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# Replicability in Empirical Legal Research<sup>1</sup>

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## ABSTRACT

As part of a broader methodological reform movement, scientists are increasingly interested in improving the replicability of their research. Replicability allows others to perform replications to explore potential errors and statistical issues that might call the original results into question. Little attention, however, has been paid to the state of replicability in the field of empirical legal research (ELR). Quality is especially important in this field because empirical legal researchers produce work that is regularly relied upon by courts and other legal bodies. In this review article, we summarize the current state of ELR relative to the broader movement towards replicability in the social sciences. As part of that aim, we summarize recent collective replication efforts in ELR and transparency and replicability guidelines adopted by journals that publish ELR. Based on this review, ELR seems to be lagging other fields in implementing reforms. We conclude with suggestions for reforms that might encourage improved replicability.

## KEYWORDS

Empirical legal research, metascience, replicability, reproducibility, verification, open science, law and economics, law and psychology

## PART I. INTRODUCTION

The replication crisis that has struck all scientific fields from medicine to economics to psychology and other fields is by now well known (Munafò et al. 2017). Robust evidence has brought to light the toll on scientific progress imposed by questionable research and publication practices including selective reporting of methods and results (Simmons, Nelson & Simonsohn 2011), use of unverifiable data and methods (Hardwicke et al. 2020a p. 15), post-hoc conjecturing (Freese & Peterson 2017 p. 155), insufficient sample sizes (Ioannidis, 2005), and a bias towards publishing only statistically significant results (Kvarven, Strømmland & Johannesson 2020). The crisis has sparked a number of reform efforts that seem to slowly be taking root despite on-going controversies around optimal approaches (Hardwicke et al. 2020a).

One such reform effort focuses on ensuring that researchers have what they need to attempt to replicate published empirical results. We characterize research as replicable if researchers are able to access all information necessary to attempt a replication.<sup>2</sup> This requires

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<sup>2</sup> Replication differs depending on the nature of the data. For studies employing data not generated by experiments, sometimes referred to in ELR as “observational studies,” replication entails using the same data (or a different sample drawn from the same population) and same methods to verify (i.e., computationally replicate) the reported results (Clemens 2015). This method is akin to auditing the original work to detect coding mistakes, computational and clerical errors, statistical problems related to insufficient power, and other such issues that might call into question the conclusions drawn from the original results (Vazire & Holcombe 2020, pp. 11-12). For studies employing data generated by experimental studies, the standard replication method is to employ the same experiment protocols used in the original study to produce a different, and often larger, dataset drawn from a similar population to determine whether the original results can be replicated (see Klein et al. 2014). Failure to replicate is usually attributed to insufficient power, publication bias, imprecise measurements, sampling error, and undisclosed researcher degrees of freedom (Munafò et al. 2017).

Researchers, of course, also make use of publically available original data and analysis materials to “extend” (using the same methods and data drawn from a different population), “reanalyze” (using different, perhaps more up-to-date, methods to analyze the same data or a sample drawn from the same population) the original results (Clemens, 2015). If these steps are taken, however, without first checking for coding errors, computational errors, and other basic mistakes, we risk compounding the errors (e.g., reusing faulty data) and spending time and resources attempting to explore and explain divergent results that arise not from different data or methods but from undetected mistakes made in the original research. Our focus here is on reforms aimed at ensuring availability of materials necessary to perform replications and whether researchers are using the information to produce and publish replication studies. Unless we first verify original results, we risk heading down an unproductive path to explore a phenomenon that might not in fact exist.

authors to make available “the...input data [and steps used to collect it], computational steps, methods, and code, and conditions of analysis” (National Academies 2019 p. 36).<sup>3</sup> Easy access to the original data, collection methods, and all analysis materials allows others to attempt to produce new results that either confirm or call into question the original study’s results and conclusions.

Replicability is central to the research process because it allows researchers to correct errors before they become generally accepted by other researchers and the public. One way that replicability promotes correction is by making replication research possible. In particular, conclusions drawn from results reported in quantitative empirical studies are more trustworthy if they are replicated by other scientists using the original methods and the original or new data drawn from the same or a similar population.<sup>4</sup> A strong commitment to replication also encourages authors of original studies to precisely state a complete list of sufficient and necessary conditions thought to lead to the original results (Howe & Perfors 2018). This leads to more useful theory in the sense that the conditions under which the theory might successfully predict behavior are clearer. Replicability also helps catch more mundane errors, such as

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<sup>3</sup> More specifically, policies that foster replication include requiring public availability of all data employed in the study along with: “a clear description of all methods, instruments, materials, procedures, measurements, and other variables involved in the study; a clear description of the analysis of data and decisions for exclusion of some data and inclusion of other; for results that depend on statistical inference, a description of the analytic decisions and when these decisions were made and whether the study is exploratory or confirmatory; a discussion of the expected constraints on generality, such as which methodological features the authors think could be varied without affecting the result and which must remain constant; reporting of precision or statistical power; and a discussion of the uncertainty of the measurements, results, and inferences” (National Academies, 2019 p. 175-176).

<sup>4</sup> Generally, replications of observational studies employ the original methods and original data, although in some cases a new sample is drawn from the same population. Replications of experimental studies draw a new sample from a similar population using the same experimental design. Replications that increase sample sizes by drawing observations from the same or similar population can help produce more precise estimates of effect sizes (Zwaan et al. 2018).

statistical mistakes and coding errors (Vazire 2019). Finally, requiring research to be replicable encourages careful work (Mitchell 2004; Munafò et al. 2017) and deters fraud (Miyakawa 2020).

In this review, we explore the state of replicability in the field of empirical legal research (ELR). We define ELR narrowly as quantitative data-driven investigations of law, legal systems and other legal phenomena.<sup>5</sup> Scholars have been producing quantitative ELR for decades, and the field continues to grow with the increasing availability of relevant data and technological tools, and the continuing interest among journal editors (Diamond & Mueller 2010). All justifications supporting efforts to encourage replicable research resonate forcefully in the context of ELR (Donohue 2015). ELR specifically is designed to address legal issues and is cited by experts testifying in front of legislative bodies, legislatures, authors of briefs filed with courts, judges, authors of comments to administrative agencies, and rule makers housed in agencies.<sup>6</sup> Indeed, serious concerns about the replicability of ELR have been raised, with little or no discernible impact (see Epstein & King 2002a, Zeiler 2016).

Our review proceeds as follows. In Part II, we establish a baseline by describing the replicability movement that has been underway for some time in the social sciences. We summarize efforts by journal editors to reform transparency and openness guidelines. We also describe the notoriously problematic results of systematic replication attempts. While much work remains to be done, efforts in the social sciences are a good baseline against which to compare progress in law. In Part III, we describe where things stand on the replicability front in law. We summarize the calls for reform, student-edited and faculty-edited law journal policies related to

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<sup>5</sup> The importance of replicability of qualitative case studies in law has been covered elsewhere (e.g., Argyrou 2017).

<sup>6</sup> Zeiler (2016) informally estimates the frequency of citation in judicial opinions of ELR published in student-edited law journals, finding that reliance on such research is common. ELR published both in student-edited and faculty-edited journals likely is also regularly cited in other types of legal documents.

replicability, and results from the first collective effort (to our knowledge) to encourage ELR replications. Although progress is underway, the picture is bleak relative to other social science fields. In Part IV, we describe a number of distinctive institutional barriers that stand in the way of law following in the footsteps of social science. In light of these hurdles, we provide a summary of suggestions others have forwarded to move ELR in the direction of fields that have progressed the most on the replicability front. We offer additional ideas. Part V proposes a roadmap for further study and concludes.

## PART II. REPLICABILITY AND REPLICATION IN SOCIAL SCIENCE

This Part describes the movement towards replicability in social science (Christensen et al. 2019) and a meta-research agenda that seeks to measure and guide that movement (Hardwicke et al. 2020a). We discuss the prevalence of replicable research practices in social science, the consequences of poor replicability, initiatives seeking to improve replicability, and early assessments of changes in the field.

### Irreplicable research and failed replications

The growing field of meta-research is finding that much of experimental social scientific research is not replicable (Hardwicke et al. 2020b). For example, a recent study examined a random sample of social scientific experimental studies published from 2014 to 2017, assessing, in part, availability of data, materials, and analysis scripts (Hardwicke et al. 2020b), along with “precise descriptions of the data collection process” (Gelman 2017, p. 38). The results suggest a low level of information availability and replicability, finding for instance only 11% of studies using publically available data and just 2% making their analysis scripts available. Other studies and reviews have found similar deficiencies in replicability in social science (Vanpaemel et al. 2015, Freese & Peterson 2017, Hardwicke et al. 2018) and beyond (Vines et al. 2014).

Further evidence for irreproducibility in experimental social science can be found in surveys of researchers in a variety of fields (e.g., psychology, economics, ecology, education) using a variety of methods (e.g., experimental, qualitative) about their practices (sometimes called questionable research practices or *p*-hacking, John et al. 2012, Necker 2014, Fraser et al. 2018, MacCoun, 2019, Makel et al. 2019). These practices often involve failing to report key methodological details, such as why outliers were excluded, how the researchers decided to stop data collection, and the conditions and measurements that did not work as predicted. Surveys find that such underreporting is common in psychology (John et al. 2012), economics (Necker 2014), and education research (Makel et al. 2019). There is also evidence for questionable research practices in political science (Franco et al. 2015) and business research (Butler et al. 2017).

[Table 1 about here]

Irreproducible practices employed in experimental work likely contribute to the surprisingly high number of false and inflated discoveries in the published literature (Simmons, Nelson & Simonsohn 2011). Notably, the results of many large-scale, pre-registered,<sup>7</sup> multi-lab efforts (Table 1) have failed to replicate substantial numbers of published social science experiments (OSC 2015, Klein et al. 2018b, Camerer et al. 2016, 2018). A recent study compared the multi-lab replication results to results reported in meta-analyses designed to produce a single estimate

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<sup>7</sup> Preregistration is public pre-commitment to a data collection and analysis plan. It promotes transparency and dissuades questionable research practices by recording the researcher's methods prior to their seeing the data (e.g., their pre-registered plan for excluding outliers can be compared to how they actually excluded outliers). It also counteracts publication bias (i.e., null findings are less likely to be published, despite providing important information) by creating a public record of the study. Preregistrations can be embargoed until the study is ready for publication. It is required by law in many jurisdictions for clinical medical trials, reflecting the importance of full reporting in that context. See Nosek et al. (2018) for further information. Bavli (2020) discusses the limitations of pre-registration and proposes an alternative.

by combining evidence from multiple studies of the same effect (Kvarven, Strømmland & Johannesson 2020). Notably, the meta-analyses used smaller samples and were not pre-registered. The comparison revealed that effect sizes reported in the meta-analyses were three times greater than those reported in the registered replications. This suggests that greater analytic flexibility—enabled by a lack of replicability—leads to less trustworthy results. While these projects have focused on replicating experiments, irreplicable practices also threaten the conclusions that can be drawn from observational studies (e.g., researchers adding covariates to their models until they find the relationship they are looking for without disclosing this in the report, see Bruns & Ioannidis 2016).

Irreplicability also makes detecting miscalculations and other mistakes in published studies more difficult. Without access to raw data and analytic code, it is hard to know whether published summary statistics are correct. This is problematic because errors do appear to be common in social scientific experimental research, and they are often used to draw faulty conclusions. For example, one study in psychology found that half of a sample of articles included a minor error and one of every eight included an error that changed reported inferences related to statistical significance (Nuijten et al. 2016).

Meta-researchers are revealing a similarly dire situation in non-experimental social science fields. Problems in the field of econometrics, for example, came to light much earlier than in experimental fields, garnering attention after Learner (1983) issued his well-known critique. Christensen and Miguel (2018) summarize evidence of widespread replicability problems in economics. Many have reported difficulty obtaining materials necessary for replication even when journals publishing the work have adopted explicit disclosure policies (Dewald et al. 1986, McCullough 2009, Glandon 2010). The *American Economic Review*'s

Annual Report of the Editors shows a concerning trend related to papers for which the journal waives data disclosure requirements—increasing fairly steadily from 6% in 2005 to 46% in 2016.<sup>8</sup>

Results reported in econometric studies regularly fail to replicate. Chang and Li (2015) attempted to computationally replicate results from 67 macroeconomics articles. Six were excluded due to the use of proprietary data. They successfully replicated results in 48% of the 61 studies. Missing data accounted for the largest portion of failures. Clemens (2015) also lists 13 studies that failed to replicate published results.

### Responses to irreplicability

The above insights into the troubling state of replicability in both experimental and non-experimental social science prompted actions and reforms aimed at improving the openness and transparency of research. These include development of organizations aimed at providing the knowledge, training, and infrastructure required for conducting replicable research, moves to promote and adopt practices designed to increase replicability, and the creation of new academic journals focused more on methods than results.

Beginning with organizations, the Berkeley Initiative for Transparency in the Social Sciences (BITSS) aims to “improve the credibility of science by advancing transparency, reproducibility, rigor, and ethics in research.”<sup>9</sup> It does this by, among other things, sponsoring workshops on research methodology and funding meta-research (National Academies 2018 p. 153). Similarly, the Center for Open Science (COS) strives to “increase openness, integrity, and reproducibility of scholarly research” (National Academies 2018 p. 114). It has led several

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<sup>8</sup> <http://dx.doi.org/10.7910/DVN/FUO7FC>. The *American Economic Review* is the top economics journal.

<sup>9</sup> <https://www.bitss.org/>

initiatives, including development of journal guidelines that promote replicable research. It also developed and maintains the Open Science Framework, a free resource for researchers to store their data and study materials, collaborate, and document research workflows and registrations.

New institutions and organizations have helped to move forward initiatives aimed at improving replicability. Notably, in 2014, a group of researchers, funding agencies, and journal editors created the Transparency and Openness Promotion (TOP) Guidelines, which are “[a]uthor guidelines for journals” that “help to promote transparency, openness, and reproducibility” (Nosek et al. 2015). Over 5,000 journals have signed a statement agreeing to consider adopting TOP to some degree, and over 1,000 have gone on to do so (Center for Open Science 2020c). There are 8 original TOP guidelines (e.g., data transparency, study preregistration), which journals can implement at three levels of rigor (e.g., for open data, they range from requiring a statement about whether data is available to requiring that data actually be posted to a trusted repository; see Nosek et al. 2015).

More recently, the Center for Open Science developed the TOP Factor, a website and database that displays how well journal policies accord with the 8 TOP guidelines and two new standards (for a total of ten). The first of those new standards is whether the journal has policies to counter publication bias.<sup>10</sup> This standard considers whether journals accept registered reports (Chambers 2019), a new journal format in which authors submit their methods for peer review prior to collecting and analyzing data. The second new standard is whether the journal offers open science badges (i.e., optional visible symbols added to articles that meet some criterion,

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<sup>10</sup> Some have also warned about author bias and proposed methods of blind data analysis adopted in the hard sciences for countering it (MacCoun & Perlmutter 2015). These methods can be especially useful when replication is not feasible (MacCoun 2018) or when researchers analyze data in ways that go beyond pre-registered plans (Dutilh et al. 2019).

such as posting data or materials online, see Kidwell et al. 2016, Rowhani-Farid & Barnett 2018).

Beyond these general guidelines, more targeted initiatives are being developed to improve replicability. For instance, one group recently used a consensus-based procedure to create a reporting checklist for authors of behavioral and social scientific studies (Aczel et al. 2020). That checklist is geared towards experiments, but improved reporting of observational studies has been widely discussed in economics (see Christensen & Miguel 2018 p. 959-60). In addition, the American Economic Association (AEA), recognized that, while trial registries (public repositories for pre-registrations, see [clinicaltrials.gov](http://clinicaltrials.gov)) are common in medical science, there was a lack of registries for social science. That group then developed a registry for randomized controlled trials (American Economic Association 2020). The COS hosts a more general registry (Center for Open Science 2020b). Together, these initiatives encourage replicability by providing tools and infrastructure (e.g., in the AEA registry) and encouraging the use of advances in checklists and other reporting guidelines. Reporting checklists are correlated with more thorough methodological disclosure, at least in some fields (Moher et al. 2009, Han et al. 2017).

Experimental psychology has also been proactive in developing outlets for research that focus on improving methodology and methodological reporting (Collabra 2020, see also Simons 2018). For instance, *Collabra: Psychology* describes its acceptance guidelines as divorced from the actual result and focused instead on the methodology employed (Collabra Psychology 2020):

The acceptance criterion for *Collabra: Psychology* is scientific, methodological, and ethical rigor. While *Collabra: Psychology* editors and reviewers do not attempt to predict a submission's impact to the field, nor employ any topic bias in accepting articles, they will check for rigorously and transparently conducted, statistically sound, adequately powered, and fairly analyzed research worthy of inclusion in the scholarly record.

Economics has also seen a number of developments to aid the production of replicable research (Christensen & Miguel 2018). Gentzkow and Shapiro (2014) developed a manual to help researchers produce replicable results. Christensen et al. (2019) published a text on how to do open science, including steps required to ensure replicability. Koenker and Zeileis (2009) discuss methods of “literate programming,” which can be used to combine into a single file all statistical analysis along with the paper draft. Racine (2019) explains in detail how to produce replicable econometrics in R, a popular programming language among economists.

At least one social science journal performs pre-publication verification. The *American Journal of Political Science* contracts with a third party to independently verify results using the data and analysis materials submitted by the author.<sup>11</sup> Although the journal does not audit data for coding errors, this verification process is useful for eliminating computational and clerical errors, which seem to be widespread.

Outside of the social sciences, some fields (e.g., cancer biology) are embracing post-publication peer review, which includes comments and critiques of published work including errors found in those works (Vazire & Holcombe 2020, Horbach & Halffmann 2018 p. 5). One service that enables post-publication review is PubPeer, which allows researchers to write comments that are attached to published articles through PubPeer’s database and search mechanism.<sup>12</sup>

### Assessing the reform efforts

An important component of the meta-research agenda is monitoring the effectiveness of reforms and initiatives (Hardwicke et al. 2020a p. 26-28). In this respect, some evidence suggests

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<sup>11</sup> <https://ajps.org/ajps-verification-policy/>.

<sup>12</sup> <https://pubpeer.com/>.

that the above responses are having their intended effect. Consider, for instance, self-reported adoption of replicability practices. A recent survey of political scientists, economics and psychologists (the “3S Survey”) found a substantial uptick in open materials, open data, and preregistration among researchers in those fields (Christensen et al. 2019). This study also found that these upticks tend to align with the timing of new institutions and technologies aimed at promoting replicability.

Additional evidence of the effectiveness of recent reforms can be found in studies attempting to measure the effect of journal policies that require replicability (Hardwicke et al. 2018, Nuijten et al. 2017). In one study, researchers compared data availability in a judgment and decision making journal that had adopted an open data policy to a comparator journal without such a policy (Nuijten et al. 2017 p. 14). They found a sharp increase in the replicability of articles published in the journal with the policy, but not in the journal that did not adopt it. That said, data made available in accordance with these policies is not always presented in a way that ensures others can easily make use of it (Hardwicke et al. 2018). In some cases the policies are not enforced (Hardwicke et al. 2018, Christensen & Miguel 2018).<sup>13</sup> On the other hand, some evidence suggests that, to trigger compliance, these policies need not be framed as requirements. Awarding articles opt-in “badges” for practices like providing open data (e.g., an “open data badge”) is associated with journal-wide improvements (Kidwell et al. 2016), but seemingly only in fields in which the need for such reforms are already salient (Rowhani-Farid & Barnett 2018).

Despite substantial progress in both experimental and non-experimental fields, especially in the last few years, much work remains to be done before the field of social science can claim

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<sup>13</sup> Even new methodology-focused papers do not always report their methods replicably (Obels et al. 2020).

to be benefiting fully from replicability policies (Gelman 2017, Christensen & Miguel 2018). Perhaps most concerning, journal editors resist publishing replication studies (Christensen & Miguel 2018). Most of the social sciences are, however, well ahead of law. In the next Part, we turn to the state of progress in ELR.

### PART III. THE STATE OF REPLICABILITY IN LAW

Calls for reform of replicability policies and practices in law go back to at least the early 2000s. Commentators seem to have valid cause for concern. Student-edited law journals regularly publish ELR, but only a few seek peer reviews, and journal editors generally do not have the requisite training to assess submission quality (Epstein and King, 2002a). After summarizing long-standing and recently revived calls for reform, we provide a current snapshot of quality control policies that top student-edited and faculty-edited law journals have adopted. We find that law lags behind other social science fields. Finally, we summarize relevant aspects of the first (to our knowledge) collective attempt to encourage and publish ELR replications. Our findings suggest that, although reform is underway, much more is needed.

#### Calls for Reform

Epstein and King (2002a) offer one of the earliest critiques of the quality of published ELR.<sup>14</sup> They lament the “unmet need for a subfield of the law devoted to empirical methods...and methodological problems unique to legal scholarship.” (fn 19) Their evaluation of all 231 articles with “empirical” in the title that were published in student-edited law journals between 1990 and 2000 uncovered rampant statistical errors--they claim that every study

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<sup>14</sup> Unsurprisingly, the strongly critical article is controversial. See e.g., Revesz (2002), who claims he made all information related to his criticized study available upon publication. In a reply, Epstein and King (2002b) push back on that claim, illustrating potential difficulties in verifying proper disclosure.

contained at least one.<sup>15</sup> They strongly endorse replicability as a necessary step to increase the quality of ELR, providing ample information about how to ensure complete disclosure of data and methods.<sup>16</sup> In the same vein, they recommend that law journal editors require authors to post all data and study materials to some public archive (132). Others joined Epstein and King in early calls for replicability.<sup>17</sup> Mitchell (2004) lays out a detailed disclosure policy that law review student-editors can cut and paste into their publication requirements.

More recently, several ELR scholars have revived the call to adopt replicability requirements and practices. Donohue (2015) emphasized the imperfections of the peer review process and called for open access to “the data and computer program” necessary to allow others to understand every step that goes into producing results.<sup>18</sup> He provides evidence of substantial methodological issues in published empirical studies of the effects of gun control laws on murder rates, the effects of the death penalty on deterrence, and other weighty issues. In 2016, four prominent intellectual property scholars published an open letter, signed by a number of other scholars, calling for, among other things, increased data and information disclosure in empirical intellectual property scholarship (Feldman et al. 2016). In the same year, Zeiler (2016) pointed to

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<sup>15</sup> They also report inferential errors in ELR published in faculty-edited journals.

<sup>16</sup> “Good empirical work adheres to the replication standard: another researcher should be able to understand, evaluate, build on, and replicate the research without any additional information from the author. This rule does not actually require anyone to replicate the results of an article or book; it only requires that researchers provide information--in the article or book or in some other publicly available or accessible form--sufficient to replicate the results in principle” (Epstein & King 2002a, 38). Arlen and Talley (2008, p. xxxvi) propose the same for experimental legal research.

<sup>17</sup> In their ELR textbook, Epstein and Martin (2014) discuss the importance of replicable datasets and provide guidance on how to construct and document such datasets. Robbennolt and Lawless (2010, 40), in their textbook on empirical legal studies, emphasize the importance of replicable research. Spitzer (2003) characterizes the push for transparency as “a superb idea.” Mitchell (2004, p. 176) calls for law journal adoption of “a set of stringent disclosure requirements for reports of original empirical research, including disclosure of detailed information about methodology, data analysis, and the availability of raw data for replication and review.” Nolasco et al. (2010) call for replicability in criminal justice research. Heise (2011, fn 2) is amenable to replication by other scholars as a necessary feature of empirical legal scholarship.

<sup>18</sup> “Since such access is virtually never available to a referee, only a process of attempted replication could begin to reveal the many serious problems that can infect even major papers in top empirical journals” (Donohue 2015, 320).

information disclosure as a key to ensuring future relevance of ELR and called for existing academic societies to take active steps to push for and implement reforms. Irvine et al. (2019, p. 346-8) proposed that experimental law and psychology researchers take steps to improve the replicability of their work.

A few legal scholars have voiced skepticism about the perceived value of replications and concern over whether the culture of legal academia discourages the checking of colleagues' work. Hubbard (2019), for example, worries that successful replications might be "branded" as "uninteresting" (p. 1). He advises researchers to "maximize their contribution to the field" by doing something more than merely attempting to replicate published results.<sup>19</sup> To us, this view seems out of sync with the burgeoning view in other social science fields, namely that replication is vital given the fallibility of the peer review process as a means of detecting, among other things, coding and computational errors (Nuijten et al. 2016). Reform movement leaders in other fields have argued that "replication has more scientific value than original discovery" (Ioannidis 2018).

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<sup>19</sup> In his comments on a specific replication study, Hubbard (2019) holds up the study as a model replication, noting that it "acknowledges the significance of the original study... follow[s] in its footsteps even as it illuminates its methodological limitations and ultimately reaches very different empirical conclusions" (p. 1). He praises the replication study for its replicable methodological contributions that advance the state of knowledge. We note that the study Hubbard praises does not attempt to replicate the original results using the original data and original methods. Instead it proposes a new method for measuring a key variable, collects new data using that method and employs them to produce results that lead to different conclusions. Although we agree that the new study makes an important and original contribution, we suggest that the study would have added something more useful to our knowledge base if it had performed a computational replication of the original results. Without verifying the validity of the original results, we are left with a literature that has produced mixed results (Zwaan et al. 2018, p. 8 "[I]n a direct replication of earlier work, the question of whether a particular method is an appropriate test of a hypothesis was previously answered in the affirmative. After all, the original study was published because its authors and the reviewers and editors who evaluated it endorsed the method as a reasonable test of the underlying theory."). Although one might offer theoretical arguments in favor of one measure over the other, we are left to contemplate which method for measuring the key variable is best. Had the authors started by attempting to replicate the original findings and found errors, they might have discovered that, once the errors were corrected, both measurement methods lead to the same results and that the measurement method is, in fact, irrelevant. Despite the authors' ability to resolve this open question, it remains open. Spellman and Kahneman (2018) argue that extensions of original research should routinely include replication. They recommend protocols to follow when extending experiments; the first step is replication. The benefits of routine replication apply in the case of non-experimental research as well (Christensen and Miguel 2018).

Hubbard (2019) also voices concern over “retaliation” by original authors whose work fails to replicate and the influence of such reactions on incentives to engage in the work of replication. Although we sympathize with this concern, we endorse the views of those who have pushed for reform despite feathers that might get ruffled. In our view, the alternative eventually will lead to a complete lack of public trust in ELR research, especially if other social science fields continue to relentlessly press forward with quality control reforms.

Despite some skepticism, calls for reform are growing in number and are now seemingly ubiquitous. We explore next whether the calls have been answered. We find that progress is severely lacking.

#### Law Journal Disclosure Requirements

We reviewed the transparency and openness guidelines of highly ranked faculty-edited and student-edited journals that publish legal research. For faculty-edited journals, we chose the top 30 by impact factor in the Web of Science’s 2019 law database (omitting the student-edited journals in this list). For student-edited journals, we chose the top 30 in the 2019 Washington & Lee Journal Rankings as sorted by their primary metric (which considers citations on Westlaw). We searched the author guidelines for encouragement or requirement of open data, materials, or analytic code, and also for mentions of preregistration or accepting registered reports (i.e., some of the TOP Guidelines mentioned above). Full details of our search and the websites we reviewed are available online.<sup>20</sup>

Beginning with the faculty-edited journals (Table 2), 14 of the 30 make no mention of any transparency guideline. Several offer some encouragement (e.g., the journal encourages authors to post their data). Two journals (*International Environmental Agreements-Politics Law*

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<sup>20</sup> For the data underlying Table 2, see <https://osf.io/vdzbp>. For Table 3, see <https://osf.io/j6fpq/>.

*and Economics and the Hague Journal on the Rule of Law*) require authors to include a statement about the availability of their data, materials, and code, and just three (*Computer Law & Security Review*, *Legal and Criminology Psychology*, and *European Constitutional Law Review*) require some form of transparency or an explanation from the author justifying non-compliance. Two journals, both affiliated with psychology, accept registered reports (*Law and Human Behavior* and *Legal and Criminology Psychology*).

The picture at student-edited journals (Table 3) is less rosy. 25 of the 30 journals make no mention of any transparency guideline. Surprisingly, four of the five journals that do include such a mention are demanding about it. These journals (Yale, Stanford, NYU, and Vanderbilt law reviews) require some aspect of replicability absent justification. The fifth (UVA Law Review) encourages open data. None mentions pre-registration or registered reports. Unlike the faculty-edited journals, the student journals that do have requirements appear clustered at the top of the rankings (they appear more evenly distributed in the faculty-edited group).

Although not applicable to mere failures to present research in a replicable way, retraction policies can aid in ensuring that materially incorrect empirical results are permanently removed from the literature.<sup>21</sup> We searched the faculty-edited and student-edited journals for retraction policies on their websites.<sup>22</sup> All but three faculty-edited journals had retraction policies through their publisher (e.g., Oxford Academic has a retraction policy that applies to all its journals). On the other hand, not one student-edited journal has a publicly available retraction policy.

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<sup>21</sup> It should be noted, however, that retracted papers seem to be frequently cited (Bolboacă et al. 2019).

<sup>22</sup> For Tables 2 and 3 with information about retraction policies added, see <https://osf.io/7q3bf/>.

## Replication Projects in ELR

Although disclosure practices and policies are crucial, their value is substantially diminished if researchers fail to use the disclosed information to check for errors by attempting to replicate results. Suchman and Mertz (2010, p. 573) voiced concern over a decade ago about the dearth of ELR replications using disclosed information.<sup>23</sup> Unfortunately, our findings suggest not much has changed over the past decade. Efforts similar to those in other fields have not made their way to law. For example, we are not aware of any large ELR-focused, multi-lab replication effort similar to those conducted in psychology and economics (Table 1).

One bright spot is a recently established annual conference focused on replication studies held by Claremont McKenna College's Program on Empirical Legal Studies in 2018 and 2019.<sup>24</sup> To our knowledge, the conference is the first collective attempt at ELR replications. The *International Review of Law & Economics* has, so far, published 18 of the 20 studies presented at the 2018 and 2019 conferences.<sup>25</sup> Of these, we categorize three as completely new studies (i.e., neither replications nor robustness checks).<sup>26</sup> Seven others test the robustness of published results by conducting alternative tests, employing either different data drawn from different populations or different methods to explore theories or conjectures that have found support in

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<sup>23</sup> Vazire and Holcombe (2020) propose a preliminary list of requirements for a system of correction that includes both transparency (information disclosure) and critical appraisal (checking each other's work).

<sup>24</sup> See Hoepfner (2019, p. 98), however, for a critique of the set of studies grounded in their general silence "about evaluating their replication results and, specifically about how their results compare to the original results." He suggests that replication authors employ Bayesian methods to evaluate how their results impact inferences we can draw from the original study, the replication and other previous findings.

<sup>25</sup> Our on-line materials include the 2018 agenda (<https://osf.io/y8x32/>), the 2019 agenda (<https://osf.io/yjmcw/>) and a summary and categorization, using Clemens' (2015) taxonomy, of all 18 published studies (<https://osf.io/qpaje/>).

<sup>26</sup> Caspi and Stiglitz (2020), Kornhauser et al. (2020) and Nyarko (2019).

previously reported data. They do not attempt to replicate previously published results to identify coding, analytical and statistical errors or to remedy sampling error or address low power.<sup>27</sup>

Most relevant for our purposes are the remaining eight studies, which do attempt to replicate original results. Seven of the eight involve non-experimental, observational data drawn from the field, and one attempts to replicate results using data previously collected through three field experiments. In all, the eight studies attempt to replicate results reported in 13 published studies.<sup>28</sup> Importantly, the authors of all eight replication studies noted some level of difficulty in obtaining all materials required to perform a full replication, reflecting the downsides of the lack of formal journal policies and current customary practice. Perhaps more concerning, authors of only two of the eight replication studies explicitly state that all of *their* data and analysis materials are publicly available.

The replication results are similar to what we've seen in other fields. The results are mixed, and some suggest major flaws that caution against applying, in any policy settings, the original study's purportedly supported claims until further study is possible. The single replication study focused on experiments successfully replicated the results reported by its three target studies using the original datasets, but found that all three studies were underpowered--the original samples were insufficiently large to detect any existing effects (Doleac 2020). Using

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<sup>27</sup> Five involve non-experimental, observational data, and two attempt to test the robustness of results from experiments. Zwaan (2018 at 4) defines an "alternative test" as a method used to "evaluate the robustness of a theoretical claim to alternative research designs." Using terminology proposed by Clemens (2015), these seven studies extend (using the same methods but different data drawn from a different population) or reanalyze (using the same data--or a different sample drawn from the same population--but different methods) previously published results and/or perform original studies (using different data and different methods). While these are important aims, we do not count them as replications. Clemens uses replication to include verification and reproduction and robustness to include reanalysis and extension, so "studies that do not appear to be replications" is not strong enough.

<sup>28</sup> All eight replication studies report results from re-analyses, refinements and/or extensions in addition to replications.

methods to refine the results to account for low power, the authors reported that the conclusions from two of the three studies do not find support in the original data. Among the 10 observational target studies, three were generally successfully replicated.<sup>29</sup> Two were found to have immaterial coding errors or immaterial differences between the original dataset and the replicated dataset;<sup>30</sup> three contain material coding errors;<sup>31</sup> two contain results that could not be fully replicated suggesting computational or clerical errors;<sup>32</sup> and four have potential power issues.<sup>33</sup>

Outside of the PELS conference context, individual replication studies in law appear to be rare. Those that have been conducted, however, make important contributions.<sup>34</sup> We point here to just three examples, but we suspect there are others. Williams et al. 2011 (reporting a failed replication attempt that prompted the original study's author to publish a correction) claim to have cast significant doubt on the original study's claims. Irvine et al. 2018 (attempting replications along with extensions) provided only mixed results for the original findings. Hazelton et al. (2010) published a replicable replication upon request of an author under attack for publishing controversial and partially incorrect results related to the influence of campaign contributions on elected judges' decision on cases. We note that unlike replications in other fields that we discuss above, almost none of the individual replication studies we found in law were pre-registered and apparently replicable. Although these findings demonstrate the importance of developing a culture of information sharing and result replication, drawing

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<sup>29</sup> Hausladen et al. 2020; Prescott and Pyle 2019; Thorley and Mitts 2019.

<sup>30</sup> Brady et al., 2019; Rao, 2019 (Van Harten target study)

<sup>31</sup> Larrimore et al. 2020 (both target studies); Sander, 2019 (Ayres and Brooks target study)

<sup>32</sup> Rao, 2019 (Van Harten target study); Sander, 2019 (Ayres and Brooks target study)

<sup>33</sup> Rao, 2019 (both target studies); Sander, 2019 (both target studies)

<sup>34</sup> A search to locate a complete set of replications is beyond the scope of our review. Note that some of the studies included here do not replicate original results but only reanalyze, extend and/or perform new studies, which in our view is not ideal but is a move in the right direction.

inferences from them about the general quality of ELR is impossible. The target studies were not drawn randomly from the population of all published studies. Weaker studies might have been targeted, although some of the weakest studies likely cannot be replicated because necessary materials are unavailable. Moreover, authors may refrain from conducting replication research that is likely to confirm the original work, or submit work that has done so, because editors might prefer replications that call original results into question (Giner-Sorolla et al. 2018, Hubbard 2019).

#### PART IV. CAN LAW FOLLOW SOCIAL SCIENCE?

While legal researchers can benefit from and rely on the lessons and infrastructures being developed in other fields, law faces distinctive challenges in improving research replicability. This Part briefly reviews those before exploring ways forward.

##### Challenges for ELR reform efforts

ELR is materially different from other research fields, and these differences pose significant barriers to reform. First, student editors stand as significant hurdles (Epstein and King 2002a). Law journals historically have operated independently, shielded from faculty influence. The potential for significant replicability reform adoption across a family of journals under, say, one publisher does not exist in law (Center for Open Science, 2020a). Also, student editors are unlikely to publish replication studies given their problematic preemption conventions (Moreno 2017, p. 423).

In addition, student editors do not face reputation effects that might influence faculty editors. Students serve on law journal editorial boards generally for one year. They face little risk of reputational hits when published authors refuse to disclose information used to produce

empirical results and when results fail to replicate. Moreover, given that most law review student editors are not qualified to look for signs of errors in empirical research and that most law journals do not require reviews of work by qualified peers, the likelihood that obvious errors are published is quite high (Epstein and King 2002).<sup>35</sup>

Second, the lack of faculty involvement on editorial boards is a barrier to adoption of policies designed to increase transparency. Student editors are likely to be unaware of methodological literatures related to transparency issues. Students are not on editors boards long enough to generate any momentum for transparency innovations. Perhaps most problematic, students likely often select empirical articles not for their methodological soundness, but rather because they wish to publish results that support their preferred descriptive or normative positions, or simply because they seem exciting.

Third, authorship norms may be standing in the way of improvements in replicability. This is because researchers trained primarily in legal methods may not have the knowledge to ensure their work is replicable. At the same time, norms in law seem to favor, at least compared to other fields, sole-authored articles or articles with few authors (Epstein and King, 2002). These restrictions may prevent legal researchers from involving methods specialists who can help improve the work's replicability (Holcombe 2019). More generally, a lack of diversity in the research process has been implicated as a source of low quality (Vazire & Holcombe 2020).

Fourth, authors that publish in law journals do not have robust incentives to disclose information necessary for replication. As we found in Part III, very few journal policies require such disclosure. Customs and norms in ELR do not seem to fill in the gaps by encouraging

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<sup>35</sup> Of course, the peer review process is not a panacea. Donohue (2011) is one among many who have highlighted serious flaws in the academy's peer-review process.

replicability. Without these incentives, authors may see replicability as simply added work that may open them up to scrutiny.

Finally, we suspect that reliance on eminence as a signifier of research quality (Vazire 2017) may be playing a role in perpetuating low-quality, irreplicable empirical legal research. Some evidence suggests the absence of a clear link between an author's status and the quality of their work (Vazire 2017), and many practices and norms in ELR may overemphasize status in publication decisions. For instance, student editors do not evaluate articles blindly, so they are likely swayed by the status of the author (see Yoon 2013; Harrison and Mashburn 2015).

### Ideas for Reform

Methods for changing research cultures range from requiring replicable practices, to incentivizing them, to making them easier to implement (Nosek 2019). Beginning with more assertive measures, we have already discussed journal transparency and openness guidelines, and their general lack of adoption among legal journals. But, while law journals have been slow to adopt these guidelines (Table 2 and 3), some prominent journals have fairly strong data transparency guidelines. These advances may help encourage others to follow suit. Similarly, increasing adoption outside of law may persuade editors to be more proactive in adjusting their guidelines. As to how those reforms may spread, there is some degree of centralization among North American law journals. For instance, there is a longstanding conference attended by law review editors (Closen & Jarvis 1992). And, in the past, these journals have worked together to collectively lower word counts in articles (Dorf 2005). Groups of empirical legal researchers (e.g., the Society for Empirical Legal Studies) may wish to circulate model guidelines, adapted from those used by other social sciences, for journals to consider and adopt. These guidelines

should reflect best practices about ethical data sharing and how to handle articles that involve proprietary datasets (Klein et al. 2018a).

Turning to incentives, some empirical legal researchers may perceive the costs of conducting their work more replicably as high and the benefits as remote and amorphous. One remedy is to raise awareness of the existing rewards of replicable research. For instance, sharing data is associated with higher citation rates (Piowar & Vision 2013). Funders also recognize the importance of replicable practices by encouraging or requiring them (Flier et al. 2017). And, as we have discussed, the broader social scientific ecosystem increasingly views replicable methods as an important component of credible research. Law journals could also incentivize replicability by providing results-blind peer review under the registered reports model (Chambers 2019) and through offering opportunities for post-publication peer review (Horbach & Halffmann 2018).

Law schools and other institutions should also consider shifting incentives towards replicability. Law schools seem to acknowledge the value of empirical research when hiring (Zeiler 2016 p. 91); but, it is less clear that they consider the replicability of that work. Hiring committees can take these factors into account either through the applicant's published research or simply through asking about their practices. Such a policy has the further advantage of aligning a school's research interests with the growing movement away from research assessment based on traditional metrics like impact factor, to the quality and replicability of research (Moher et al. 2020).<sup>36</sup> Other school policies could also incentivize replicability, such as by asking faculty whether they plan to make their data and materials available when applying for internal funding.

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<sup>36</sup> The fact that U.S. News and World Report just started ranking law schools based on citation counts, however, poses a significant challenge. <https://www.usnews.com/education/blogs/college-rankings-blog/articles/2019-02-13/us-news-considers-evaluating-law-school-scholarly-impact>.

Outside of law schools, the influential Washington & Lee Law Journal Rankings take into account court and academic citations to articles in the ranked journals. This methodology does not take into account replicability, despite the fact that for articles to properly be used in court, their methods and data should be fully examinable (Chin, Ribeiro & Rairden 2019). To incentivize such practices, the rankers should consider including replicability as part of their system.

Turning to making replicable research easier and less daunting, groups like the Society for Empirical Legal Studies (SELS) might consider using meetings to offer workshops and training on replicability methods. Organizations like the Framework for Open and Reproducible Research Training (FORRT) provide syllabi and other resources to assist in such initiatives. Empirical legal researchers might also find that, although earlier tools for sharing data and materials were initially designed for technical audiences (e.g. Github), more user-friendly websites have been developed over the past several years. Training and emerging best practice guidelines can also assist authors in navigating the ethical and legal issues that arise in making their work more accessible (Klein et al. 2018a).

Finally, ELR should join the meta-research movement that many cognate fields are engaged in (Hardwicke et al. 2020a). We hope our preliminary review of journal guidelines will help and can be revisited in the coming years to check for progress. And, as we saw in Part III, many legal scholars conduct observational research, yet there do not seem to be any reporting guidelines tailored to this type of work unlike in other fields that use observational data (e.g., medical researchers use the STrengthening the Reporting of OBServational studies in Epidemiology (STROBE) Statement to encourage replicability in their field).<sup>37</sup> Development and

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<sup>37</sup> The Consolidated Health Economic Evaluation Reporting Standards seem to be followed in health research but not in economics (Christensen & Miguel 2018).

testing of a consensus-based reporting statement for law and economics studies represent a straightforward way to improve ELR.

#### PART V. CONCLUSION

Replicability is foundational for scientific progress. Recent evidence demonstrating a serious lack of replicability of published social science research has spurred a robust and sustained movement to reform empirical research production procedures and publication requirements. The field of empirical legal research faces significant obstacles to achieving necessary reforms. Thus, it is unsurprising that ELR lags behind other social scientific fields. This is troubling. ELR is designed to address questions of law and policy; its credibility is essential to these aims. Indeed, the field's credibility hinges on its willingness to implement mechanisms that detect and correct error.

Additional basic research is necessary to gain further insight into the extent of irreproducibility of ELR. We don't yet have a sense of how widespread the problems are. Specifically, we need systematic study of the rates of inadequate disclosures, the rates of failure in replication attempts, whether journals that have adopted disclosure requirements strictly enforce them, whether consumers of ELR, including judges, legislators, regulators and other legal actors, rely on reported findings despite potentially high error rates, and perceptions of the problems and potential solutions in the minds of authors and editors. So far, we have seen just the tip of the iceberg.

Most important, from our perspective, is the need for a culture shift in ELR. The field lacks coherence on fundamental issues including what replication entails. It seems not to adequately value replications designed only to verify whether the author's data and methods actually support the reported results. LoPucki (2015, fn 118) argues that requiring all legal

empiricists to disclose information required for replicability would “make pervasive empiricism impossible.” This might be true, but, in our view, the potential cost of reduced quantity is well worth the benefit of improved quality. LoPucki also claims that one of the downsides of law schools hiring PhD-trained scholars is that they “impose the cultures of their disciplines on legal scholarship” (abstract). In the case of replicability efforts, we believe ELR stands to benefit from the cultures of other social science fields.

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TABLE 1. LARGE MULTI-LAB REPLICATION PROJECTS OF EXPERIMENTS IN SOCIAL SCIENCE

The results of six multi-lab replication projects of experiments in social science.

Project	Field	# Replication studies	Statistically significant in the same direction as the original
Estimating the replicability of psychological science	Psychology	97	36%
Evaluating replicability of laboratory experiments in economics	Economics	18	61%
Investigating Variation in Replicability (Many Labs 1)	Psychology	16	88%
Many Labs 2: Investigating Variation in Replicability Across Samples and Settings	Psychology	28	54%
Many Labs 3: Evaluating participant pool quality across the academic semester via replication	Psychology	9	33%
Evaluating the replicability of social science experiments in Nature and Science between 2010 and 2015	Social Sciences	21	62%

TABLE 2. AUTHOR GUIDELINES AT FACULTY-EDITED LAW JOURNALS

The top 30 faculty-edited journals law by impact factor in the Web of Science’s “Law” (2019) category. Author guidelines were examined for mentions of data, materials, and analytic code transparency, as well as pre-registration and registered reports (RR). Guidelines were coded for whether these factors were encouraged or required, or if a statement about them was required. 0 = no mention as a guideline; 1 = encourages sharing; 2 = Requires an availability statement; 3 = requires sharing (subject to reasons why it is not possible). See supplementary material for more details and links to the journal websites that were reviewed: <<https://osf.io/vdzbp/>>.

Journal	Data	Materials	Code	Prereg	RR	IF
European Journal of Psychology Applied to Legal Context	0	0	0	0	0	4.91
International Data Privacy Law	0	0	0	0	0	4.16
Common Market Law Review	0	0	0	0	0	3.52
Regulation & Governance	0	0	0	0	0	3.38
Transnational Environmental Law	1	1	1	0	0	2.64
Annual Review of Law and Social Science	0	0	0	0	0	2.59
International Environmental Agreements-Politics Law and Economics	2	2	2	0	0	2.51
American Journal of International Law	1	1	1	0	0	2.48

Journal of Law and the Biosciences	0	0	0	0	0	2.28
Antitrust Law Journal	0	0	0	0	0	2.26
Law and Human Behavior	0	0	0	0	Accepted	2.15
Feminist Legal Studies	0	0	0	0	0	2.10
Journal of International Economic Law	1	0	1	0	0	2.0
Computer Law & Security Review	3	1	1	0	0	1.85
Journal of Legal Analysis	0	0	0	0	0	1.73
Legal and Criminology Psychology	3	0	0	0	Accepted	1.67
Psychology Public Policy and Law	1	1	0	0	0	1.67
Journal of Environmental Law	1	0	1	0	0	1.63
Hague Journal on the Rule of Law	2	2	2	0	0	1.6
European Law Journal	1	1	1	0	0	1.59
International & Comparative Law Quarterly	1	1	1	0	0	1.57
European Constitutional Law Review	3	3	3	0	0	1.53
Modern Law Review	0	0	0	0	0	1.51

Icon – International Journal of Constitutional Law	0	0	0	0	0	1.51
World Trade Review	1	1	1	0	0	1.5
European Journal of International Law	0	0	0	0	0	1.48
Medical Law Review	1	0	1	0	0	1.46
International Journal of Transnational Justice	1	0	1	0	0	1.45
Law & Society Review	0	0	0	0	0	1.43
International Journal of Law and Psychiatry	1	0	1	0	0	1.34

TABLE 3. AUTHOR GUIDELINES AT STUDENT-EDITED LAW JOURNALS

The top 30 student-edited journals according to the W&L Law Journal Rankings (2019), based on its “Combined Score” metric. Impact factor (IF) from the Web of Science is also included.

Coding and columns are the same as in Table 1. For more information, see:

<https://osf.io/j6fpq/>.

Journal	Data	Materials	Code	Prereg	RR	W&L / IF
Yale Law Journal	3	0	3	0	0	1 / 6.59
Harvard Law Review	0	0	0	0	0	2 / 7.14
Stanford Law Review	3	0	0	0	0	3 / 3.37
Columbia Law Review	0	0	0	0	0	4 / NR
University of Pennsylvania Law Review	0	0	0	0	0	5 / 4.62
Georgetown Law Journal	0	0	0	0	0	6 / 3.26
California Law Review	0	0	0	0	0	7 / 1.95
Notre Dame Law Review	0	0	0	0	0	8 / 1.23
University of Chicago Law Review	0	0	0	0	0	9 / 3.22
New York University Law Review	3	0	0	0	0	10 / 2.46
Iowa Law Review	0	0	0	0	0	11 / 1.0

UCLA Law Review	0	0	0	0	0	12 / 2.64
Minnesota Law Review	0	0	0	0	0	13 / 1.57
Texas Law Review	0	0	0	0	0	14 / 2.67
Michigan Law Review	0	0	0	0	0	15 / 3.53
Virginia Law Review	1	0	0	0	0	16 / 2.60
Duke Law Journal	0	0	0	0	0	17 / 3.08
William & Mary Law Review	0	0	0	0	0	18 / NR
Vanderbilt Law Review	0	3	3	0	0	19 / 2.43
Fordham Law Review	0	0	0	0	0	20 / 1.02
Boston College Law Review	0	0	0	0	0	21 / NR
Boston University Law Review	0	0	0	0	0	22 / 2.43
Cornell Law Review	0	0	0	0	0	23 / 1.66
Northwestern University Law Review	0	0	0	0	0	24 / 1.07

Cardozo Law Review	0	0	0	0	0	25 / NR
Washington University Law Review	0	0	0	0	0	26 / 2.05
Southern California Law Review	0	0	0	0	0	27 / 1.60
U.C. Davis Law Review	0	0	0	0	0	28 / NR
George Washington Law Review	0	0	0	0	0	29 / 1.24
Emory Law Journal	0	0	0	0	0	30 / NR