What Good is a Regression?

Inference to the Best Explanation and the Practice of Political Science Research[*]

Arthur Spirling[†]   Brandon M. Stewart[‡]

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Abstract

We consider the claim that political science research has become too focused on causal inference at the expense of substantive concerns. We contend that debates on this question are unproductive because they fail to recognize that almost all empirical social science research uses "Inference to the Best Explanation" (IBE) and must be assessed in that light. Noting its wide acceptance and use elsewhere, we review the basic principles of IBE, and examine its ubiquity in our discipline. We show that disputes regarding the merits of different approaches can be reduced to debates over the relative weight scholars place on quality of inference versus quality of explanation. We argue that many pathologies of current practice can be both explained and potentially ameliorated this way. These include incentives to p-hack, the undervaluing of description, and the confusion over the value of non-plausibly causally identified studies. Explicitly embracing IBE helps structure discussions in the discipline.

[†]Professor of Politics and Data Science, New York University (`arthur.spirling@nyu.edu`)
[‡]Assistant Professor of Sociology and Office of Population Research at Princeton University (`bms4@princeton.edu`)

1 Introduction

The use of regression is ubiquitous in social science research. Despite this dominant status—or perhaps because of it—what one can learn from a given regression has become increasingly contested. This is partly a question of whether a given association can be plausibly claimed to be causal (e.g. Samii, 2016), but it is much broader than that. It includes the appropriate role that regression might play in suggesting explanations (e.g. Huber, 2013), and in testing them (e.g. Ashworth, Berry and Bueno de Mesquita, 2021). This paper provides a framework for understanding, and potentially resolving, the differences implied by these seemingly opposed positions. That framework, known as "abductive inference" in general, and "Inference to the Best Explanation" (IBE) in particular, is by no means novel to us (it has been known at least since Peirce, 1878). But we believe it is almost entirely unknown to scholars in the discipline (though see Dowding and Miller, 2019; Tavory and Timmermans, 2014). This is surprising because, as we will argue, almost all published work is doing some version of the IBE process—including qualitative studies. This matters: it means that much of the controversy around the use of regression, and indeed quantitative work more generally, is missing an interrogation of the underlying inferential framework on which the work is built. In particular, our contention is that in making their claims and counter-claims, researchers are in fact advocating for the importance of different elements of the *same* mode of inference. With this is mind, they have much more in common than they initially realize. In short then, the answer to "what good is a regression?" depends on what element of that inferential framework you value most. And while there may be good reasons to value some particular element more than others, researchers should make that case explicitly when critiquing other approaches. To be clear: our argument is not that scholars *should* do IBE; it is that they are *already* doing IBE, and so should be aware of what that means for their work.

Inference to the Best Explanation (IBE)—a phrase coined in Harman (1965)—is a mode

of inference that describes everyday reasoning. Given a set of facts, we infer that from a set of possible explanations, the one that 'best' explains the evidence is the one most likely to be true. The best explanation might be one that is simpler, more complete, or possessing some other desirable property. IBE is a form of 'abductive inference" inference (sometimes "abduction"), in that it involves a non-deterministic link between observation and conclusion, via a (typically causal) story that explains the outcomes we saw. Unlike deductive inference, the conclusions are not direct, logical consequences of a set of premises. We are most confident in IBE when the candidate set of explanations are *ex ante* plausible and the evidence discriminates well between them. Because we cannot do deduction except in special cases such as formal theory[1], and inductive reasoning (strictly defined) is rare, IBE is essentially how arguments always proceed. Social scientists will recognize it most clearly in work where authors lay out multiple theories (explanations) and adjudicate between with them with a variety of regression-based tests (facts/evidence). This is not just a difference of terminology: IBE emphasizes that the *candidate explanations* have as important a role in the credibility of the inference as the evidence.[2] Thus, credible estimation of a descriptive fact or causal query is only one piece of the larger inferential framework. Any one fact is consistent with countless possible stories, so we are *always*—even if only implicitly—adjudicating between competing explanations.

We use 'regression' as a catch-all for any parametric or non-parametric model in which there is an outcome (dependent variable) that is a function of at least one predictor (independent variable). Typically, we are using it to characterize a property of the conditional distribution of the outcome given the predictors; it tells us how the outcome will change as we vary a particular input. This includes linear regression as a special case, but also subsumes generalized linear models, and various techniques traditionally deemed part of

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[1]We can make a deductive inference about a property of the model but still need IBE to connect to real-world behavior.

[2]See e.g. Gelman and Imbens (2013, 3) for a related discussion on when patterns "need an explanation."

"machine learning" (e.g. random forests).[3]

This paper begins by reviewing the place of regression in the discipline. In Section 3 we then provide a definition of IBE before showing how it is used (knowingly or not) in the theory and practice of social science research. In Section 4 we explain why understanding current practice as IBE matters. We are explicit in terms of implications for research. In our view, many common concerns, including the danger of HARKing (Kerr, 1998) and p-hacking (Franco, Malhotra and Simonovits, 2014), and the relative importance of description (Gerring, 2012; Munger, Guess and Hargittai, 2021), should be understood as differences of opinion about what part of IBE is most important. We make no claims that acknowledging the role of IBE will solve all problems, and we are explicit in that section about currently open issues. We conclude in Section 5.

2   The State We are In: Current Methodological Debates in Political Science

Consider a common scenario. An author presents a regression table and discusses the "effects" of a given (independent) variable on an outcome of interest. The data is observational, meaning that assignment to treatment and control is not within the power of the researcher. From a causal inference perspective, one of the variables is a treatment (in the sense of e.g. Gelman and Hill, 2006), but the author does not necessarily use that term. In addition, the author controls for (conditions on) various other variables in the regression, but does not provide particularly clear assertions about the nature of the assumed confounding, any potential post-treatment bias, the plausibility of conditional ignorability in this case, or the implied Directed Acyclic Graph (in the sense of Keele, Stevenson and Elwert, 2020). Over

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[3]See e.g. Aronow and Miller (2019) for a textbook account of linear regression as an approximation of an unknown conditional expectation function.

various specifications, the relationship between the treatment and the outcome is consistent, insofar as the sign and direction of a coefficient (or computed quantity such as a risk ratio) remains similar across the model specifications. Put crudely: what is this regression good for, and how might we assess its merits? How might we judge the worth of this regression (or these regressions) relative to one where, conditioned on confounders, the treatment is plausibly as-if randomized to units? Suppose in addition, that in this latter case only one potential explanation for the data-generating process is offered. Finally, assume that prediction (in the sense of Cranmer and Desmarais, 2017) is not the goal. These abstractions are perhaps extreme, but capture the spirit of many examples in modern published works.

In a thin sense, there is no dispute about what a regression does in either case: it approximates a conditional expectation function.[4] But when, and in what ways, that conditional expectation function is useful is the subject of considerable debate. This is not simply a matter of disagreement over what assumptions are plausible when; it is, in practice, a question of competing visions about what the work of the discipline should be and where effort is most profitably applied.

Although a relatively recent development in the history of social science, the "credibility revolution" (Angrist and Pischke, 2010) is a natural place to begin this discussion. The central idea here is that making causal statements is difficult with observational data, and can only be done in a more limited set of circumstances than may be initially realized. Indeed, per Samii (2016) (see also e.g. Gelman and Hill, 2006; Gerber et al., 2014; Keele, Stevenson and Elwert, 2020), regressions without thought about these issues may be actively misleading. Consequently, scholars must search for a "strong design" in order to make

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[4]Claims regarding regressions in published work often revolve around the fact that a coefficient is stable across specifications. It is worth articulating what this means. Consider a binary treatment variable. The coefficient in the table is the parametric approximation of the difference in means between the treated and untreated averaged over the strata defined by the other covariates. The stability of the coefficient implies that (given the model approximation) this difference remains approximately the same as we change the subgroups over the different models in the table.

"persuasive" causal claims (Sekhon, 2009, 503). This is hard to achieve even in seemingly propitious circumstances where, for example, treatment and control may be randomized but the groups thus created are not comparable (e.g. Sekhon and Titiunik, 2012). While the technical claims of these scholars are not in doubt, there has been much disagreement about what the credibility revolution means, or should mean, for the focus of political science research in general.

Some scholars, like Huber (2013) (see also Huber 2017, Ch 6; Clark and Golder 2015; Binder 2020), argue that the turn to causal inference designs is potentially troubling. The first reason for this is that many substantively interesting phenomena do not naturally lend themselves to such work (because e.g. the treatment cannot be plausibly randomized), and thus we see less effort to study such questions. The second reason is that focussing on identification opportunities crowds out theory development: the claim is that traditional (not plausibly causal) regression designs help us refine our understanding of relationships in observational data. In the context of randomized controlled trials (RCT), Deaton and Cartwright (2018) make an allied argument. That is, the results of (necessarily) specific RCTs cannot be easily extrapolated to broader questions of interest in a field. By contrast, scholars like Samii (2016) contend that these fears are somewhere between overblown and exactly wrong. More specifically, we should avoid using traditional designs that generate "pseudo-general pseudo-facts" (Samii, 2016, 1). And such entities are a bad basis for either trying to understand phenomena or building theories about them. Thus, to the extent that the credibility revolution has changed practices, it has done so in a way that moves authors away from actively misleading themselves from their results. A related but distinct concern comes from those who contend that causal empiricists and formal theorists are not communicating with each other—they are "pulling apart", when they should be cooperating (Ashworth, Berry and Bueno de Mesquita, 2021).

For others, the priority is not producing causal claims (of whatever plausibility), but

description. Thus we see work by Gerring (2012) that emphasizes the importance of the descriptive task *per se* and independent of theory-testing. Indeed, scholars have proposed entire journals to help counter the fact that "[c]ausal research that asks the question *why* has largely taken the place of descriptive research that asks the question *what*" (Munger, Guess and Hargittai, 2021, 3, emphasis as original). Here then, regressions are informative about the state of the world in terms of associations—nothing more and nothing less. Partly in an attempt to connect this associational logic to the goal of inference, researchers have recently argued that flexible machine learning approaches—capable of including non-linear interactions—ought to be more broadly deployed for political science tasks (e.g. Montgomery and Olivella, 2018). Whatever the estimation approach, the fundamental challenge is that the associations are conditional on many variables. But this can make interpreting them—in terms of an all-else equal logic—difficult (Ashworth, Berry and Bueno de Mesquita, 2021).

Whatever one's priority for the purpose of regressions, there has been increasing agreement on what their properties ought to be. In particular, the importance of replication and robustness in results. At one level, the concerns regard the potentially malign motivations of researchers to "p-hack" or else leave insignificant results in the "file-drawer" (e.g. Franco, Malhotra and Simonovits, 2014); for others, there are broader issues of "forking paths" wherein researchers make ad-hoc but crucial decisions about data and estimation Gelman and Loken (2014). Scholars have proposed various solutions, from "multiverse analysis" of all possible choices (Steegen et al., 2016) to more focused efforts at assessing sensitivity (e.g. Imai and Yamamoto, 2010; Blackwell, 2014; Cinelli and Hazlett, 2020). Related in spirit, but different in practice, other authors have suggested methods for incorporating distributional assumptions about bias (as in, the difference between an estimated coefficient and the 'true' causal effect) into more nuanced interpretations of regression results (Little and Pepinsky, 2021).

Of course, the logic thus far assumes that authors are sufficiently clear about what they

are estimating to effectively connect the results to explanations or broader theory in a credible way. Lundberg, Johnson and Stewart (2021) point out that this is often not the case. This leads to situations where debates can be entirely about disconnects over the target estimand. An older literature, (e.g. Lieberson and Horwich, 2008) worries that the link between theory and evidence has so substantially frayed that social science is merely 'mimicking' science. A necessary first step in understanding how evidence informs theory is understanding what the author believes the evidence to represent.

The sheer diversity of inferences about and from regression might suggest that the discipline has no unified way to communicate about these issues. At the very least, it would seem that authors have instinctual understandings of what makes their arguments compelling but that those visions have no common framework in which to be placed and assessed "scientifically". Our contention is more positive, however. We claim that a framework already exists for accommodating all of these positions, and that it is Inference to the Best Explanation. We now define IBE in more detail, before making this point with reference to studies in the field.

3   What is *Inference to the Best Explanation*?

Abductive inference, in the form of IBE, is ubiquitous in scientific enquiry (e.g. Harman, 1965; Boyd, 1984; Douven, 1999; Lipton, 2003). The study of politics is no exception (Dowding and Miller, 2019). Despite the fact that it is believed to capture 'everyday' reasoning, it is not always easy to find precise statements as to what IBE is. Introductory accounts (e.g. Psillos, 2002; Douven, 2021) will typically give a definition along the following lines:

> Given some data D (some observations, or facts about the world), and some candidate explanations or hypotheses $E_1, \ldots, E_n$ that potentially explain D, the one that is most compatible with D is most likely to be true.

Unsurprisingly, there is considerable philosophical debate as to what constitutes an "explanation" *per se* (e.g. Achinstein, 1983). But at this level of abstraction, it is straightforward to give examples of how such a process might work in principle. Suppose we observe that as countries become more developed (say, in per capita income terms) they are generally more likely to become democracies. One explanation might be that of *Modernization Theory* in the sense of Lipset (1959): essentially, that changing social conditions make middle class citizens (in particular) more likely to embrace democratic ideals. An alternative explanation might follow this basic logic, but specify that middle class sympathy is a product of particular social relations some centuries before (Moore et al., 1993). Other explanations might focus on the role of income inequality (e.g. Boix and Stokes, 2003) or elite responses to the threat of revolution (Acemoglu and Robinson, 2001). Which of these explanations is most plausible is not our central interest here.[5] Our point instead is that this is a near-universal undertaking: scholars observe (essentially) the same data, and attempt to provide a 'best' explanation for this data. And when they do this, they are doing IBE. We can push this point further. When scholars gather *new* data and suggest a 'best' explanation for those observations—relative to other explanations or even simply a null hypothesis—they are also doing IBE. Consequently in empirical social science, almost everyone, all of the time, is doing IBE. We will expand on this idea below, but before doing so we clarify the position and nature of IBE more broadly.

First, abductive inference is in contrast to both *deduction* and *induction*. If we observe data D, then deduction requires that our inference is a logical consequence of that D. In social science, such reasoning is rare outside of formal theory work where we specify predicates (say, assumptions of a theoretical model) and generally agree on what operations one can undertake on those predicates (say, what makes for an 'equilibrium' in a given game). Logic

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[5]Though obviously political science as a whole has invested considerable effort in this question (see, e.g., Przeworski et al., 2000)

of this kind extends to the "deductive approach" to causal inference—which requires careful specification of reality and the representation of that reality (Pearl, 2014).[6] But this does not mean that a given *empirical* study using such tools is itself "deductive" in terms of its inferential approach: it cannot be if its conclusions do not follow *with certainty* from its data. In contrast, *induction* does not involve guaranteed conclusions. This is the case even though we may accept the truth of the premise D. In line with our comparative politics example above, an inductive inference might be that a randomly chosen rich country is very likely to be democracy. Unlike for deduction though, we make no claim that this *must* follow from D—we acknowledge that there are a small number of rich non-democracies. In addition, and crucially unlike in abductive inference, induction does not require we offer a causal 'story' as to *why* we expect a rich country to be democratic. Induction can simply assert that these features generally co-occur. IBE requires the extra step.

Second, however common, IBE is not (claimed to be) a perfect strategy for inference. First, IBE contains a logical fallacy: "affirming the consequent." From our case above, for example, if *Modernization Theory* is correct, then it follows that we would see a particular pattern of democratization. But seeing that particular pattern cannot be conclusive evidence that *Modernization Theory* is correct (Clarke and Primo, 2012, make a similar point in their discussion of 'models'). Second, we have no guarantees that the set of explanations from which we are purportedly selecting the 'best' one contains the truth. Indeed, the fact that political scientists continue to propose new explanations for the development data we observe suggests that the field as a whole has not yet reached the end point of this search. Finally— and a point that belies the simplicity of abstract definitions of IBE—it is unclear exactly what constitutes a 'best' explanation. It seems reasonable to prefer explanations that have better predictive accuracy (even within sample), but how that is traded off against the qualities of

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[6]That is, that a causal quantity (defined in a certain way) can be identified as a particular operation on a structural causal model is a deductive conclusion that follows from the assumptions of that structural causal model.

the explanation itself (e.g. its parsimony) may be ambiguous (see, e.g, Barnes, 1995).

Of course, one does not need to believe that a given inference method is perfect (or even coherent—see, e.g., Van Fraassen, 1989; cf Douven, 1999) for it to be popular in practice. And if it is popular, it is important to understand its characteristics and implications.

3.1   IBE in Social Science: Theory

To be more specific about the machinery of IBE in social science, it is helpful to think about how it works in a "classic" case, such as medicine (the discussion of the Semmelweis case in Lipton, 2003, is in this vein). Generally, the moving parts here are

1. characterization of the symptoms (the data)

2. the enumeration of possible explanations (causes of symptoms)

3. tests that discriminate between explanations that are or are not consistent with the data (or that are more or less consistent with the data)

The order is important: to avoid overfitting, one cannot use the *same* data to both create explanations and test those explanations. Nonetheless, there can be iteration: medical doctors may update their diagnosis after positing a particular condition, but then testing for it, and finding it absent. But there are limits, as we note below.

In the context of social science, steps (2) and (3) take specific forms. First, our explanations are almost always *causal* in nature (King, Keohane and Verba, 1994; Clarke and Primo, 2012) though exactly how precise they need to be is debatable (Dowding, 2015, Ch3). Second, in quantitative work, it is often the case that only one explanation is offered, and that it is tested against *only* a null hypothesis (Gross, 2015). Of course, the null hypothesis is not itself an explanation for the data (see McShane et al., 2019). So failing to reject the null is potentially awkward in the world of IBE. That is, we have not found evidence consistent

with our preferred E_i, but nor have we made an inference to another, 'best', explanation. This does not mean that the study is of no value. Obviously, as with medicine, it can be important in an individual case to show that a particular explanation is unlikely to be correct (e.g. we ascertain that patient does not have an allergy to shellfish, and can likely eat shrimp). More optimistically, at a *field* level of surveying many studies regarding similar data, we can presumably claim that a process consistent with IBE is taking place. That is, when we look at a literature at a high level, we are collectively generating new explanations, and making an inference to the best one by rejecting those that are not supported. But some care is required in this weaker, aggregated understanding of IBE: simply generating a large number of bad (unsupported) explanations is not especially helpful.

3.2   IBE in Social Science: Practice

One part of our argument is that everyone is doing inference to the best explanation, whether they know it or not. IBE is ubiquitous because it is often the only thing we can do—no single exercise is going to unambiguously show that we are right (in the way that a deduction would). We now show how different types of social science work proceed on this score.

3.2.1   IBE in Observational Studies

In one of the most highly cited articles ever published in the *American Political Science Review*, Fearon and Laitin (2003) seek to explain why the 20th Century saw a notable rise in civil conflict. They pose the question: "What explains the recent prevalence of violent civil conflict around the world?" (75). They then enumerate a set of candidate explanations from conventional wisdom as to what makes countries susceptible to civil war: the end of the Cold War and associated changes in the international system, ethnic or religious diversity, and ethnic or broad political grievances. Their fourth (preferred) explanation is conditions that favor insurgency, including weak central governments, positive shocks to

insurgent capabilities, and rough terrain. Much of the article is devoted to detailing 10 different empirical facts (framed as hypothesis tests) that would be implied by different explanations for civil wars. While the explanations themselves are essentially causal in nature, the tests are mostly descriptive or predictive in nature. Like the doctor examining symptoms, Fearon and Laitin (2003) describe the conditions that they would expect to see in the world were each explanation best. They then test these conditions using five different regression models—containing thirteen predictors—on cross-country data.

Fearon and Laitin (2003) has been the subject of critiques on methodological grounds, many of which have emphasized the concerns around post-treatment bias (e.g. Acharya, Blackwell and Sen, 2016) and thus challenged the empirical credibility of the tests (e.g. Samii, 2016). Focusing on claims about the relationship between economic shocks and violence, Ashworth, Berry and Bueno de Mesquita (2021, Ch 9.2) note that the entangled nature of the mechanisms concerned make it hard to know what association we would expect to see between economic performance and civil conflict even if the economy was driving the conflict. This is one of the challenges of IBE: even when trying to apply descriptive or predictive tests, causal reasoning is generally necessary to determine what we would expect to see given that the explanation were true. Of course, some explanations can be more easily removed from consideration: Fearon and Laitin (2003) quickly dismiss the first common wisdom explanation—the increase is due to the changes at the end of the Cold War—by showing in their first figure that civil war had been steadily rising since at least 1950.

Regardless of whether one finds the tests convincing, the strategy here is one of inference to the best explanation. Thus we can evaluate the contribution of the work not just on the quality of empirical evidence, but also on the development of the candidate explanations. Even if we believe the tests of the 10 hypotheses are convincing, this does not rule out other explanations they do not consider, it simply suggests that their preferred explanation is the best of the four they offered. This makes the credibility of their conclusion—and

the policy implications they draw from it at the end of the article—turn as heavily on this candidate set of explanations as the empirical credibility of the tests. We think this fact that is under-appreciated in the methodological literature.

3.2.2   IBE in Experiments

While IBE helps to reinterpret what is happening in observational studies, it is also the mode of inference used in randomized experiments that target specific quantities. This is most obvious in the context of laboratory experiments that capture some abstract form of behavior that is intended to generalize to a broader class of real-world settings. In their paper "Winners don't punish", Dreber et al. (2008) aim to assess claims about the role of self-sacrificing punishment in maintaining cooperation. They bring subjects into the lab to play a form of repeated Prisoner's Dilemma games where—in addition to the usual cooperate and defect—there was a third option to "punish" by paying 1 unit of money to have the other player lose 4 units. They find that those who perform well don't use the ability to exact the punishment and that "this suggests that costly punishment behaviour is maladaptive in cooperation games and might have evolved for other reasons" (Dreber et al., 2008, 1). That is, their understanding is that a common set of behaviors governs the way humans behave in many settings. Here the data is the result of this simplified experiment, but their inference is that this is reflective of behaviors in a much wider array of settings.

The logic of IBE also pertains to field experiments. Consider for example, the audit study in Bertrand and Mullainathan (2004) to study discrimination. The motivating observation is that, in the United States, Black prospective workers are twice as likely to be unemployed as white prospective workers. This is consistent with a range of explanations including racial discrimination at the point of application or differences in the supply of applicants (presumptively due to prior history of racial discrimination). The authors design a method of data collection that adjudicates these explanations: they submitted the same resumes to

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different jobs, swapping out the names with a set intended to convey that the applicant was white or Black. The data is that a list of (protypically) white names received 50% more callbacks than a list of (protypically) Black names. Since the resumes were the same and we might reasonably believe that employers do not have especially strong preferences towards names *per se*, they infer that the best explanation is a discrimination on race at the point of application.

Note that even this claim involves an inference that the driving feature is signaling race and not socio-economic status or some other property of the applicant. Bertrand and Mullainathan (2004) attempt to adjudicate among these explanations by showing that the gap between the names is no higher for jobs that rely on soft skills or interpersonal interactions. They also show that their evidence is not well explained by either of the two major economic theories usually used to explain discrimination: taste-based and statistical discrimination (Becker, 1957; Arrow, 2015). They suggest then that an alternative model based on a heuristic screening might be a better explanation for the behavior they observe.

Experiments are attractive because the randomization makes the correspondence between the observed association and the causal estimand particularly plausible. However, this 'merely' establishes a particularly reliable fact about the world; it does not directly evaluate a theory. IBE is the framework that moves us from the particular facts we observed to the broader explanation. Even in the case of experiments, the set of possible candidate explanations plays a crucial role in our understanding of what is a reasonable inference.

4   What IBE Means for Applied Research

To reiterate: scholars are doing IBE almost all the time. We now turn to the practical implications of this fact. We emphasize three ideas: first, IBE often involves the bringing together of multiple pieces of evidence: e.g. many different regressions in various places,

in addition to a qualitative case study on a potential mechanism. Second, explanation is obviously vital to the practice of IBE, and should be taken as seriously as inference is. Third, that exploratory analysis, because it encourages the development of explanations, is similarly crucial and currently undervalued.

4.1   Imperfect Tests Are Compatible with IBE

IBE suggests that we want to find pieces of evidence that discriminate between competing explanations of the underlying phenomenon. In practice, most pieces of evidence are consistent with more than one possible explanation (even in causally identified experiments as we demonstrated above). But this frees us from the need to search for a single glorious test that will uniquely demonstrate that our favored explanation is true—likely no such thing exists. Instead, we simply need to rule out other competing possibilities. Thus work done in the framework of IBE will often involve cobbling together imperfect evidence that jointly make a compelling case. An immediate consequence is Implication 1.

Implication 1 *The "Causal Two-Step" is not compatible with IBE. Researchers should clearly state their causal ambitions.*

The "Causal Two-Step" arises when a researcher includes controls *as if* to make the estimated effect of the treatment more causally plausible, yet *simultaneously* claims that the data does not allow for a causal interpretation of coefficients. This is not IBE. What is compatible with IBE is researchers either making the causal claim they want to make and acknowledging the possible imperfection of any one piece of evidence (regression, case study, interview) or being clear how the (non-causal) descriptive evidence helps to adjudicate between competing (causal) explanations of the world. That is, a causal identified regression coefficient is only one type of evidence that can be consistent or inconsistent with an explanation.

4.2 Explanation is as Central as Inference

Our contention is that the purported debate—between those who embrace (only) causally plausible designs and those who eschew them as unhelpful—is confused. That is, everyone is doing IBE, though their focus may on different parts of it. If the analyst takes the position that what matters (most) is causal identification over generating explanations, they are focused on the *quality of inference* in IBE. That is, whatever the—potentially very narrow— set of explanations, they want to believe that the inference they make is the correct one. By contrast, scholars like Huber can be seen as emphasizing the importance of *quality of explanation* in IBE. That is, they want the data and models to tell us about the relative plausibility of many explanations, or actively develop new explanations. And they want to do this, even if that same machinery cannot do a particularly good job of determining which one of those explanations is 'best'. Notice that this is *not* a question of "external" versus "internal" validity (in the sense of, e.g. Morton and Williams, 2010; McDermott, 2011): the problem is not that one is sacrificing generalizability for (local) credible estimation of causal effects. The issue instead is the preferred tradeoff between inference and building explanations. This leads to Implication 2.

> **Implication 2** *A study can focus on the "I" aspect of IBE, or the "E" element, or some combination of the two. We should not judge studies as good or bad simply as a product of that choice.*

If the primary concern is tests of extant theory, then quality of inference must be the priority. But this will not help with theoretical progress *per se*: it is trivially true that an infinite number of theories is consistent with a particular (well-identified) causal effect. Relatedly, focusing on quality of explanation is not *a priori* wrong. But if the tests of that explanation are weak in the sense described by Samii (see, also, Aronow and Samii, 2016), IBE may not lead to the correct inference.

A different and more positive way to express our sentiments here is to consider the value of a regression study for which a relevant treatment effect is not well-defined. An example might be Fearon and Laitin (2003) above, but there are many of similar nature. We would contend that this regression—which by extension most quantitative observational work done in the discipline until very recently—is not deleterious to our general efforts unless one of two conditions holds. First, that it leads the reader to the *wrong* inference about which explanation is best. Second, that it suggests that a new *wrong* explanation should be included in the set of explanations from which a best one will hopefully emerge. If one wishes to critique a study, being as explicit as possible about which of these applies, and how, leads to a more useful dialogue.

> **Implication 3** *A lack of a causally identified parameter is not a problem per se, as long as it provides evidence which helps to adjudicate between explanations. Evaluate studies in explicitly IBE terms.*

The centrality of explanation does not mean there are no standards for such entities. On the contrary, ruling out explanations by showing they are inconsistent with available evidence is only compelling if the candidate explanations are themselves compelling. Rejecting strawmen is unhelpful: the explanations must also be sufficiently elaborate that they can imply observable differences in the world. Thus Implication 4.

> **Implication 4** *Rejecting strong explanations is more informative than ex ante implausible ones (as often found with null hypotheses). Hypotheses—even those which are rejected—should map to compelling, elaborated explanations of the phenomenon under study.*

A hypothesis implies an explanation for a given scientific phenomenon. For us to do IBE from a regression, that hypothesis should be testable from that regression. Unfortunately, it is not uncommon to see lists of hypotheses that are not constructed in a way that implies

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a particular test in the ensuing regression. Or worse, researchers write out hypotheses that involve no IBE explanatory logic at all. That is, vague statements of expectations that do not help us distinguish between the plausibility of the elements of a set of explanations (including the special case of one explanation versus the null hypothesis), at all. This should be avoided.

If the hypothesis being tested are compelling, then ruling explanations *out* is as useful as ruling them in. For example, it is helpful to know that an explanation previously thought to be "best" is in fact weaker than believed. Or that none of the usual explanations for this sort of case (say, a rich country that is not a democracy) do not work well. Specifically, this is knowledge of value to a field *as a whole*—beyond a particular study. And it is at that more aggregated level that developing explanations and assessing the evidence for them should take place. This yields Implication 5.

> **Implication 5** *IBE is the process of ruling out alternative explanations. Report null or negative results even for preferred explanations, so that the community understands when there is an absence of evidence or conflicting evidence.*

Of course, researchers have long noted that null results are underreported (e.g. Franco, Malhotra and Simonovits, 2014). When this is combined with author and journal selection effects such as the "file drawer problem" (in the sense of Rosenthal, 1979) publication bias may be inevitable (Dickersin, 1990). Our point here is that IBE—which is the primary mode of inference in social science—gives a new and specific impetus to improving practice on this matter.

4.3 Exploration Matters

Explanations are developed from exploration. In an instructive analogy, Tukey (1977, 1) notes that the data analyst should be a "detective" attempting to fact find, before—in a

separate stage–the jury system makes a ruling. This judicial role is where "Confirmatory Data Analysis" (CDA) steps in. Since CDA involves hypothesis *testing*, it is different to exploratory data analysis (EDA), which involves

> a focus on tentative model building and *hypothesis generation* in an iterative
> process of model specification, residual analysis, and model respecification

in the words of Behrens (1997, 132, emphasis added).

But in much modern political science work, the EDA (hypothesis generation) is not kept separate to CDA (hypothesis testing). The result is that researchers often try to do both and end up doing neither. Thus we see statements of hypotheses in papers that do not comport with either Fisher or Neyman-Pearson decision theory. That is, there is no explicit null hypothesis, nor is there discussion of a test statistic; nor ultimately, is there a mutually exclusive decision to reject or fail to reject the null.[7] Instead, scholars write of statistical evidence "partially consistent" with a hypothesis (Thompson, 1975, 474); or they talk of hypotheses which are "strongly supported" by the data (Lau, 1985, 130).[8] This gives rise to Implication 6.

Implication 6 *Exploratory Data Analysis (generating explanations) should be valued per se and kept separate from Confirmatory Data Analysis (testing explanations). Be explicit when doing exploration.*

We are not the first to explicitly link the idea of EDA to Peirce's original (1878) work on abductive inference. Behrens and Yu (2003, 40), for example, note that after EDA we should " "abduct" only those [hypotheses] that are more plausible for subsequent confirmatory experimentation." Though not motivated by IBE itself, one tool that may encourage

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[7]See Mayo (1996, Ch 12) for a nuanced discussion on the historical relationship between Peirce's work and Neyman-Pearson statistics.

[8]We use older studies here, because our goal is not to critique specific scholars, but rather to give examples of the types of language that can be found in many modern works.

such practices is pre-analysis plans; these limit the extent to which researchers can add explanations after performing the (confirmatory) analysis stage (itself coming after the EDA).

By valuing exploration, and encouraging the separation of EDA from CDA, IBE deincentivizes other problematic practices. These include Hypothesizing After the Results are Known (HARKing), defined as "presenting a post hoc hypothesis in the introduction of a research report *as if* it were an a priori hypothesis" (emphasis added) (Kerr, 1998, 197). The idea is that authors run several (perhaps many) different analyses involving statistical tests, and then write hypotheses consistent with the (strongest) results they find. Given that hypotheses should be first derived from theory, and thus offer a way to test the implications of that theory, that HARKing reverses this basic process is immediately concerning. And indeed, the problems with this kind of interpretative overfitting have become well known. They include specific issues, such as a general tendency for Type-I errors ("false positives") to be presented as findings (Ioannidis, 2005). Related, researchers may overfit in a purely statistical sense by "p-hacking"—trying different model and data specifications in a deliberate attempt to reach a particular level of significance (Simmons, Nelson and Simonsohn, 2011). Or they might do such things less consciously, but ultimately resulting in the same problem of findings that cannot be replicated (Gelman and Loken, 2014).

HARKing occurs because scholars explore the data and then pick a particular explanation to (superficially) "test" via a hypothesis. But under IBE, this subterfuge is pointless. If analysts want to explore data to suggest new explanations, they should just do this without artificially stating hypotheses *as if* testing a pre-existing theory. And, for all the usual reasons, those explanations should not be tested with the data used to generate them. But that is a separate matter. Second, IBE suggests that description is *per se* important. In the medical example we gave above, the physician needed symptom data to make a diagnosis. So, anything that devalues "mere" description—and the incentives behind HARKing certainly do—is anathematic to IBE and the scientific approach that embodies.

A special case of these problems—and a solution that is provided by a proper consideration of IBE—is presented by much "text as data" work. There, scholars will use unsupervised techniques—including topic models (Quinn et al., 2010; Roberts et al., 2014, e.g.)—designed for summarizing and organizing a corpus. Yet researchers routinely use the output of these models to make statements about the plausibility of various theories. The issue here is not simply about the availability of forking paths arising from decisions one must make in fitting the models (see, e.g., Denny and Spirling, 2018). It is that, fundamentally, unsupervised techniques are primarily methods of discovery which can only be repurposed as tools of measurement with substantial care and validation (Grimmer and Stewart, 2013; Grimmer, Roberts and Stewart, 2022). The danger is what we term "PEACHing": [P]resenting [E]xplorations [A]s [C]onfirmable [H]ypotheses. This is, in a sense, the opposite of HARKing. In HARKing, techniques (like regression) that allow for hypothesis tests are used to assess a large number of possible hypotheses, and those with suitable p-values presented as if *a priori* theorized. In PEACHing, techniques that do *not* naturally facilitate hypotheses tests are used to suggest hypotheses that cannot be "tested" at all, at least on that data and with that approach (though see, e.g., Egami et al., 2018, on testing with an explicit heldout sample). What IBE makes clear is that these approaches can be used to help suggest new explanations, but the role of explanation generation must be carefully separated from the testing of those explanations. These insights yield Implication 7.

> **Implication 7** *Post hoc hypotheses do not provide the same inferential value because the explanation is built to fit the available facts and thus cannot be effectively distinguished from other explanations with that same set of facts. Do not engage in HARKing or PEACHing.*

An additional interesting consequence of valuing exploration is that it recasts some supposed contrasts between quantitative and qualitative methods. In that literature, there have been considerable recent efforts to use mixed methods for *inference* (e.g. Glynn and Quinn,

2011; Humphreys and Jacobs, 2015). Such attempts are not uncontroversial, in the sense that some scholars of quantitative (e.g. Beck, 2006) and qualitative (e.g. Collier, Brady and Seawright, 2004) approaches argue those techniques can and should be used for fundamentally different purposes. But from an IBE perspective, where the goal might be to generate new explanations, it is not clear that a distinction between "within-case" rather than "between-case" variation makes much difference for the problem at hand. That is, even if one asserts that the (causal) *inferences* possible with different methods vary, it is not obvious that one is obviously preferable to the other for the initial component of abductive inference. Implication 8 makes this point.

Implication 8 *Description aids exploration, and this may usefully be quantitative or qualitative in nature. Explicitly acknowledge when generating new explanations.*

4.4   Open Problems in IBE

As we hinted at above, exactly how one *does* IBE is often unclear (see, e.g., Van Fraassen, 1989). To ameliorate this, some scholars have drawn links between IBE and potentially more rigorous frameworks like Bayesianism (Douven, 2013; Schupbach, 2017; Henderson, 2020). But ambiguity persists. To see why, consider two separate studies that examine the same phenomenon, but come to different conclusions as to that phenomenon's ("best") explanation. This could include, say, culture versus institutions as the cause of some political outcome like the effective number of parties in a system (see e.g. Neto and Cox, 1997). But it would also extend to situations in which some scholars assert a given (say, positive) causal effect of a treatment from data (e.g. Kroenig, 2013), while others find the opposite (negative) effect or no effect at all (e.g. Sechser and Fuhrmann, 2013).

What does IBE have to say about which of these (hypothetical) studies is "better"? To answer this, recall that abductive inference has only two places from which a judgement may proceed. First, in terms of the nature of the *explanations* themselves. But exactly

what properties are desirable, and how they should be traded off is unclear. For example, *parsimony* might be preferable for some scholars but not others (relative to, say, *generality*). Second, studies can differ in the way that they move from data to explanation: that is, for a fixed set of explanations, the purported inference to the "best" one is of higher or lower quality. In practice here, there is presumably some notion of "plausibility" or "credibility" (in the sense of e.g. Angrist and Pischke, 2010). But the requisite standards are not obvious and have not been constant over time.

5   Discussion

The credibility revolution has been a "catastrophic success"; it has so profoundly changed the practice of social science, that it has created new problems in its wake. In particular, scholars have claimed that it is crowding out questions, and approaches to those questions, that also deserve attention. Consequently, the argument goes, the discipline is all the poorer. We agree that we see more attention to ensuring claims of causal effects are plausible, but this isn't the full story. Our argument above was that both improving inference and improving explanations have a natural role in political science. A given researcher or research agenda may value one aspect over another, but they are not necessarily in direct competition. Furthermore, the abductive inference mode in which they must both simultaneously exist as crucial elements—known as *Inference to the Best Explanation*—is both ubiquitous and of long-standing. We argued that this was true of essentially all empirical set ups: from observational regressions where conditional ignorability may or may not be plausible, to randomized lab experiments where it is generally agreed that a treatment effect has been identified.

Understanding that almost all such research is some variant of IBE allows for a common framework of discussion across the discipline. But it does more than that. By clarifying the

importance of explanations—their development and their plausibility—IBE ameliorates a series of debates in political science. We argued that if explanation matters, then exploring and describing data becomes *per se* valuable. And if that is true, there is no reason to misrepresent ('causal') hypotheses as if they occurred to the reader after running the relevant regression: the incentive to "p-hack", or to hide null results away, is reduced. More generally, we argued that recognizing the centrality of IBE means avoiding unhelpful and uncharitable comments about whether a given research question is "big" or "small"—and thus worthy of answering (or not), and with different degrees of precision. Our position is that these arguments are in fact mostly displays of researchers' preferences—over what they value in terms of the components of the IBE framework. If these preferences are made explicit, progress is more likely.

In short then, the answer to "what good is a regression?" depends on what you want from it. There are reasonable answers that arise from the same philosophical framework being used (implicitly) by those that particularly promote causal inferences. This does not mean anything goes. Just as an example, IBE for a given problem is not served by including controls that make most sense if the goal was the estimation of a causal effect yet asserting (caveating) that the regression results should not be interpreted causally.

We sought to make the basic point that the fact that a regression coefficient does not have a plausibly causal interpretation—or that it does, but not to the standard required by a given reader—is not, and cannot be, a reason to say that the regression is *per se* unhelpful. Given this focus, we have not given a deep philosophical treatment or resolved all issues. This is a problem, because IBE has no immediate solution for many concerns that are required for working across the discipline. For instance, as others have noted, it is not universally clear what makes for a "best" explanation. Nor is it clear how explanations ought to be derived, or how different they need to be from one another in practice. We leave such tasks for future work.

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