

Legal Scholarship Makes the World a Better Place

by Neil H. Buchanan, The George Washington University Law School

Abstract: This article responds to claims that law professors are engaged in scholarly pursuits that fail to serve important social functions. I argue that legal scholarship “matters” in important ways, and in particular that the legal academy has improved its service to society by embracing interdisciplinary approaches to studying the law.

Early in my academic career (or, more accurately, early in my *first* academic career, since my current life as a legal scholar was preceded by a career as an economics professor), I learned that I should not expect my work to have a noticeable impact on the world. One of my professors told me that it was important to do good work, and that the combination of good work by various academics could change the world for the better. However, a book like John Maynard Keynes’s *The General Theory* comes along only once in a century or so, and even then, no small amount of luck and happenstance must conspire to allow the great book to become seen as great, and eventually to change the world. It is our fate as academics, my professor told me, never to know when or how we have had our greatest impact.

Chastened and more than a bit disappointed, I pursued the point. Surely, I insisted, there are academics who testify before Congress, or who give advice to Presidents and Prime Ministers, and who rely on their academic work to inform their policy suggestions. Yes, my professor replied, and sometimes a policy maker will even credit a professor for making an argument that supports a policy initiative. However – and here is the truly deflating point – the policy makers generally “shop” for academics who have written things that support the already-formed policy

agenda of the politicians who are running the show. Academic writers do not necessarily influence policy makers, she said. Policy makers choose the academics to whom they will pretend to listen.

Cynical? Absolutely. Overstated? Quite possibly. I share that anecdote here, however, specifically to mock the title of this paper, “Legal Scholarship Makes the World a Better Place,” and its author – or, if not to mock myself, then at least to make clear that scholars might have to settle for making the world a better place in ways that have nothing to do with (directly) changing the way policy makers think. If the making of policy is generally such a cynical process, as my former professor claimed (and as everything I have seen in the decades since then has confirmed), how do academic writers make a difference? In particular, how do the writers of *legal* scholarship make a difference?

Certainly, there are plenty of people who claim that legal scholars are especially useless. We have been accused of being dilettantes, for reasons that I will describe (and attempt to refute) at the end of this essay. We have also been accused of deliberately making ourselves irrelevant, by writing on abstract topics rather than on questions of legal doctrine. Some judges (prominently including Harry Edwards, of the D.C. Circuit) have complained that law professors have largely abandoned what used to be their exclusive role, which was to write articles that were useful to sitting judges. Yet these and other complaints miss the point that legal scholarship has not become less relevant or less expert, but instead that our relevance and expertise has expanded, and our combination of skills has positive effects on real-world debates and policies.

I will start with an example of a “close call,” in which scholarly output seems *almost* to have changed the world in a rather “big” way, but not quite. This example, not coincidentally, has to do with my own work (which I have co-written with the constitutional law scholar Michael C. Dorf, of Cornell Law School), regarding the legal issues surrounding the federal “debt ceiling” statute. Starting in 2011, the then-new Republican majority in the House of Representatives broke with longstanding custom by refusing to increase the maximum limit on federal debt by an amount sufficient to accommodate the borrowing necessitated by Congress’s own spending and taxing laws. The new political strategy was to claim that it was the President who “wanted” more debt, and thus that it was merely a difference of opinion over policy that was at stake, because the new majority in the House deplored increases in the national debt.

This radical new political strategy took everyone by surprise. The very idea that a political party would threaten to allow a default on federal obligations for the first time in the country’s history had, in fact, been so unthinkable that there was virtually no existing scholarship pertaining to the questions raised by this threat. After some in-the-moment blog posts and op-eds by various legal scholars, the politicians averted the immediate threat by creating the ill-fated “super-committee,” which ultimately failed, and which then led to the disastrous “fiscal cliff” at the end of calendar year 2012.

Although none of the other scholars who had engaged in the initial debate about the debt ceiling chose to write anything further, Professor Dorf and I wrote what turned into a series of four academic articles (all published in the *Columbia*

Law Review), in which we argued that the President's most important constitutional duty is to honor the country's legal obligations to pay its bills, even if that meant that he had to issue debt in excess of the statutory ceiling. This response was, of course, also radical in its own way. It seemed to suggest that the President could "re-write the law" (which is not what we argued), or that the President could unilaterally "blow past" the debt ceiling (which is even further from our actual argument).

More to the point, ours was not a position that made *either* side of the political divide happy. The White House insisted that the only way to avoid default was for Congress to increase the debt ceiling sufficiently to allow the country's bills to be paid, in full and on time. This was true, as far as it went, but it simply avoided the real question, which is what the President should do if Congress ultimately refuses to be sensible. To be told by two professors that the President must, as a constitutional matter, avoid a larger constitutional violation by treating the debt ceiling statute as unconstitutional was not what the political advisors to the President wanted to hear. And as it happened, in each of the ensuing big showdowns over the debt ceiling from 2012 through earlier this year, the President succeeded in getting the other side to blink, which allowed him to avoid having to make the choice that we described.

Did anything we wrote matter? As my opening paragraphs to this essay suggest, I am completely willing (indeed, I have long been conditioned) to believe that the answer is a resounding no. Allow me, however, at least to imagine that this line of legal scholarship might have had some impact. Professor Dorf and I have been told, for one thing, that our views were circulated on Capitol Hill and in the

White House, and that the presidential strategy that we outlined was seriously considered. Moreover, because we had argued that the Republicans would actually be giving the President *more power* by allowing him to decide which laws to obey, it is at least imaginable that the Republicans' ultimate capitulation in each showdown was fed in part by their having learned (directly or indirectly from our work, which is the only source of this argument) that their attempt to tie the President's hands could end up making him stronger.

Still, we have no way of knowing what was happening inside the heads of the relevant players in this drama. Is there any objective evidence of our arguments having an impact on this series of crises? Actually, there is. One of our arguments was that the President is prohibited by the Constitution from "prioritizing" payments. That is, if a day were to come when the government was due to pay money to two different parties, both of whom held legally binding rights to be paid in full and on time (because of previous laws passed by Congress), but there was insufficient money to pay both parties, we showed that the President has no authority to decide who is the more deserving party, or that the other party must accept an extended IOU from the government. (And, as we demonstrated in our fourth paper, any promise to pay the currently disappointed party would also be "debt," which means that the President's decision to default on one of the payments would not even succeed in preventing a breach of the debt ceiling.) The legislative process determines priorities, and for the President to re-write those priorities, we argued, would be a more fundamental violation of the separation of powers than it

would be for the President to make an explicit decision to honor those priorities by issuing additional debt.

Even if one disagrees with our argument (although I should note that no one has, to this point, written a response that engages with our constitutional argument, in a law review or even in a “tweet”), there is evidence that our argument changed the political/policy dynamic. Republicans implicitly acknowledged the problem that we identified by proposing legislation specifying how the President should choose whom to pay (and whom to stiff), if the moment of truth were to arrive. Of course, they could not pass that legislation, because the problem that we identified essentially said that any such prioritizing legislation would have to be in the form of new spending laws, which are the source of the President’s conundrum in the first place. The point, however, is that the political actors proceeded differently than they would have, if there had never been scholarship that described the impossible position in which the Republicans’ strategy would put the President.

One additional point bears mentioning here. Although most of the analysis in the Buchanan-Dorf series of papers was based on principles of constitutional law, it mattered that one of the co-authors is an economist (and that the other is well-read in economics). One of the original arguments regarding the debt ceiling revolved around whether certain actions by the government were likely to increase the likelihood of default on the debt. Some legal scholars suggested in op-eds that issuing more debt would by itself increase the likelihood of default, but they were wrong. As economists know, U.S. government debt is repayable in dollars, which means that the *only* way that the government could ever default on its debt is if the

political system goes haywire (as it nearly did from 2011 through 2014, and as it might well do in the Spring of 2015). Not knowing a basic economic fact thus caused other scholars to make avoidable errors.

I have spent the bulk of this paper recounting my direct experience with writing scholarship that might have “mattered,” in some sense of that word. This emphasis is explained both by my having intimate knowledge of the to-and-fro of that particular set of arguments, and by my hope that our articles might yet help to prevent the return of the threat of a debt ceiling disaster in the future. Beyond my own work, however, there are certainly plenty of examples of legal scholarship over the past generation or so that have been enormously influential. I will briefly name just a few.

Professor Catherine MacKinnon’s legal writing (as well as her advocacy) brought about a revolution in real-world law that has changed – and, in my view, dramatically improved – the lives of women (and men). This is a person, after all, who essentially created the legal concepts of sexual harassment, hostile work environments, and similar ideas that were directly turned into laws at the federal and state levels. If any academic career ever had widespread influence, it is hers.

Or, to look at a single piece of legal scholarship that has had an important impact, the 2000 article *Michigan’s Majority Graduates in Practice: The River Runs Through Law School* (by Richard Lempert, David Chambers, and Terry Adams), changed the debate over affirmative action in law schools and elsewhere, inspiring a series of responses from dissenting and concurring scholars alike. Like MacKinnon’s work, the Lempert et al. paper changed the path of policy and the law,

even though it was not the kind of article that judges were accustomed to reading. In addition, that article (again like MacKinnon's work) went far beyond traditional legal doctrine, drawing on sociology and other social sciences to inform its conclusions.

The examples that I have described thus far revolve around "big issues" of various sorts, which are the fodder for front-page news coverage and debates among policy makers and partisans. There are plenty of issues that are not usually headline news, however, that matter a great deal, and that can end up affecting policy. As a law professor, Senator Elizabeth Warren wrote profoundly important work on the causes of personal bankruptcies, showing that the reality is far from the moralistic belief that people who go bankrupt are wastrels who gamed the legal system. Instead, her work showed that most people in that unfortunate position are there because of either medical disasters or divorce, the latter leaving millions of women and their children in, or on the brink of falling into, poverty. That she was able almost single-handedly to create a federal regulatory agency (the Consumer Financial Protection Bureau), and now to push legislation in the Senate, is notable, but it was her initial scholarship that began the process.

One of my colleagues at GW Law, Professor Joan Meier, just days ago was awarded a large grant from the National Institute of Justice to study how courts award custody in cases where there is evidence of abuse by one parent. (Sadly, the evidence that is currently available shows some troubling trends). Her work will directly inform the world about how the legal system could and should be changed, and it will draw from an interdisciplinary bag of tools to do so. Similarly, another

clinical professor at GW, Jessica Steinberg, has written about the provision of legal services to low-income clients, using a statistical study to investigate whether it makes sense to provide “a little help” to a larger number of clients, or full-scale legal assistance to a lucky few. (Her surprising finding: Clients who receive legal assistance at the initial stages do no better than those who receive no help at all.) This, too, could have important effects on how the legal system works, and it could do so by changing how legal clinics operate, even if it never leads to legislation.

Finally, I should emphasize that I am not arguing that the exclusive measure of importance in scholarly work is the direct impact that the work has on policy or even practice. The creation of knowledge for its own sake is important, and I sometimes think that I should spend more of my own time on “impractical” matters, because one never knows when things will become practical. (Same-sex marriage is an outstanding recent example.) I will say, however, that legal scholars who “dabble” in other fields – history, sociology, economics, and so on – are uniquely positioned to turn their scholarship to society’s immediate advantage. That is, it is specifically because we are open to being accused of being dilettantes that we are able to have real-world impact. More and more, legal scholars are arming themselves with knowledge from other academic fields, and we are combining that knowledge with our legal training to create real value to society. We should always commit to trying harder, to having more impact, and to learning from our mistakes. Even in our currently imperfect state, however, it is not an exaggeration to suggest that, as the title of this essay immodestly suggests, legal scholars are making the world a better place.