory logics. Certainly, the way in which Taagepera mathematically combines his quantities differs from the explanatory configurations used by Sartori; but both remain sceptical of models with many variables where each one additively explains a small part of the phenomenon being analysed.

It is curious that this is also the primary reason why an author like Charles Ragin argues that the comparative method is superior to the statistical method in several respects:

"First", and I am quoting from Ragin's 1987 book devoted to comparative analysis, "the statistical method is not combinatorial; each relevant condition typically is examined in a piecemeal manner" (Ragin 2014); and both Sartori and Taagepera could not agree more.

Ragin then provides other arguments in favour of the comparative method. For instance: "applications of the comparative method produce explanations that account for every instance of a certain phenomenon" (Ragin 2014), where "all exceptions to working hypotheses must be addressed and resolved" (Ragin 2023).

And here, one cannot help but recall Sartori's criticism of Duverger's Laws concerning the relationship between electoral systems and party systems, and of their operationalizations as mere 'frequency laws' unable to account for multiple outliers. Sartori, on the other hand, combines electoral rules and the structuring of party systems to address those 'exceptions' and to reach generalizations with greater explanatory power (Sartori 1994).

Finally, it is again Ragin who writes: "the comparative method does not require the investigator to pretend that he or she has a sample of societies drawn from a particular population so that tests of statistical significance can be used. The boundaries of a

comparative examination are set by the investigator"; an approach that has much to do with scope conditions and Sartori's rules for defining what is comparable and what is not.

Unlike Taagepera, Ragin does not seem like a 'strange bedfellow', given his considerable affinity with Sartori in his approach to the logic of comparison.[3] However, at least judging by their respective bibliographies, Ragin and Sartori do not seem to hold each other in particularly high regard. Ragin never cites Sartori, not even in the re-edition of his *The Comparative Method* (Ragin 2014), nor in the more recent *Analytic Induction for Social Research* (Ragin 2023). And Sartori himself, in his *Comparing and Miscomparing* (Sartori 1991), references Ragin almost solely in a footnote, specifically the one regarding the potential superiority of the comparative method over the statistical one.

One possible reason for this mutual suspicion might be their differing beliefs regarding the purpose of comparison: for Sartori the reason for comparison is control,[4] whereas Ragin explicitly writes that the point is not to test theories or hypotheses but to interpret cases.[5]

Returning to the differences between the comparative method and the statistical method, and the preference for the former over the latter, Arend Lijphart argued precisely the opposite: "there is [...] no clear dividing line between the statistical and comparative methods; the difference depends entirely on the number of cases. [Thus], if at all possible one should generally use the statistical [...] method instead of the weaker comparative method" (Lijphart 1971).

This methodological divergence is not the only difference between Sartori and Lijphart. There is also a substantive one, which concerns the feasibility of Israel's institutional experiment with the direct election of its prime minister. Lijphart considered it feasible, as a sort of *sui generis* presidentialism sustained by the mutual threat of executive no-confidence and legislative dissolution, while Sartori foresaw the institutional structure stalling and frequent elections occurring (Lijphart 1999; Sartori 1994). As it turned out, Sartori was right, and Lijphart was wrong on this point; and this should at least give pause to anyone considering experimenting with something similar again.

However, there is one point on which Sartori and Lijphart certainly agreed: the importance of applied knowledge. Whether they were discussing the effects of electoral systems, the rigidity of government forms, or the institutional arrangements best suited to challenging contexts, their analyses were never detached from some prescriptive purposes for the real world (Crepaz et al. 2000; Lijphart 2008; Sartori 2011). "Above all, ... my emphasis has always been on the conversion of theory into practice, and thus on 'operative' (not operational) science. In my view political scientists are ... required to know (at least better than laymen) how problems can be solved, which reforms are likely to work and, in a nutshell, to have 'know how'" (Sartori 1997: 96).

Conversely, this was one of the trends that Sartori most deplored in the recent developments of political science, namely, favouring pure research at the expense of the

[3] For example, Sartori would endorse Ragin's statement that "the comparative method is based on 'logical methods' [...]; it uses two of Mill's methods of inductive inquiry: the method of agreement and the indirect method of difference" (Ragin 2014: 15).

[4] "One can engage in comparative research for a variety of reasons, but THE reason is control" [of theses, propositions, expectations] (Sartori 2011: 217).

[5] "Many comparativists, especially those who are qualitatively oriented, are not often involved in 'testing' theories per se. Rather, they apply theory to cases in order to interpret them" (Ragin 2014: 11).

theory-practice linkage. In his view, it was as though political science had forgotten its original mission, one of the few areas where Sartori thought economists were better than us political scientists.[6]

So, returning to the title that I initially had in mind: if it is hard for Italian political scientists not to be somewhat Sartorian, recognizing the importance of theory and the centrality of comparison, I believe that for many of us, following him in answering the question "a science for what?" has been more challenging.

This is not a specifically Italian shortcoming, since it was recently observed across Europe in the survey of the profession conducted by Giliberto Capano and Luca Verzichelli. The authors dedicated their book on the fate of political science "to the next generations of political scientists. So that they will never forget that being a political scientist does not only mean carrying out excellent research, writing brilliant, original papers, and teaching students; it also means, first and foremost, contributing towards a better, democratically sustainable society" (Capano and Verzichelli 2023).

In Italy, it seems to me that other disciplines are more attentive to this aspect and cultivate their public engagement more systematically. There are, for instance, two major economics festivals, a sociology festival, as well as festivals for philosophy and law. Perhaps it is time to consider a political science festival, independent and separate from our annual conference. I believe this would be yet another way to be Sartorian on this third front as well.

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[6] It is curious that in the latest issue of the Annals of the American Academy of Political and Social Science, Alan Blinder, an economist celebrated precisely for his ability to bridge research and public engagement, lamented instead the absence of his own discipline on this front, and recommended that his colleagues reduce the gap between economic theory and political relevance. He concluded his piece with a wonderful quote from a baseball player of Italian descent, Yogi Berra: "In theory, there is no difference between theory and practice. In practice, there is." (Blinder 2024: 13).

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