

# The Failure to Examine Failures in Democratic Innovation

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## INTRODUCTION

Deliberation scholars have changed the landscape of democratic theory irreversibly, providing us with a coherent set of principles that undergird contemporary approaches to democracy making (e.g., Bohman 1998; Gutmann and Thompson 1996). Deliberation has become a normative project owned and invested in by practitioners of democracy worldwide (see Bherer, Gauthier, and Simmard 2016). The relevance of deliberative democracy is clear, as it seeks to respond to a set of disquieting contemporary phenomena that increasingly manifest in declining social capital and trust, and increasing deference to extremist populist appeals.

Beyond the deliberative turn in democratic theory, an empirical turn in deliberative democracy has also by now been undertaken. Buoyed by the active praxis of early adopters (Dienel and Renn 1995; Fishkin 1991), the empirical study of democratic innovations has diffused in line with the diffusion of these innovations themselves. A distinct subfield of political research has convened with the express goal of explaining the nature and impact of democratic innovations—“institutions ‘designed specifically to increase and deepen citizen participation in the political decision-making process’” (Smith 2009, 1).

An exploratory analysis of a sample of seven journals in political science<sup>1</sup> shows that since 2006 around 9% (200/2272) of the articles published in these journals held deliberation as a primary focus. There is no clear trend of increase or decrease in number of published articles on deliberation (see figure 1), which is indicative of the maturation and stabilization of the subfield within the discipline. The majority (151/200=76%) of these studies have an empirical focus and analyze a democratic innovation or a laboratory experiment designed to explore the inner-workings of deliberation (see table 1). The ratio between empirical vis-à-vis strictly theoretical work (the latter incorporating normative political philosophy and conceptual modelling) is fairly stable over the 10 years we have reviewed (see figure 2).

However, when analyzing the argument put forward by this large body of empirical studies in further detail, a striking result emerges. The vast majority of articles focus on best practices (64%). Only 18% of empirical articles explore the varying quality of implementation of democratic innovations, and just seven studies (4%) investigate deliberative initiatives that according to the author(s) themselves are failures.

For example, the vast majority of case studies of deliberative polls, citizens' assemblies, and participatory budgeting are members of the first category. The articles in this first category often highlight some minor issues and problems, but overall portray the innovation as a best practice or a step towards a best practice. Articles in the second category instead explore more significant problems emerging in cases of democratic innovation. Typical members of the second category are randomized controlled trials that analyze how certain commonly used designs generate negative outcomes (e.g., Karpowitz et al. 2012, Spada and Vreeland 2013), articles that explore the conditions that moderate the impact of democratic innovations (Kosack and Fung 2014; Wang and Dai 2013), and articles that discuss the degradation of innovations (Baiocchi and Ganuza 2014). Lastly we found only seven empirical articles that explored cases that the authors themselves categorize as failures or extremely problematic (Hughes 2016; Kamenova and Goodman 2015; Griffin et al. 2015; Ravazzi and Pomatto 2014; Smith 2013; Lopes Alves and Allegretti 2012; Gaynor 2011). For a discussion of the coding protocol see the online appendix.

While the number of articles analyzing the variety of these new democratic institutions including failures appears to slowly increase over time (figure 3), it is important to keep in mind that the vast majority of these articles have been published in the *Journal of Public Deliberation* and in *Politics and Society*. There is not a single article analyzing a failure in any of the top five journals in political science (second column of table 1).

Does this mean that the vast majority of cases of democratic institution-building are resounding successes? We do not think so. Various specialized monographs have highlighted how many instances of deliberation fail to promote democratic goods (Fuji-Johnson 2015; Hendricks 2012 and Smith 2009), many provide a façade of democracy or are co-optation programs with largely undemocratic aims (Wampler 2007), and some do not even survive long enough to generate detectable impacts. Why then has this been elided by the literature? One theoretical reason may be that, unfortunately, the sub-discipline lacks a clear grasp on what might count as failure which could be used to systematically explore the success rate of democratic innovations (something we return to below). For example, if we consider survival over time as the most basic characteristic of a successful democratic innovation, then the data generated by the Brazilian Participatory

Budgeting census (Spada 2012) available on Participedia.net offer a grim outlook on the survival rate of this family of democratic innovations. On average only half of these processes survive four years of implementation (see table 2). The other half are discontinued.

We argue that this lack of representativeness in the real-world cases of deliberation that command the attention of political scientists is currently a major barrier to understanding

disconnect between disciplinary focus and real-world outcomes and offers recommendations for design of empirical studies that can provide better feedback to conceptual and normative debates. We begin by discussing some of the pitfalls of research and analysis in an emerging field. We then discuss perverse incentives that affect the relationship between gatekeepers and researchers. We turn to the familiar problem of publication bias as it pertains to the

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democratic improvements. Without a comparison of success and failure, our models for successful outcomes will be chronically overdetermined, which ultimately reduces their chances of adoption in practice.

Why do we see this pattern of “failure neglect” in top journals? This article explores some explanations for a

subfield, before considering some causes for optimism and ways forward.

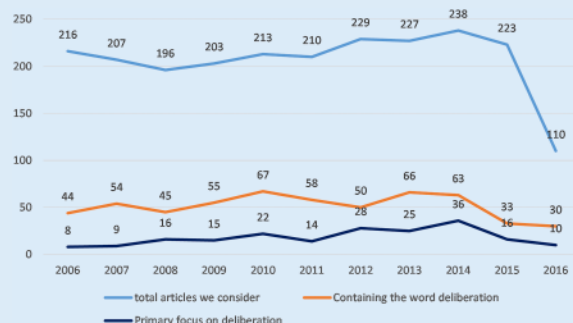
#### ROADWORKS NEEDED AT THE EMPIRICAL TURN

The achievements of empirical studies of deliberation in practice are significant. Research has helped us to understand preference change in deliberation among randomly selected groups (Fishkin 2009, Nabatchi et al. 2012), deliberation’s effect on efficacy and political participation (Gastil, Dees and Weiser 2002), as well as the effect of facilitators on group discussion (Fung 2006) to give just a few important examples. Though not fully comprehensive across all existing journals, the trend uncovered above challenges both the overall validity of the empirical turn as it stands and how such work can and should influence the normative project of democratic deepening. We wish to make some suggestions in aid of both exploring and mitigating the causes of this trend.

#### Sampling and Interpretation Bias

The sub-discipline we address is quite fortunate in that it is characterized by regular engagement between political philosophers, political scientists, and practitioners on merit. Many of the movers and shakers in the discipline can justifiably claim expertise across these categories. This is no mean achievement. However, despite the positive relationship that

**Figure 1**  
**Trends in Published Articles in the Top 5 Journals in Political Science + PAS + JPD**

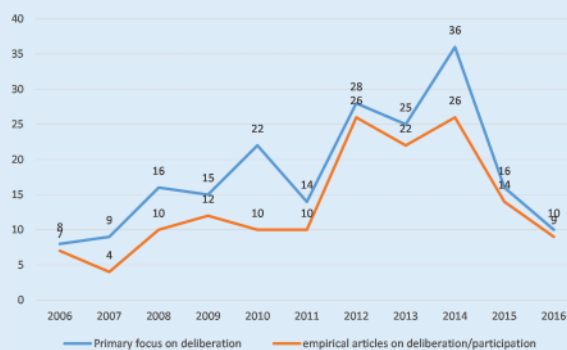


Note: We conducted the mapping in July 2016, data from AJPS, PA, ARPS, APSR, Governance, PAS and JPD.

**Table 1**  
**Aggregate Articles on Deliberation in the Last 10 Years (top 5 journals + PAS + JPD)**

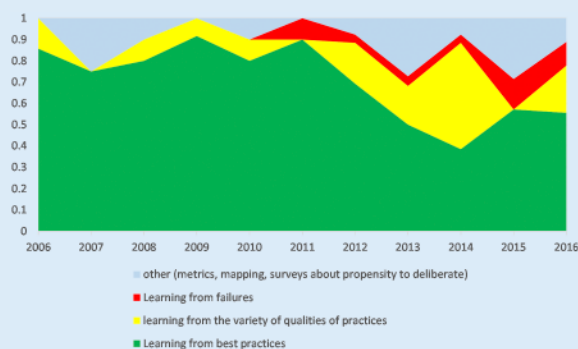
	Total	Top 5 Journals	PAS+JPD
Total articles published (2006–July 2016)	2272	1886	386
Containing the word deliberation <sup>6</sup>	557	352	205
Primary focus on deliberation <sup>7</sup>	200	26	174
Empirical articles on deliberation <sup>8</sup>	151	15	136
1) Learning from best practices	97 (64.2%)	6 (40%)	91 (66.9%)
2) Learning from the variety of quality of practices	28 (18.5%)	6 (40%)	22 (16.2%)
3) Learning from failures	7 (4.6%)		7 (5.1%)
4) Other (e.g., Quality of deliberation metrics, mapping, surveys about citizens' propensity to deliberate)	19 (12.6%)	3 (20%)	16 (11.8%)

Figure 2  
Empirical Analysis on Deliberation



Note: We conducted the mapping in July 2016, data from AJPS, PA, ARPS, APSR, Governance, PAS and JPD.

Figure 3  
A Breakdown of Empirical Articles on Deliberation



Note: We conducted the mapping in July 2016, data from AJPS, PA, ARPS, APSR, Governance, PAS and JPD.

has been built between philosophy and systematic data collection in the sub-discipline, we argue that more needs to be done to refine and integrate the lessons of practice and theory. This is not a new refrain (see, for example Mutz 2008 and Thompson 2008), but it is one whose nature we wish to update. There is a healthy and necessary tension between normative and empirical work in the discipline (Sabl 2015). We argue that the advancement of any such sub-discipline is hampered when this tension is either too strong or lost altogether; and that both problems arise in the context of work on deliberative institutions.

First we contend that some of the pattern witnessed above can be explained by a lacuna of work on conceptualizing so-called democratic innovations themselves in order to establish standards for invoking empirical cases of the class. The meaning of almost every major concept in social sciences can be contested. While a fixation on definitional issues can be poisonous to the advancement of a discipline, it is hard to overstate the difficulty that defining a class by the quality of being innovative creates for the scientific process of comparison. A normative project of democratic deepening

presupposes that improvements on the status quo are necessitated. The gestation of the research area within normative circles has had a lasting influence in the naming of the sub-discipline itself and the expectation of what is studied empirically. Studying innovation implies a commitment to understanding the process of experimentation oriented to identifying and implementing improvements. But what counts as innovation for the purposes of systematic comparison can be confusing because a solutionist approach automatically connotes an idiosyncratic improvement on whatever has gone before.

This scenario presents clear dangers as sampling of exceptional cases is incentivized. An entire class is selected on the dependent variable (or a positively skewed almost-constant as it were). A correction towards the mean is made difficult because each case can be presented as both unique and a member of the class—where what defines the class is the quality of being unique in some way. Such an account is stylized in the sense that no serious scholar will make the explicit claim that a case is both unique and the same in terms of characteristics relevant to the study at hand. And of course if a sub-discipline did not depart from what went before there would be no need for the sub-discipline. But it is the lack of lucid conceptual analysis (which requires both theoretical and empirical contributions) that allows for this selection bias to manifest in a dearth of studies of failures. Failures will simply not be as salient where a class is defined by solutionist claims.<sup>2</sup>

There are very few occasions that we are aware of where empirical scholars have been clearly able to map out or even borrow from theory a set of necessary conditions that distinguish what a non-case of a democratic innovation under investigation is from a case with a negative outcome based on a chosen dependent variable. If the first cases brought to attention are all those with exceptional outcomes, we might expect that in reality the typical case cannot be so, and we cannot be sure of what the typical case looks like and how it differs from our exceptional ones. We would expect much like in Galton's (1886) original that the child's height would regress from that of the exceptional parent (say the British Columbia Citizen's Assembly archetype) towards the mean (something less deliberative or well-planned perhaps). The problem here is that we don't have a good sense of a population and we don't know what the mean might look like. If we took a sample of participatory processes over time we would probably expect that the results in Porto Alegre were atypical. It is not that variation does not exist, it is that the standards for communicating and interpreting that variation do not. Current conceptions of deliberative or democratic innovations either are too exclusive of real-life consultations that are regularly replicated by a plethora of governments and agencies; or can be too inclusive of non-deliberative interventions whose effect departs from standard outcomes—a criticism that has been levelled at the burgeoning deliberative systems literature (Owen and Smith 2015). Where that tension between normative and empirical work is weakened, conceptual clarity will suffer.

This leads us to the second related problem which manifests not when the tension between normative and empirical analysis is too weak, but when it is too strong. What should researchers

do when presented evidence that falsifies a hypothesis? Where normative commitments are strongly held, it is all too easy to categorize failures post-hoc as instances of non-deliberation or unintended consequences of otherwise successful processes.

We should be wary of temptations towards concept-shifting (i.e., oh well that wasn't really deliberation then) as a response

constraints on relationships between researchers and gatekeepers that generates a low supply of these type of studies.

The competitive nature of the emerging democratic innovation 'industry' and the scarcity of long-term funds implies that firms that specialize in the facilitation and support of democratic innovations crave academic validation, that is, research

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to evidence of negative effects on democratic outcomes generated by democratic innovations. If deliberative scholars are overzealous in their normative commitments, evidence that reduces the odds that mechanisms designed to improve democratic deliberation are successful in certain contexts will be ignored or downplayed and the project of democratic deepening will suffer. Where such a situation persists a vicious circle is created whereby negative portrayals of deliberative democracy in certain contexts are not seen as part of the field of democratic innovation (Hibbing and Theiss-Morse 2002, Shapiro 2003); and the sub-field becomes no more than a self-referencing echo-chamber. Following Mutz (2008) we reiterate calls that contributors to debates on democratic institution-building should make clear distinctions between what they expect deliberative democracy to deliver, and what democratic goods they expect specific instances of deliberation to deliver. In other words, deliberative democrats should reflexively consider the scope of their arguments in light of the evidence. That is, we need a better idea of how deliberative democracy is likely to achieve democratic goods in different contexts; at the expense of wasting time trying to interpret what might make a context 'deliberative' or not. It is only once this work is done that we can begin the crucial empirically-informed normative work of deciding what kind of deliberation should be prioritized and when.

#### **Pressure at the Gate—Supply Failure**

Another factor that contributes to the scarcity of interest in failures and fragilities of democratic innovations is increasing

that cheerleads their efforts and products. This system is born not of excessive greed or narcissism on the part of for-profits or non-profits that make up the sector, but of a survival imperative. Perverse incentives are generated by market forces, political competition and austerity measures. The problem is not to compete with other providers of similar services, but to survive a crowded marketplace for political reform, beholden to ideological competition. Often democratic innovations are adopted in the midst of fierce political competition and thus any critique, no matter how small and abstract, might be used to justify abandoning the project.

Inevitably the opportunity spaces for research are limited or mollified, when small organizations are tasked with collecting process data that can be used to review their own performance. The critique of evaluation in the industry is not new (Lee 2015), but intensifies as the public participation professional (PPP) space becomes more crowded and competition among small organizations for survival becomes more pronounced in times of austerity. Academics are often involved in these evaluations but their skill-set can be used as much to avoid serious scrutiny as to provide it. The request for signing of a confidentiality agreement (see an example in appendix 2) is becoming common for academics that join the research boards of organizations implementing democratic innovations.

Thus academics when invited to evaluate democratic innovations are forced to inhabit a difficult space because they have to generate knowledge that is rigorous, but at the same

Table 2

### **The Survival of Participatory Budgeting (PB) among Brazilian Cities with more than 50,000 Inhabitants**

	1989–1992	1993–1996	1997–2000	2001–2004	2005–2008	2009–2012
Number of cities implementing PB	11	29	62	129	119	99
Cities that abandoned PB		4	12	23	64	66
Survival Rate		64%	59%	53%	51%	45%
Cities with a population larger than 50,000 inhabitants in 1992 (excluding Brasilia)	464	468	468	468	468	468

The time periods reflect the city government four-year term in Brazil. The cities considered are those that have a population larger than 50,000 in 1992 excluding Brasilia. Four cities became independent in 1992.

Source: Authors' calculation based on the Participatory Budgeting Census



time cannot be used to damage the reputation of the innovation itself. In many cases the innovation is promoted by well-meaning organizations that open their doors to research. The incentive for the academic is to stop asking the difficult questions (or at least answering them publicly for now), and concentrate on ancillary debates in which significant positive impact can be shown. Most evaluations of deliberative institutions, for example, show increases in learning and internal efficacy of participants—something that is quite important—but very few enter into system-level questions of real policy impact and long-term empowerment.

In order to overcome these problems, academics have developed and implemented innovations themselves. The developer/researcher is a role that has become well understood in the discipline and which funding bodies have supported. However, internalizing development does not automatically shield research from market or political forces in the long-run. When initial exploratory grants run dry, the innovation developed by an academic has to self-fund on the basis of its own merits and not on the basis of the research output. Often developer/researchers have been criticized for not being forthcoming with their data or allowing external impact evaluation of their innovations (Lupia 2004). As a community we are left sleepwalking into a scenario where researchers who have fought long and hard to develop good reputations are criticized because of information constraints not entirely of their own making.

Thus the bias towards generating evidence that is positive is rooted in a mix of moral and personal incentives. Developers, activists and academics themselves are for the most part committed to the normative project of democratic deepening and are interested in the proliferation of these programs so that they can generate more studies and obtain more data.

### Publication Bias—Demand Failure

Debates above may of course be exacerbated by a familiar bias for publishing ‘findings’ (Gerber, Green and Nickerson 2001) and innovation studies that have desirable consequences (Rogers 1983, Sveiby et al. 2009). In 1983 Rogers in reviewing the literature on the diffusion of policy innovation found only 0.2% of studies investigated unintended negative consequences. This evidence bears out what we suggested about disciplinary framing above. If you don’t find an ‘innovation’ you don’t have much to shout about.

Our exploratory analysis is the first tailored to the specificity of the subfield of democratic innovations and it highlights that the top five journals in political science do not contain a single empirical paper analyzing a failure of a democratic innovation in the past ten years. We find such papers in *Politics and Society*, (49th in the last Thompson and Reuters ranking) and in the *Journal of Public Deliberation* (unranked). Are studies of failures not conducted? Or are journals not publishing them because they are less likely to receive attention?

All new fields have to prove their worth in terms of demand. But beyond that there are also very specific characteristics of the democratic innovation sub-field that might be exacerbating this problem. A failure to reach out across sub-disciplinary boundaries has likely resulted in a lack of excitement among

editors of generalist journals about what is at stake in replications of democratic innovations which tend to be small-scale with little immediate policy impact (Goodin and Dryzek 2006). To most social scientists it is not surprising that these experiments fail, and therefore the prospects for learning from expected failures may seem limited (unless they are deemed exceptional). The field has established itself as a normative critique but not yet an empirically grounded one, even if we are now witnessing hundreds of thousands of new deliberative institutions bubbling up around the world.

### CONCLUSION: SIGNALS OF HOPE?

By almost any measure the study of deliberative and participatory democratic innovations is established as a sub-discipline of political science. There is more funding available for, and awarded to, both researchers and practitioners. This has led to an increase in the number of cases existing as well as the amount of data accessible to scholars, the establishment of networks among researchers and practitioners (e.g., *participedia.net*), and increasing academic outputs in the form of books, symposia, and articles in top journals. However, a very simple review of the top five journals in political science has uncovered significant biases in the information flows that reach around the sub-discipline. In this paper we have begun to tease out some explanations for this state of affairs. While this situation might be simply a feature of the novelty of the field, we think at least three emerging approaches give indication of what can be done to hasten positive change.

First, the Participedia Project has recently refocused to establish an international network of research centers (26) and other academics (more than 50) working together to create a global map of democratic innovations. This is a long-term project, but it is specifically designed to overcome selection bias by sourcing case studies from participants themselves and mapping their key characteristics. Participedia is just one example of an emerging family of national or multinational mapping projects that strive to chart both successes and failures (e.g., ‘Latinno’, ‘Cherry-Picking’, and the ‘Brazilian Participatory Budgeting Census’).<sup>3</sup>

Second, a new breed of academic/developers is emerging. These new figures can obtain funding that is bound not to promotion of specific innovations, but to explore a variety of different innovations under different local conditions. For example the Democracy Matters project implemented two different designs of Citizens’ Assemblies in two UK municipalities at the same time to explore the interaction between local conditions, design and outcomes.<sup>4</sup> On a larger scale Archon Fung and Stephen Kosack, together with other colleagues, are implementing randomized controlled trials both in Indonesia and Tanzania to explore again how different local conditions affect the impact of democratic innovations.<sup>5</sup> Both these projects are designed to generate different impacts, and respond to the call for specific theory-testing and reflexive theory-building.

Third, the newly established Empatia project has developed an interesting embargo approach that might help navigate the tension between practitioners and academics. Empatia is a European Research Council funded project

that is implementing a new integrated platform for multi-channel engagement in four European cities (Lisbon, PT; Wuppertal, DE; Milan, IT; and Říčany, CZ). Empatia is piloting an embargo system to allow politicians and implementers to have a few months to prepare a communication strategy to manage the potential negative impact that the release of information will generate. This compromise allows Empatia to include in its researcher-gatekeeper compact a very rigorous impact evaluation component, while at the same time protecting its adopters. At the heart of this approach is the belief that democratic innovations always have challenges to overcome in real-world contexts, and that there may be no perfect solutions. What is necessitated is a robust management system for such problems that does not cover-up failures, but at the same time is not suicidal in the midst of political competition.

Overall we take these projects as welcome signals of hope, but the system of bias-generation is complex and thus we do not expect an immediate change. In particular, we think that it is important that journals themselves start experimenting with new solutions specifically designed to overcome all forms of publication bias. One such experiment conducted in 2015 by *Comparative Political Studies* has offered an interesting set of results about the impact of results-free review on publications (Findley et al. 2016). Of course these reforms will need to be cognizant of their plurality to different methods and organizing perspectives.

One of the most important next steps for the field is to start theorizing what constitutes a failure. We do not require a definitive answer; the answer may depend, for example, on where one stands with regard to debates in democratic theory about the independence of procedures and outcomes in establishing the democratic character of institutions (Barber 1984; Estlund 1997; Ingham 2013 and Landemore 2013); but we do require a more coherent framework of answers. Our coding here was limited to considering failure only where it was interpreted as such by the author itself. And even without a consensus around the concept of failure for now, we suggest contributors to the field should move more systematic analyses of problems, survival and unintended negative consequences into the spotlight.

#### SUPPLEMENTARY MATERIAL

To view supplementary material for this article, please visit <https://doi.org/10.1017/S1049096517000579>. ■

#### NOTES

1. We have surveyed the top five journals in political science according to the 2016 Thompson-Reuters ranking (AJPS, PA, ARPS, APSR, Governance) and we have also included the Journal of Public Deliberation, a non-Thomson Reuters ranked journal dedicated to empirical studies on deliberation, as well as Politics and Society (PAS), a journal outside the top-5 that is attentive to the topic of deliberation. We included PAS and JPD as a validity test to provide some assurance that what we were observing was not an artefact of publishing practices associated with top-ranked journals. The data provided in this article was coded by one of the authors with all the limitations that a non-blind coding protocol implies. A full mining of journals within and beyond the broadly defined field of political science was beyond the scope of this article. The data should be treated in accordance with this limited scope.
2. The concept of solutionism cannot be traced to a particular author, it appears in many disciplines and has a variety of flavours. For a discussion on the origin of this pejorative term in philosophy, architecture, pedagogy

and civic technology see Morozov popular book "To Save Everything Click Here." (Morozov 2013).

3. For a description of Latinno see: <http://www.latinno.net/en/>; for a description of the Cherry Picking project see <https://cherrypickingproject.wordpress.com/>; for a description of the Brazilian PB Census see <http://participedia.net/en/content/brazilian-participatory-budgeting-census>
4. For a description of the project see: <http://citizensassembly.co.uk/>
5. For a description of the project see: <http://evans.uw.edu/faculty-research/leadership-research-real-world-issues>
6. The word could be simply contained in a footnote or even the title of a cited article.
7. The abstract of the article discusses deliberation or democratic innovations.
8. The abstract of the article specifically mentions a case study or a lab experiment or empirical data on deliberation

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