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Bélot, Céline, Magnette, Paul, and Saurugger, Sabine, eds (2008). *Science politique de l'Union européenne* [Political Science of the European Union]. Paris: Economica.

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I

Paradoxically, the study of comparative politics in France has long been, along with the political economy of collective choice, a blind spot in a professional community that was fathered by Maurice Duverger in a country where Emile Durkheim

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made it the only possible scientific substitute for experimentation.[1] The only printed journal dedicated to the discipline (the *Revue internationale de politique comparée*) was created in Belgium (*Sociétés et politiques comparées*, of an entirely different persuasion, is available online at www.fasopo.org/reasopo.htm); the best epistemological book available in French is by two Belgian authors (Jucquois and Vielle, 2000); and the only recent methodological book was prepared at the University of Louvain and was published in English, just like Dogan and Pelassy's earlier book (Dogan and Pelassy, 1982, 1990; Kazancigil and Dogan, 2001; Rihoux and Ragin, 2009). As far as I know, Badie and Hermet's handbook has not had a more recent edition than that of 2001 (Badie and Hermet, 2001), and apart from Mény's classical textbook on the comparative politics of the democratic states (Mény and Surel, 2009; also available in English with a different coauthor and fewer cases, Mény and Knapp, 1998) and the study of Western democracies testing the Lipset–Rokkan model of "frozen cleavages" by the Swiss Hans Peter Kriesi (1994), the only systematic book that does not simply pile up chapters on national cases has been published by two young scholars (Grossman and Sauger, 2007). Although often cited and highly respected, the late Charles Tilly is not imitated (Tilly, 1975; Tilly and Goodin, 2006). Leaving aside for the time being the incipient comparatism evolving from the study of the European Union, we have to go back, in specific fields such as parties and cleavages, to Jean and Monica Charlot (1985), while allowing for, once again, three Belgian scholars' works (Seiler, 2000, 2003; Delwit, 2002; De Waele, 2004).[2]

Mainstream comparative studies in international political science usually look like this: let's suppose a process, a mechanism, a set of organizations or of behaviors, extracted from various contexts; embrace (within reason) as many cases as possible given the nature of the mechanism or the unit to be studied and the requirement of feasible research; sort out independent and dependent variables; find reliable and meaningful indicators; use sophisticated statistics when needed, elicit controlled correlations and move toward well-grounded hypotheses backed by a causal theory of some sort; check the output by putting the relevant concepts along a suitable ladder of abstraction and sometimes adding a detailed case study that complements statistical causality with socio-historical causality; should the occasion arise, substitute a longitudinal analysis of intertemporal variables for the historical narrative. I have exposed here the "hard" version; there may be softer ones using controlled descriptions and narratives of fewer cases. The main rationale is still basically the same: to find an explanation that is as univocal and parsimonious as possible and applicable to different contexts. The French reluctance to use that approach does not stem from a lack of training in statistics. It is the other way around; the opposition to the mainstream analysis brings about a distaste for comparative quantitative research often deemed an ideological artifact when applied to too many cases and used without the care required by the search for a "thick" meaning of indicators. The main bone of contention lies in the status of history. Everyone, be it "mainstream" historians or "socio-historians," admits that the past is a foreign country: they do things differently there (a formula dear to the hearts of British historians). However, in the case of the former, that does not prevent them from hypothesizing that comparable mechanisms may be at work in historical worlds where the *concatenation* of mechanisms may be always singular, and if this holds true, one should not

abstain from "comparing nations," as Rokkan famously did by locating them within the same epoch. Such an outlook entails that the large units one is comparing are more or less parts of a common trend involving an evolutionist view of the empirical world, whether Whiggish or neo-Darwinist-functionalist (sometimes it may be both). Even the "strategic" readings of "democratization" pay tribute to that *zeitgeist* which, despite its recent rebuffs, is far from fading away. Many French scholars strongly disagree (Lagroye et al., 2006). The thrust of their argument has not so much to do with the underlying *zeitgeist*, congenial to most of them, as with a consequence of their conception of history: the past is a foreign country where a plurality of political units do things *their own way*. So, short of very careful and painstaking scrutiny, we should not put different cases under the same conceptual label. The comparison by similarities and contrast may fail if we are spellbound by the illusion of typologies and we forget that the Weberian ideal-types are not construed to describe an empirical reality but to stipulate that a dominant mechanism may be operating. Jean-François Bayart is a sophisticated exponent of such an approach: as "historicity" is the main feature of what is to be compared we should resist any comparison of "entities," advocate the comparison of *contingent* practices, processes, and moments, and abandon causal reasoning. The motto is "to compare in order to singularize. To singularize in order to universalize," which raises a host of tricky issues that cannot be dealt with here (Bayart, 2008).

Hence the French tendency is to reduce the "small-*n*" to two cases, one of them being the genuine object of the inquiry and the other just a way to confirm the intrinsic interest of a study actually focusing on a single case. Hence a somewhat excessive interest in minute historical and quasi anthropological studies of local objects revealing the poverty of the usual concepts such as "citizenship," "mobilization," "voting," "party," "pluralism," "democracy," "rule of law," "state," a critique familiar to the practitioners of "subaltern studies." Admittedly, there are some embarrassing stumbling blocks: to avoid becoming stuck in a strict historical narrative, the authors of such studies cannot do without a modicum of conceptual apparatus and so they resort to stipulative catch-all concepts which can be used to apply to any time and place or cover a huge range of events, "social construction," "fields" (*champs*), "sectors," "transactions," "instruments," "*gouvernementalité*" (Foucault's legacy is pervasive here), "configurations" borrowed from the hugely popular Norbert Elias, "intellectual capital," "*habitus*," and "strategies" (the latter three from Pierre Bourdieu). They are used more as an illustration of what is described and interpreted than as a tool for comparison. Let us hasten to add that several of them are also used in a part of English political science, witness the vogue for "the discursive construction."

In two subfields, the picture looks fairly different: "area studies," ill considered by the rational-choice political economists but still widely practiced in mainstream political science, and "European studies," and both of these are the topics of the books under review.

II

The scholars dealing with Arab politics suffer from two handicaps: the lack of reliable longitudinal political data, and the vastly different meanings given to certain political words (say, democracy, pluralism, social class, religion, parties).

Moreover, the overall puzzle arising from the relative impossibility of disentangling normative and descriptive uses of the same statements is more pervasive in these areas and in their "social scientific study." The thick and historical descriptions that are useless to the economists (or so they tell us) seem inevitable, whether or not they use quantitative data (and why should they refrain from doing so?). What is usually considered a shortcoming of several French political studies, too much reliance on history, anthropology, socio-linguistics, religious studies and too many vast generalizations backed up by a succession of case studies, becomes a significant asset. Gilles Kepel's and Olivier Roy's achievements, not commented upon here since they have immediately been translated into English, bring evidence of their influence.

French scholarship is not short of good collective works on "the Arab world," some of them designed like the collections on Europe commented upon below. For example, Picard's book, coming out almost 30 years after Hudson's justly famous book at the time (Hudson, 1977), is very different from the latter: instead of a two part book comprising a splendid essay on a key problem still pressing today (legitimacy) and a country-by-country study, we are presented with a collection using several general concepts as headlines to deal with the area: authoritarianism, primordial ties, political mobilizations, law and justice, gender, party systems, corporatism, political space, political economy, public policy, international relations. Far from being a juxtaposition of country studies, this is a genuine comparative book with the qualities and drawbacks of this kind of approach. Each chapter, deploying a vast array of research data, supplies a framework for further analyses of specific countries and problems, which makes it an indispensable tool for students and scholars (and, for once, it is completed by a bibliography and an *Index rerum*). Accordingly, what is missing is the identification of discrete variables allowing a comparison by similarity and contrast. The balance leans toward the positive qualities, although each author, being a specialist in only one part of the area, tends to extend her own partial vision to the whole. However, the editor has provided enough guidelines to keep the collection under control.

Although presented as a country study, Michel Camau and Vincent Geisser's book has a more ambitious purpose, pursued also by the Dabène-Geisser-Massardier collection offered to Camau as a *Festschrift*. Of the three editors of the latter, only Geisser is a specialist of the Arab world, Dabène being an authority on Latin America and Massardier working on the Western democracies. The core idea is that, whatever we may think of the existence (or not) of "authoritarianism" as a type of regime, we'd better consider it first as a "situation" or, in Camau's terms, as a "syndrome" to be found, in variable proportions, in any kind of regime. Authoritarian mechanisms operate in democratic and liberal regimes, either in specific "zones of exception," as Gilles Massardier puts it in his excellent contribution, or as ordinary means used by a government which cannot count on the goodwill or sheer rational self-interest of all to be effective, a commonplace emphasized by an overwhelming majority of political thinkers of all times, cultures, and persuasions. Last, it is always useful to recall that electoral politics, however free and fair, may be controlled by a small elite which, rightly or not, can afford to dissolve the government if it does not like the winning party. Hence rigid and dichotomous typologies opposing "authoritarianism" to "democracy" reveal their weakness when checked against concrete empirical contexts, and all the more so as the dichotomy is complemented by its opposite: the idea that there is a "transition from

authoritarian rule to democracy" which makes democracy the "natural" future of authoritarianism by a magic move from a zero-sum game to a win-win game.

Notwithstanding, it is worth maintaining a distinction between authoritarianism as a regime and as a situational property, lest we stay confined to mere platitudes such as "any kind of power is authoritarian" and "every regime is more or less authoritarian." More important, it would be hardly possible to document one of Camau's more stimulating theses, to wit that there may be an "authoritarian equilibrium" analogous to a "democratic equilibrium," both of them located along a continuum (Colomer, 2001). We have to keep in mind that authoritarianism is not just a property of the rulers; their subjects may participate in its working, not so much because of a widespread societal authoritarianism in many "non-political" sectors as because of the more or less "consensual" acceptance of the authoritarian processes by the ordinary people, be it either out of a feeling that it is "the only game in town" or as a result of a "trickle down effect" which may tend to convince them that it brings some benefits for want of something better or for fear of something worse such as radical Islam (see Hibou, 2006, who stresses with some excess the phenomenon of "willing servitude"). As Jean-Noël Ferrié (2009) puts it in his excellent review, an authoritarian government may be linked to its society by variegated transactional flows and its "liberalization" or "pluralization," far from bringing it to an end to start a process of democratization, may indeed *consolidate* it: after all, Napoleon III's "liberal" then "parliamentarian" empire could have survived a long time, had France not been defeated in 1870 in the war against Prussia.

If Camau's theses seem rock solid, not all the implications drawn by the editors of the Dabène-Geisser-Massardier book are unimpeachable. I do not think that the ever cursed "ethnocentrism" (needless to say, always "Western") is the source of the misconceptions pointed out earlier, since the "natives" opposing authoritarianism share the same beliefs when holding that everything going wrong in their country is due to that feature of the regime, unless of course we decree that all of them are "Westernized elites," which is deeply mistaken. Likewise, it is wrong to hold that Western advice to promote "democratization" and "good governance" is by itself a cause of authoritarianism in "legitimizing an authoritarian and security-driven normalization of the opposition's behavior in order not to jeopardize the new macro-economical and macro-political equilibria" (p. 35), in other words a Pinochet-style regime wearing democratic clothes. It may be true that we are witnessing an attempt at "bureaucratizing the world" and submitting people to the famous "discipline" excoriated by a Foucault impervious to the chaos brought about by the absence of a law-abiding and fair bureaucracy. It remains true, too, that the masses demanding some part of the pie may resort with good reason to actions far remote from the ideal of a civil society advocated by the UNDP and the World Bank as well. Yet, it looks rather like a paradox to draw the conclusion that the new regime should be as authoritarian as the previous one. I wonder if there is not a confusion between authoritarianism and Easton's "*authoritative* allocation of values." Besides, there is no obvious causal connection between the international prescriptions and a new transition to authoritarianism.

As often, causality is the nagging problem of those constructions. They may point out specific cases where a causality of some sort is operating but they have trouble with generalizations. The idea that economic liberalization brings about authoritarianism (still the "Pinochet-syndrome" in reverse) is simply false, as well

as the opposite idea that it brings about democracy. As Ferrié (2009) reminds us, there is no obvious causal connection in either direction, the relationship depending on the context and circumstances. Moreover, the so-called "liberalization" is often a phony policy keeping intact the real roots of a regime, what Jean-François Bayart and Béatrice Hibou have called "the privatization of the state" driven by the sheer desire of the political power-holders to maintain their grip on the society and to keep their benefits, be it under a "socialist" or a "liberal" guise. If this is the case, the international prescriptions should be seen as constraints to be accepted when there is no other way to survive in a dramatic crisis of resources (although Zimbabwe and North Korea are nice examples of "heroic" resistance, to the detriment of their citizens), manipulated, then abandoned when things start to get better. We will return to some of these problems in a quite different area, the European Union.

III

In a nutshell, the European Union today looks like a slightly better version of the American Articles of Confederation adopted shortly after the Revolution and before the publication of the *Federalist Papers* and the resolution to hold the convention of Philadelphia. They announced that "each state retains its sovereignty, freedom, and independence, and every power, jurisdiction, and right, which is not by this Confederation expressly delegated to the United States in Congress assembled." The EU has no executive authority and no "national" (meaning "European") judicial system (even though the decisions of the European Court of Justice are binding to the states and their own judiciaries). One of the most important powers of modern national government, the power to tax, is by and large missing. Unlike the Confederation, it holds the power to regulate commerce, the main source of the Commission's and European Parliament's power, but it has no power in matters of defense and foreign affairs. So, most of the policy-making depends on a complex process involving national governments, the Commission, and the Parliament in a context vastly different from the 18th century's: Europe is a powerful economy and is made up of states most of which have a long history and all of which are overloaded by the growth of their tasks in a geographical zone not isolated from the turmoils of the global world. To grasp that process, we need to get a proper understanding of *both* the European level and the national levels. Here, comparison comes in: national institutions, parties and party systems, and types of leadership cannot be studied as "cases" since we need to compare them in order to grasp their operation in the European decision-making process. This is the reason why the book under review edited by Céline Bélot, Paul Magnette, and Sabine Saurugger is original, at least as a preliminary starting point. Instead of breaking down the object of study by domains of public policies, as was famously done in a well-known collection (Wallace et al., 2005), or by relevant concepts, say, legitimacy, efficiency, governance, federalism, and "Europeanization," these editors organize their volume by subdisciplines and objects of general political science in order to assess their value in the study of the EU and vice versa to inquire if the EU as an empirical phenomenon elicits new insights and brings some added value to political theory, international relations, public policies, political anthropology, public opinion, citizenship, interest groups, parliaments, high civil servants, cities and regions, among others. Unfortunately, as in most

of the other books reviewed here, there is no index, an effect of the inexcusable penuriousness of many French publishers. Some glaring omissions betray the reluctance and shortcomings of French research (leadership, regimes, although one of the editors has produced a very good book on the "European political regime" [Magnette, 2006], and above all political economy)[3] or are due to their extensive treatment in several collections of the Presses de Sciences-po (democracy, constitution, voting). There is some heterogeneity and sloppiness in the choice of chapters: some of them are not quite up to the overall level of quality of the book (for example, Law and Politics, actually a study of the politics of lawyers' interests, political parties, and cleavages). Other rubrics do not quite fit in the logic of the project (government by committees, enlargement).

Despite those remarks the book is well worth reading, since, while succeeding in bringing evidence of the growing involvement of French political science in the international field of European studies without concealing its obvious weaknesses (one of the editors is refreshingly candid in her final remarks), it may also be used as an introduction to future comparative researches. In this respect, Yves Déloye's chapter "socio-histoire" is enlightening: a staunch advocate of the "historical sociology of politics" (1997; also Déloye and Voutat, 2002), he shows that this approach, far from being adverse to comparative analysis, may contribute to its expansion by shedding some light on the vexing problems of European identity, the "Europeanization of citizenship," and above all the determination of the character of what is commonly and confidently named "the European polity," whose main feature is to be "something," recalling the story of the blind men and the elephant (Leca, 2009). His presentation of and commentaries on Gary Marks's (1997) essay and Stefano Bartolini's (2005) impressive study using Rokkan's categories are particularly well crafted.

That does not mean that all is right with the world of political science and that we live together like a big happy family, since the technological (the use of statistical methods for comparison) and theoretical (the status of historical description) "gaps" remain wide. A special issue of the valiant journal *Politique européenne*, edited by Sabine Saurugger, tackles the question head on and it can be read as a follow up to the previous book. Once again, the contributions are of unequal value. Among the best ones, Frédéric Mérand from Montreal (who publishes mainly in English) gives us a careful and balanced analysis entitled "Should (American) Institutionalists Read (French) Sociologists?" (the reverse title would be equally apposite). The most interesting argument is not so much the worn out critique that American Institutionalists should include conflict and domination in their toolbox as the emphasis on the poverty of the abstract dichotomy between the logic of consequences and the logic of appropriateness, famous since March and Olsen first put it forward. That is an example among many others of the French distaste for "dualism," "individual–society," subjective–objective," etc., and therefore of all the dichotomic variables. Niilo Kauppi and Mikail Rask Madsen contribute a very solid epistemological argument for getting rid of the "dualist" lexicon ("individual–institution", "supranational–national", "socialization–calculation"). On the last duality ("social constructivism–rationalism"), Sophie Jacquot and Cornelia Woll take the same stance in a more fruitful contribution dealing with empirical research strategies. The distrust of dualism may go so far as to urge Kauppi and Madsen to hold in contempt "one more Grand Theory" (and it is true that most of those theories are based on an opposition to another concept).

This is a mistake: although classical sociology dealing with Durkheimian "societies" with "high moral density" may be rightly suspicious of those theories (Goldthorpe, 2000), they are necessary when dealing with something "in the making" (and, by the same token, "falling apart"), unless we are satisfied with staying trapped in the dark and content ourselves with some underhand "Grand Theory" holding that people need not know what they are doing provided they do it out of "strategies of transaction and domination."

One of the most interesting contributions is Bernard Jullien and Andy Smith's chapter on the EU and the regulation of industry, which calls for a political sociology of the industrial sector. Starting from the distressing commonplace that has been a pain in the neck of all the federalists and advocates of "Great Power Europe," to wit the strength of the logics operating in various sectors and the weakness of the inter-sector mediation and coordination, they proffer a research program mainly based on French sociological concepts to find a way to cross-breed the institutionalist theories devised in industrial economics and policy analysis. More important, they propose to focus the analysis on "two levels of articulation": the articulation of the logics of economic production and political interests and the ever specific articulation of scales and arenas where rules are negotiated. Such a framework calls for a move from international to transnational comparison (Hassenteufel, 2005).

An analogous strategy is seemingly used by Yves Buchet de Neuilly in an unexpected domain. Note that his book is entitled *L'Europe de la politique étrangère*, an inversion of the expected title *La politique étrangère de l'Europe*. It is highly representative of a current of research in French political science combining a theory based on Bourdieu and Elias with a method using as primary sources a careful description of organizational and interorganizational networks and "circuits," narratives of specific cases, and in-depth interviews with a selected number of actors.[4] The overall aim is to account for the institutional construction of the *Politique étrangère et de sécurité commune*, using as a starting point not a definition of what a foreign policy is, since the term itself refers to different stakes depending on the various actors and sectors that use it, but a core distinction between two basic "competitive configurations": 1) the cleavage between, on the one hand, the national and supranational actors of the community sectors and, on the other hand, the national and supranational actors of the diplomatic sector and 2) the cleavage between the national actors and the representatives of the Commission. This is a clever way to eschew the classical and obsolete distinction between the "community" and "intergovernmental" methods. The overall conclusion is that the conception of two kinds of clear-cut interests, "European" and "national," is by and large an illusion since the office holders, citizens of the same state, may hold different views of the national interest of their state, depending on their positions and the dynamics of their interaction in a political or administrative body. The same could be said, *a fortiori*, of the European interest. The "integration" taking place in the sectors of "international relations" is not the same "integration" as in other sectors. Interesting findings, indeed. The problem is that such statements amount to throwing the baby out with the bathwater: instead of inquiring about the conceptual meaning of "integration" and testing if we can reasonably use it in the foreign policy field (the same could be said of the catch-all term "Europeanization," whose meaning varies widely according to the cases), the author

contents himself with a careful description of the organizational fabric of the EU's would-be foreign policy without asking the obvious questions: "to what extent does the working of various organizations dealing with international relations make up the foreign policy of a polity? What kind of coordination is required and how could it operate?" We have to answer those questions the author is careful not to ask, perhaps for fear of being accused of "methodological nationalism," a term coined by Ulrich Beck, and certainly to avoid any "reification" of collective groups such as endowing the states with "a will" of their own. For this purpose we need a comparative inquiry about what the member states are doing, why and how, when they "decide" to take widely differing positions toward foreign states, for example the recent German "decision to abandon" Areva, the French nuclear company, in favor of Russia's Rosatom to build a nuclear power station, with the effect of shaking "European equilibrium" (a "Westphalian" concept dating back to the 19th century). A different picture of "Europe in international relations" would certainly appear. The will to "deconstruct" and "disaggregate" the empirical units routinely used in the study of politics, such as "government," "state," "civil service," etc., however useful, may lead us into dead ends. I wonder what we can do with the chapter on, once again, historical sociology (decidedly a French craze), very different from the contribution pointed out earlier. There is nothing wrong with using sociology of knowledge and sociology of trajectories and positions of the social agents and groups that make up the "European political space," yet is it enough, and is it essential? Is it so obvious that we should get rid of the familiar oppositions between what is "national" and what is "European," what is at "the top" and what is at "the bottom," as well as between "North," "South," and "East," or "Old" and "New" members to substitute a *summa divisio* between the agents who, given their social position, *believe* in the reality of a European space and those who do not? Besides the obvious point that it may seem a little superficial to attribute beliefs to positions in an institution since it begs the question "why and how were those institutions created?" (a puzzling oversight, coming from scholars who insist on a *historical* sociology and advocate a method based on biography and prosopography), and to hold that, once formed, those beliefs will shape the agents' actual behavior (the classical confrontation of "ideas" and "interests"), why should we leave out of the picture, say, "national," "European," "northern," southern," "eastern," old," "new" beliefs? More important, one may ask whether we are not witnessing a mere transfer of jargons to substitute the language of Pierre Bourdieu's sociology to account for what is usually expressed in mainstream language as a "compound polity" where each problem and policy process works in a way that cannot be put in the same conceptual category as another. Anan Menon, in a friendly yet sharp and pungent commentary (entitled "French follies?"…), notes that the term "*usage politique*" "seems superficially similar to much of the political science literature focusing on multilayered political systems, which has coined terms such as 'credit assignment' and 'blame shifting' to characterize the ways in which political actors from one level of government make use of other levels."

IV

If we dare to venture a general conclusion from the present review, dealing, I say this again, with a small part of the French political scientists' output,

we will have to risk a Freudian hypothesis: it looks as if many "young scholars" (i.e. most of those in their mid-forties) were keen to forget and deny their origins. After all, French political science issued from the *Facultés de droit*, just as American political science issued from the departments of government. Yet the study of institutions is conspicuously wanting, as shown by the insufficient utilization in this specific field of tools such as the logic of consequences, entrepreneurship, and isomorphism, which are not only part and parcel of the "American toolbox" but were always the concern of lawyers (admittedly under other names). In the long gone 1950s, Georges Burdeau could dispose of the "behavioralists" in a flippant, misinformed, and unfair footnote of his monumental *Traité de science politique*, now nearly forgotten. Today, although all of us despise them on a basis as fragile as Burdeau's, we are all behavioralists or "post-behavioralists," given the heavy emphasis we put on the study of actors. Institutions are usually considered as "resources" to be "utilized," rarely as "identity-builders" and next to never as shaping behaviors. Admittedly, French European studies deal at some length with the European Parliament as a provider of identity, resources, social positions, and status to its members, but the comparative study of its symbolic value to the elites and European citizens is still wanting.[5] The "second order" character of the European elections and the prevalence of national over European issues are taken for granted and widely documented by electoral studies without being considered as a pressing problem (as signaled above, that may be explained by French and more generally international political scientists' aversion to a clear-cut distinction between European and national interests). Beliefs, *habitus*, and interests reign undaunted. Or if they are treated as "instruments," they seem to evolve from various "socio-logics" and not from a conscious rational design. Sometimes, consciousness comes in as a "manipulator," always to serve a "party" (although we find examples of "unconscious manipulations"), rather than to "engineer" so as to serve what is (construed as) "the public interest," which makes the same scholars look somewhat at cross-purposes when they recommend institutional changes in their own country (they usually get away with it by saying they are voicing their opinion "as citizens" and not as "scientists"). Despite the frequent references to institutionalist theories, institutions are not studied "for their own sake." To be sure, such an outlook is useful in much empirical research to put us on the alert against reducing the study of politics to the study of formal legal institutions, but it forgets that the study of government, which is still, or should be, the main job of political scientists, cannot leave them out of the picture. Some area studies may justify this neglect when institutionalization is conspicuously missing, although Michel Camau, himself a former lawyer, is well aware of their relevance. The emphasis he puts on the rule of law and pluralism as key elements of the characterization of regimes should not be taken too lightly since not only are too many countries conspicuously wanting in this respect, but above all there can be no self-sustaining rule of law without efficient and accountable institutions, understood as such by the common man who should, at the very least, *believe* in them (here, beliefs are important to express some support). It is to be hoped that European studies, once they have exhausted the joys of "overlapping arenas," "multilevel governance," "governance by networks," and so on (there is no longer any French terminological exceptionalism), will return to their essential concern.

Notes

* The present review is not intended as a critical overview of the discipline in France and French-speaking countries. Neither is it a review of the best works in French-speaking political science. It is meant to point out a recurrent issue and to use the examples of two subfields to show how it has evolved.

1. Duverger has still a few discreet heirs, Owen (2002).
2. See, however, Martin (2007), referring to the Lipset–Rokkan model.
3. Such an omission is surprising since some good research has been carried out by French or France-based scholars … usually writing also in English and German: Woll (2004, 2008), Jabko (2006). Even so, they have their misgivings about a rational choice approach practiced by most of the political economists, so they leave this field to classical economists and turn either to an interest group approach (Grossman, 2003; Saurugger, 2003) or to a "political sociology of economy" taking its cue from Max Weber (Beckert, 2002, 2009; Beckert et al., 2007; Fligstein, 2001). More on that below.
4. For a good assessment of the utilization of Bourdieu's sociology in European studies, see Kauppi (2003).
5. Olivier Costa and a few others are a relative exception in the field: Costa et al. (2004) and the Costa–Rozenberg contribution to Bélot et al. under review. However, among the three research domains explored by the French-speaking political scientists according to Costa and Rozenberg, the European Parliament's contribution to the development of European politics, the institutional logics, and the progressive differentiation of the European Parliament's elites, the second one looks somewhat neglected. Besides, the comparison of national parliaments to assess their contribution to an emerging European polity is the province of non-French-speaking research.

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