
TWO TYPES OF EXPERIMENTATION: WHERE DO INSTITUTIONAL BYPASSES FIT?

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Experimentation is in vogue in discussions of institutional design, but the concept contains an important ambiguity. That ambiguity is highlighted by Mariana Prado and Michael Trebilcock's work on institutional bypasses. Prado and Trebilcock define an institutional bypass as a type of institutional reform that involves creating a separate institution which operates in parallel with and performs the same function as the original institution (Prado and Trebilcock 2019, 6–7). They promote bypasses as ways of opening up room for experimentation. At the same time they distinguish institutional bypasses from randomized controlled trials, one of the best-known types of experimentation (2019, 10–11). In drawing this distinction Prado and Trebilcock highlight the fact that literature on experimentation in institutional design covers at least two different conceptions of experimentation. In some literature experimentation is exemplified by randomized controlled trials, in others it is exemplified by the more open-ended processes associated with experimentalist governance.² This short essay elaborates on the distinctions between the two types of experimentation, argues that institutional bypasses are likely to fit best with the second type, and emphasizes that the two types of experimentation have different advantages and disadvantages as modes of learning and reform.

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1. RANDOMIZED CONTROLLED TRIALS

The idea of institutional experimentation was thrust into public prominence when the 2019 Nobel Prize in economics was awarded to three scholars, Abhijit Banerjee, Esther Duflo and Michael Kremer, “for their experimental approach to alleviating global poverty” (The Nobel Prize 2019). Banerjee, Duflo and Kremer are known for pioneering the use of one particular type of experimentation, “randomized controlled trials,” to learn about the effects of interventions designed to combat poverty (The Committee for the Prize in Economic Sciences in Memory of Alfred Nobel 2019).

In its simplest form, a randomized controlled trial begins with a population of actors and an intervention whose effects are to be investigated. Each member of the

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² For a distinction between strong and weak forms of experimentalism, see Davis 2010, 548-549 and Davis and Prado 2014, 215–16.



population is randomly assigned to one of two groups, a process which will result in two groups whose characteristics are expected, on average, to be identical. One group, the treatment group, is subjected to the intervention. The other, the control group, experiences the status quo. Next the groups are compared along any dimensions which the intervention could possibly affect. If the experiment is well-designed, and if the randomization process produced very similar treatment and control groups, then any post-treatment differences between the two groups can be attributed to the fact that one group was “treated” with the intervention. In addition, the average difference is considered to be an estimate of the average effect of the treatment (The Committee for the Prize in Economic Sciences in Memory of Alfred Nobel 2019).

Suppose, for example, the objective is to learn about the effect of introducing a new approach to policing. In principle, one could investigate this question using a randomized control trial. The first step would be to randomly assign communities to either a treatment or a control group. The communities in the treatment group would be policed using the new approach. Communities in the control group would be policed in the same way as before. If the two groups of communities were in fact identical then any post-treatment differences between the communities in the treatment group and those in the control group can be attributed to the new policing, and the average difference will equal the average effect of the intervention. The great virtue of randomized controlled trials is that they allow investigators to isolate these sorts of causal effects with a fair amount of confidence (The Committee for the Prize in Economic Sciences in Memory of Alfred Nobel 2019).

The validity of causal inferences from randomized controlled trials depends critically on separation of the groups—everyone in the treatment group and no one in the control group must get the treatment.³ Unless the groups are separated in this way there is no reason to infer that post-treatment differences between the two groups reflect the effects of the intervention. For example, our hypothetical policing trial will not serve its purpose if outcomes in the control group are affected by the treatment. The same is true if the treatment group is affected by the presence of the control group. There are several ways in which these kinds of spillovers might occur. The police using the new approach might stray into or be spotted from within communities in the control group. Or criminals in the treatment communities might flee into the control communities to escape the new forms of policing (or vice versa). Yet another possibility is that the police in one or both groups will be influenced by impulses to compete with one another.

Not all forms of experimentation display the virtues of randomized controlled trials. Consider institutional bypasses (Prado and Trebilcock 2019). Prado and Trebilcock list “creating room for experimentation” as one of the advantages of institutional bypasses (2019, 9, 136). By this they mean that a bypass generates

³ For further discussion see Davis 2019.

information about the effects of an institutional reform under the actual conditions in which the reform would operate if implemented fully. They acknowledge that randomized controlled trials can provide similar information but argue that bypasses may be preferable because the trials “present significant financial, logistical, ethical and political obstacles in many circumstances” (Prado and Trebilcock 2019, 11).⁴ The difference appears to be that institutional bypasses leave the original institution in place so that members of the affected population are exposed to both institutions at once. As a consequence, no one is deprived of access to a potentially superior institution. This increases the odds that a bypass can be introduced and abandoned without producing significant ethical concerns or political opposition. In addition, in cases where members of the population can choose whether to use the relevant goods or services, exposing the population to both institutions makes it possible to elicit information about users’ preferences. None of these benefits can arise in a randomized control trial because the members of the treatment and control groups are, by design, exposed to different institutions.

While it may be true that institutional bypasses avoid some of the drawbacks of randomized controlled trials, they also sacrifice one of the main benefits, namely, the ability to draw inferences about the effects of exposing the population to only the new institution. Recall that by definition a bypass serves the same population as the institution it reforms. This means that any outcomes reflect the influence of the two institutions operating side-by-side. Even if outcomes in some portion of the population improve following the introduction of the bypass, there is no way to know whether the improvement would have occurred in any event under the original institution or will occur if the bypass ousts the original institution. Better outcomes in the communities who use the bypass may be explained by overall improvements in social or economic conditions that would have caused improved outcomes under the original institution as well. Or, the superior performance of the bypass may be attributable to its interaction with the old institution. For instance, the old institution might be dealing with the hard cases in the system. Or alternatively, the presence of the old institution might be pushing the people who staff the bypass to compete. As a result, the only thing we can learn about with confidence is how a system that combines the bypass and the original institution compares with a system which, at an earlier point in time, included only the original institution. For example, we may learn that a healthcare system works better after the addition of emergency health care units. But that does not mean that it works better *because* of the new units. Nor does it mean that the system will work better if those units completely replace existing emergency rooms.

2. EXPERIMENTALIST GOVERNANCE

⁴ See also Davis 2010, 550-552 (discussing ethical and practical obstacles to employing experimentalism as an approach to lawmaking in developing countries).



Although institutional bypasses do not exemplify the kind of experimentation favored by proponents of randomized controlled trials, they do qualify as examples of the kind of experimentation associated with the concept of “experimentalist governance.” In the literature on experimentalist governance pioneered by Charles Sabel, experimentation refers to processes that generate information about the effects of a sequence of incremental reforms as opposed to wholesale replacements of institutions (Sabel and Zeitlin 2012). Learning arises from comparison of outcomes both over time and across groups that have implemented different reforms. Examples of this kind of experimentation have been documented in fields as disparate as management of river basins, regulation of food safety, child protection and community policing (Sabel and Zeitlin 2012, 172). Sabel, his collaborators and other scholars suggest that this kind of experimentation is particularly well-suited for institutions that operate across international borders in areas such as environmental protection, protection of human rights and regulation of transnational bribery (Sabel and Zeitlin 2012; De Búrca, Keohane, and Sabel 2014; De Búrca 2017; Davis 2019b).

A distinctive feature of the concept of experimentalist governance is that it explicitly extends to the practice of reviewing and analyzing both the outcomes of intervention and the processes for evaluating those outcomes. That review and analysis involves more than just evaluation of an experimental design in accordance with agreed scientific criteria. It also involves reflection on the criteria themselves.⁵ Proponents of experimentalist governance acknowledge that actors operating in a given field might disagree about either the outcomes that institutions should seek to achieve or how to evaluate those outcomes.⁶ This is especially likely when information about outcomes is imperfect and the relevant actors are associated with different institutions or are accountable to different communities, conditions that are especially common in the context of transnational regulation of illicit activities (Davis 2019b).

Institutional bypasses fit the description of experimentation used in the literature on experimentalist governance. First, institutional bypasses involve incremental reforms. Second, in principle, the evaluation of institutional bypasses can involve review and analysis of both the outcomes of implementing the bypass and the evaluation process itself. Third, although bypasses may be less prone to controversy than randomized controlled trials, they are still likely to open up room for disagreement and reflection.

Consider a concrete illustration, an intervention that involved extending the operations of a foreign law enforcement agency to cover a form of corruption that

⁵ According to Sabel and Zeitlin (2012, 170): “...goals, metrics, and decision-making procedures themselves are periodically revised by a widening circle of actors in response to the problems and possibilities revealed by the review process.”

⁶ According to Sabel and Zeitlin (2012, 177): “...too many participants with sharply different perspectives may make it hard to reach an initial agreement on common framework goals.”

previously was only subject to regulation by dysfunctional local institutions. This is essentially what happened when the U.S. Department of Justice began to prosecute officials of *Fédération Internationale de Football Association* (FIFA), football's highest governing body, and its regional affiliates, for corruption in U.S. courts (Department of Justice 2018; 2015). This intervention qualified not only as a form of experimentation but also as an institutional bypass. On the positive side, it may be possible to learn something about the efficacy of the U.S. intervention by examining indicators of corruption in sport before and after the reform. However, for the reasons discussed above, that analysis will not tell us anything about whether it would be appropriate for the U.S. Department of Justice and U.S. courts to completely displace local enforcement institutions and adjudicators. There also may be profound disagreements about how to evaluate the outcomes of the intervention. Some of those disagreements may be over questions of fact. Has corruption declined in the aftermath of the intervention? Have talented businesspeople been frightened away from serving in sporting organizations by the prospect that their conflicts of interest will trigger criminal liability? Other disagreements may be over questions of values. What term of imprisonment is proportionate to the wrong represented by this form of bribery? Should fines levied on offenders be kept by the U.S. Treasury or returned to the organizations that were defrauded? Is U.S. extraterritorial enforcement inherently illegitimate?

Notice that the mere fact that this intervention left the existing local institutions in place did not guarantee that it would avoid ethical objections and opposition. At least when it comes to coercive interventions, disagreements on ethical or political grounds seem inevitable. In my own work on the regulation of transnational bribery I argue for a form of "inclusive experimentalism" which involves seeking out, confronting and trying to resolve these potential disagreements in collaboration with as broad a group as possible of affected parties (Davis 2019a).

3. CONCLUSION

The idea of an institutional bypass draws some of its appeal from its association with the idea of experimentation. Experimentation in institutional design can be a useful method of learning about alternative designs. For that reason, experimentation also represents an appealing approach to reform of dysfunctional institutions. However, experimentation comes in different flavors. Many proponents of experimentation idealize randomized controlled trials. Institutional bypasses can never fulfill the promise of that type of experimentation as a means of identifying causal effects. Bypasses are more compatible with the type of experimentation associated with experimentalist governance, which typically involves implementing and learning from incremental reforms as opposed to complete institutional displacement. The epistemological, ethical and political implications of the differences between these two types of experimentation warrant greater attention. At the same time, there should be explicit acknowledgement that

both types can, and ideally should, involve review and evaluation of both the outcomes of experiments and the process of experimentation itself. That acknowledgement should be accompanied by frank recognition of the potential for disagreements between actors with different perspectives as well as a commitment to developing inclusive processes for identifying and trying to resolve those disagreements.

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