COMPARATIVE CONSTITUTIONAL STUDIES: TWO FIELDS OR ONE?

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Comparative Constitutional Studies: Two Fields or One?

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Abstract

This article reviews recent comparative research on constitutionalism and judicial review and argues that it is best understood as falling into two fields: comparative constitutional law (CCL) and comparative judicial politics. While both fields are directed at the same phenomenon – the global spread of constitutionalism and judicial review – their purposes and methods are quite different. CCL, for its part, is aimed at constructing constitutional law doctrine, understanding the methodologies of judicial recourse to foreign law, and investigating how similar-seeming constitutional principles take on different meanings in different legal systems. Comparative judicial politics, by contrast, consists of a range of political science research on the origins and ongoing dynamics of judicially enforced constitutionalism. The recent call for CCL to progress into comparative constitutional studies should accordingly be treated with caution. While there are opportunities for productive dialogue between the two fields, such dialogue needs to recognize the distinct identity of each.

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INTRODUCTION

Two recent assessments of the state of comparative research on constitutionalism and judicial review proceed in very different ways. For Tushnet (2014), there is still merit in demarcating at least some of the research in this area by the term 'comparative constitutional law' (CCL). While acknowledging CCL's close connection to research in comparative politics, Tushnet presents CCL as a self-contained field aimed at studying 'general *themes* in constitutional law around the world', including constitution-making, the 'structures of constitutional review' and the 'structure of rights analysis' (p. 2).[1] This presentation of the field contrasts markedly with Hirschl's (2014) appraisal of the health and future direction of CCL.[2] In Hirschl's view, CCL is undergoing a 'renaissance' but is currently 'rid[ing] on a fuzzy and rather incoherent epistemological and methodological matrix' (p. 5). The problem, he thinks, is that the formerly vibrant field of CCL has been appropriated by legal academics with little understanding of theory-building through causal inference (p. 164). The way to remedy this problem is to reconceive CCL as 'comparative constitutional studies' (CCS): a pluralist, eclectic, multidisciplinary field dedicated to the study of global constitutionalism using a range of different methodological approaches – from small-N qualitative case studies to large-N quantitative approaches.

Who is right? Or rather, what is the better conception of research in this area, its current state of health and future direction?

Part 2 of this article surveys the comparative literature on constitutionalism and judicial review and argues that it consists of several distinct enterprises: CCL Type 1, which is concerned with the use of foreign law to clarify the content of, and in certain cases, construct constitutional law doctrine; CCL Type 2, which is concerned with understanding the methodologies of constitutional comparison for doctrinal purposes; CCL Type 3 which is about research on the way in which similar-seeming constitutional structures and doctrines take on different meanings and functions in different legal systems; and comparative judicial politics, which consists of a diverse range of social science research on the origins and ongoing dynamics of the post-1945 global spread of constitutionalism and judicial review.

[1] See also the more emphatic articulation of the point in Tushnet (2006a, p. 67): 'There is of course a large field of comparative studies of governmental organization, conducted by political scientists as well as by lawyers, and some of the field overlaps with the field of comparative constitutional law. But, there is also one large difference between the fields. Comparative constitutional law involves doing law.'

[2] Tushnet (2016) reviews Hirschl's book sympathetically and welcomes its professed methodological pluralism. In drawing out differences between Tushnet and Hirschl's approaches, this review should not be understood as attributing to Tushnet the reading of Hirschl (2014) presented here.

Against this background, Part 3 interrogates Hirschl's assessment of the state of CCL and his suggestions for its future development. Rather than a single field dominated by academic lawyers, this part argues, research into judicially enforced constitutionalism is best thought of as falling into two separate fields – CCL and comparative judicial politics. Each of these fields has its own distinctive purposes and methods. At several points in his argument, Hirschl recognizes this and commits himself to disciplinary and methodological pluralism. At other points, however, Hirschl appears to valorize causal-explanatory social science research over legal-interpretive approaches. This preference is particularly apparent in his classification of various contributions to the literature along a continuum from explanatorily weak single-country case studies to explanatorily powerful large-N causal theory-building and testing. If adopted, Part 3 concludes, this classificatory framework would subsume CCL under comparative judicial politics. This part of Hirschl's project should accordingly be resisted.

If all forms of comparative research on constitutionalism and judicial review are to be equally valued, Part 4 argues, their distinctive purposes need to be acknowledged. This means that certain types of interdisciplinary research, such as research that tries to combine an external social science perspective and the participatory-insider perspective of CCL Types 1 and 2, make no sense. On the other hand, some of the insights emerging from CCL Type 3 research might usefully be taken up by more causally oriented scholars. In particular, the differences in constitutional culture that legal academics have identified as a factor influencing the transnational migration of constitutional ideas might assist political scientists in better operationalizing law as a comparative variable.

LITERATURE ON JUDICIALLY ENFORCED CONSTITUTIONALISM

This section briefly surveys the comparative literature on judicially enforced constitutionalism, not with a view to being comprehensive, but with the aim of highlighting the main types of research that are being conducted. Only the most representative, 'classic' or otherwise recent and interesting research is covered.

For legal academics, the main point of entry into the literature has been studying the effect that the proliferation of judicially enforced constitutionalism has had on the practice of constitutional law in different parts of the world. Legal academics who style themselves comparative constitutional lawyers thus tend to be national constitutional lawyers in the first instance and comparativists second. Their interest in CCL is typically driven either by the experience of a constitution-making process in their home country or by a shift in judicial

citation practices towards greater reference to foreign law. They are not, in the main, comparative lawyers who happen to be interested in CCL. While it is thus notionally possible to classify CCL as a sub-field of comparative law, in truth there is little connection between the two enterprises. With a few notable exceptions (Bomhoff 2013, building on Lasser 2004), legal researchers who study foreign constitutional orders are not all that familiar with debates in comparative law. Rather, they are drawn to CCL because that is part of what being an effective constitutional lawyer in their home country requires. Once drawn into this field, to be sure, they become part of a global scholarly community that is engaged in generating a self-standing body of knowledge about constitutional concepts, structures and methods. But the initial impetus for their involvement in those conversations is typically a desire to contribute to constitutional practice in a particular country.

For legal academics, the spread of constitutionalism and judicial review is thus not so much a phenomenon whose *causes* need to be explained as a change to the way in which constitutional law is produced that needs to be *understood* and *rationalized*. In pursuit of this broad objective, legal academics have engaged in at least three main types of research. The first type is purely doctrinal in the sense that it uses foreign law to give content to constitutional law norms in a particular legal system ('CCL Type 1'). Exactly how this is done depends on the roles legal scholars play in the legal system concerned and on what the internal rules of reference to foreign law are in that system. For continental European legal scholars, used as they are to being at the center of the legal system, CCL as 'doctrinal constructivism' comes naturally (Von Bogdandy 2012, p. 26). Scholars engaged in this type of research see themselves as participants in the construction of constitutional law doctrine – not just describing it from without, but 'shap[ing] it from within' (Von Bogdandy 2012, p. 26). Anglo-American legal academics, by contrast, tend to position themselves as somewhat more detached 'observers' of the way judges draw on foreign law (Rosenfeld 2012, p. 39). Their function is not so much to participate in the construction of constitutional law doctrine as to reflect on the methods judges use when relying on foreign law, and to criticize particular instances of reliance (or non-reliance). The exact form that CCL Type 1 takes also depends on whether reliance on foreign law is explicitly endorsed by the constitutional order in question (as is the case in South Africa, for example) or a more contested practice that needs to be justified (as is the case in the United States).

Examples of CCL Type 1 include virtually any modern constitutional law commentary in jurisdictions in which reference to foreign law is a matter of common practice together with law journal articles exploring the relevance of foreign law to particular

doctrinal questions. Woolman & Bishop (2013), for example, is a five-volume, multi-author treatise on South African constitutional law that makes extensive reference to foreign law in almost every chapter. An example of the more focused, single-issue version of this form of CCL research is Stone (2001). In this article, Stone argues that the judicially created 'implied freedom of political communication' in Australia is capable of being understood as a personal right notwithstanding its origins in a constitutional system traditionally more preoccupied with federal limits on legislative power. To substantiate this argument, she draws extensively on US constitutional law, showing how conceptions of freedom of speech as a personal right in that setting are not wholly alien to the Australian context.

Closely related to this type of research is research on the way judges in different jurisdictions have responded to the demand that they take account of foreign law in constitutional decision-making (CCL Type 2). This form of research is a logical extension of Anglo-American legal scholars' more detached role in doctrinal construction. Starting in the late 1990s in the US with the controversy over Justice Breyer's reference to foreign law in his dissenting opinion in *Printz v United States* 521 US 898 (1997) (Tushnet 1999, Waldron 2005, Jackson 2005, Choudhry 2006, Dixon 2008, Rosenfeld 2012), CCL Type 2 has since broadened into studying the way in which judges in different parts of the world have responded to the globalization of judicial review (Saunders 2006, Jackson 2010). While doctrinally oriented in one sense – knowledge generated by this form of research may conceivably be fed back into the practice of constitutional law in a particular jurisdiction – the point of CCL Type 2 is to contribute to a legal-system-independent body of knowledge about the characteristic ways judges respond to the demand to take account of foreign law and, by extension, to contribute to transnational understanding of the methodologies of doctrinally oriented comparison. Jackson (2010) has been particularly influential here in distinguishing between 'postures' of 'resistance', 'convergence' and 'engagement' – responses that are notionally applicable to all jurisdictions in which recourse to foreign law occurs. On this approach, neither a universalist faith in the eventual emergence of a common understanding of all constitutional structures and doctrines, nor an expressivist denial of the value of constitutional comparison, is winning or should win the day.

The third major form of CCL research ('CCL Type 3') concerns research into the way in which particular constitutional doctrines, reasoning methodologies, structures, and rights are instantiated in different legal orders. For a while it was contended by some that constitutional law in different jurisdictions was converging on similar conceptions of core principles (Dorsen, Rosenfeld, Sajó & Baer 2003, Beatty 2004), or that there might even be

such a thing as a body of 'generic constitutional law' (Law 2005). This is no longer the dominant view, however. Rather, in line with the ascendancy of conceptions of reference to foreign law as forms of transnational 'engagement' (Jackson 2010) or 'dialogue' (Choudhry 1999, Teitel 2004), the purpose of CCL Type 3 is today seen as being to clarify the different ways in which local legal cultures and institutional settings influence the meaning given to similar-seeming constitutional doctrines, reasoning methods, structures and rights. This form of research, in other words, takes the expressivist insight that constitutional law, like all law, needs to be viewed through the lens of legal culture (Cotterrell 2006) and uses it as a basis, not to give up on the enterprise of CCL, but to drive the project of enhanced understanding through contrast and comparison.

The major topics of interest in CCL Type 3 have been the widespread use of proportionality analysis as a method for reconciling either competing rights or competing legislative goals and rights (Beatty 2004, Kumm 2007; Kumm 2010, Cohen-Eliya & Porat 2010, Cohen-Eliya & Porat 2011, Cohen-Eliya & Porat 2013, Barak 2012, Möller 2012, Bomhoff 2013, Jackson 2015), the doctrine of 'unconstitutional constitutional amendments' (Halmai 2012, Dixon & Landau 2015, Albert 2016), forms of judicial deference and levels of review (Tushnet 2007, King 2012), social rights (Dixon 2007; Tushnet 2007, Bilchitz 2007, King 2012, Young 2012), and the merits of the so-called 'Commonwealth' or 'weak-form' model of review (Gardbaum 2013). Of these, the two most active sub-literatures have been on proportionality and social rights.

After Beatty's (2004) somewhat optimistic announcement of proportionality as a generally applicable, normatively unassailable method for adjudicating constitutional rights, research on this topic is now based on greater skepticism, both about proportionality's universality and about its capacity to legitimate judicial review. The main topics currently under discussion include the cultural and institutional setting in which proportionality emerged in German constitutional law in the late 1950s (Bomhoff 2013, Cohen-Eliya & Porot 2013, Hailbronner 2015), the reasons why the US Supreme Court has thus far been resistant to adopting proportionality (Bomhoff 2013, Cohen-Eliya & Porot 2013), the analytic similarities nevertheless between US balancing and German proportionality (Cohen-Eliya & Porot 2013), and the possible benefit to the US of some 'moderate increase in the use of proportionality' (Jackson 2015, p. 3098). While some of this work has sought to explain the diffusion of proportionality across different jurisdictions (Cohen-Eliya & Porot 2013), the focus has been on understanding the way the operation of proportionality as a doctrine changes with, or adapts itself to, variations in legal culture. In this way, discussions of

proportionality have tracked the general trend in the CCL literature from 'convergence' to 'engagement'. US balancing may not be the same as German proportionality, scholars involved in this line of research now agree, but exploring the reasons why this is so helps to clarify the role these doctrines play in the constitutional systems concerned while at the same time providing a lens through which to compare them.

The thriving CCL literature on social rights appears to call into question Tushnet's (2014, p. 70) view that 'it is unlikely that any generalization about rights-protection will withstand scrutiny'. In fact, however, what this literature is mostly about is not the search for a universal understanding of the doctrinal content of social rights, but building a body of comparative knowledge about the roles that constitutional courts perform when adjudicating these rights (Young 2012) and how these roles may best be justified according to (or by extending) standard liberal-democratic conceptions of the judicial function and the separation of powers (King 2012). The focus of this research has thus fallen on appropriate standards of review for judicial enforcement, including whether the so-called 'minimum core' standard developed in international human rights law may be applied at the domestic level (Bilchitz 2007), the normative justification for social rights given concerns over their impact on democracy and the formation of macro-economic policy in particular (Bilchitz 2007, King 2012), and whether weak- or strong-form remedies are better suited to fulfilling the pro-poor promise of these rights while at the same time meeting democratic objections to their judicial enforcement (Sunstein 2001, Tushnet 2007, Bilchitz 2007, Landau 2012, King 2012).

Whether in the form of Type 1, 2 or 3, the characteristic feature of CCL, this brief survey reveals, is its legal-interpretive and normative character: legal scholars engaged in this form of research are either seeking to solve particular doctrinal controversies or to understand how constitutional doctrines come to take the form that they do. In engaging in such research, legal scholars draw either on the argumentative frameworks and internal values of a particular legal tradition or on notionally supra-systemic standards such as those generated by normative political theory. Rather than causal explanations, they tend to stress the way in which institutional and legal-cultural factors both influence the migration of constitutional ideas and limit the growth of a generic body of comparative constitutional law.

CCL thus conceived is markedly different from the research that has been conducted on the global spread of constitutionalism and judicial review over the last twenty years in comparative politics. Despite one early objection (Gillman 1994), research by political scientists on this phenomenon has almost exclusively taken the form of causal-explanatory inquiry into its political origins and ongoing dynamics. Within that focus, behavioralist and

rational choice approaches have prevailed over historical-institutionalist and ethnographic approaches of the sort advocated by Smith (2008) and Scheppele (2004). The reasons for this have not been examined, but presumably have to do with the higher status of the former two approaches in US political science and with the difficulty of developing a detailed, hermeneutic understanding of constitutional politics in more than a handful of countries. In any event, the consequence of this epistemological slant has been that comparative judicial politics scholars have tended not to stress the constitutive and constraining role of law (Hilbink 2008, pp. 1099-1100). Rather, constitutional courts are modelled as political institutions motivated by the justices' ideological values and institutional power-building aspirations (Hirschl 2008, Vanberg 2015). Other actors in constitutional politics, too, such as constitutional designers, are modelled in similar ways (Ginsburg 2012). This feature of comparative judicial politics thus further emphasizes its distinctiveness from CCL.

Ironically, the main thing that the two lines of research have in common is that both have a somewhat awkward relationship with their parent field. While technically a sub-field of comparative politics, comparative judicial politics is better understood as an outgrowth of the US political science field of law and courts (aka 'political jurisprudence' or 'judicial politics').[3] Just as comparative constitutional lawyers tend to be national constitutional lawyers in the first instance, so do comparative judicial politics scholars tend to be US law-and-courts scholars who study constitutional politics in non-US settings. In line with these origins, one of the main purposes of the field has been to extend the methods that were developed to study US constitutional politics to study constitutional politics in the rest of the world. It has only been in the last few years that a significant number of European political scientists have started to become involved in the field (Dyevre 2010, p. 299). But this has occurred too late for them to influence the culturally specific, post-realist view of law that underpins it. By contrast, sizeable pockets of scholars based in the major European constitutional democracies (Germany, France and Italy) and in Australia do research in CCL. These scholars have been quite resistant to the post-realist view of law's relationship to politics that pervades comparative judicial politics (Möllers & Birkenkötter 2014, Von Bogdandy 2016), and their presence in CCL complicates calls for interdisciplinary law/social science scholarship on the US model.

[3] These sorts of comparisons are not conclusive, but note for example that the *Oxford Handbook on Law and Politics* (Whittington, Kelemen & Caldeira 2008), includes six chapters on comparative judicial politics (and four others on international law) while the *Oxford Handbook on Comparative Politics* (Boix & Stokes 2007) has only one such entry.

Against this background, the rest of this section briefly turns to consider the main topics of interest in comparative judicial politics. While this sort of exercise is always a little subjective, the following six topics give a sense of the main preoccupations of the field: (1) the political origins of judicial review and the causes of the spread of global constitutionalism; (2) the conditions under which constitutional courts build their institutional legitimacy and play consequential roles in national political systems; (3) the contribution that the establishment and consolidation of judicial review may make to democratization; (4) the dynamics of, and political rationale for, authoritarian constitutionalism; (5) constitutional 'endurance' and other statistically analyzable phenomena, such as the declining influence of the US Constitution; and (6) the applicability of the main empirical models of US Supreme Court decision-making to constitutional decision-making outside the US. As before, the aim is not to give a comprehensive account of the literature relating to each of these topics but to give a flavor of the sort of research that is being conducted.

Measured by the depth of the theorizations and the number of countries covered, the most extensive work to date has been conducted on topic (1). The two major contributions to the literature are Ginsburg's (2003) 'insurance theory' of judicial empowerment and Hirschl's (2007) 'hegemonic preservation theory' of the judicialization of politics. Ginsburg's theory, for its part, posits that ruling elites and aspirant political power-holders adopt judicial review as a form of insurance against future electoral defeat (p. 18). It further posits that the strength of the judicial review system adopted will correlate to the degree of political uncertainty at the time of adoption (p. 24). Thus, a threatened elite that fears an electoral loss, but which still has considerable bargaining power at the time of adoption, will insist on relatively strong judicial review powers. That demand may be conceded by an aspirant political power-holder that sees constitutional negotiations as the most realistic route to power and which in any case reckons on its capacity to control judicial review after power has been transferred. The situation is reversed where the incumbent power-holders do not fear electoral defeat or where the aspirant power-holder has greater bargaining power at the time of adoption (p. 24).

Hirschl's 'hegemonic preservation theory' also explains the spread of constitutionalism and judicial review as a response to political uncertainty, but gives that response a more particular motivation. According Hirschl's theory, the rapid global rise of constitutionalism and judicial review is a function of strategic action on the part of political, economic and judicial elites to respectively 'preserve or enhance their political hegemony', 'promot[e] a free market', and 'enhance their political influence and international reputation'

(p. 12). He proceeds to illustrate this thesis in a comparative study of the judicialization of politics in Canada, Israel, South Africa and New Zealand. Like Ginsburg (2003), Hirschl's theory stresses interest-based factors over differences of legal culture and institutional tradition in explaining the nature and timing of the judicialization of politics. In his treatment of South Africa, for example, that country's long tradition of legalism rates barely a mention. Instead, all the emphasis is placed on the interests of power-preserving elites.

It is perhaps not surprising that interest-based theories should have dominated the literature on the political origins of judicial review. If they are going to work anywhere it is during the moment of constitutional pacting, which most closely fits these theories' pared down vision of social reality. As soon as the focus shifts to the second area of concern – the conditions under which constitutional courts are able to build their institutional legitimacy and play a consequential role in national politics – the theorizations become more diverse. Here, the literature focuses on the ongoing dynamics of judicial review, particularly in new democracies – a topic that can be studied historically for older democracies, as it has been in in the case of the US and Australia, for example (Knight & Epstein 1996, Galligan 1987).

The first major theorization in this area, the so-called 'political fragmentation' thesis, holds that an important condition for the expansion of judicial power, especially in new or otherwise fragile democracies, is the existence of significant political competition. The more fragmented the political system, the argument goes, the more likely it is that multiple political players will look to the judiciary, first, to protect their fundamental political interests and, secondly, to safeguard the democratic rights required to ensure a fair electoral process. Chavez (2004, 2008), for example, has argued that variations in judicial independence at the national and provincial level in Argentina may be attributed to fluctuations in the concentration of political and economic power. Ginsburg (2003) similarly points to political diffusion as an important condition for the emergence of strong constitutional courts in Asia.

The next set of theorizations of the conditions under which constitutional courts become forceful actors in national politics directly addresses the question of judicial agency and asks whether there is anything constitutional courts may do, assuming a reasonably favorable political context, to build their institutional power. The leading approach in this respect is Epstein, Knight & Shvetsova's (2001) 'tolerance interval' theory of judicial empowerment, which also features in Ginsburg (2003) and in the so-called 'strategic' approach to judicial decision-making (Vanberg 2005, Staton 2010, Helmke 2012). According to all these theorizations, constitutional judges may and do act strategically to build their institutional power. They do this principally by anticipating the likely political repercussions

of their decisions, and adjusting their decisions so as to maximize the chances that they will be enforced (Epstein, Knight & Shvetsova 2001, p. 128, Ginsburg 2003, pp. 65-89). According to Epstein, Knight and Shvetsova's (2001) version of the theory in particular, this kind of strategizing may produce a virtuous cycle in terms of which each decision that is enforced widens the policy 'interval' into which a decision may be placed, making it easier and easier to avoid negative political repercussions (p. 128).

There is a slight difference in the thrust of these theories between stable and unstable settings. In stable settings, the purpose of strategic decision-making is said to be to maximize the court's policy influence (Epstein & Knight 1998), whereas in unstable settings, the point of strategic action is to build the court's institutional legitimacy, or at least to avert some or other institution-threatening attack or personal judicial setback (Helmke 2012, Ramseyer & Rasmusen 2001, Staton 2010). The common thread, however, is the notion that judges may take their court's institutional fate into their hands to a certain extent, and exploit whatever political space exists to maximize their influence in national politics. This set of theories is thus distinctly different from, but not necessarily incompatible with, the political fragmentation thesis. Whereas the latter stresses external political factors, the former stresses judicial choices. But they may be reconciled in so far as the capacity for judicial action is seen to be at least partly structured by the external political environment.

There has been surprisingly little work done directly on the role of constitutional courts in democratic consolidation (topic (3)). While Mietzner (2010) offers an explanation of the Indonesian case, he does not seek to generalize it. The same is true of Maveety and Grosskopf's (2004) work on the Estonian Supreme Court. Issacharoff (2015) offers a comprehensive account of the role of constitutional courts in sustaining democracy in different settings, but eschews political science theorizing in favor of analyzing 'the role of law in the structure of constitutional democracy' (p. 10). The closest thing to a general theory is Ginsburg's (2013) account of the role of constitutional courts in democratic consolidation in four Asian countries. By examining common themes in the constitutional politics of South Korea, Taiwan, Thailand and Pakistan, Ginsburg provides support for a tripartite classification of constitutional courts as 'upstream triggers of democracy', 'downstream guarantors' of authoritarian 'exit bargains', and 'downstream democratic consolidators'. This classificatory scheme is a useful start. As Ginsburg's himself notes, however, it is less than a 'complete theory' (p. 63). Rather, it provides a way of distinguishing the types of role constitutional courts may play, thereby facilitating exploration of such issues as the mutual compatibility of the roles and their particular contribution to the consolidation of democracy.

Comparative judicial politics scholarship on authoritarian constitutionalism (topic (4)) has recently been the subject of a specialized review in this journal (Moustafa (2014)). This topic will thus not be considered here, save to mention Tushnet (2015) as an important contribution that has since been published.

Constitutional 'endurance' (topic (6)) has recently emerged as a major area of research interest following the establishment of the Comparative Constitutions Project, a large-scale, database-driven project aimed at generating empirical data to support constitutional-design advice, but also increasingly used for other research purposes. The first study emerging from this project (Elkins, Ginsburg & Melton 2009) provided a wealth of fresh data and insights into the average lifespan of constitutions and the factors that condition constitutional longevity. Related studies include large-N dataset analyses of the declining influence of the US Constitution (Law and Versteeg 2012) and the clustering together of world constitutions into two contrasting ideological sets (Law and Versteeg 2011).

The final major area in the comparative judicial politics literature (topic (6)) is the voluminous large-N quantitative work investigating the determinants of constitutional decision-making outside the US (Iaryczower, Spiller & Tommasi 2002, Carruba, Gabel & Hankla 2008, Amaral-Garcia, Garoupa & Grembi 2009, Garoupa, Grembi & Ching-pin Lin 2011, Kapiszewski 2011, Garoupa, Gomez-Pomar & Grembi 2013, Escresa & Garoupa 2013). This work mainly consists of attempts to test whether the three empirical models of US Supreme Court decision-making – the legalist, attitudinal and strategic models – can explain constitutional decision-making in other parts of the world. The conclusion emerging from this work is that these models have some purchase, but generally need to be adjusted to take account of the differing institutional and political circumstances in which foreign constitutional courts operate (Roux 2015).

TWO FIELDS OR ONE?

In light of the foregoing brief survey, this section considers how comparative research by legal academics and political scientists on judicially enforced constitutionalism should be understood. Is this group of scholars contributing to one reasonably coherent, multidisciplinary field? Or is their research better understood as falling into the separate fields of CCL and comparative judicial politics? The foil for this analysis will be Hirschl's (2014) argument that CCL needs to develop into the multidisciplinary field of CCS in order to realize its full potential. The section will contend that Hirschl's argument is overdone in

several respects and that, if followed, his suggestions would run the risk of subsuming CCL under comparative judicial politics.

It is necessary first to clear some terminological ground about the meaning of the terms 'field' and 'discipline'. The intention is not to be dogmatic, but to define these terms in a way most favorable to Hirschl's argument so that it can be fairly assessed.

While the two terms are often used interchangeably, one way of distinguishing an academic field from a discipline is to stipulate that the former draws its identity primarily from the set of phenomena being investigated while the latter refers to a branch of knowledge that is curated by scholars with a distinct professional training and outlook. Although a field, on this definition, may be the exclusive preserve of a particular discipline, this is not necessarily the case. Because the identifying feature of a field is the phenomenon studied, fields lend themselves to interdisciplinary and multidisciplinary scholarship – to the contribution of a range of disciplines investigating roughly the same phenomenon from different perspectives. In order to make progress in a field, however, there still needs to be a shared conception of what its main purposes are. Failing that, what may look in the first instance like a single field may in reality be multiple fields – two or more groups of scholars from different disciplines investigating roughly the same phenomenon, but with such different purposes in mind, and deploying such radically different conceptual frameworks and methodologies, that it makes more sense to think of them as working in separate domains.

On Hirschl's (2014) account, CCL is a single field, but not a very coherent one. 'There is considerable confusion', he says, 'about its aims and purposes, and even about its subject—is it about constitutional systems, constitutional jurisprudence, constitutional courts, or constitutional government and politics?' (p. 4) In Hirschl's view, the reason for all this 'confusion' is that CCL has abandoned its roots in the writings of Aristotle, Jean-Jacques Rousseau and Henri-Benjamin Constant (p. 153). Instead, Hirschl argues, the field has been 'appropriat[ed]' (p. 164) by academic lawyers who do not really understand the rules of 'inference-oriented' comparison (p. 225). While they have done some interesting classificatory work, he concedes, academic lawyers too often engage in lazy or just unproductive comparisons, and tend to focus on a core group of 'usual suspect' countries (p. 211). CCL is only likely to progress, he concludes, once academic lawyers, as the numerically dominant group, commit to learning from social scientists about how to conduct rigorous, 'inference-oriented' comparative research (p. 244).

This provocative analysis of the state of CCL and Hirschl's call for its progression into CCS has rightly been welcomed as a major contribution (Dixon 2016, Tushnet 2016,

Gardbaum 2016). In its breadth of learning and the forcefulness of the arguments presented, it injects new life and significance into comparative research on constitutionalism and judicial review. Nevertheless, there are several problems with Hirschl's analysis that cast doubt, both on his assessment of the state of CCL and also on his suggestions for its future development.

First, Hirschl's claim that CCL is confused is a function of the way he defines the field as a broad, multidisciplinary endeavor. As we saw in the previous section, if CCL is defined more narrowly as the work that academic lawyers have been doing on judicially enforced constitutionalism, it emerges as a self-contained field with a reasonably coherent set of purposes. In the beginning, perhaps, there was some confusion over whether CCL was about the convergence of constitutional law in different legal systems on a common set of concepts and methods, or instead about understanding the diverse impact on the practice of constitutional law of the post-1945 spread of judicially enforced constitutionalism. It is now clear, however, that CCL is about the latter undertaking. For some scholars, as we have seen, that means working through the 'migration of constitutional ideas' (Choudhry 2006) on a doctrinal level. For others, it means reflecting in a more detached way on the methodologies of constitutional comparison that judges and other involved in doctrinal construction are deploying. For yet others, it means examining how similar-seeming concepts take on different meanings in different legal systems. The common thread in all of this research, however, is an interest in *understanding* the impact of the spread of judicially enforced constitutionalism on the practice of constitutional law. As such, CCL is quite distinct from comparative judicial politics, which is directed at *casually explaining* the political origins and ongoing dynamics of this phenomenon. CCL, according to this sense of things, only becomes confused once it is assumed to be a continuation of the formerly broad field of comparative constitutionalism with the divergent set of purposes that Hirschl attributes to it. Defined in that way, it is indeed uncertain what the real aim of CCL is. But that uncertainty is a product of Hirschl's definitional move rather than an inherent property of the field.

The second problem with Hirschl's analysis is that his call for CCL to reconnect with its roots in classical political philosophy and at the same time to get over its obsession with constitutional courts is in tension with his own understanding of the phenomenon at the heart of the field. In so far as the object of study is taken to be the post-1945 'global spread of constitutionalism and judicial review' (Hirschl 2014, p. 1) it is neither surprising nor necessarily regrettable that (a) responses to this phenomenon have occurred from within political science and law; and (b) constitutional courts have been at the center of the analysis. In the eighteenth and early nineteenth century, when the classical political philosophers

Hirschl admires were writing, law and political science did not exist as separate academic disciplines. To suggest that we should return to the broader nature of these authors' inquiries is thus a little anachronistic. The disciplines of law and political science have arisen in order to formalize different types of research undertaking and it is an open question whether scholars might be able to achieve more working within the constraints of these disciplines as opposed to engaging in a broader multidisciplinary field. Likewise, CCL's focus on constitutional courts is a consequence of the central role that such courts have been given in the enforcement of the post-1945 Constitutions. In the wake of this phenomenon, academic lawyers have been drawn to analyzing the methodologies of judicial recourse to foreign law. They have at the same time paid attention to the legitimacy of the institutional roles that constitutional courts have been performing from the perspective of liberal constitutional theory. These features of CCL are entirely appropriate given the nature of the subject matter being investigated.

A third less than fully convincing aspect of Hirschl's analysis is his depiction of legal academics as having 'appropriated' the field of CCL (p. 164). Pointing to the dominance of legal academics in the *Oxford* and *Routledge Handbooks of (Comparative) Constitutional Law* (Rosenfeld & Sajó 2012, Tushnet, Fleiner & Saunders 2013), another compilation by Elsevier (Ginsburg & Dixon 2011) and in the *International Journal of Constitutional Law*, Hirschl argues that legal academics have reduced the focus of CCL to their narrow concerns. The problem with this argument is that it is once again dependent on Hirschl's construction of CCL as a broad, multidisciplinary endeavor. If we define CCL instead as research on the changing nature of constitutional law *practice* in the wake of the global spread of constitutionalism and judicial review, things look very different. From that perspective, CCL is dominated by legal academics, not because they have appropriated the field from political scientists, but because the field is primarily legal-interpretive and normative in character. As before, in other words, what Hirschl presents as evidence of his thesis is in fact an artifice of his definitional choice. Had he reversed the question, and asked what proportion of scholars in comparative judicial politics work principally in political science, he would have found a similar numerical dominance the other way.[4]

The fourth and final problem with Hirschl's analysis is that his call for CCL to progress to CCS, while couched in the language of big-tent multidisciplinarity (pp. 13-15, 18, 191), at times has a harder edge to it – one that comes close to the subsumption of CCL under

[4] See, for example, the comparative section (Part III) of the *Oxford Handbook of Law and Politics* (Whittington, Kelemen & Caldeira 2008).

a monolithic social science conception of the field. Hirschl's book is thus peppered with disparaging remarks about the quality of legal research, and doctrinal research in particular.[5] Hirschl's apparent disdain for legal scholarship comes to a head in chapter 6, where he identifies 'four modes of comparative inquiry in constitutional law': (1) single-country studies, (2) 'self-reflection or betterment through analogy, distinction and contrast', (3) the generation of 'concepts and analytical frameworks for thinking critically about constitutional norms and practices', and (4) 'theory-testing and explanation through causal inference'. While ostensibly setting these four modes out in methodologically neutral terms, it is clear by the end of the discussion that Hirschl is in fact ranking them in ascending order of scholarly worth, and that the normative baseline he is using for this is a social science standard of explanatory power. He thus concludes his analysis by saying that 'comparative constitutional law scholarship, its tremendous development in recent years notwithstanding, often (though certainly not always) falls short of advancing knowledge *in the manner sought by most social scientists*' (p. 244, emphasis added). Quite so, but this criticism only makes sense if 'advancing knowledge in the manner sought by most social scientists' is the purpose of all 'comparative constitutional law scholarship', which is clearly not the case. This criticism thus says more about the disciplinary lens Hirschl is using to assess CCL research than it does about the quality of scholarship in the field.

These four problems with Hirschl's analysis mean that his call for CCL to progress to CCS needs to be treated with caution (Möllers & Birkenkötter 2014, Von Bogdandy 2016, Gardbaum 2016, Jackson 2016, Young 2016). At best, the case he makes for the desirability of a social science turn in CCL is stronger in relation to CCL Type 3 than it is in relation to CCL Types 1 or 2. In relation to CCL Type 1, the call for academic lawyers to embrace social science standards of inference-oriented comparison makes no sense in so far as the purpose of their research is not causal theory-building but contributing to the construction of constitutional law doctrine in a particular legal system. Since the methodological standards for this sort of work are set by the legal tradition in which the scholar is working, the extent to which social science methods are relevant depends on the practices of that tradition (Roux 2014). Similarly, the purpose of CCL Type 2 is not to develop general theories about the

[5] See, for example, his reference to 'mere doctrinal accounts' on p. 13; his statement on p. 160 that '[v]irtually all the grandmasters of 20th-century constitutional design literature' and the 'literature on the transition to and consolidation of democracy' have been political scientists' (a gratuitous slight since those are not principally areas of legal-academic research); his lament on p. 163 that CCL scholarship is 'court-centric' (a misplaced criticism since constitutional courts are quite appropriately central to the phenomenon to which academic lawyers are responding); and his comment on p. 165 that 'constitutional jurisprudence' is 'considered the central component of the constitutional universe' (which criticizes doctrinal researchers for being doctrinal).

political origins and ongoing dynamics of the global spread of judicial review, but to understand the different ways in which judges have responded to this phenomenon. That is principally a legal-interpretive question, not a causal-explanatory one, and thus the sorts of social science methods Hirschl advocates are again inappropriate.

Hirschl's call for academic lawyers to familiarize themselves with the rules of inference-oriented comparison is most relevant to CCL Type 3. Even here, however, the argument needs to be more attentive to the distinctive purposes academic lawyers have been pursuing. For many researchers, the point of examining doctrines like proportionality and social rights is not to ask why they have spread, but to understand the different forms that they take in different legal cultures (Bomhoff 2013). That is a classic comparative law question, and there is no reason necessarily to confound it with a positivist social science interest in causal explanation. Occasionally, to be sure, legal scholars working on these topics do make causal claims. Thus, Cohen-Eliya & Porot (2013) include a chapter in their book on *Proportionality and Constitutional Culture* on the reasons for the spread of this doctrine, and Landau (2012) asks whether strong-form or weak-form remedies for social rights violations have had a greater impact on alleviating poverty. Both of those are causal questions that need to be answered using the methods Hirschl advocates. But in fact, in both these instances, the scholars concerned recognize this and conform to the methods he prescribes.[6] There are, of course, other examples where legal academics have been less careful, and Hirschl is right to call those scholars to account. But to use social science methods of inference-oriented comparison as the *general* standard from which to critique academic lawyers' work in CCL is another matter.

The fact that there are a number of problems with Hirschl's argument does not mean that his call for CCL to progress into CCS has no merit. There *is* such a thing as disciplinary myopia, and thus legal researchers and political scientists working on judicially enforced constitutionalism do need to engage with each other. This conversation, however, needs to start from the premise that CCL (as defined here) has its own purposes and methods that cannot always be easily combined with a positivist social science interest in casual explanation. The final section of this article sets out this different conception of the future direction of comparative research on constitutionalism and judicial review.

[6] Hirschl (2014) praises Cohen-Eliya & Porat (2013) as an example of 'methodologically astute small-N research design' (p. 258). That praise is due in respect of that part of their work that does seek to investigate a causal claim, but it is beside the point in respect of their broader interpretive project.

FUTURE DIRECTIONS

Any move towards greater dialogue between CCL and comparative judicial politics needs to recognize both the distinctive purposes of each of these fields and also the limits on interdisciplinary research across law and political science.

To begin with, it needs to be acknowledged that genuinely interdisciplinary research across these two disciplines, like all interdisciplinary research, is 'hard to do' (Fish 1994, pp. 231-42). Law, if it amounts to a distinctive discipline at all, is distinctive because of the participatory-insider perspective that legal academics adopt (Roux 2014). That perspective is difficult to marry with the external-observer perspective of political science work on constitutionalism and judicial review. Even historical-institutionalist approaches, while more attentive to the constitutive and constraining role of law, do not seek to contribute to the construction of legal doctrine. Researchers will thus usually need to choose which audience – a legal-professional or scholarly audience – they would rather address (Roux 2015).

The extent to which social science perspectives may be incorporated into legal research is also contingent on the extent to which this already happens as a matter of professional legal practice. The post-realist US understanding of the relationship between law and social science is very different from that in Europe, say. On the one hand, this means that interdisciplinary law/social science research comes more naturally to scholars in the US than it does to scholars elsewhere. On the other, it means that incorporating social science perspectives into legal research may not actually be all that interdisciplinary in societies where law has already absorbed such perspectives into its argumentative logic (Balkin 1996). In national research fields, these differences in law's relationship to social science do not really matter, but they begin to matter in transnational fields like CCL where one researcher's natural, even unconscious, interdisciplinarity is another researcher's clash of paradigms.

It is presumably for these sorts of reasons that the new legal realist movement in socio-legal studies in the US has settled on a conception of interdisciplinary law/social science scholarship as 'translation' between these two paradigms, rather than actual synthesis (Erlanger, Garth, Larson, Mertz, Nourse & Wilkins 2005, Suchman & Mertz 2010, Macaulay & Mertz 2013). The idea is that 'the best learning from the social sciences can be brought to bear on legal problems *without losing the nuances and priorities of either field*' (Tomlins 2006, emphasis added). In research on judicially enforced constitutionalism, adopting this approach would mean that legal scholars and political scientists should familiarize themselves with each other's research paradigms as sources of productive challenge to, and inspiration for, their own work. But CCL and comparative judicial politics should continue as

separate research endeavors. At some meta-level, of course, the collective efforts of scholars working in those two fields could be thought of as falling into the multidisciplinary field of CCS. Any formal institutional move to that effect, however, would need to acknowledge that neither law nor political science could unilaterally set the research agenda for the field or police its methodological standards.

Engaging each other's research paradigms as distinctively different enterprises in this way, both political scientists and comparative constitutional lawyers would have much to gain. As noted earlier, comparative politics research on judicially enforced constitutionalism has been dominated by behavioralist and rational choice approaches that tend to ignore the constitutive and constraining role of law. The problem with these approaches is that they assume that law's autonomy from politics is everywhere the same – or worse, everywhere the same as it is in the US. And yet, the relative degree of law's autonomy from politics, as Nonet & Selznick (1978) long ago argued, is one of the main differences between legal cultures. It is thus also a core variable for political science research on judicially enforced constitutionalism (Hilbink 2008). Indeed, one of the central questions that comparative judicial politics scholars need to address is the conditions under which fidelity to law's internal constraints might emerge as a distinct motivation for human action. That is an old sociological question, of course. In comparative judicial politics, however, it has been suppressed by the field's dominance by US political scientists, whose post-realist conception of the law/politics relation functions as an unconscious premise for their research. Legal academics, and especially non-US legal academics, could be quite helpful here in addressing this blind spot. Some of the insights emerging from CCL Type 3 research on the influence of constitutional culture on the migration of constitutional ideas, for example, could be used to distinguish between societies on the basis of their commitment to an ideology of law's autonomy from politics. In this way, the interpretive insights of CCL could be used to inform conceptualizations of key variables in causally oriented comparative judicial politics research.

Comparative constitutional lawyers, in turn, need to be more disciplined about how they utilize social science research findings or themselves engage in such research. As with academic lawyers in other areas, comparative constitutional lawyers do sometimes slide into making causal-empirical claims or frame research questions in ways that can be answered only by using appropriate social science methods. In those instances, academic lawyers should either desist from making such claims and asking such questions or acquire the skills needed to do the job properly.

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