

STANDARDS, NETWORKS, AND THE POLITICAL ECONOMY OF INTERNATIONAL LAWMAKING: A RESPONSE TO STAVROS GADINIS

*Paul B. Stephan**

*Three Pathways to Global Standards*¹ broadens our understanding of structures that undergird international cooperation. Stavros Gadinis argues that different kinds of lawmaking networks propagate differently. Private networks depend on market success, in the sense that the demand for their products rests on competition in the private sector. Regulators succeed when they cooperate with true peers. States use power to work their will. I have some second-order criticisms of the article, offered in the spirit of respectful engagement with good scholarship. These reservations, however, do not detract from a view that Gadinis has identified significant issues in international relations and has proposed useful theses about them as well as good strategies for their validation.

For roughly two decades now, international law professors have sought to open up the black box that is the nation state to examine how variation in domestic structures and politics affects outcomes in the production of international law. Inspired by the work of Anne-Marie Slaughter, many law professors and a few political scientists have tried to demonstrate how informal cooperation among officials and related stakeholders have managed to overcome obstacles to international cooperation. Networks, not states, became the focus of scholarly research. Slaughter focused on high profile topics such as human rights, but her followers in legal academia mostly have clustered more in the mundane field of economic regulation. Much of the work has looked at regulation of globalized industries, of which financial services provides an excellent example.

Successful proposals invite revisionism. Pierre-Hugues Verdier demonstrated that, at least in the financial sector, regulatory networks do less than network enthusiasts expect and, for structural reasons, are likely to continue to disappoint.² In this article, Gadinis seeks to mediate between enthusiasts and skeptics by arguing that different kinds of networks function differently. He identifies three types of networks, investigates how they succeed in diffusing regulatory standards, and identifies different sets of normative arguments that might apply to each type.

Rather than highlight the article's ample accomplishments, I will use my limited space to focus on what left me unsatisfied. As Gadinis offers his work as an invitation for further empirical investigation of other networks, I will raise the questions that I hope he and others will address before going down this road much further.

* *Professor of Law at the University of Virginia School of Law.*

Originally published online 05 August 2015.

¹ Stavros Gadinis, *Three Pathways to Global Standards: Private, Regulator, and Ministry Networks*, 109 AJIL 1 (2015).

² Pierre-Hugues Verdier, *Transnational Regulatory Networks and Their Limits*, 34 YALE J. INT'L L. 113 (2009); *The Political Economy of International Financial Regulation*, 88 IND. L.J. 1405 (2013). Gadinis himself has expressed some skepticism about the normative appeal of these networks. Stavros Gadinis, *The Politics of Competition in International Financial Regulation*, 49 HARV. INT'L L.J. 447 (2008).

Inputs—What is a Network?

Gadinis proposes a theory in which the type of network determines the mechanisms for diffusion. A particular network type is thus an input (independent variable), and adoption of a particular regulatory framework is an output (dependent variable). For the theory to work, one needs a reasonably clear sense of what counts as a network and how it fits into the three types that Gadinis identifies.

I question, however, the usefulness of Gadinis's third type. He wants to distinguish between the upper reaches of an executive, where tradeoffs among policy areas can occur, and regulators with a defined and narrow competence. But a ministry network (his term) comprising senior executive-branch officials is not really a network at all. As noted above, the network project started as an effort to unpack the state to look at the impact of disaggregated components on international relations and international law. But executive branches, I had always thought, stand for the aggregated state.

What is distinctive about a network of states? How is it different from an international organization (IO)? Is it the level of staffing at the international level? Although IOs may tend toward bureaucratic bloat, instances of minimally staffed institutions have been observed in the wild. The GATT, one of the most spectacularly successful IOs during its life from 1948 to 1994, began with a handful of contract employees. Is it the absence of a treaty constituting the institution? This distinction would beg an important question, namely whether formal charters do any interesting work. Does the influence of the Financial Action Task Force (FATF), the ministry network that Gadinis studies, turn on the absence of a treaty constituting its authority? I doubt it.

To be fair, Gadinis here is following in the steps of other scholars that seem to define a network by the lack of a formal treaty structure rather than the nature of its participants.³ I don't blame him for not challenging preexisting scholarly practice, however problematic it may be. But I am not convinced that a ministry network, as Gadinis uses the term, is anything other than an IO. Look, for example, at his Table 4, where he assigns private, regulator and ministry status to a number of international regulatory institutions. In the blank spot for trade and product specifications/ministry, why not insert the WTO? And for human rights/ministry, what about the Human Rights Commission (or, for that matter, the Council)? Treaties, to be sure, embody the WTO as well and the Human Rights Commission and Council. But why does this matter?

Either ministry networks are instances of IOs, in which case the network concept no longer addresses disaggregation, or networks are distinct from IOs because of the absence of a treaty, in which case the network concept drops out and we look instead to the distinction between hard and soft law. Either way, we have strayed rather far from the original project, which was to look at the dwindling significance of nation states in the age of globalization. When addressing the question that Gadinis asks, namely how it is that networks differ, one might respond that some structures are not networks.

If I am right, then Gadinis's finding about the FATF is both more and less significant. On the one hand, what he says about the FATF doesn't tell us anything about networks, because a more stringent definition of the concept would exclude the FATF. On the other hand, by demonstrating that the success of the FATF in imposing its rules on states turns on power above all else, it suggests something generally about IOs. Power more than normative appeal might explain IO success, as many international relations specialists seem to believe and as many academic international lawyers seem to deny (or at least find discomfiting).

Once we kick out ministry networks, we are left essentially with two categories, which can be recharacterized as private and public. Gadinis then helps us to understand the differences in both the content and successful

³ E.g., Chris Brummer, *How International Financial Law Works (And How It Doesn't)*, 99 GEO. L.J. 257 (2011); Richard K. Gordon, *On the Use and Abuse of Standards for Law: Global Governance and Offshore Financial Centers*, 88 N.C. L. REV. 501 (2010); Kenneth S. Blazewski, *The FATF and Its Institutional Partners: Improve the Effectiveness and Accountability of Transgovernmental Networks*, 22 TEMP. INT'L. & COMP. L.J. 1 (2008).

adoption of regulatory standards originating in the private and public spheres. This result is not as granular as that proposed by his article, but it has the attraction of rigor and coherence.

Outputs—What Counts as Cooperation?

Gadinis tests the effectiveness of networks by studying the adoption by states of particular products generated by the networks. In each of his three case studies, the dependent variable is accession to an agreement. The beauty of this approach is that this variable is easy to specify and to locate in a particular year. The downside is its avoidance of questions about the significance of signatures.

A deep and interesting debate addresses generally the distinction between formal and substantive adherence to international obligations.⁴ One cannot criticize Gadinis for ducking these big questions. But the issue has particular salience to the claims that he seeks to test. To be concrete here, I question whether the timing and sequence of joining the Multilateral Memorandum of Understanding (MMOU) produced by the International Organization of Securities Commissions (IOSCO) tells us anything important. As this is what Gadinis measures, he obviously thinks that the phenomenon is significant. Let me explain the basis for my doubts.

To this casual observer, the obligations entailed in the MMOU look like a less rigorous version of those contained in the various bilateral executive agreements that U.S. antitrust regulators have entered into over the past four decades with many of that country's significant trading partners.⁵ The MMOU is, as the name indicates, multilateral, but otherwise the commitments to positive and negative comity look, to my untutored eye, very similar. Yet the bilateral antitrust agreements are not network products, as the most important of them long antedated the Clinton Administration's creation of the International Competition Network. Moreover, the network for financial regulation, IOSCO, functioned for decades before coming up with the MMOU. Why should a late-in-life accomplishment define, or even illuminate, an old institution?

To me, the MMOU looks like a copy-cat move prompted more by bureaucratic envy than a desire to optimize global investment regulation. (Not, mind you, that it is inconsistent with optimal enforcement). What evidence do we have that it does anything more than confirm practices that might exist in its absence, and which might be modeled on cooperation among competition-regulation peers? In particular, is there any correlation between adoption of the MMOU and participation in comparable competition-regulation agreements?

To be clear, I don't know the answers to these questions. Perhaps the MMOU has accomplished a lot. But I wonder. I do know that the United States has agreed in principle to recognize another state's disclosure regime as an adequate substitute for its own, but only on a bilateral basis.⁶ The MMOU is not such a mutual recognition commitment, and the fact that it is not indicates something important about its limits.

This specific question points to a more general observation. The revisionist critique of the early network scholarship is that networks help to solve some coordination problems, but lack the capacity to address issues with significant distributional consequences. Gadinis's case study of IOSCO and the MMOU does not belie that point. I don't quarrel with the observation that MMOU acceptance tells us something about the influence of regulatory networks on dissemination of regulatory standards. But the ability of networks, rather than states, to address significant problems remains unproved.

This leads to a deeper issue that Gadinis understandably, but regrettably, avoids addressing. Each of his case histories involves a success story, in the sense that formation of a network (as he uses the term) was followed

⁴ One of my favorite explorations of this distinction is Oona A. Hathaway, *Do Human Rights Treaties Make a Difference?*, 111 YALE L.J. 1935 (2002). The discomfort and pushback that this article generated suggests, at least to me, that Hathaway was on to something important.

⁵ For a compilation, see *Antitrust Cooperation Agreements*, THE UNITED STATES DEPARTMENT OF JUSTICE.

⁶ Pierre Verdier, *Mutual Recognition in International Finance*, 52 HARV. INT'L L.J. 55 (2011).

at some later date by widespread state adoption of a particular regulatory standard. What this inquiry lacks are data about failed regimes. Gadinis notes in passing that some unsuccessful state-to-state and IO efforts had taken place before the International Accounting Standards Board (IASB) produced the International Financial Reporting Standards. But what explains the inability of these earlier projects to obtain support? What characteristics might allow us to predict success or failure?

This question relates to another point that Gadinis makes in passing. He asserts that his data “helps refute the claim that the substantive character of each area calls for a particular institutional actor.” His evidence, however, is only that, as a matter of history, different kinds of networks sought to take up the regulatory standards that IASB, IOSCO and FATF ended up promulgating. But why isn’t it significant that some networks succeeded in producing standards that states then adopted and others did not? The right question to study, I would have thought, is not which notions strike which groups, but rather what kinds of problems—coordination or collective action, for example—find their solutions (to the extent they do) in particular kinds of international structures? Is it so clear that the success of particular international structures in promoting international cooperation is independent of the type of problem that cooperation might address?

I realize studies of failure are depressing. Moreover, an objective definition of failure, the dependent variable to be investigated, may present challenges. But research that looks only at success stories can be seriously misleading. A famous example from financial economics involves the purported equity premium, namely the apparently inexplicable risk-related rate of return on equity investments as evidenced by records of publicly traded stocks. Researchers finally realized that a hidden issue in measuring stock performance is survival bias. The earlier work did not account for market collapse, i.e., the wiping out of an entire market of securities due to war, invasion, revolution or similar disaster. By focusing only on the United States and Great Britain, which had not experienced such an event over the last two centuries, the researchers missed something important. Adding results from, say, Russia, the Netherlands and Germany into the studies significantly reduces the premium.⁷ Similarly, until empirical studies take on the problem of failed international regulatory projects, our knowledge of what networks do will be importantly incomplete.

The Inevitability of Political Economy

In keeping with my skeptical (some might say depressing) tone, I now move from failure to resistance. International lawyers like international cooperation, because without some kind of coordinated international behavior we have nothing to study and nothing to offer the wider world other than our unfiltered normative aspirations. Most of us don’t like to consider arguments about why nation states should restrict and obstruct particular projects. Especially unappealing is the idea that states might create constitutional commitments to impede particular kinds of international cooperation.

Gadinis reviews the concerns some have raised about international regulatory cooperation. I am grateful that he includes my work here. In his necessarily brief coda responding to these questions, he focuses largely on reforming the network process, especially by broadening the range of stakeholders brought into the formulation of standards. I wonder if this is the right response.

First, the global administrative law project on which he draws offers much more in the way of normative vision than it does empirically based positive analysis of the effect of institutional design on regulatory outputs. He might have done well instead to look at research on private legislatures, which suggests that broadening of

⁷ Stephen J. Brown et al., *Survival*, 50. J. FIN. 853 (1995).

inputs tends to produce not clearer rules but rather greater delegation of discretion to downstream law-enforcers.⁸ This work suggests, although it certainly does not prove, that international administrative cooperation works as it does for a reason, and that for the most interested parties its deficiencies may be a feature, not a bug. Greater inclusiveness in the international regulatory structure may produce even worse outcomes, whether they be barriers to entry into regulated markets or greater uncertainty about the applicable rules.

An alternative strategy for discouraging bad regulatory outcomes is to make it easier for states to reject the products of regulatory networks. A commonly voiced concern about regulation based on transnational cooperation is that individual states, when deciding to adopt the standard, face a difficult take-it-or-leave-it choice. Accepting the outcome on offer, however flawed, might be better than becoming an outsider. Making it harder to accept such offers through domestic checks, whether constitutional or political, might lessen this concern.

For examples of what these check might look like, consider what national judiciaries have done. One important U.S. court has floated the idea of recognizing (others would say inventing) constitutional obstacles to some kinds of executive-branch participation in international regulatory cooperation.⁹ The Luxembourg Court similarly blocked the first U.S.-E.C. antitrust cooperation agreement on treaty grounds.¹⁰ Such national resistance probably makes it harder to implement all products of regulatory cooperation, but might disproportionately weed out the more problematic ones.

* * * *

I congratulate Gadinis on his illuminating and important article. International regulatory networks are not going away any time soon. He helps us to understand how they vary in what they do and what they produce. I especially welcome the publication in the American Journal of a rigorous empirical study that combines case studies with quantitative analysis. This is another example of how international legal scholarship, at least in the United States, is becoming more like legal scholarship generally.

⁸ Alan Schwartz & Robert E. Scott, *The Political Economy of Private Legislatures*, 143 U. PA. L. REV. 595 (1995). For application of these insights to international lawmaking, see Paul B. Stephan, *The Futility of Unification and Harmonization in International Commercial Law*, 39 VA. J. INT'L L. 743 (1999).

⁹ *NRDC v. EPA*, 464 F.3d 1 (D.C. Cir. 2006).

¹⁰ Case C-327/91, *Fr. v. Comm'n*, 1994 E.C.R. I-3641.