Research Registries: Taking Stock and Looking Forward

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Abstract: We analyze one prominent policy solution to the credibility crisis in experimental research—research registries—with a focus on the AEA registry. We find that a majority of economics field experiments do not register, only half of registrations are prior to intervention, and most of these preregistrations lack sufficient detail. We compare these facts to ClinicalTrials.gov and find broad similarities. As such, we conclude by advancing an economic model to explore potential improvements to registries generally. The model shows that banning late registration and linking Institutional Review Board applications to registrations can significantly increase registry effectiveness.

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1 Introduction

"There is a property common to almost all the moral sciences, and by which they are distinguished from many of the physical... that it is seldom in our power to make experiments in them."
Mill (1836)

An immutable fact among economists is that there is rarely broad agreement about an issue of positive and normative import. Interestingly, one area where economists had seemingly agreed was on how empiricism can be used to learn about the world. Whether it was John Stuart Mill in 1836, Milton Friedman in 1953, Joan Robinson in 1977, or William Nordhaus and Paul Samuelson in 1985, the tenor was that, unlike chemists, physicists, and biologists, economists do not have the luxury of data generation via controlled experiments and therefore must rely on experiments that happen to occur (Friedman (1953), Robinson (1977), and Samuelson and Nordhaus (1985)). The general feelings of these icons were widely shared throughout the 19th and 20th centuries.

The last several decades, however, have brought a significant change in the empirical landscape in economics. While the use of historical evidence remains invaluable, new approaches to generate data in the lab and field have opened up several unique lines of research into the "whys" behind observed behaviors (see Harrison and List (2004)). While the experimental approach has helped to clarify identification, control, statistical inference, and interpretability, critics in the broader social sciences have recently called for the experimental movement to proceed more cautiously. Indeed, an active debate has emerged over claims that the experimental approach faces a "credibility crisis" (see Jennions and Møller (2003), Ioannidis (2005), Nosek, Spies and Motyl (2012), Bettis (2012), Maniadis, Tufano and List (2014), and Dreber et al. (2015)).

The debate has evolved into several lines of inquiry, but the thread connecting them relates to false positives, external validity, and lack of replication generally. This charge follows from the fact that data are ultimately finite, so that researchers must choose which hypotheses to test, report, and trumpet in a system where publication incentives imply that not all results are equally likely to get published. Economists, along with researchers in other empirical disciplines, have recognized that these limitations could lead to a departure from socially optimal experimental conduct. By now, a growing literature has developed that provides meta-analyses and policy prescriptions aimed at

improving the social usefulness and credibility of the experimental approach (Young (2018), Vivalt (2018), Andrews and Kasy (2019), Meager (2019), and Vivalt (Forthcoming) for meta-analyses, and see Glaeser (2008), Coffman, Niederle and Wilson (2017), Christensen and Miguel (2018), Al-Ubaydli, List and Suskind (2019), Dufwenberg and Martinsson (2019), McCloskey and Michaillat (2020), and Abadie (Forthcoming) for policy prescriptions).

This paper conducts an empirical and theoretical examination of one such policy prescription—the establishment of the American Economic Association's registry for randomized controlled trials (the AEA RCT Registry) in May 2013. This registry provides a venue for researchers to document their experiments in a manner that is searchable by external audiences. In principle, if used appropriately, the registry can tackle key issues in the credibility crisis. Our empirical work begins by evaluating the extent to which the registry has been successful in combating two issues that have received particular attention:

- *The file drawer problem*, namely that not all experimental results are published and are therefore relegated to the "file drawer."
- *Scope for p-hacking and results manipulation*, namely that researchers often make ex-post decisions adaptively in a manner that is not accounted for in the empirical analyses.[1]

The registry can address the file drawer problem to the extent that it provides searchable records of all economics RCTs started and their outcomes. The registry can limit the scope for p-hacking to the extent that researchers pre-specify their experimental design before trial commencement.[2]

Though still relatively new, the AEA RCT Registry is the most commonly used registration database in economics (see Section 4.2).[3] The largest research registry overall is ClinicalTrials.gov, which is maintained by the National Institutes of Health and contains 302,850 medical trial regis-

[1]The extent to which results of empirical studies are manipulated in practice has been studied by Brodeur et al. (2016), Brodeur, Cook and Hayes (2020), and Vivalt (2018).

[2]The file drawer and p-hacking problems apply to empirical research generally, but existing registries mainly focus on RCTs. One reason is that RCTs are low hanging fruit——each RCT is ostensibly designed to test a small set of interventions and has an explicit start and end date. A notable exception is the Open Science Framework (OSF) Registries Network, which permits the registration of observational studies. Other web services, such as AsPredicted, also facilitate recording any research hypothesis. However, unlike a registry, AsPredicted does not provide a way to search the recorded hypotheses.

[3]For a discussion of the role of the AEA registry in terms of promoting transparency see Christensen and Miguel (2018).

trations from 208 countries as of April 1, 2019.[4] A large literature (reviewed in Appendix C) has assessed the mixed effectiveness of ClinicalTrials.gov. To the best of our knowledge, we are the first to provide a systematic assessment of the AEA RCT Registry.

We make two specific contributions. First, we evaluate whether the AEA RCT Registry has been effective, for economics field experiments, at solving the file drawer problem and limiting the scope for p-hacking.[5] Second, we suggest alternative registry designs that might improve outcomes compared to the status quo. Our theoretical analysis focuses on one concrete design issue, namely that the registry accommodates *late registration*.[6] While typical motivations for promoting registration rely upon the assumption that it is done prior to experimentation,[7] the AEA RCT Registry permits the registration of completed trials.[8]

Unfortunately, we find little evidence that the AEA RCT Registry is sufficiently addressing either the file drawer or p-hacking problems for economics field experiments. A theme that emerges from our analysis is that the social norm of registration appears rather limited. Many trials fail to register and those that do register often fail to provide a detailed description of their experimental design beyond the mandatory registration requirements. Insofar as these requirements are fairly weak (which was a deliberate choice in order to encourage participation and help *establish* a norm for registration), this unfortunately implies that the impact of registration on credibility is fairly weak as well.

Regarding the file drawer problem, while the universe of started field economics experiments is unobserved, we are able to perform a census of papers conducting field experiments published in select economics journals and working paper series. Roughly 50% of the field experiments

[4] In contrast, the AEA RCT Registry lists 2,444 studies located in 133 countries as of April 1, 2019 (although the reader should bear in mind that the AEA RCT Registry is much newer than ClinicalTrials.gov).

[5] A previous version of this paper considered both field and lab experiments. However, as we explain in Section 2, the AEA RCT Registry is primarily targeted at the registration of field experiments, making it more difficult to interpret any success or failure in the registration of lab experiments. Our focus on field experiments is to prevent this distinction from interfering with the interpretation of our conclusions.

[6] The AEA RCT Registry chose to allow late registration primarily to facilitate the registration of RCTs that started prior to the registry's establishment. Our understanding is that there was no plan to revisit this design choice at a later date.

[7] For instance, because researchers may be more likely to "relegate an experimental finding to the file drawer" if the results are negative. If researchers do not attempt to publish experiments with negative results, then they may not have sufficient incentives to distribute results ex-post.

[8] ClinicalTrials.gov also allows late registration although several categories of experiments are required to preregister by law. As far as we know, no existing laws require either the registration or preregistration of economics experiments.

published in top journals (both general interest and field) in economics—between 2017 and the end of 2019 Q2—are registered. We note that this is a selected sample which likely overstates the overall fraction of studies registered.[9] Perhaps more telling is that only 20% of working papers on field experiments in the New Economics Papers Report on Experimental Economics (NEP-EXP) are registered. Overall, 44% of the field experiments in our sample registered, which likely overstates the prevalence of registration given our focus on top venues. On the same theme, we also show that the AEA RCT Registry is not currently effective at capturing RCT outcomes. Only one-third of registered trials follow-up with any outcome data as of April 1, 2019.[10]

We similarly find little evidence that the AEA RCT Registry is succeeding at limiting the scope for p-hacking. On the one hand, the vast majority of registered trials (roughly 90%) do not provide a pre-analysis plan. On the other hand, the registration information that is provided is often not specific enough to tie researchers to *one* experimental design. We make this determination via an assessment of the primary outcomes reported by 300 randomly chosen preregistrations. We had one set of RAs assess the specificity of the primary outcomes reported by each preregistration. We then had a second set of RAs label each preregistration as either a lab or field experiment according to the definitions in Harrison and List (2004).[11]

We find that 241 of the 300 preregistrations are for field experiments. Focusing on this subset, the average preregistration reports just over 3 primary outcomes, but even the most detailed of these outcomes fails to specify either a specific variable construction or measurement timeframe. As we discuss more patiently below, even the most restrictive outcomes are similar to "number of fruits each experimental subject consumes" rather than to "number of apples each experimental subject consumes in March, 2019." We are able to match working papers to 100 of these pre-registrations.[12] Comparing these working papers to the preregistrations, we find that researchers change the construction of a primary outcome 10% of the time (e.g. report a number of vegetables consumed rather than a number of fruits consumed) and add a primary outcome 46% of the time

[9] For instance, studies may register late in order to be considered by AEA Journals, and, in fact, we find this is the case for most papers published in these journals as we discuss in Section 3.2.

[10] This issue is not unique to economics. As we discuss in Appendix C, ClinicalTrials.gov also faces problems with capturing outcome data.

[11] The AEA RCT Registry does not collect data on whether a given RCT is a field experiment or lab experiment.

[12] There are no associated published papers.

(i.e. highlight an unregistered variable in their abstract, introduction, or conclusion).

Assessments of ClinicalTrials.gov provide a useful benchmark for our results on the AEA RCT Registry and an indication of whether these patterns hold more generally. We extend the existing literature by examining the restrictiveness and fidelity of primary outcomes reported by 300 randomly chosen preregistrations from the first five years of ClinicalTrials.gov. We find that the ClinicalTrials.gov preregistrations are only slightly more restrictive than the AEA RCT Registry preregistrations. We also find that papers associated with the ClinicalTrials.gov preregistrations and the AEA RCT Registry preregistrations have similar fidelity to the registered primary outcomes. This result combined with the literature review in Appendix C suggests that if ClinicalTrials.gov gives a sign of where the AEA RCT Registry is headed, then there is little reason to be optimistic that the current approach will significantly dent the credibility crisis in economics.

In an effort to understand these data patterns and provide guidance moving forward, we next construct a simple model of registration.[13] The model features a researcher endowed with an experiment on an underlying hypothesis and an outside "consumer" of research. The researcher first chooses whether to preregister and conduct the experiment. The researcher then chooses whether to register late and receives a payoff based on the outsider's updated belief about the underlying hypothesis. Preregistration allows researchers to signal confidence in their hypotheses, for instance due to strong intuition based on prior work or domain expertise. But late registration is tempting due to option value—there is a chance that registration is not worth it ex-post.

The value of the model is twofold. First, we are able to formally scrutinize how registration influences the decision to experiment, particularly for researchers on the margin.[14] Second, we provide comparative statics that help to determine how counterfactual policies influence registration decisions. In particular, the model allows us to speak to the implications of banning late registration. One might conjecture that allowing late registration could only increase the number of registered trials by providing researchers more opportunities to register. To the contrary, our

[13] A number of recent theoretical models seek to capture researcher incentives in order to speak to optimal design and conduct of experimentation. However, we are not aware of any models that speak to registration. For examples and further discussions of this growing literature, see Di Tillio, Ottaviani and Sorenson (2019), Libgober (2020), Al-Ubaydli, List and Suskind (2019), Tetenov (2016), and Anderson and Magruder (2017).

[14] For instance, we highlight that incentivizing registration may discourage experimentation by raising the bar for a study's results to appear significant, reflecting similar issues raised by Duflo et al. (2020).

model highlights that late registration is partially *at the expense of* preregistration. As a result, a ban may actually increase registrations *overall*. We use a calibration exercise to argue that this insight is empirically relevant for the AEA RCT Registry. Given parameter values that match current registration rates, we show that banning late registration can strictly increase registrations. This suggests taking seriously the idea that banning late registration could effectively address both the file drawer problem and p-hacking.

So where do we go from here? Our first recommendation is to explore prohibiting late registration, while simultaneously providing incentives for researchers to preregister their work (such as mandating preregistration as a condition for publication). Insofar as the ultimate goal of the registry is to maximize preregistration, this dual approach can move us in that direction. Yet this approach does not solve two other issues that our analysis highlights: (1) current registrations lack the specificity needed to limit the scope for p-hacking and (2) registration costs can reduce experimentation. Our second recommendation tackles these issues. Since RCTs require Institutional Review Board (IRB) approval, we propose having researchers submit their IRB materials directly to the registry. While IRB materials are admittedly heterogeneous across schools, in our experience they contain enough uniformity and detail to provide a check on p-hacking. Further, this policy avoids large additional costs since researchers can simply upload the IRB forms that they have already completed.

The remainder of our paper is organized as follows. Section 2 provides background information on the AEA RCT Registry—readers already familiar with the registry should feel free to skip this section. Section 3 presents our empirical assessment of whether the registry is currently solving the file drawer and p-hacking problems for economics field experiments. Section 4 compares the AEA RCT Registry to ClinicalTrials.gov and discusses other registration venues as well. Section 5 presents our model of a researcher's registration decision. Section 6 concludes. All tables, figures, and proofs are in the respective appendices.

2 Background

Research registries emerged as a way to address the unfortunate reality that a large number of research results are never reported. In particular, academic journals tend to selectively publish studies that reject a null hypothesis to the exclusion of studies that confirm a null hypothesis or provide inconclusive results. Robert Rosenthal coined the term the file drawer problem in 1979 to describe the bias this selection introduces into the scientific literature.[15] This selection also directly gives researchers an incentive to repeatedly re-choose their data, outcome variables, and analysis method until they are able to reject the null hypothesis of interest at conventional levels of statistical significance. The process of repeatedly re-choosing data, outcome variables, and analysis method is commonly referred to as p-hacking. Together, these two effects can undermine public trust in empirical research and cause inefficient resource allocations.

The AEA launched the AEA RCT Registry in May 2013 to capture ongoing, completed, and terminated RCTs in economics and other social sciences (see About the Registry on the AEA registry webpage).[16] At the time, existing registries, such as ClinicalTrials.gov, focused on medical trials.[17] The AEA chose to implement a streamlined registration process to encourage participation—registration only requires answering a few questions and researchers are able to register at any time *even after the RCT is completed*. The required questions ask for a title, short abstract, start date, primary outcomes, treatment arms, and IRB approval number.[18] The AEA decided to focus on RCTs since experiments have a fairly distinct beginning and end. While one could imagine allowing non-RCT projects to register as well, it is difficult to implement a credible registration approach

[15]For example, consider 100 researchers who each conduct an experiment to test the null hypothesis that some parameter is less than or equal to 0 against the alternative that the parameter is greater than 0. At least 5 of the researchers are likely to find that the parameter is greater than 0 at a 5% significance level. If journals only publish significant results, then only these 5 studies will be published. Seeing 5 out of 5 studies rejecting the null, outside researchers might incorrectly conclude that there is strong evidence that the parameter is greater than 0.

[16]We are indebted to Rachel Glennerster for providing context about the registry's creation.

[17]The International Initiative for Impact Evaluation (3ie) Registry, which focuses on experiments in developing countries, and the Evidence in Governance and Politics (EGAP) Registry, which focuses on political science experiments, were launched contemporaneously.

[18]Many RCTs in economics require IRB approval, but the IRB approvals are not made publicly available. A policy that either made external registration a condition for IRB approval or made IRB approvals public would directly help solve the file draw problem. Informed by our model, we also argue in the conclusion that requiring researchers to upload their IRB materials during registration could significantly improve the registry's ability to attenuate p-hacking at little cost.

for research on pre-existing data.[19]

Note that a registration is distinct from a pre-analysis plan though in our experience they are often conflated. A registration is essentially metadata and a list of primary outcomes and treatment arms. In contrast, Duflo et al. (2020) propose that a pre-analysis plan should answer two detailed questions: “What are the key outcomes and analyses?" and "What is the planned regression framework or statistical test for those outcomes?" Put another way, a pre-analysis plan goes beyond a registration by specifying (1) a set of primary analyses and (2) the content of those analyses. A more detailed pre-analysis plan may go even further and specify *all* steps involved in analyzing the data. Of note, researchers have the ability to upload a pre-analysis plan as part of their registration (see Ofosu and Posner (2019) for an analysis of the pre-analysis plans that have been added to the AEA RCT Registry).

Though not explicitly stated on the website, the AEA RCT Registry is primarily focused on capturing economics field experiments. While many lab experiments have chosen to register, some of the registry questions, e.g. questions on subject randomization, are less natural for certain lab experiments. In the same spirit, the AEA journals require that field experiments, but not necessarily lab experiments, be registered as a condition for publication.[20] In any case, no economics journal requires that any experiment *pre*register—instead allowing registration to be done at the time of submission.[21] In contrast, most medical journals require preregistration of clinical trials.

The timing of an AEA RCT Registry registration can be determined from its listing in the registry database. All RCTs are listed side-by-side with the preregistered trials marked by a small orange clock in the upper left corner of the trial entry. Trials that registered after data collection began are instead marked by a grey clock (see Figure I). That said, it is not clear to us whether this distinction is salient or appreciated by consumers of research (or referees and editors). Unfortunately, we are not able to precisely study the extent to which the time of registration is dis-

[19] See Burlig (2018) for a more thorough discussion of issues related to the registration of and pre-analysis plans for non-RCT empirical studies.

[20] The specific policy is “As of January 2018, registration in the RCT registry is mandatory for all applicable submissions. This applies to field experiments. Laboratory experiments do not need to be registered at this time.”

[21] The official policy states, emphasis added, “If the research in your paper involves an RCT, please register (registration is free), prior to submitting. We also kindly ask you to acknowledge compliance by including your RCT ID number in the introductory footnote of your manuscript. **Registration ideally happens before the project launches, but registering at the time of submission is also acceptable.**”

tinguishable to someone who searches the registry. Our own conjecture is that the distinction is minor,[22] though researchers may emphasize that a study was preregistered in the corresponding written paper.

Finally, a few other aspects of the AEA RCT Registry prove important in practice. First, it is possible to update a registration after it is initially submitted—although, as we document below, this is rarely done. Second, the registry sends automatic reminders to encourage researchers to complete fields that become relevant during and after the RCT. For example, after the trial has concluded, researchers are asked to link to any data, program files, or results that they have made public. Third, researchers are able to hide several fields in the registration from public view until later dates (specifically, the trial's location, intervention description, experimental design, names of any sponsors or partners, and supporting documents). On this last point, we stress that, as is, allowing these fields to be temporarily hidden does not eliminate the possibility that registering could invalidate an RCT's experimental design. One oversight is that the registry does not permit hiding the researchers' names, experiment title, and start date. This oversight is problematic for any RCT where identification relies on the intervention's occurrence being undisclosed to participants in the control and/or treatment arms.[23]

3 Analysis of AEA RCT Registry

We start with an empirical examination of the AEA RCT Registry. Specifically, we examine the extent to which the registry is currently capturing the universe of economics RCTs and the extent to which it succeeds in pre-committing researchers to assessing a specific set of outcome variables. We consider the AEA RCT Registry from its launch on May 15, 2013 up through April 5, 2019. The AEA RCT Registry is primarily targeted at the registration of field experiments, making it more difficult to interpret any success or failure in the registration of lab experiments. As such, we focus our analysis on field experiments when possible in order to prevent this distinction from

[22] Anecdotally, despite our own familiarity with the registry, we never realized these clock icons existed until starting this project. Likewise, we informally discussed this paper with several colleagues and most were not aware of this distinction prior to our informing them.

[23] This issue was raised to us anonymously after we first circulated this paper. A specific concern is that the public might inadvertently discover the RCT through web searches for the researchers' names and historical paper titles.

interfering with the interpretation of our conclusions (see Harrison and List (2004) for definitions of the various experiment types).

3.1 File Drawer Problem

We first assess whether the AEA RCT Registry is effective at solving the file drawer problem for field experiments. Informally, a registry can address the file drawer problem to the extent that

1. Every field experiment that is started is added to the registry
2. Experiment results are added to the registry at the conclusion of the experiment

Because the universe of started field experiments is unknown, we cannot determine the fraction of RCTs that register with accuracy.[24] As such, we start by examining the registration rate for field experiments published in leading economics journals and working paper series. Table I presents the registration rates for RCTs appearing in the following outlets over 2017, 2018, and the first two quarters of 2019:

- American Economic Review (AER)
- American Economic Journal: Microeconomics (AEJ-Mic)
- American Economic Journal: Applied Economics (AEJ-AE)
- American Economic Journal: Economic Policy (AEJ-EP)
- Journal of Political Economy (JPE)
- Quarterly Journal of Economics (QJE)
- Review of Economic Studies (ReStud)
- Journal of Development Economics (JDE)
- Experimental Economics (EE)
- New Economics Papers Report on Experimental Economics (NEP-EXP)

We find that field experiment registration rates across journals are heterogeneous and overall quite low. The AER, QJE, and AEJ-AE have the highest field experiment registration rates—75%, 69%, and 62% respectively—over this period. The remaining journals have registration rates below 60%—with only 14% of the field experiments published in the JPE registering. Of note, the AEA journals require that field experiments be registered as a condition for publication as of January 2018. Table II investigates the effectiveness of this requirement by reporting the registration rates for field experiments by journal and year. Despite the requirement, we find a registration rate of

[24] As mentioned above, while IRB approvals could conceivably be used to determine this, they are not publicly available or searchable.

only 63% for field experiments published in the AEA journals considered here over 2018 and the first two quarters of 2019. This could be due to ambiguity in the designation of a given RCT as a field or lab experiment. Also of note, we find only *two* registrations of lab experiments across our sample—a stark contrast with the sizable number of field experiment registrations we observe. This result suggests that it is not a norm within economics to register lab experiments.

The second step in addressing the file draw problem is reporting outcomes. The registry data speaks immediately to whether outcomes are added to the registry at the conclusion of the RCT. Unfortunately, the AEA RCT Registry does not collect data on whether a given RCT is a field experiment or lab experiment. As such, we examine outcome reporting across all registered trials. We find that few registered trials add their outcomes. Of the 1,654 registered trials that ended before December 31, 2018, only 21% provided preliminary results or a link to a working paper by April 1, 2019. In fact, only 32% provided *any* follow-up information about the trial, e.g. intervention completion date, final number of observations, and whether there is public data available. This result is not driven by the short horizon. Of the 1,210 trials that ended before December 31, 2017, only 28% provide preliminary results or a link to a working paper and only 41% provide any follow-up information by April 1, 2019.

3.1.1 Late Registration

The AEA RCT Registry allows researchers to register RCTs even after completion. Allowing late registration might help solve the file draw problem by facilitating more registrations. Here it is not per se important that the trial is registered immediately, just that it is registered. That said, late registration can also incentivize researchers to not register, insofar as they may attempt to delay registration and subsequently neglect to do so if not seeking to publish the study. This point is made more formally via our model, which highlights that allowing late registration can come at the cost of diminishing preregistration.

In practice, it is not generally possible to tell if a given trial was registered late because the researcher did not know the registry existed or if the researcher purposely waited to register the trial.[25] Should the first case dominate, then allowing late registration helps to establish a census of

[25]For example, consider an unregistered project that a researcher is about to submit to the journal. There are at least

trials. However, should the second case dominate, then allowing late registration may exacerbate the file draw problem.

Fortunately, we are able to partially disentangle these two possibilities for the subset of researchers who register multiple trials over time. If a researcher registers her first trial late and then preregisters all of her future trials (i.e. those started after the first registration), then the late registration was likely due to not knowing about the registry. In contrast, if a researcher is repeatedly late in registering trials started at future dates, then the researcher is likely registering late on purpose.[26] Table III displays all registrations made by three primary investigators. Each primary investigator here registered their first trial in 2014, proceeded to register multiple new trials (started after 2014) late, and registered their most recent trial over a year after the intervention began.

To investigate whether researchers purposely register late, we consider the subset of 1,209 distinct primary investigators who register a trial with a start date after January 1, 2014—note that the registry itself opened in May 2013 and that it is infeasible to subset this analysis to field experiments because the registry does not collect data on whether a given trial is a field or lab experiment. To be further conservative, we only consider a registration as late if it occurred more than a week after the intervention began. 319 of the 1,209 primary investigators registered multiple trials. Of interest are the 231 primary investigators who registered at least one of their multiple trials late. 98 of these researchers registered multiple trials late at dates more than a quarter apart. This combination of observations suggests that many researchers register late on purpose. As such, we pay special attention to late registration as a model feature in Section 5.

3.2 P-Hacking

We now assess whether the AEA RCT Registry is effective at attenuating p-hacking. Informally, a registry can reduce p-hacking to the extent that

1. RCTs register before the intervention begins, i.e. preregister

two ways that this project enters the registry. First, the researcher may be unaware of the registry. On submission, the researcher learns of the registry from a referee and chooses to register. Second, the researcher may be aware of the registry. Before submission, the researcher decides to register and so is able to report that the paper was registered, which might be a signal of quality to the journal, or required by the journal (as it is for the AEA journals).

[26] At some point it becomes untenable to conclude that the researcher is just disorganized.

2. Registrations fully specify at least the primary outcome variables
3. Published or working papers on the RCTs match the registrations

We examine each of these issues in turn.

3.2.1 Preregistration

We start by examining the fraction of RCTs that preregister. As the registry does not collect data on whether a given trial is a field or lab experiment, we are unable to subset this analysis to field experiments. To allow time for researchers to learn about the registry's existence, we examine the subset of 1,792 trials whose start date is after January 1, 2014.[27] Of these trials, only 47% registered before their intervention began. Another 30% registered before the intervention ended.[28] Figure II presents the cumulative number of preregistrations and late registrations over time and Figure III presents the number of preregistrations and late registrations each quarter. These figures illustrate that while the fraction of RCTs that are preregistered has been weakly growing over time, the registry is still dominated by late registrations. We also conduct an ANOVA test to examine the drivers of preregistration. Table IV assesses whether quarter, the first trial topic keyword listed, the first JEL code listed (two-digit classification), the first trial location listed, and primary investigator affiliation predict the preregistration status. We find that quarter is the strongest predictor followed by affiliation and keyword.

Finally, we verify that preregistration is still uncommon among published papers. Table V presents the preregistration rates for field experiments published in the journals considered above. We find that only one-third of these papers preregistered their trial. Columns 1-3 report the number of papers with a registered RCT published in each journal by year. Columns 4-6 report the number of these papers whose RCT started post 2013. And Columns 7-9 report the preregistration rates for this subset. Notably, of the 12 registered RCTs published by AEA journals that were started after 2013, only 5 preregistered. This suggests the registry is still largely capturing studies that are already going through the publication process, which limits the registry's impact on experiment

[27] The registry became widely known after David McKenzie's October 14, 2013 World Bank Development Impact blog post.

[28] The registry also allows trials to report a data collection completion date. Only 146 of these trials report this value. 76 of the 146 registered before the data collection completed.

design and p-hacking.

3.2.2 Restrictiveness

We next examine the extent to which the preregistrations specify the experimental design. Note that researchers are required to describe their:

- Primary outcomes[29]
- Randomization method
- And planned number of observations and treatment arms

Researchers are not required to submit a pre-analysis plan (and correspondingly, only 11% of the 1,792 trials post a pre-analysis plan).

As a first pass, we focus on whether researchers specify their primary outcomes in enough detail to tie their hands to particular variable constructions. We randomly sampled 300 preregistrations and had one set of RAs assess the specificity of the primary outcomes reported by each preregistration. We instructed these RAs to independently count the number of primary outcomes listed and score the outcome descriptions on a scale of 0 (not specific) to 5 (very specific). We gave the following example scale: "Mark "health" as a 0, "nutritional intake" as a 1, "number of fruits consumed" as a 2, "number of fruits consumed at school per week" as a 3, "number of fruits consumed at school per week during Spring quarter" as a 4, and "number of bananas consumed at school per week during Spring quarter" as a 5." Appendix E provides the full instructions.[30] We then had a second set of RAs label each preregistration as either a lab or field experiment according to the definitions in Harrison and List (2004).[31] We find that 241 of the 300 preregistrations are for field experiments.

We find that the field experiment preregistrations leave significant latitude.[32] Table VI reports the assessed restrictiveness. The average preregistration specified just over 3 primary outcomes. The average minimumly restrictive outcome and the median restrictive outcome are classified as

[29] Secondary outcomes are an optional field. 25% of trials list a secondary outcome.

[30] The work was carried out by 10 RAs. Each RA was assigned two sets of 30 preregistrations. The average correlation of the restrictiveness scores across RA pairs was 70%. Our analysis is based on the average of the two assessments.

[31] The AEA RCT Registry does not collect data on whether a given RCT is a field experiment or lab experiment.

[32] Qualitatively, the results are largely unchanged when including the lab experiment preregistrations.

a 2—these outcomes are only as precise as "number of fruits consumed." The preregistrations generally do not specify a precise measurement unit (say number of bananas) nor a measurement timeframe. The average maximumly restrictive outcome is classified as a 2.5—so somewhere between "number of fruits consumed" and "number of fruits consumed at school per week." Only the 90th percentile maximumly restrictive outcome specified a precise measurement timeframe. No outcome was as precise as "number of bananas consumed at school per week during Spring quarter."

Delecourt and Ng's preregistration of "Unpacking the Gender Profit Gap: Evidence from Micro-Businesses in India" provides a useful example. The authors plan to "test whether giving men and women the same business closes the gap in profitability. We set up our own market stalls, to which we randomly assign male and female vendors. We thus exogenously vary gender, holding the business constant." The authors' primary outcomes are (at the vendor level) "daily profit, daily revenue, number of "missed" clients, number of purchasing clients" and (at the product level) "quoted price, price paid." Note that profit, revenue, and number of purchasing clients are specific expect for missing a timeframe; quoted price and price paid are missing both a specification of the products to be considered (likely the primary outcomes of interest will actually be price indexes) and a timeframe; and number of "missed" clients is missing both a specification of how missed will be measured and a timeframe. The two RAs assessing this preregistration agreed that the maximumly restrictive outcome here is a 4 and the minimally restrictive outcome is a 2.

The RAs were also instructed to compare the most recent version of the registration to the preregistration to explore if any primary outcome or sample specification changed. The last two rows in Table VI report the results. We find that 4% of the 241 assessed field experiments changed one of their primary outcomes after the preregistration. Similarly, 4% of the assessed field experiments changed some aspect of their sample specification after the preregistration.

3.2.3 Fidelity

We finally assess the extent to which the primary outcomes reported in the associated working and published papers match the preregistered primary outcomes. The p-hacking concern here is that

authors might change the construction of primary outcomes in order to achieve significant results, add additional outcomes that have a significant relationship, or not report outcomes that do not have a significant relationship. 227 of the 241 field experiment preregistrations listed an intervention end date and 182 ended before June 2019. However, only 7 of the preregistrations provided a link to a working or published paper. As such, we instructed the RAs to use the reported link if present else to try to find an associated paper through Google Scholar via searching for the title and authors. The RAs conducted this search over August 2019 and found working papers for 100 of the preregistrations (there were no associated published papers). Given the above, we expect that this is close to the complete universe of working papers.

Table VII reports the assessed fidelity of the working papers associated with the field experiment preregistrations. On average, 90% of the primary outcomes in a given working paper match their preregistered construction. However, this figure is somewhat misleading because the vast majority of preregistered primary outcomes were unspecific—to use Delecourt and Ng's example, there are many ways to construct a variable that reports the "price paid" for products sold by micro-businesses in India. More troubling, roughly a quarter of the working papers report additional primary outcomes (i.e. the working papers highlight an unregistered variable in their abstract, introduction, or conclusion—see Appendix E). The average working paper reports 0.5 additional primary outcomes. Similarly, roughly a quarter of the working papers fail to report a primary outcome with the average working paper under-reporting 0.4 primary outcomes.

An important caveat to our analysis is that there are many valid reasons for researchers to deviate from their preregistered experimental design. For example, a sudden influx of monetary or technological support may enable a field experiment to record additional primary outcomes midway through an intervention. Rather than a binding constraint on what researchers can do and journals can publish, preregistration can be thought of as useful additional information for outside researchers. Preregistration provides value by distinguishing the initial hypotheses and testing procedures from additional hypotheses and tests that became available or were developed during the course of the experiment.

4 ClinicalTrials.gov and Other Registries

We now compare the AEA RCT Registry to other venues. First, we examine the restrictiveness and fidelity of preregistrations with ClinicalTrials.gov. Second, we examine whether economists use other research registries in addition to or in place of the AEA RCT Registry. We emphasize that our analysis of ClinicalTrials.gov here builds on a large literature and provide a survey of this literature in Appendix C. That said, we are not aware of any previous comparisons to the AEA RCT Registry, which is the main contribution of this section.

4.1 Restrictiveness and Fidelity of ClinicalTrials.gov

We conduct a new survey of ClinicalTrials.gov which serves to more precisely benchmark our results on the restrictiveness of AEA RCT Registry preregistrations and on the fidelity of published or working papers to those preregistrations. We proceed in the same manner as Section 3.2. We find that preregistrations from the first five years of ClinicalTrials.gov are somewhat more restrictive than the AEA RCT Registry preregistrations. We also find that published and working papers associated with the ClinicalTrials.gov preregistrations and with the AEA RCT Registry preregistrations have similar fidelity to the registered primary outcomes. This result combined with the literature review in Appendix C suggests that if ClinicalTrials.gov gives a sign of where the AEA RCT Registry is headed, then there is little reason to be optimistic that the current approach will significantly dent the credibility crisis in economics.

More precisely, we randomly sampled 300 trials that preregistered with ClinicalTrials.gov between March 1, 2000 and July 1, 2005. This period runs from the start of the ClinicalTrials.gov website up through the enforcement of the International Committee of Medical Journal Editors' (ICMJE) policy requiring investigators to preregister trials as a condition for publication. We then employed a new set of RAs to independently review each trial. Using the same rubric as for the AEA RCT Registry, each RA assessed (1) the extent to which the trial's preregistration specifies the primary outcomes in detail and (2) whether the primary outcomes reported in the latest published or working paper match those registered.[33] Appendix F repeats this analysis for preregistrations

[33]The RAs assessed the first available registration for each clinical trial. However, the ClinicalTrials.gov database

with ClinicalTrials.gov after the implementation of the Final Rule for Clinical Trials Registration and Results Information Submission and reaches similar conclusions.

Table VIII reports the assessed restrictiveness of the 300 randomly selected ClinicalTrials.gov preregistrations. The average preregistration specified 2 primary outcomes—1 less than the average AEA RCT preregistration. The average minimumly restrictive outcome is classified as a 2.8, the median restrictive outcome as 3, and the maximumly restrictive outcome as 3.4—each roughly 1 unit more restrictive than the equivalent value for the AEA RCT preregistrations. Put another way, the median primary outcome from a ClinicalTrials.gov preregistration is roughly as specific as "number of fruits consumed at school per week." In contrast, the median primary outcome from an AEA RCT Registry preregistration is just "number of fruits consumed."[34]

We were able to associate published or working papers with 278 of the 300 ClinicalTrials.gov preregistrations. Table IX reports the assessed fidelity of the primary outcomes reported in these papers to those in the registration. On average, 80% of the primary outcomes in a given paper match their registered construction—as compared to 90% of the the AEA RCT Registry primary outcomes.[35] However, as with the AEA RCT Registry results, this figure is misleading because the vast majority of registered primary outcomes are vague enough to match with multiple possible variable constructions. More telling, the average paper reported 0.4 primary outcomes that were not registered and failed to report 0.4 registered primary outcomes. These values closely match those found for the AEA RCT Registry.

4.2 Other Research Registries

A separate open question is whether economists use other research registries in addition to or in place of the AEA RCT Registry. To answer this question, we first directly examine whether

was reset on June 23, 2005. As such, the first available registration for the majority of trials in the sample period is the version as of June 23, 2005. Because investigators may have updated their registration between the initial submission and June 23, 2005, the following analysis provides an upper bound on the restrictiveness of the preregistrations and on the fidelity of the reported primary outcomes.

[34]The last two rows in Table VIII report empirical results from comparing the latest version of the registration to the first available registration. We find 51% of the 300 assessed preregistrations later changed a primary outcome and 64% changed their sample specification. These results are an order of magnitude above those for the AEA RCT Registry. This difference could be due to the longer future horizon available for the ClinicalTrials.gov preregistrations.

[35]Of note, Ewart, Lausen and Millian (2009) find a similar 70% fidelity rate for primary outcomes registered with ClinicalTrials.gov.

economists register in two specific alternative registries—the Registry for International Development Impact Evaluations (RIDIE) and the Evidence in Governance and Politics (EGAP) Registry. Figure V displays the number of economics registrations and the total number of registrations in RIDIE and EGAP by quarter over 2018 and 2019. We find that there is no single quarter with more than 25 economics registrations in either registry. This exercise provides some evidence that economists primarily use the AEA RCT Registry.

We also examine if and where each RCT published in Experimental Economics over 2016-2019 registered. This second exercise is motivated by the fact that no paper published in Experimental Economics registered with the AEA RCT Registry. Appendix E describes the exact search process. Surprisingly, we find no registrations. This result confirms our earlier conjecture that registration is not yet a norm for economists performing lab experiments. Economists running lab experiments generally do not register (or preregister) anywhere.

5 A Model of Registration

We now introduce a simple model that articulates a researcher's incentives to register and the implications of the registration timing decision. The model provides two sets of results regarding registration patterns. The first set considers the impact of banning late registration. We show that banning late registration can increase the total number of experiments that register—improving the registry's usefulness in solving the file draw problem. We also show that banning late registration always weakly increases preregistrations—improving the registry's ability to attenuate p-hacking. The second set of results considers a change in the informativeness of a registration (explained in detail below). Here, we show that increasing informativeness further increases the number of preregistrations. We end with a numerical calibration that provides some support for concluding that banning late registration will increase the number of economics experiments that register with the AEA RCT Registry.

5.1 Model Description and Assumptions

Our model posits a two-stage experimentation problem. We consider a researcher who is endowed with an experiment related to state $\theta \in \{T, F\}$—for instance, reflecting whether an intervention causes a significant treatment effect. The researcher receives a signal on this underlying state in each stage, but an outsider (e.g. the public or journal editor) is only able to observe the signal from the second stage, as well as the registration decision and time. The specific timing of the researcher's actions in our model (illustrated in Figure VI) is:[36]

- First, the researcher observes an initial signal s_1 and then decides whether to conduct the experiment as well as whether to *preregister* (which we also refer to as *registering early*).
- After conducting the experiment, the researcher observes a second signal s_2 and, if the study was not preregistered, decides whether to *register late*.

We first describe our assumptions on the information environment and payoffs to the researcher. We then define a class of partitional equilibria wherein the researchers who register at each stage are those who have the most favorable signals.

5.1.1 Information Environment

We assume that the researcher and outsider initially share a common prior p_0 over $\theta \in \{T, F\}$. The researcher then receives two signals, $s_1 \in [\underline{s}_1, \overline{s}_1]$ and $s_2 \in [\underline{s}_2, \overline{s}_2]$, each with a continuously differentiable density:

- The researcher's signal in the first stage is drawn according to $s_1 \sim f(\cdot \mid \theta)$ where we assume $\frac{d}{ds_1} \log f(s_1 \mid T) \geq \frac{d}{ds_1} \log f(s_1 \mid F)$, and take the distribution over s_1 to be larger in first-order stochastic dominance (FOSD) when $\theta = T$ than when $\theta = F$.
- In the second stage, the researcher observes a second signal $s_2 \sim g_\gamma(\cdot \mid \theta)$ if she registered in the first stage where $\gamma \in \Gamma$ is exogenous and fixed. Else the researcher receives the second signal $s_2 \sim g_0(\cdot \mid \theta)$. We assume $\frac{d}{ds_2} \log g_{\tilde{\gamma}}(s_2 \mid T) \geq \frac{d}{ds_2} \log g_{\tilde{\gamma}}(s_2 \mid F)$, for all s_2 and

[36] A ban on late registration corresponds to an otherwise identical decision problem, except where the researcher is *only* able to preregister or not register at all.

$\tilde{\gamma} \in \Gamma \cup \{0\}$, and take the distribution over s_2 to be larger in the FOSD if $\theta = T$ than if $\theta = F$.

Note that this framework accommodates the possibility that a researcher who does not register early may be able to p-hack. Under this interpretation, we take $g_0(\cdot \mid \theta)$ to be the distribution over experiment outcomes induced by any such p-hacking.[37] Importantly, $\gamma \in \Gamma$ parameterizes the informativeness of the second period signal following registration. For example, the process of registering may help experimenters think through additional contingencies that lead to an improved experimental design. We take the impact of registration on informativeness to be exogenous, although we study comparative statics in γ as well.

While only the researcher (directly) observes s_1, both the researcher and the outsider observe s_2 as well as the registration decision $d \in \{\emptyset, 1, 2\}$ (i.e. no registration, registration at $t = 1$, or registration at $t = 2$ respectively). The outsider therefore updates p_0 from the observation of d and s_2. We denote the updated belief of the outsider that $\theta = T$ by $\hat{p}_d(s_2)$. We think of s_1 as reflecting intuition or prior knowledge on the part of the researcher or information on the propensity of her sample to show treatment effects (for instance, as in the model of scaling results in Al-Ubaydli, List and Suskind (2019)). In contrast, s_2 reflects the experimental findings, which can be conveyed verifiably. Notably, registration influences the second signal but not the first. The assumptions on the signals are standard technical assumptions that ensure that higher signals lead to positive updates on the truth of the hypothesis (which we verify in Appendix D). All signals are also assumed to have full support.

5.1.2 Researcher Payoffs

The researcher incurs a cost of $c_E \geq 0$ when conducting the experiment and also incurs a cost $c_R \geq 0$ whenever registering the experiment (whether registration is early or late). The researcher

[37] One caveat is that, under this interpretation, taking g_0 to be common knowledge does implicitly assume that any such p-hacking would be anticipated and correctly inferred by the outsider. Accommodating the potential for asymmetric information over p-hacking would take us too far afield with (in our view) little added relevant insight for the topic at hand. Also note that, by taking the hypothesis to be exogenous, we are implicitly assuming that all such p-hacking is "vertical," i.e., relating to the same hypothesis (e.g., resampling data). In principle, a researcher may p-hack by drawing multiple hypotheses. While we do not model this possibility, it is less clear whether registration would prevent this "horizontal" p-hacking (aside from its influence on researcher payoffs, which we do address), nor how this should influence the resulting inference .

receives a payoff of 0 if the experiment is not conducted. Otherwise, the payoff depends on the registration decision and the outsider's belief, $\hat{p}_d(s_2)$. We denote the payoff following registration as $b_R(\hat{p}_d(s_2))$ and the payoff following non-registration as $b_N(\hat{p}_\emptyset(s_2))$. We assume $b_N(p) \leq b_R(p)$ for all $p \in [0,1]$. We also assume that $b_i(p)$ is continuous and increasing in p—reflecting a preference for positive results (see Brodeur et al. (2016) and Andrews and Kasy (2019) for empirical evidence suggestive of this preference). For some results below, we assume $b_R(p)$ is weakly convex in p, reflecting a (weak) preference for informative experiments. While we assume s_2 is commonly known, we do not necessarily assume that the researcher and the (passive) outsider share the same preferences over information.[38]

5.1.3 Partitional Equilibrium

For some general results below and our calibration, we focus on the following class of equilibria:

Definition 1. *A **partitional equilibrium** is characterized by thresholds $s^*_{1,\emptyset}, s^*_{1,R}, s^*_{2,R}$ such that:*

- *The researcher conducts the experiment whenever $s_1 > s^*_{1,\emptyset}$,*
- *The researcher preregisters the experiment whenever $s_1 > s^*_{1,R}$, and*
- *If the researcher does not preregister, then the researcher registers the experiment late whenever $s_2 > s^*_{2,R}$.*

Partitional equilibria are convenient to work with because the threshold signal is indifferent between actions on each side of the threshold—that is, a researcher with signal $s^*_{1,R}$ should be indifferent between preregistration and not, and likewise for other signals in this definition.[39] We

[38] One can imagine that a researcher would obtain some independent benefit from conducting an experiment, for instance due to assistance with future work. If this benefit is additively separable, this can be captured simply by decreasing c_E or c_R appropriately, although in this case the assumption that these costs are positive has more bite.

[39] Note that in a partitional equilibrium of our model, if a study is registered late, the distribution of the observed second period signal will be truncated at $s^*_{2,R}$, though pre-registered studies will display no such *second period* truncation. There is empirical support for this contrast, when using our preferred interpretation of s_2 as the experiment results; Adda, Decker and Ottaviani (2020) show empirically that experimental results on ClinicalTrials.gov do not display a clustering just above the significance threshold, even though this is frequently found in published studies across disciplines. Insofar as late registration may be a requirement for publication among studies not registered early, we view this result as supportive of our formulation of preregistration as well as this particular equilibrium.

are able to derive the comparative statics results described below by studying these indifference conditions.[40]

The following assumption is necessary to ensure that the second period registration takes the partitional form for all registration costs c_R:

Assumption 1. *The difference in payoffs between registration decision, $b_R(p) - b_N(p)$, is strictly increasing in p.*

This assumption says that the gain to registration is higher when the outsider's belief is more optimistic. Equivalently, this assumption says that additional optimism benefits the researcher more following registration, suggesting complementarities between beliefs and registration. Researcher payoffs as a function of beliefs may arise from a variety of sources (e.g., reputational considerations). In Appendix D, we discuss a few simple microfoundations of payoffs which provide more context for when this assumption is satisfied. However, we do not take a stand on microfoundations for this complementarity.[41]

A technical difficulty is that, while increasing differences is necessary for the second period signal to be a partition for all c_R, this is not enough to ensure the same holds for the first period registration decision.[42] While Propositions 2 and 3 take this as given, in Appendix D, we provide a sufficient condition which gives that the local indifference condition ensures equilibrium holds globally. This condition states that as researchers grow more optimistic that $\theta = T$, their preference for preregistration over late registration increases as well. This condition assists our numerical calibration, where we verify that it holds and therefore that the global conditions for equilibrium

[40] Note that this model will always possess a pooling equilibrium whereby registration is seen as a *negative* signal. That is, consider a profile where researchers never preregisters, and any deviation is inferred as coming from the researchers with the worst possible signal. In this case, there is no incentive to preregister, since it is both seen negatively and sacrifices option value. Note that this equilibrium requires off-path beliefs susceptible to criticisms in the spirit of the intuitive criterion—assuming that earlier registration is interpreted more negatively may be unpalatable since it is the researchers with higher initial signals who have lower option value, and hence would have the least to lose by preregistering.

[41] In Appendix D, we show that, under Assumption 1, the second period registration decision does not convey information regarding the first period signal. This natural property can fail more generally.

[42] The reason is the following: if the first period signals makes the researcher sufficiently optimistic that the second period signal will be favorable, *independently of the registration decision*, then the added benefit to registering earlier may decrease as well. The potential for non-monotonicity in signalling games is a well-known theoretical issue; see Feltovich, Harbaugh and To (2002) for a discussion of countersignalling equilibria, as well as Liu and Pei (2020) for a general treatment of non-monotonicities in signalling games. Note that the latter paper shows single-crossing by itself does not ensure monotonicity.

are satisfied given indifference at the threshold signals. We omit the technical details from the main text.

For some of our comparative statics results in Section 5.2, it is important to rule out edge cases wherein all registration is early. To do so, we use the following assumption:[43]

Assumption 2. *Consider a model coinciding with the one described above, but where* ***every*** *conducted experiment must be preregistered. For this alternative model, consider the lowest signal, $s_1^* > \underline{s}$, that obtains at least as high a payoff from conducting the experiment as from not conducting it, provided the outsider conjectured all $s_1 > s_1^*$ experimented. Then if the option to experiment without registering were introduced, a researcher with signal s_1^* would have a profitable deviation to do so if this induced the outsider to believe that $s_1 = s_1^*$.*

While this assumption is restrictive in that it does rule out certain model parameters, it does not appear this case is empirically relevant as we are quite far from having universal voluntary preregistration. When this assumption holds, we can compute the threshold in a simple way—illustrated in Figure VII. For each signal s_1, we consider the payoff from (a) preregistering and (b) experimenting without registration if we were to set $s_{1,R}^* = s_1$. Assumption 2 states that the payoff corresponding to (b) is higher than the payoff corresponding to (a) at the lowest signal. The equilibrium threshold signal $s_{1,R}^*$ is then at the intersection of these two lines.

5.2 General Results

We present two sets of results. The first set considers the impact of banning late registration. We show that banning late registration can increase the total number of experiments that register—improving the registry's usefulness in solving the file draw problem. We also show that banning late registration always weakly increases preregistrations—improving the registry's ability to attenuate p-hacking. The second set of results considers a change in the informativeness of the second period signal following registration. Here, we show that increasing the informativeness of registration further increases the number of preregistrations. We also articulate a subtle trade-off between

[43] Note that this assumption can be checked directly from model primitives and is not an assumption on equilibrium.

incentivizing preregistration and incentivizing experimentation, reflecting and formalizing similar concerns related to the social costs of pre-analysis plans by Duflo et al. (2020).

5.2.1 Implications of Banning Late Registration

We now consider the effect of banning late registration, i.e. prohibiting registration in the second stage. Ignoring researcher incentives, one could imagine that allowing late registration would lead to more trials registering. For instance, suppose the researcher simply decides to register in each period with some probability (independent of all other variables). In this case, a late registration ban would simply stop registrations that would have otherwise occurred. While this direct effect of banning late registrations is present in our model, the picture is more complicated because researchers substitute between early and late registration. The following proposition identifies conditions under which the substitution overwhelms the direct effect, resulting in a net increase in the fraction of studies that register under a late ban:

Proposition 1 (Overall Implications on a Late Registration Ban)**.** *Suppose $g_0 = g_\gamma$ and fix all other parameters besides c_R, c_E, and the distribution over s_1. Let $\hat{p}_{s_1}(s_2)$ be the posterior belief that $\theta = T$ given signals s_1 and s_2. If the distribution over s_1 yields $\max_{s_2} \hat{p}_{\overline{s}_1}(s_2) - \hat{p}_{\underline{s}_1}(s_2)$ sufficiently small, then there exists a set of c_R, c_E (in particular, c_E small and c_R sufficiently small but positive) such that banning late registration increases the overall number of registrations.*

The intuition is as follows. When deciding when to register the experiment, the researcher faces a tradeoff between the option value of delay and the potential to signal their confidence based on their initial information. When the initial signal is not too informative, the signalling benefit is low relative to the potentially significant option value. However, under a late ban, the researcher has no option value, and the tradeoff is instead between the expected benefit from registering or not. This larger difference induces them to register earlier even when the initial information is less favorable. The large increase in preregistration can overwhelm the lack of late registrations under a ban, leading to an overall increase in registrations. We note that this argument requires the registration cost to be intermediate—if it is too low, then there is no option value, but if it is too high, then the expected benefit may not be worth the cost.

The general comparative statics on the impacts of a late ban emerge by studying the incentives of the indifferent type:

Proposition 2 (Other Implications of a Late Registration Ban)**.** *Suppose Assumption 2 holds and that the researcher's indifference conditions determine a partitional equilibrium. Then there exists an equilibrium under a late registration ban where:*

- *A weakly larger fraction of experiments preregister and*
- *Weakly fewer experiments are started*

These increases are strict if the threshold signals $s^*_{1,\emptyset}$, $s^*_{1,R}$ and $s^*_{2,R}$ are all distinct and interior.

The proof and intuition are straightforward and come from considering the incentives of the marginal researcher indifferent between actions (i.e. experimentation and registration). Again, banning late registration eliminates the researcher's option value from registering late. Thus the researcher that was marginal between registration decisions when the late registration is allowed will strictly prefer to register (early) under a ban. Similarly, the researcher that was marginal between experimenting or not when late registration is allowed will now strictly prefer to not experiment. We verify that the former change leads to more experiments preregistering, whereas the latter change leads to fewer experiments starting. The effect of diminishing the value of experimentation without registration highlights a potential trade-off between inducing preregistration and inducing experimentation.[44]

5.2.2 Environmental Comparative Statics

Our second finding articulates conditions under which increasing the informativeness of registrations causes an increase in preregistrations. The logic behind this result closely follows our previous comparative statics. Namely, this change increases the payoff to preregistration, and so encourages researchers who previously chose to delay to instead register early:

[44] While the proposition does not rule out an increase in $s^*_{1,R}$ when there are multiple equilibria—for instance, it need not be that *all* $s^*_{1,R}$ thresholds with a late registration ban are lower than *all* $s^*_{1,R}$ thresholds without a late registration ban—this intuition suggests the *most plausible* change following a late registration ban would be a decrease in $s^*_{1,R}$. If the outsider maintained a conjecture that the equilibrium $s^*_{1,R}$ were unchanged following a ban, then researchers with s_1 just below $s^*_{1,R}$ would have a strictly profitable deviation to preregister. Understanding that it is the *lower* s_1 signals with the strict incentive to preregister suggests the outsider should conjecture a lower threshold.

Proposition 3 (Informativeness Comparative Static)**.** *Suppose Assumption 2 holds and that the researcher's indifference conditions determine a partitional equilibrium. Consider a change in γ that makes preregistered experiments more Blackwell informative.*[45] *If $b_R(\hat{p})$ is strictly convex, then there exists an equilibrium where the first period registration threshold weakly decreases (and strictly if the threshold is interior).*

Note that convexity is necessary in order to ensure that researchers gain from having more informative experiments.[46]

Insofar as the ultimate goal of registries is to maximize preregistration, we then recommend changes to the registration process that increase the informativeness of the subsequent experiment. These changes may include but are not limited to (1) requiring more detailed information about the experimental design at the time of registration, (2) requiring a pre-analysis plan, or (3) providing a mechanism for eligible subjects, be they individuals or communities, to join an experiment (as is possible with ClinicalTrials.gov). We emphasize that this is a normative statement. The issue is that increasing the informativeness of registration in this manner likely raises the cost of registration (see, for instance, Olken (2015) for discussion of the significance of planning costs in the context of pre-analysis plans) which may counteract the desired effect.

5.3 Numerical Calibration

We conclude by using a numerical calibration of the above model to explore the impact of banning late registration for the AEA RCT Registry as suggested by Propositions 1 and 2. In our view, it is a priori unclear which specifications of the parameters are most compelling. We therefore seek to be permissive in the specifications we consider, while focusing on an information acquisition technology which allows us to tractably vary signal informativeness. Specifically, we let the first and second period signals have the distribution

- If $\theta = T$, then $s_t \sim f(s_t \mid T) \propto s_t, s_t \in [\underline{s}_t, 1 - \underline{s}_t]$

[45] Several characterizations of the Blackwell order exist; one is that an experiment $\mathcal{I}_1$ is Blackwell-more informative than an experiment $\mathcal{I}_2$ iff $\mathcal{I}_2$ can be represented via some (potentially stochastic, but θ-independent) transformation of the outcome of $\mathcal{I}_1$ (See Blackwell (1953) and the literature following).

[46] Libgober (2020) shows that this convexity condition is naturally generated if follow-on work is proportional to beliefs and if the researcher prefers follow-on work when $\theta = T$.

- If $\theta = F$, then $s_t \sim f(s_t \mid F) \propto (1 - s_t), s_t \in [\underline{s}_t, 1 - \underline{s}_t]$

for $t = 1$ and $t = 2$ respectively. We assume that $0 < \underline{s}_1 < 0.5$ in order to keep the first period belief bounded away from 1 and 0. And we take $\underline{s}_2 = 0$. Note that the second period signal is conditionally independent of the first period signal and that the informativeness of the first period signal is decreasing in $\underline{s}_1$. For simplicity, we next assume that the payoff functions are linear—taking $b_R(\hat{p}) = \hat{p}$ and $b_N(\hat{p}) = 0.8\hat{p}$. This choice reflects that registration is required for publication in the AEA journals. This choice also ensures that the signal informativeness does not influence payoffs directly.

The remaining model parameters are the cost of experimentation c_E, first period signal lower bound $\underline{s}_1$ (introduced above), the initial prior p_0, and the cost of registration c_R. We take as given that all researchers experiment, and so set $c_E = 0$.[47] Then guided by the observed timing of registrations with the AEA RCT Registry, we focus on values for $\underline{s}_1$, p_0, and c_R that produce equilibria wherein the percent of RCTs that preregister closely matches the percent of RCTs that register late.

Table X presents the results. Columns 1 through 3 report the input $\underline{s}_1$, p_0, and c_R. Column 4 gives the percent of RCTs that preregister in equilibrium. Column 5 confirms that this value matches the percent of RCTs that register late. Column 6 displays the total registration rate. Note that the total registration rate is increasing in $\underline{s}_1$. That is, the registration rate is decreasing in the informativeness of the first period signal.

Of interest, Table X Column 7 reports the registration rate under a ban on late registration. We find that, in all cases, banning late registration causes a sharp increase in preregistration. At the least, the percent of experiments that preregister nearly doubles. We also find that, in many cases, banning late registration causes an increase in overall registration—with the benefit being higher when the first period signal is less informative. When the first period signal is $\underline{s}_1 = 0.33$ and $\underline{s}_1 = 0.35$, banning late registration causes a small decline in overall registration. Whereas when the first period signal is $\underline{s}_1 = 0.38$ and $\underline{s}_1 = 0.4$, banning late registration causes a large increase

[47] This decision avoids considerations of how registration timing influences the external margin of experimentation. Note that this margin is unobserved because we are unable to determine how many potential experiments are not conducted.

in overall registration.

These results demonstrate the empirical relevance of Proposition 1 for the AEA RCT Registry. The calibrations confirm that banning late registration increases overall registration under parameterizations of the model that generate qualitatively similar patterns to the AEA RCT Registry data. Of course, our simple model omits other elements guiding registration decisions that may be significant, and we caution against the assertion that banning late registration *must* increase overall registration. That said, insofar as early registrations may be especially valuable, our theoretical analysis shows that a late ban is unambiguously beneficial if the relative preference for preregistrations is sufficiently large.

6 Conclusion

This paper provides a relatively sobering assessment of the AEA RCT Registry—suggesting that thus far it has not been transformative in tackling the major issues at hand. Most experimentalists do not register and many registrations are done for trials that are already at the journal submission phase. Perhaps most disconcerting is that even when preregistrations are completed, they often do not provide enough information to attenuate p-hacking concerns. Hence, even in the best case scenarios, we see limited progress towards solving the file drawer problem and p-hacking.

By introducing an economic lens that clarifies the costs and benefits inherent in this knowledge creation market, we are able to provide two policy recommendations. First, we recommend prohibiting late registration, while simultaneously providing incentives for researchers to preregister their work (such as mandating preregistration as a condition for publication). This dual approach can help maximize preregistrations. But this approach does not solve two other issues that our analysis highlights: (1) current registrations lack the specificity needed to limit p-hacking and (2) registration costs can reduce experimentation. Our second recommendation tackles these issues. We propose having researchers submit their IRB materials directly to the registry. While IRB materials are heterogeneous across schools, in our experience they contain enough uniformity and detail to provide a check on p-hacking. This policy avoids large additional costs since researchers can simply upload the IRB forms that they have already completed.

Importantly, we find it valuable to acknowledge that much of the behavior regarding registration is undoubtedly guided by norms. In our economic model, this takes the form of treating the benefits and costs as exogenous. Certain norms might make publishing without preregistration very difficult. If this were to occur, then our analysis suggests that this feature alone could induce a higher bar for undertaking an experiment in the first place and a lower bar for registration. We suspect that this trade-off is something policymakers are cognizant of, but which our analysis formalizes.

Finally, while data generation via field experiments represents one of the strongest growth areas in the social sciences, greater confidence in the received results is possible. Where will the registry approach to publication bias lead us in the long run? While we have some hints from our discussion of ClinicalTrials.gov, new norms might lead to other changes in experimental conduct that would need to be considered. For instance, we do not observe researchers repeating an experiment multiple times with a new registration each time. But this behavior might emerge if the requirement to register early is sufficiently stringent. We should note that the impact of this behavior on the informativeness of experiments is generally ambiguous (see, for instance, Di Tillio, Ottaviani and Sorenson (2019) and Glaeser (2008)). We view it as important to take such concerns seriously when considering optimal policy in the knowledge creation market, as the credibility of our science is critically linked to key features of the registry.

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A Tables

Table I: Number of papers with an RCT published over our timeframe by journal and experiment type

	Count		Fraction Registered	
	Field	Lab	Field	Lab
Journal				
AEJ-Mic	0	7	NaN	0.00
AEJ-EP	5	1	0.60	0.00
AEJ-AE	29	1	0.62	0.00
AER	16	3	0.75	0.33
JDE	39	2	0.44	0.00
EE	5	70	0.00	0.00
JPE	7	2	0.14	0.00
NEP-EXP	44	62	0.20	0.00
QJE	16	1	0.69	0.00
ReStud	10	9	0.50	0.11

Table II: Number of published papers with a field experiment and fraction that registered with the AEA RCT Registry

	Number Published			Fraction Registered		
	2017	2018	2019*	2017	2018	2019*
Journal						
AEJ-EP	1	2	2	0.00	0.50	1.00
AEJ-AE	9	13	7	0.67	0.62	0.57
AER	5	6	5	1.00	0.50	0.80
JDE	11	19	9	0.36	0.42	0.56
EE	2	3	0	0.00	0.00	NaN
JPE	3	2	2	0.00	0.50	0.00
NEP-EXP	14	11	19	0.07	0.27	0.26
QJE	6	7	3	0.50	0.71	1.00
ReStud	2	4	4	0.50	0.50	0.50

* Data for the first two quarters of 2019

Table III: Three primary investigators who knew about the AEA RCT Registry, but failed to pre-register multiple future RCTs

(a) Investigator A

	First Registered On	Start Date	Intervention Start Date
0	2014-10-07	2014-03-04	2014-04-29
1	2015-10-02	2015-08-14	2015-09-15
2	2016-04-06	2016-04-08	2016-04-29
3	2018-03-14	2016-02-09	2016-12-15
4	2018-06-26	2018-03-23	2018-08-01
5	2018-11-20	2019-02-01	2019-05-01
6	2019-03-16	2019-04-15	2019-04-22
7	2019-03-26	2016-02-09	2017-01-16

(b) Investigator B

	First Registered On	Start Date	Intervention Start Date
0	2014-03-31	2014-03-26	2014-03-26
1	2014-10-06	2014-08-01	2014-09-11
2	2015-10-16	2015-03-21	2015-10-27
3	2015-10-23	2015-09-28	2015-11-04
4	2016-12-14	2015-10-27	2016-12-15
5	2017-10-13	2016-04-01	2016-04-01

(c) Investigator C

	First Registered On	Start Date	Intervention Start Date
0	2014-05-07	2014-04-30	2014-04-30
1	2018-02-02	2018-05-01	2018-05-01
2	2018-09-10	2018-08-06	2018-08-06
3	2019-03-08	2017-01-01	2017-01-01

Notes: Each primary investigator here registered their first trial in 2014, proceeded to register multiple new trials (started after 2014) late, and registered their most recent trial over a year after the intervention began.

Table IV: ANOVA test of predictors of preregistration status

	Partial Sum of Squares	DF	F Stat	PR(>F)
Intercept	0.01	1	0.06	0.81
Quarter	68.03	99	4.42	0.00
Keyword	3.44	13	1.70	0.05
JEL Code	15.37	83	1.19	0.12
Trial Country	8.28	93	0.57	1.00
Affiliation	94.27	444	1.37	0.00
Residual	267.54	1722		

Notes: ANOVA test of whether quarter, the first trial topic keyword listed, the first JEL code listed (two-digit classification), the first trial location listed, and primary investigator affiliation predict the preregistration status of trials in the AEA RCT Registry. Quarter, keyword, and affiliation prove strongly predictive of preregistration.

Table V: Number of published papers with an RCT that registered with the AEA RCT Registry, number whose RCT started after 2013, and fraction of papers whose RCT started after 2013 that preregistered

	Number Registered			Started Post 2013			Fraction Preregistered		
	2017	2018	2019*	2017	2018	2019*	2017	2018	2019*
Journal									
AEJ-EP	0	1	2	0	0	1	NaN	NaN	1.00
AEJ-AE	6	8	4	0	2	2	NaN	0.00	0.00
AER	5	3	4	1	3	3	1.0	0.67	0.33
Develop Econ	4	8	5	0	2	1	NaN	0.00	1.00
Exp Econ	0	0	0	0	0	0	NaN	NaN	NaN
JPE	0	1	0	0	0	0	NaN	NaN	NaN
NEP-EXP	1	3	5	1	2	4	1.0	0.00	0.50
QJE	3	5	3	1	3	2	0.0	0.33	0.00
ReStud	1	2	2	0	1	1	NaN	1.00	0.00

* Data for the first two quarters of 2019

Table VI: Assessment of the extent to which 241 randomly chosen AEA RCT Registry field experiment preregistrations precisely specify their primary outcomes

	Mean	Std	Min	10%	25%	50%	75%	90%	Max
Number of Outcomes	3.26	2.43	0.0	1.0	2.0	3.00	4.0	6.0	20.5
Maximumly Restrictive Outcome	2.48	1.03	0.0	1.0	1.5	2.50	3.0	4.0	4.5
Minimumly Restrictive Outcome	1.94	1.09	0.0	0.5	1.0	2.00	2.5	3.5	4.5
Median Restrictive Outcome	2.22	0.99	0.0	1.0	1.5	2.25	3.0	3.5	4.5
Outcome Changed (Yes/No)	0.04	0.16	0.0	0.0	0.0	0.00	0.0	0.0	1.0
Sample Changed (Yes/No)	0.04	0.17	0.0	0.0	0.0	0.00	0.0	0.0	1.0

Notes: Preregistrations were randomly sampled from the period May 15, 2013 to April 1, 2019. Each registration was assessed by two RAs. The values presented are based on the average of the two assessments. The RAs were instructed to market unspecific outcomes as a 0 and very specific outcomes as a 5. The instructions (which include a scoring example) are presented in Appendix E.

Table VII: Assessment of the extent to which working and published papers report the primary outcomes preregistered with the AEA RCT Registry

	Mean	Std	Min	10%	25%	50%	75%	90%	Max
Fraction of Matching Outcomes	0.90	0.22	0.0	0.65	0.91	1.0	1.0	1.00	1.0
Number of Additional Outcomes	0.46	1.05	0.0	0.00	0.00	0.0	0.5	1.65	7.0
Number of Missing Outcomes	0.43	0.90	0.0	0.00	0.00	0.0	0.5	1.10	4.0

Notes: Working papers were found for 100 of the 241 field experiment preregistrations.

Table VIII: Assessment of the extent to which 300 randomly chosen ClinicalTrials.gov preregistrations precisely specify their primary outcomes

	Mean	Std	Min	10%	25%	50%	75%	90%	Max
Number of Outcomes	1.95	1.18	0.5	1.0	1.0	1.5	3.00	4.0	6.0
Minimumly Restrictive Outcome	2.77	0.99	1.0	1.5	2.0	3.0	3.50	4.0	5.0
Maximumly Restrictive Outcome	3.35	0.99	1.0	2.0	3.0	3.5	4.00	4.5	5.0
Median Restrictive Outcome	3.04	0.90	1.0	2.0	2.5	3.0	3.52	4.0	5.0
Outcome Changed (Yes/No)	0.51	0.45	0.0	0.0	0.0	0.5	1.00	1.0	1.0
Sample Changed (Yes/No)	0.64	0.45	0.0	0.0	0.0	1.0	1.00	1.0	1.0

Notes: Preregistrations were randomly sampled from the period March 1, 2000 to July 1, 2005. This period corresponds to the first five years of the ClinicalTrials.gov registry and predates the ICMJE policy requiring preregistration for publication in most medical journals. Each registration was assessed by four RAs. The values presented are based on the median of the four assessments.

Table IX: Assessment of the extent to which working and published papers report the primary outcomes preregistered with ClinicalTrials.gov

	Mean	Std	Min	10%	25%	50%	75%	90%	Max
Fraction of Matching Outcomes	0.80	0.41	0.0	0.32	0.58	1.0	1.0	1.0	5.0
Number of Additional Outcomes	0.39	0.94	0.0	0.00	0.00	0.0	0.5	1.0	10.5
Number of Missing Outcomes	0.38	0.84	0.0	0.00	0.00	0.0	0.5	1.5	7.0

Notes: Working or published papers were found for 278 of the 300 preregistrations.

Table X: Equilibrium registration rates for various model specifications

$\underline{s}_1$	p_0	c_R	% Preregister	% Register Late	% Register	% Preregister (Late Ban)
0.33	0.124	0.100	3.87	3.85	7.72	6.49
0.33	0.159	0.120	3.99	3.99	7.98	6.24
0.33	0.198	0.140	3.96	3.95	7.91	5.71
0.35	0.149	0.100	5.3	5.3	10.6	9.75
0.35	0.169	0.110	5.46	5.46	10.92	9.66
0.35	0.190	0.120	5.55	5.58	11.13	9.45
0.38	0.148	0.080	7.88	7.85	15.73	18.86
0.38	0.172	0.090	8.28	8.26	16.54	19.07
0.38	0.198	0.100	8.68	8.64	17.32	19.18
0.40	0.102	0.050	8.98	8.88	17.87	28.88
0.40	0.167	0.075	10.06	10.57	20.63	30.31
0.40	0.242	0.100	12.1	12.02	24.12	31.84

Notes: Each simulation takes $c_E = 0$, $\underline{s}_2 = 0$, $b_R(\hat{p}) = \hat{p}$, and $b_N(\hat{p}) = 0.8\hat{p}$. Columns 1 through 3 report the input $\underline{s}_1$, p_0, and c_R. Column 4 presents the percent of experiments that preregister in equilibrium. Column 5 confirms that this value match the percent of experiments that register late. Column 6 displays the total registration rate. Column 7 reports the registration rate (which is also the preregistration rate) under a ban on late registration.

B Figures

Figure I: The AEA RCT Registry. Trials that register late are marked with a gray clock and trials that preregister are marked with an orange clock.

Figure II: Cumulative number of AEA RCT preregistrations and late registrations

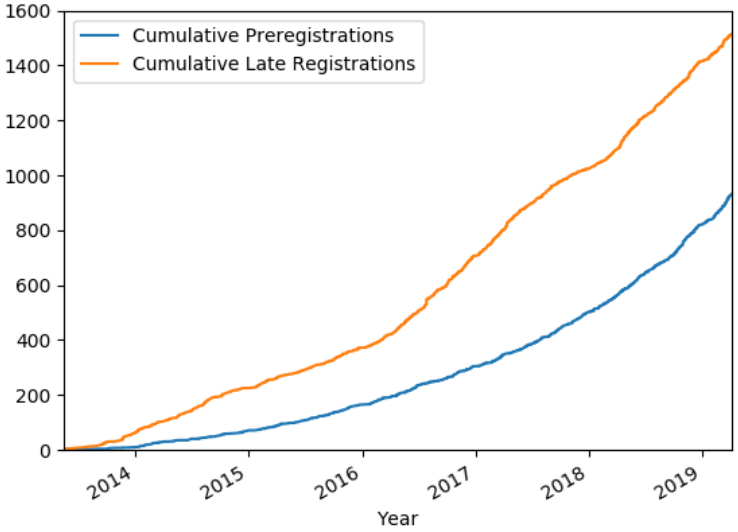


Figure III: Number of AEA RCT preregistrations and late registrations by quarter

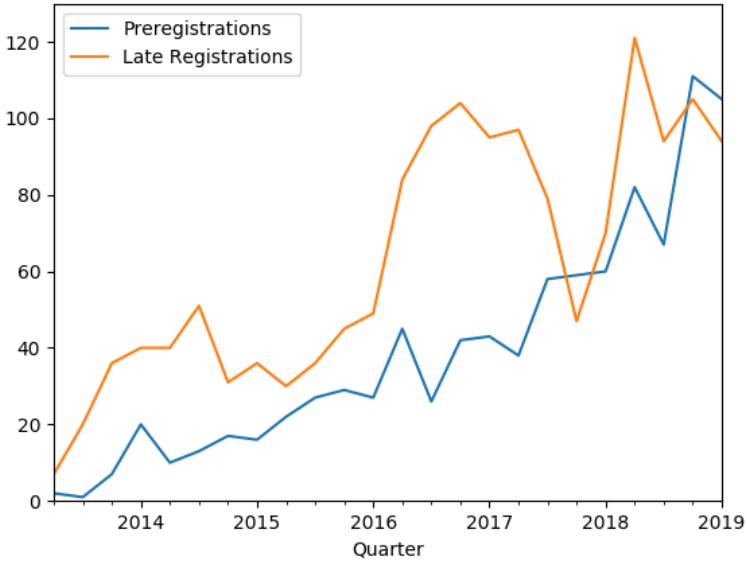


Figure IV: Days between intervention start and AEA RCT registration for RCTs started after January 1, 2014. Positive values indicate that the intervention began after the registration.

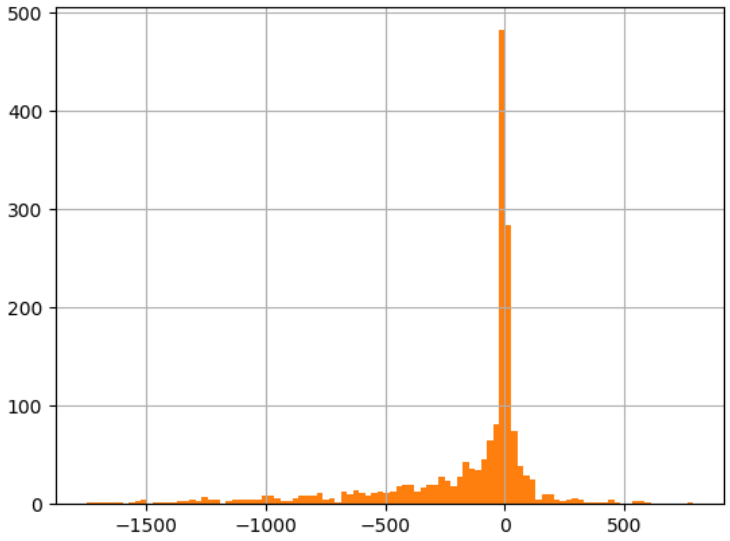


Figure V: Registration in the Registry for International Development Impact Evaluations (RIDIE) and in the Evidence in Governance and Politics (EGAP) registry over time. Solid lines display the total number of registrations while dashed lines present economics registrations

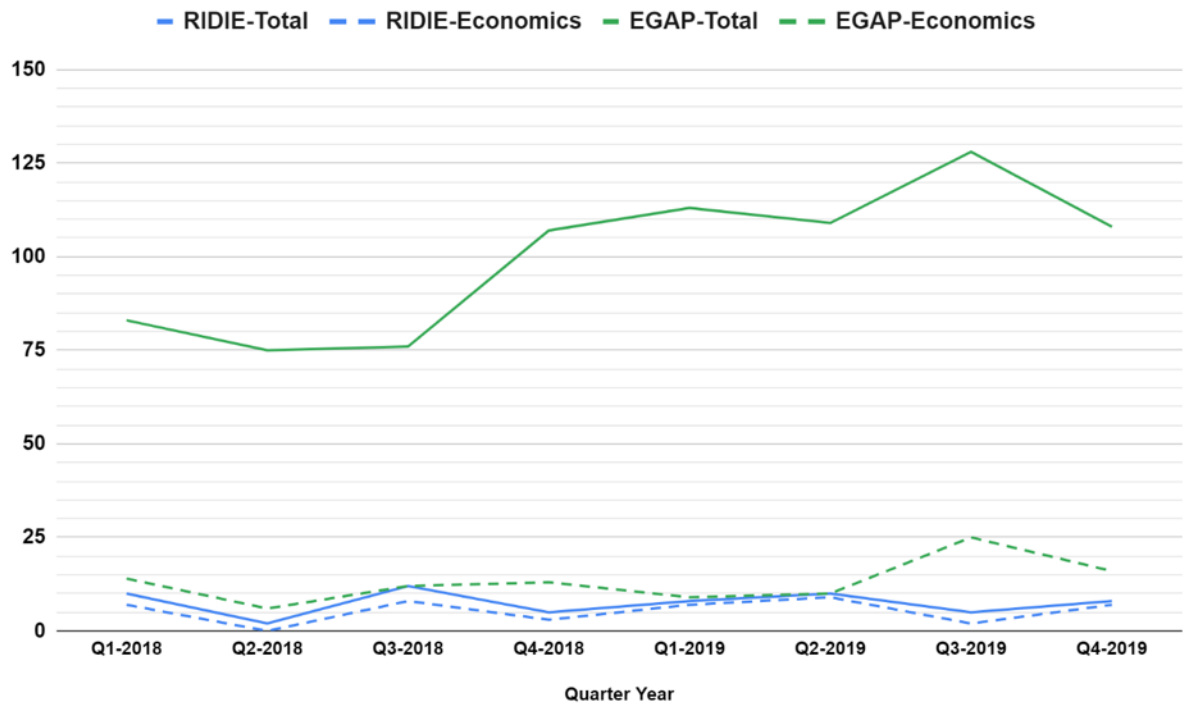


Figure VI: Timing of moves in the model

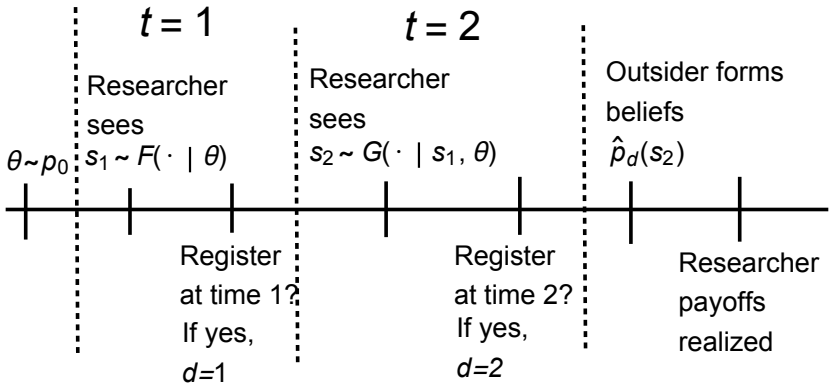
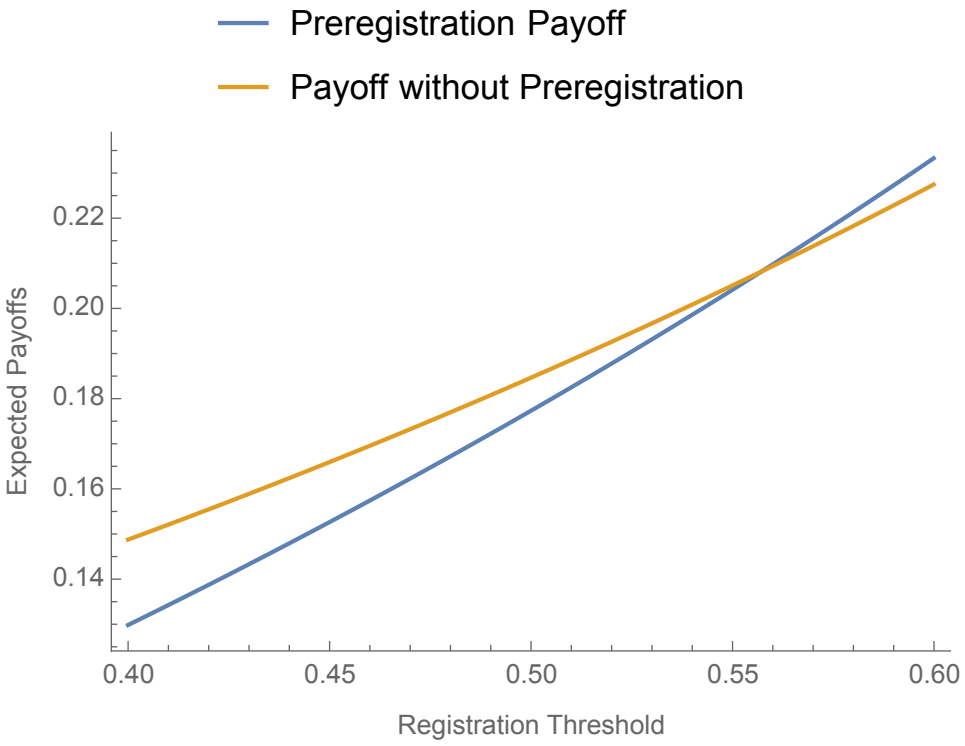


Figure VII: Researcher payoff upon receiving signal $s_1 = s^*_{1,R}$ assuming the equilibrium registration threshold is conjectured by the outsider to be $s^*_{1,R}$



Notes: Assumption 2 is seen to hold, since at the left endpoint, the orange line is higher than the blue line. The (conjectured) equilibrium threshold is the intersection point of these two lines. Payoffs and information structure are as in Table X, with $\underline{s}_1 = .4$, $c_R = .1$, and $p_0 = .25$.

C Survey of ClinicalTrials.gov Literature

Assessments of ClinicalTrials.gov provide a useful contrast between economics and medical disciplines. Since ClinicalTrials.gov (launched in February 2000) has a much longer history than the AEA RCT Registry, these assessments may also provide hints about how the AEA registry could evolve going forward. Unfortunately, we find that the success of ClinicalTrials.gov in solving the credibility crisis is largely mythical. Previous studies show that ClinicalTrials.gov has foundational problems similar to the AEA registry.

First, ClinicalTrials.gov, by itself, does not capture a census of all relevant trials. In an early survey of industry-sponsored phase III drug trials, Manheimer and Anderson (2002) found that 25% of prostate cancer drug trials and 40% of colon cancer drug trials failed to register with ClinicalTrials.gov (or any other applicable registry). Dickersin and Rennie (2003) raised similar concerns for academic trials. In response to this issue, the International Committee of Medical Journal Editors (ICMJE) mandated that clinical trials register before the onset of patient enrollment as a condition of consideration for publication.[48] This policy change provides a rough upper bound on the voluntary registration rate. Zarin et al. (2007) document that ClinicalTrials.gov received an average of 30 new registrations per week prior to the full implementation of the ICMJE policy in September 2005 and 220 new registrations per week after. These values imply that fewer than 14% of all clinical trials voluntarily registered with ClinicalTrials.gov.[49]

Second, many trials that do register do not provide sufficient information. Zarin et al. (2011) examine the primary outcome measures from 100 randomly selected non–phase I trials that registered with ClinicalTrials.gov in August 2010 and find that 61% lacked either a specific metric and/or time frame. Zarin et al. (2017) repeat this analysis for 80 articles published in the New England Journal of Medicine and the Journal of the American Medical Association over 2015-2016

[48] The policy required new trials to preregister from July 1, 2005 on and existing trials to register by September 13, 2005. The policy did not specify a required registry, but the announcement noted that only ClinicalTrials.gov currently fulfilled the ICMJE's specifications. See DeAngelis et al. (2005).

[49] 14% is likely a high upper bound because the ICMJE policy does not impact most industry-sponsored trials. Also, enforcement of the ICMJE policy increased over time. Mathieu et al. (2009) find a 73% registration rate for trials in three medical areas (cardiology, rheumatology, and gastroenterology) indexed in the ten general medical journals and specialty journals with the highest impact factors in 2008. Meanwhile, Huser and Cimino (2013) find a 96% registration rate for trials published in five ICMJE founding journals over 2010-2011.

and find that 42.6% of the primary outcomes listed in the associated ClinicalTrials.gov registrations lacked either a specific metric and/or time frame.[50] More surprisingly, even basic ClinicalTrials.gov information fields are often completed incorrectly. Chaturvedi et al. (2019) survey registrations over 2005-2015 and find that 17% of the listed primary investigator names are not those of real persons, but instead, to use their term, "junk information."

Third, most registered trials fail to report their results. ClinicalTrials.gov launched a results database in September 2008 to implement Section 801 of the Food and Drug Administration Amendments Act of 2007 (FDAAA), which requires the submission of "basic results" for most clinical trials of drugs and biologics within one year of their completion.[51] Despite this law, Law, Kawasumi and Morgan (2011) find that fewer than 13% of relevant registered trials completed between October 2008 and May 2010 reported results on time. Prayle, Hurley and Smyth (2012) and Anderson et al. (2015) show similarly poor reporting compliance rates for registered trials that completed in 2009 and over 2008-2012 respectively. Examining longer time frames, Nguyen et al. (2013) note that 50% of cancer drug trials failed to report results three years after completion. And Fain et al. (2018) find that 25% of industry-sponsored trials failed to report results even seven years after completion.[52] Adda, Decker and Ottaviani (2020) show an excess in the number of significant results in Phase III investigation relative to Phase II investigations for small industry sponsors; they argue this is consistent with the selective reporting of results.

Finally, when registered trials do report results these often differ from the published results. Hartung et al. (2014) explore these inconsistencies by taking a 10% random sample of Phase III and IV trials that both proceeded to publication and reported results on ClinicalTrials.gov before January 1, 2009. The authors find that 80% were inconsistent in the number of secondary outcomes considered, 35% inconsistently stated the number of individuals with a serious adverse event, 20% had inconsistencies in a primary outcome value, and 15% described a primary outcome inconsistently. Becker et al. (2014) similarly find that nearly all trials published in high-impact journals that

[50] The authors also find that 33% of the trials that registered over 2012-2014 registered more than three months after their start date.

[51] The FDAAA also mandates the registration of most non-phase I trials of FDA-regulated drug, biological, and device products.

[52] In a partial counterpoint, Oostrom (2020) finds that requirements to preregister psychiatric drug trials with ClinicalTrials.gov help limit the effect of financial sponsorship on reported drug efficacy via capturing negative results.

reported results on ClinicalTrials.gov had a least one significant discrepancy. Perhaps more ominously, Earley, Lau and Uhlig (2013) highlight differences between the number of deaths reported on ClinicalTrials.gov and in corresponding published papers.

D Proofs

This appendix is organized as follows. First, we present proofs related to equilibrium beliefs. Then, we present proofs related to finding and checking partitional equilibria. With these results in hand, we present proofs of the comaprative statics from the main text. We subsequently verify the conditions for partitional equilibria which we use for our numerical calibration, and conclude with some additional discussion of the microfoundations of preferences which would lead to Assumption 1 being satisfied.

D.1 Properties of Beliefs

Proof that $\frac{d}{ds_1}\log f(s_1 \mid T) \geq \frac{d}{ds_1}\log f(s_1 \mid F) \Rightarrow \hat{p}(s_1)$ *is increasing.* While likely familiar, this argument is included for completeness. Note that

$$\hat{p}(s_1) = \frac{\mathbb{P}[\theta = T]f(s_1 \mid T)}{\mathbb{P}[\theta = T]f(s_1 \mid T) + \mathbb{P}[\theta = F]f(s_1 \mid F)}.$$

We take the derivative and obtain $\hat{p}'(s_1)$ has the same sign as:

$$\begin{aligned}
&\left(\mathbb{P}[\theta = T]f(s_1 \mid T) + \mathbb{P}[\theta = F]f(s_1 \mid F)\right)\mathbb{P}[\theta = T]f'(s_1 \mid T) \\
&\qquad - \left(\mathbb{P}[\theta = T]f'(s_1 \mid T) + \mathbb{P}[\theta = F]f'(s_1 \mid F)\right)\mathbb{P}[\theta = T]f(s_1 \mid T) \\
&\quad = \mathbb{P}[\theta = F]f(s_1 \mid F)\mathbb{P}[\theta = T]f'(s_1 \mid T) - \mathbb{P}[\theta = F]f'(s_1 \mid F)\mathbb{P}[\theta = T]f(s_1 \mid T),
\end{aligned}$$

which, since $\mathbb{P}[\theta = T] \in (0, 1)$, is greater than 0 if and only if:

$$\frac{f(s_1 \mid F)}{f'(s_1 \mid F)} \geq \frac{f(s_1 \mid T)}{f'(s_1 \mid T)} \Leftrightarrow \frac{d}{ds_1}\log f(s_1 \mid T) \geq \frac{d}{ds_1}\log f(s_1 \mid F),$$

as desired. □

Lemma D.1. *In any equilibrium,* $\hat{p}_1(s_2)$ *is increasing.*

Proof of Lemma D.1. As it will be useful for a later proof, we consider $\hat{p}_d(s_2)$ for any d. Let us first consider the fictitious environment where s_1 were observable to the outsider. Call this

$\tilde{p}_{s_1,d}(s_2)$. Differentiating $\tilde{p}_{s_1,d}(s_2)$ (which is equal to $\hat{p}_d(s_2)$ once integrating over the distribution of s_1 conditional on d), we have that it is proportional to:

$$g'_\gamma(s_2 \mid T)f(s_1 \mid T)\mathbb{P}[T] \cdot g_\gamma(s_2 \mid F)f(s_1 \mid F)\mathbb{P}[F] \\ - g'_\gamma(s_2 \mid F)f(\tilde{s}_1 \mid F)\mathbb{P}[F]g_\gamma(s_2 \mid T)f(s_1 \mid T)\mathbb{P}[T].$$

Following similar logic as the previous proof, we have:

$$f(s_1 \mid T)\mathbb{P}[T]f(s_1 \mid F)\mathbb{P}[F])(g_\gamma(s_2 \mid T)g_\gamma(s_2 \mid F)) \cdot \left(\frac{g'_\gamma(s_2 \mid T)}{g_\gamma(s_2 \mid T)} - \frac{g'_\gamma(s_2 \mid F)}{g_\gamma(s_2 \mid F)}\right),$$

which must be greater than 0 since $\frac{d}{ds_2}\log g_\gamma(s_2 \mid T) \geq \frac{d}{ds_2}g_\gamma(s_2 \mid F)$, and in addition since all other densities and probabilities are positive as well.

It remains to show that $\hat{p}_d(s_2)$ is increasing in s_2. Letting $\sigma(\cdot \mid s_2, d)$ denote the equilibrium measure over s_1 given s_2 and d, by the martingale property of beliefs:

$$\hat{p}_d(s_2) = \int_{s_1} \tilde{p}_{s_1,d}(s_2)\sigma(s_1 \mid s_2, d)ds_1,$$

as $\hat{p}_d(s_2)$ is simply the expectation over $\tilde{p}_{s_1,d}(s_2)$ after observing s_1, in addition to s_2 and d.

Now, if $d = 1$, then $\sigma(s_1 \mid s_2, d)$ is mechanically independent of s_2 on its support. So,

$$\hat{p}_1(s'_2) = \int_{s_1} \tilde{p}_{s_1,1}(s'_2)\sigma(s_1 \mid d = 1)ds_1 \geq \int_{s_1} \tilde{p}_{s_1,1}(s''_2)\sigma(s_1 \mid d = 1)ds_1 = \hat{p}_1(s''_2),$$

as claimed. □

Lemma D.2. *Under Assumption 1, the outsider's belief satisfies $\hat{p}_2(s_2) = \hat{p}_\emptyset(s_2)$ in equilibrium (that is, the belief following late registration is equal to the belief following no registration).*

Proof. Let $h(s_1, s_2 \mid \mathbf{1}[d = 1], \theta)$ denote the joint distribution of signals given the state θ and registration decision at time 1 and γ, and let $\sigma(d \mid s_1, s_2)$ denote the probability the registration

decision is d given signals s_1 and s_2. Note that $\hat{p}_d(s_2)$ is:

$$\frac{\int_{s_1} \sigma(d \mid s_1, s_2) h(s_1, s_2 \mid \mathbf{1}[d=1], T) \mathbb{P}[\theta = T] ds_1}{\int_{s_1} \sigma(d \mid s_1, s_2) h(s_1, s_2 \mid \mathbf{1}[d=1], T) \mathbb{P}[\theta = T] ds_1 + \int_{s_1} \sigma(d \mid s_1, s_2) h(s_1, s_2 \mid \mathbf{1}[d=1], F) \mathbb{P}[\theta = F] ds_1},$$

noting that σ also includes the event that the researcher undertakes the experiment.

Consider any signal s_2 where the researcher were to mix over the registration decision. At any such signal, we must have $b_R(\hat{p}_2(s_2)) - c_R = b_N(\hat{p}_\emptyset(s_2))$, since otherwise there would be a strict incentive to deviate. Since $b_R(\hat{p}) - b_N(\hat{p})$ is increasing, there can only be at most one belief where this indifference is satisfied, say p^*. Since there is only one belief that can be induced in order for the sender to be willing to mix over registration, we must therefore have that the registration decision is uninformative, i.e., that the same belief is induced for each registration decision.

However, note that for any strategy, $\mathbb{E}_{d \sim \sigma}[\hat{p}_d(s_2)]$ is equal to the probability that $\theta = T$ conditional on s_2 alone, by the martingale property of beliefs. Following the proof of Lemma D.1 to consider the event that $d \neq 1$, this belief is increasing in s_2, since it is increasing in s_2 for all s_1 and thus also increasing when we take an expectation over s_1 as well, for any measure over s_1. Since we require $\hat{p}_2(s_2) = \hat{p}_\emptyset(s_2) = p^*$ in order for the researcher to be willing to mix, it follows that there can only be a single signal s_2 where the registration decision is informative.

This shows that we must have a deterministic registration decision, for almost every s_2, and in particular one that is not informative. Thus, $\sigma(d \mid s_1, s_2)$ is independent of s_1 on the support of the signals that delay registration. Upon inspecting the expression of the outsider's beliefs, we observe that the second period registration decision does not influence the distribution over s_1. □

Lemma D.3. *Under Assumption 1, $\hat{p}_2(s_2) = \hat{p}_\emptyset(s_2)$ is increasing in s_2.*

Proof of Lemma D.3. Replicating the proof of Lemma D.1, Lemma D.2 implies that $\sigma(d \mid s_1, s_2)$ is also independent of s_2 when $d \in \{\emptyset, 2\}$, under Assumption 1. Thus, the same argument applies to this case as well. □

Remark 1. *The above arguments rely upon the increasing differences conditions in order to ensure that late registration does not convey information. Without this assumption, mixed strategy equilibria may emerge and cannot be ruled out immediately. To see why, suppose the increasing differ-*

ences condition is violated. Then, we can find p_1, p_2 such that $b_R(p_1) - b_N(p_1) = b_R(p_2) - b_N(p_2)$. We can then also find c_R such that $b_R(p_1) - c_R = b_N(p_1)$, which also implies $b_R(p_2) - c_R = b_N(p_2)$.

Now, to illustrate that this Lemma can fail, suppose for simplicity that the distribution of beliefs as a function of s_2, integrating over s_1, is a strict subset of (p_1, p_2). Then if the first period signal (among types that do not register) is sufficiently informative, there exists a strategy $\sigma : S_1 \times S_2 \to \{\emptyset, 2\}$ such that $\hat{p}_\emptyset(s_2) = p_1$ and $\hat{p}_2(s_2) = p_2$; that this can be done under the stated conditions follows immediatley from, for instance, Gentzkow and Kamenica (2016); their result implies that, given any "integrated out" second period belief $\hat{p}(s_2) \in (p_1, p_2)$, the martingale constraint alone dictates whether a distribution of beliefs can emerge under some information structure if s_1 fully reveals the state, and that this conclusion holds as long as the first period signal is sufficiently informative (how close to fully informative will depend on the parameters). By the stated conditions, given these beliefs, the researcher is indifferent between registration decisions. Note this actually describes two *equilibria; one could instead let $\hat{p}_\emptyset(s_2) = p_2$ and $\hat{p}_2(s_2) = p_1$.*

While many of the features of the above construction rely upon the assumption that the second period signal alone puts beliefs in (p_1, p_2), this is not strictly necessary and the argument would still work if there were registration strategies which put the support of the outsider's second period belief in $\{p_1, p_2\}$. We also do not see an easy way of ruling out "sufficiently informative" first period signals a priori. These equilibria are simpler to see in the extreme case where $b_N(p) = b_R(p) - c_R$ for all p. In this case, any *second period registration strategy forms an equilibrium for the researcher, including ones which reveal information about the first period signal. While theoretically interesting, our conjecture is that most researchers in practice would not vary their registration decision after experimenting based on information not conveyed in an experiment's results. As we view Assumption 1 as appealing, we leave an analysis of other conditions which would yield Lemma D.2 to future work.*

Lemma D.4. *Consider any partitional equilibrium, where decision d is taken by researchers with $s_1 \in [s_{min}, s_{max}]$. Then $\hat{p}_d(s_2)$ is uniformly increasing (i.e., increasing for all s_2) in s_{min}.*

Proof. We use the above characterization of $\hat{p}_d(s_2)$ which uses the martingale property of beliefs, i.e. that $\hat{p}_d(s_2) = \int_{s_{min}}^{s_{max}} \hat{p}_{d,s_1}(s_2) \mathbb{P}[s_2 \mid s_1 \in [s_{min}, s_{max}]]$. Consider $\mathbb{P}[s_2 \leq s' \mid s_1 \in [s_{min}, s_{max}]]$.

We claim that this is FOSD larger if s_{min} increases. The result is immediate given the claim, since $\hat{p}_{d,s1}(s_2)$ is increasing in s_2, which means integrating against an FOSD larger distribution leads to $\hat{p}_d(s_2)$ increasing, for all s_2. On the other hand, the claim is immediate as well; if we consider drawing s_1 and then using the resulting draw to determine s_2, increasing s_{min} simply increases the probability of using a more favorable s_1 draw. Hence the conclusion follows. □

D.2 Partitional Equilibria

In this Section, we walk through some additional details on partitional equilibria and conditions which ensure the indifference condition determines an equilibrium. While our sufficient condition for an equilibrium to be a partitional threshold equilibrium is restrictive, it is useful in that we are able to numerically verify it in many cases of interest, particularly the region where we calibrate our model to the data.

We first show that Assumption 1 implies the existence of the threshold equilibrium strategies in the second period:

Proposition 4. *In any equilibrium under Assumption 1, there exists a threshold s_2^* such that a researcher who has not registered at time 1 will do so at time 2 if $s_2 > s_2^*$.*

Proof. By Lemma D.2, the outsider's belief at time 2 depends only on s_2 and $\mathbf{1}[d = 1]$. So consider the range of $b_R(p) - c_R - b_N(p)$ over all s_2 given $d \neq 1$; note that this is either always positive, always negative, or positive for some values and negative for others. In the first two cases, the registration decision is degenerate and hence trivially of a threshold form (taking the thresold to be outside of the support of the signal distribution). In the latter case, since by assumption $b_R(p) - c_R - b_N(p)$ is increasing and continuous, by the intermediate value theorem we have there is some belief in the range of possible second period beliefs where this is equal to 0, say p^*, which corresponds to a signal s_2^*. Furthemore, we previously showed that the second period belief is increasing in s_2. Since it is also continuous in s_2, we thus have the researcher registers when $s_2 > s_2^*$ and not when $s_2 < s_2^*$, as desired. □

We now present our sufficient condition which ensures the existence of a partitional equilibrium

in the first period:

Definition 2. *We say that a registration strategy has increasing gains to early registration if:*

$$\int_{-\infty}^{\infty} b_R(\hat{p}_1(s_2))\mathbb{E}[g_\gamma(s_2 \mid \theta) \mid s_1] - b_R(\hat{p}_2(s_2))\mathbb{E}[g_0(s_2 \mid \theta) \mid s_1]ds_2 \tag{1}$$

is increasing in s_1.

Note that this condition does not depend on b_N, and in fact does not depend on the researcher's registration threshold at $t = 2$; it says that given a researcher *will* register, it is even better to register early rather than late when the initial signal is higher. This reflects our intuition that researchers with more favorable results are generally more eager to register, whenever they may be on the margin.

Proposition 5. *Suppose a registration strategy satisfies increasing gains to early registration. Then under Assumption 1, then the first period registration decision must be of a partitional form.*

Proof of Proposition 5. Denote $\tilde{p}(s_1)$ as the *researcher's* belief that $\theta = T$ given a signal of s_1, and recall that $\tilde{p}(s_1)$ is a strictly increasing function of s_1. Furthermore, the researcher's benefit is independent of the *realized* s_1 (since this is not observed by the outsider). As a result, we can write the researcher's payoff without any reference to s_1 at all, and only the researcher's belief $\tilde{p}$. And to prove the theorem, it suffices to show that the payoff from registration increases more than the payoff from non-registration when $\tilde{p}$ increases.

Making this change of variables, we have the researcher's payoff is:

$$-c_R + \int_0^1 b_R(\hat{p}_1(s_2))\left(\tilde{p}f(s_2 \mid T) + (1-\tilde{p})f(s_2 \mid F)\right)ds_2. \tag{2}$$

The payoff from registration at time 2 is:

$$\int_{s_{2,R}}^1 \left(b_R(\hat{p}_2(s_2)) - c_R\right)\left(\tilde{p}f(s_2 \mid T) + (1-\tilde{p})f(s_2 \mid F)\right)ds_2 + \int_0^{s_{2,R}} b_N(\hat{p}_2(s_2))(\tilde{p}f(s_2 \mid T) + (1-\tilde{p})f(s_2 \mid F))ds_2.$$

Consider the difference between these two expressions, which can be written:

$$\int_{s_{2,R}}^{1} \left(b_R(\hat{p}_1(s_2)) - b_R(\hat{p}_2(s_2))\right)\left(\tilde{p}f(s_2 \mid T) + (1-\tilde{p})f(s_2 \mid F)\right) ds_2 \\ + \int_{0}^{s_{2,R}} (b_R(\hat{p}_1(s_2)) - c_R) - b_N(\hat{p}_2(s_2))(\tilde{p}f(s_2 \mid T) + (1-\tilde{p})f(s_2 \mid F))ds_2.$$

Rewriting this slightly, we wish to show that if:

$$\int_{0}^{1} \left(b_R(\hat{p}_1(s_2)) - b_R(\hat{p}_2(s_2))\right)\left(\tilde{p}f(s_2 \mid T) + (1-\tilde{p})f(s_2 \mid F)\right) ds_2 \\ + \int_{0}^{s_{2,R}} (b_R(\hat{p}_2(s_2)) - c_R) - b_N(\hat{p}_2(s_2))(\tilde{p}f(s_2 \mid T) + (1-\tilde{p})f(s_2 \mid F))ds_2 > 0.$$

then this also holds at any $\tilde{p}' > \tilde{p}$. Note that this expression considers the difference as the sum of two terms: The first term is the *belief increase* due to registration, and the second is the loss due to *option value*.

Now, if this is positive at some $\tilde{p}$ but not at $\tilde{p}' > \tilde{p}$, then it must be due to option value, since the first term is always positive, since the proposition considers equilibria where the increasing gains to early registration condition is satsfied. Thus, it suffices to show that the second integral is increasing in s_2.

To see this, first note that first order stochastic dominance is maintained under monotone transformations,[53] and that $\hat{p}_2(s_2)$ is a monotone transformation of s_2. As a result, consider the distribution over second period beliefs, say $g(p_2 \mid \theta)$. Since $f(s_2 \mid T)$ first order stochastically dominates $f(s_2 \mid F)$, we also have $g(p_2 \mid T)$ first order stochastically dominates $g(p_2 \mid F)$. we have this integral is:

$$\int_{0}^{s_{2,R}} (b_R(p_2) - c_R - b_N(p_2))(\tilde{p}g(p_2 \mid T) + (1-\tilde{p})g(p_2 \mid F))dp_2.$$

Now, recall $b_R(p_2) - b_N(p_2)$ is assumed to be increasing, and increases in $\tilde{p}$ yield increases in first order stochastic dominance shifts in the distribution over second period beliefs. Since the

[53] For a quick proof for reference, note that if $\mathbb{P}[A \leq x] \leq \mathbb{P}[B \leq x]$, for all $x \in \mathbb{R}$, then for any monotone f we have $\mathbb{P}[f(A) \leq f(x)] \leq \mathbb{P}[f(B) \leq f(x)]$ for all x. Then we also have $\mathbb{P}[f(A) \leq y] \leq \mathbb{P}[f(B) \leq y]$, for all $y \in \mathbb{R}$—either y is in the image of f in which case this is immediate, or it is not in which case either both probabilities are equal to 0 or both probabilities are equal to 1.

expectation of an increasing function of a random variable increases when the random variable distribution increases in first order stochastic dominance, we have that this integral increases as well.

We have thus showed that if some signal s_1 prefers to register, then so do all higher types as well. Likewise, if some signal s_1 prefers to not register, then so do all lower types. It follows that the registration decision partitions the support of the first period signal, as desired. □

Proposition 6. *Under Assumption 1, the first period experimentation decision takes a partitional form.*

Proof. Consider the researcher's payoffs from early registration, (2), as well as late registration, 2. As the proof of Proposition 5 states, both of these expressions are increasing in s_1. Hence if some type s_1 does not prefer to undertake the experiment, then neither do any lower types, since this implies both of the expressions are negative at s_1 and are therefore also negative at higher s_1. Likewise, if some type s_1 prefers to undertake the experiment, then it means at least one of these is positive, and hence is also positive at higher s_1, as desired. □

Note that this proposition does not rely upon the increasing gains to early registration condition. Indeed, it only relies upon the assumption that s_1 signals make $\theta = T$ more likely, and that b_R, b_N are increasing.

The following simple Corollary, which follows immediately from the above proofs, illustrates that to show that a particular partition is in fact an equilibrium, it suffices to check the increasing gains to early registration condition:

Corollary D.4.1. *Consider the strategy arrived at via the following algorithm:*

- *First, compute the second period beliefs that are indifferent between late registration and not registering, and*
- *Second, compute the first period signal which makes the researcher indifferent between registration decisions, given this signal.*

If the increasing gains to early registration condition is satisfied, then these thresholds define a partitional equilibrium.

Proof. Immediate from the above; given the indifference thresholds, higher first period signals imply higher payoffs to undertaking the experiment, and higher payoffs to registration. Hence the indifference conditions suffice to charactrize the equilibria. □

This corollary is used in our calibration, since it allows us to avoid computing the second period signal threshold to check that a conjectured strategy is in fact an equilibrium. In contrast, the increasing gains to early registration condition is straightforward to check across ranges of parameters. In general, we note that the second period threshold will depend on the first period threshold, and visa versa, since the former influences the payoffs of registering early and the latter influences the beliefs of the outsider.

D.3 Main Text Comparative Statics Proofs

Proof of Proposition 1. Consider a fictitious environment where early registration convinces outsiders that $s_1 = \overline{s}_1$, noting that early registration will give researchers lower payoff than this. Hence the payoff from early registration is at most:

$$-c_R + \int_{-\infty}^{\infty} b_R(\hat{p}_{\overline{s}_1}(s_2))\mathbb{E}_\theta[g(s_2 \mid \theta) \mid s_1]ds_2. \tag{3}$$

Since the worst-case from late registration is that the first period signal is $\underline{s}_1$, we have that the from late registration is at least:

$$\int_{-\infty}^{\infty} \max\{b_R(\hat{p}_{\underline{s}_1}(s_2)) - c_R, b_N(\hat{p}_{\underline{s}_1}(s_2)\}\mathbb{E}_\theta[g(s_2 \mid \theta) \mid s_1]ds_2. \tag{4}$$

Under the assumption that $\hat{p}_{\overline{s}_1}(s_2) - \hat{p}_{\underline{s}_1}(s_2) < \delta$, for some δ, continuity of b_i gives us that we can find some ε such that (4) is greater than:

$$-\varepsilon + \int_{-\infty}^{\infty} \max\{b_R(\hat{p}_{\overline{s}_1}(s_2)) - c_R, b_N(\hat{p}_{\overline{s}_1}(s_2)\}\mathbb{E}_\theta[g(s_2 \mid \theta) \mid s_1]ds_2.$$

If this equation is larger than (3), then we have that the payoff from late registration is higher

than the payoff from early registration, which is our desired result. Subtracting this from (3) yields:

$$\varepsilon + \overbrace{\int_{-\infty}^{s_2^*} ((b_R(\hat{p}_{\bar{s}_1}(s_2)) - c_R) - b_N(\hat{p}_{\bar{s}_1}(s_2)))\mathbb{E}_\theta[g(s_2 \mid \theta) \mid s_1]ds_2}^{(*)}.$$

Now, in the limit we consider, we take the initial signal to be uninformative, but fix the informativeness of the second signal. While ε may be arbitrarily small given a sufficiently uninformative first period signal, $(*)$ is bounded away from 0 and negative, as long as c_R is chosen so that some types would not register in the second period. Hence, it follows that this difference is negative. Therefore, the researcher's payoff from registering late is larger than registering early.

Now, recall $\hat{p}_{s_1}(s_2)$ is increasing in s_2, for all s_1. It follows that for all s_1, since $b_R(p) \geq b_N(p)$, with strict inequality for some beliefs, we further have, for all s_1:

$$\kappa(s_1) := \int_{-\infty}^{\infty} b_R(\hat{p}_{s_1}(s_2)) - b_N(\hat{p}_{s_1}(s_2))\mathbb{E}[g(s_2 \mid \theta) \mid s_1]ds_2 > 0.$$

Hence, as the initial signal becomes uninformative, we have that $\kappa(s_1) \to \kappa$, for all s_1. It follows that as long as $c_R < \kappa$, researchers prefer registration at time 1 to non-registration. Note that at $c_R = \kappa$, the researcher must have second period beliefs which would lead them to strictly prefer registering, meaning that $(*)$ is negative provided c_R is not too low.

We now make the main comparison of interest: if late registration is allowed, then by the above, the payoff from late registration is larger than the payoff from early registration, so all researchers register late. On the other hand, we also argued that there exists c_R such that researchers would still be willing to register early if forced to do so. Thus, we have that all researchers register early in this instance. We conclude that banning late registration leads to all researchers registering early, whereas some researchers never register, as claimed.

The above argument takes $c_E = 0$, and analogous reasoning shows this still applies when $c_E > 0$. □

Proof of Proposition 2. Note that a late ban is equivalent to adding the following to this researcher's

payoff:

$$\int_{s_2^*}^{\infty} b_N(\hat{p}_2(s_2))\mathbb{E}_\theta[g_0(s_2 \mid \theta) \mid s_{1,R}^*] - (b_R(\hat{p}_2(s_2)) - c_R)\mathbb{E}_\theta[g_\gamma(s_2 \mid \theta) \mid s_{1,R}^*]ds_2.$$

By convexity, this term will increase if g_γ is replaced by g_0. On the other hand, by definition, when $s_2 > s_2^*$ we have $b_N(\hat{p}_2(s_2)) < b_R(\hat{p}_2(s_2)) - c_R$, so that the entire above expression is negative. Note that, by assumption, we cannot have $s_{1,R} = s_{1,\emptyset}$, since we are focused on the case where both counterfactuals lead to researchers registering late with positive probability.

Given this, we first show that weakly fewer experiments are conducted under a late ban, i.e. $s_{1,\emptyset}^*$ increases. Indeed, if this signal leads to a payoff of 0 from conducting an experiment when late registration is allowed, it therefore leads to negative payoff when late registration is banned. Hence the type that is indifferent between not conducting the experiment and conducting the experiment with delayed registration must increase.

Now consider $s_{1,R}^*$. Similarly, we have that now, these types strictly prefer to register. Note that $s_{1,R}^* = s_{1,\emptyset}^*$ coincides with the case where there is no delayed registration at all. By Assumption 2, the payoff of early registration in this instance is lower than delayed registration revealing the lowest type. Hence by the intermediate value theorem, there exists a new threshold value, say $\tilde{s}_{1,R}$ which makes the reseacher indifferent between delaying and not, with this threshold being between the original $s_{1,R}^*$ and $s_{1,\emptyset}^*$. □

Proof of Proposition 3. Follows the same reasoning as proposition 2; an increase in the informativeness of registration leads to a mean preserving spread in beliefs (see Blackwell (1953)). Hence because $b_{R,\gamma}$ is convex, the researcher who is indifferent between registering and not registering prior to an increase in the informativeness does stricly better by registering. The same reasoning allows us to conclude the threshold lowers. □

D.4 Additional Model Discussion

We first present some examples microfounding the increasing differences condition:

Example 1. *Suppose that whether publication ultimately occurs only depends on $\hat{p}_d(s_2)$, with*

this probability being denoted by $\pi(\hat{p}_d(s_2))$ for an increasing function $\pi(\cdot)$. However, the ultimate venue depends on registration; the expected value of a registered publication is β_R and the expected value of a non-registered publication is β_N. In this case, the increasing difference condition is satisfied, since the difference in payoffs is $(\beta_R - \beta_N)\pi(\hat{p}_d(s_2))$.

In the previous example, registration does not impact whether publication occurs, but it does impact the expected tier of the ultimate venue, for instance due to the AEA requirement that experiments register in order to be published. We can also consider the opposite case, where the tier of the final outcome is irrelevant, but registration leads to additional independent possibilities for publication (again, due to the fact that more possible journals are available).

Example 2. *Normalize the benefit of publication to 1, but suppose that the probability of publication is $1 - (1 - \pi(\hat{p}_d(s_2)))^{\beta}$, where $\beta = \beta_R$ when registered and $\beta = \beta_N$ when not registered, where $\beta_R > \beta_N$, for a differentiable and increasing $\pi(\cdot)$. Taking derivatives and simplifying, we have that the increasing difference condition is satisfied whenever:*

$$\beta_R(1 - \pi(\hat{p}_d(s_2)))^{\beta_R - 1} > \beta_N(1 - \pi(\hat{p}_d(s_2)))^{\beta_N - 1}$$

The expression $\beta(1-\pi)^{\beta-1}$ is increasing in β, for $\pi \in (0, 1)$, whenever $1+\beta \cdot \log(1-\pi) > 0$, which can be rewritten as $\pi < 1 - e^{-1/\beta}$. Hence, this is satisfied whenever the probability of publication is low, relative to the number of venues. Considering a case where $\beta_R = 5$ and $\beta_N = 4$ (an extreme view of the relative importance of top 5 publications), increasing differences reduces to the requirement that $\pi(\hat{p}_d(s_2)) < .2$ (note that this condition implies that the maximum probability of publication is less than 0.67).

To emphasize, these examples are simply meant as a way to assist the reader in calibrating the increasing differences assumption. This assumption is standard in the signaling literature, and the complementarity may come from other sources not explicitly considered in the above examples.

D.5 Numerical Calibration

In this Section, we describe details in showing that our specification satisfies increasing gains to early registration. We first write out the outsider's beliefs, given that the first period signal is in some interval $[s_*, s^*]$, under the particular experimentation technology, and observation of the signal s_2. This is:

$$\begin{aligned}\hat{p}_d(s_2) &= \frac{p_0 \int_{s_*}^{s^*} 2s_1 2s_2 ds_1}{p_0 \int_{s_*}^{s^*} 2s_1 2s_2 ds_1 + (1-p_0)\int_{s_*}^{s^*} 2(1-s_1)2(1-s_2)ds_1} \\ &= \frac{p_0((s^*)^2 - (s_*)^2)s_2}{p_0((s^*)^2 - (s_*)^2)s_2 + (1-p_0)((1-s_*)^2 - (1-s^*)^2)(1-s_2)} \\ &= \frac{p_0(s^* + s_*)s_2}{p_0(s^* + s_*)s_2 + (1-p_0)(2 - s_* - s^*)(1-s_2)}.\end{aligned}$$

Regarding the first period beliefs, we note that the expression for the first period is exactly the same as the previous expression in the special case where $s_2 = 1/2$. Thus, the highest possible belief corresponds to the case where $s^* = s_* = 1 - \overline{s}$, meaning that the threshold is less than:

$$\frac{p_0(1-\overline{s})}{p_0(1-\overline{s}) + (1-p_0)\overline{s}},$$

which approaches p_0 as $\overline{s} \to 1/2$ and 1 as $\overline{s} \to 0$. We numerically verify that the condition in Proposition 5 is satisfied for all p_0 and possible first period belief thresholds. This calculation is done in Mathematica and is available from the authors' webpage or upon request.

E RA Instructions

E.1 Restrictiveness

Rubric for assessing pre-registration restrictiveness:

Use the Trial History button to get to the last pre-registry version before the Intervention Start Date with a +1 week buffer.

Primary Outcomes

- Number of outcomes listed ____

Note: Be mindful of indices. In some cases, PIs may list the variables which make up an index to be more specific. In these cases, the index itself should be counted as one primary outcome variable and the variables that make up the index should not be counted. Some of this information may appear in the "Primary Outcomes (explanation)" field.

- Specificity of outcomes listed

Score each outcome based on the example scale below and report the

  - Minimum ____
  - Maximum ____
  - Median ____

Example Scale: Mark "health" as a 0, "nutritional intake" as a 1, "number of fruits consumed" as a 2, "number of fruits consumed at school per week" as a 3, "number of fruits consumed at school per week during Spring quarter" as a 4, and "number of bananas consumed at school per week during Spring quarter" as a 5.

- Did the number of outcomes or their descriptions change after the Intervention Start Date?
  - Yes = 1
  - No = 0

Notes: Please click on View Changes and check that significant changes have been made. Minor semantic changes or typos do not count as changes.

Sample Information (found in Experiment Characteristics under Experimental Details):

- Estimate or prediction for final sample size____

Use field ***Sample size: planned number of observations.*** Put 0 if a specific number is not given

- Number of populations used ___

Add 1 for each population used.
For example, Put 3 if the analyses are run for all, then for men, then for women

- Did the sample size or sample splits change after the Intervention Start Date?
  - Yes = 1
  - No = 0

E.2 Fidelity

Rubric for assessing fidelity of working/published paper to registration

Compare latest version of the paper available to the pre-registered version assessed above. You will likely need to search for the paper by title and then by authors. Titles will change.

Primary Outcomes

- Fraction of variables whose construction remains true to the pre-registry ___

  Example:

  - If 1 out of 5 variables changes, then report 0.80
  - The construction of a variable changes if the pre-registration lists "number of bananas consumed at school per week during Spring quarter" but the paper reports "number of bananas consumed at school per week during summer".

- Number of primary outcomes introduced in the paper but not previously registered ___

- Number of primary outcomes listed in the registry but not in the paper ___

Note: For this section, a primary outcome is a variable mentioned in the abstract, introduction, or conclusion.

Sample Information

- Number of observations reported in the paper ___

- Number of populations introduced in the paper, but not registered ___

For example, the paper may repeat analyses for rich household and for poor households. If these sub-populations are not mentioned in the preregistration, then put 2.

- Number of populations listed in the registry, but not mentioned in the paper ___

E.3 Experimental Economics Registrations

We instructed an RA to:

- Assemble a list of RCTs published in Experimental Economics between the years 2016 and 2019
- Find registrations corresponding to these RCTs via
    1. Searching Google for the paper title plus the word "register"
    2. Searching three registries (https://ridie.3ieimpact.org/, https://egap.org/registry/, and https://cos.io/prereg/) for the paper title and for the authors

F ClinicalTrials.gov after the Final Rule

Section 801 of the Food and Drug Administration Amendments Act of 2007 (FDAAA 801) established requirements for clinical trials to register with and report results to ClinicalTrials.gov. These requirements were clarified and expanded by the the Final Rule for Clinical Trials Registration and Results Information Submission (Final Rule), which went into effect on January 18, 2017. See ClinicalTrials.gov for a comprehensive overview.

FDAAA 801 and the Final Rule may have increased the restrictiveness of trials' preregistrations with ClinicalTrials.gov along with the fidelity of trials' results to their preregistrations. To examine this hypothesis, we repeat our assessment of ClinicalTrials.gov for the year following the effective date of the Final Rule.[54] Specifically, we randomly sample 100 ClinicalTrials.gov preregistrations that occurred between January 18, 2017 and December 31, 2017. We then instruct two RAs to assess each preregistration and associated working or published paper using the same rubric as in Section 4.

Table XI reports our assessment of the extent to which these preregistrations precisely specify their primary outcomes. We find that the preregistrations over January 18, 2017 to December 31, 2017 are slightly *less* specific than over the March 1, 2000 to July 1, 2005 period examined in Section 4. We also find that fewer trials changed their outcome or sample—likely due to the shorter follow-up period here.

Table XII reports our assessment of the fidelity of working or published papers associated with the preregistrations. We identify working or published papers for 37 of the 100 preregistrations. These papers show slightly more fidelity to the preregistrations than those from the earlier March 1, 2000 to July 1, 2005 period. 90% of the primary outcomes reported by the average paper matched the construction specified in the preregistration, but the average paper still reported 0.36 additional primary outcomes and failed to report 0.15 primary outcomes.

[54] This choice provides at least two years of follow-up for all trials.

Table XI: Assessment of the extent to which 100 randomly chosen ClinicalTrials.gov preregistrations precisely specify their primary outcomes

	Mean	Std	Min	10%	25%	50%	75%	90%	Max
Number of Outcomes	1.87	1.59	1.0	1.00	1.0	1.0	2.00	3.10	8.0
Minimumly Restrictive Outcome	2.60	1.10	0.5	1.50	1.5	2.5	3.12	4.05	5.0
Maximumly Restrictive Outcome	2.84	1.14	0.5	1.50	2.0	3.0	3.50	4.50	5.0
Median Restrictive Outcome	2.70	1.13	0.5	1.45	2.0	2.5	3.50	4.50	5.0
Outcome Changed (Yes/No)	0.12	0.28	0.0	0.00	0.0	0.0	0.00	0.50	1.0
Sample Changed (Yes/No)	0.34	0.46	0.0	0.00	0.0	0.0	1.00	1.00	1.0

Notes: Preregistrations were randomly sampled from the period January 18, 2017 and December 31, 2017. This period corresponds to the first year after the implementation of the Final Rule for Clinical Trials Registration and Results Information Submission (42 CFR Part 11). Each registration was assessed by two RAs. The values presented are based on the average of the two assessments.

Table XII: Assessment of the extent to which working and published papers report the primary outcomes preregistered with ClinicalTrials.gov

	Mean	Std	Min	10%	25%	50%	75%	90%	Max
Fraction of Matching Outcomes	0.90	0.25	0.0	0.71	1.0	1.0	1.0	1.00	1.0
Number of Additional Outcomes	0.36	0.64	0.0	0.00	0.0	0.0	0.5	1.25	2.0
Number of Missing Outcomes	0.15	0.33	0.0	0.00	0.0	0.0	0.0	0.75	1.0

Notes: Working or published papers were found for 37 of the 100 preregistrations.