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## Randomizing ... What? A Field Experiment of Child Access Voting Laws

by

Daniel E. Ho\*

We explore randomizing legal information when it may be infeasible to randomize law per se. If citizens are underinformed about a legal entitlement, randomizing information about the entitlement may yield critical insight into its potential effect. We illustrate with a field experiment with the League of Women Voters of Georgia in the 2008 general election. We randomly informed roughly 10,000 of 20,000 recently registered mothers of young children about their statutory right to bring their child into the voting booth. We find the treatment had a moderate (but statistically insignificant) turnout effect, but caused a (statistically significant) shift toward early voting. (JEL: C93, D72, K00)

### *1 Introduction*

Numerous scholars have advocated widescale field experiments in law (e.g., Abramowicz, Ayres, and Listokin, 2011; Chilton and Tingley, 2013; Greiner and Patanayak, 2012; Madison, 2014; Manski, 2013; Ouellette, 2015; Walker, 1988). Abramowicz, Ayres, and Listokin (2011), for instance, lucidly advocate “randomly assigning individuals or firms to different legal rules” (p. 929). These proposals respond to an increasing recognition of the fragility of causal inferences about law. Observational studies may not meet conditional exogeneity or may have little power, as laws rarely change and then only highly nonrandomly after considerable political and legal wrangling (Angrist and Pischke, 2010; Ho and Rubin, 2011). Laboratory experiments may lack external validity. Bringing randomization directly into the

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legislative and administrative realm would indeed do wonders for evidence-based law and policy. In the first-best world, field experiments would be widely adopted.

Yet what if randomizing law per se proves infeasible, intractable, or ethically fraught? This article explores an alternative approach, which preserves the benefits of randomization in actual legal settings, while circumventing many of the well-known challenges to randomizing legal entitlements. The approach capitalizes on decades of research documenting that research subjects are sorely underinformed about their legal entitlements.<sup>1</sup> Adapting Angrist, Imbens, and Rubin (1996), we can therefore bring one element critical to law's influence under the researcher's control: information. Some (potentially large) subset of the population may act as if the law did not exist. Randomizing information about a legal entitlement and observing real-world outcomes enables researchers to learn about the law's potential influence on individuals whose legal awareness is randomly activated.

We illustrate this approach with a field experiment involving election law. We focus on *child access laws*, which provide voters the right to bring their child into the voting booth. These laws, which exist in roughly half of U.S. states, and vary widely across time and jurisdiction, are animated, at least in part, by the desire to reduce the cost of voting to parents. Randomly assigning parents the ability to bring children into the voting booth would be politically infeasible. It also is not necessary, as many parents are unaware of the law. With the League of Women Voters of Georgia, we designed an intervention to randomly inform roughly 10,000 recently registered mothers of the right to bring their child into the voting booth. Collecting individual-level turnout information, we find weak evidence that the intervention increased turnout, but stronger evidence that it shifted mothers to vote early, in person. These results shed light on one of the principal motivations for child access laws.

This article proceeds as follows. Section 2 discusses well-known limits to large-scale social experiments that directly attempt to randomize law. Section 3 adapts ideas from research designs that use a randomized intervention as an *instrument* for the researcher to gain control over the treatment assignment. Randomizing information similarly allows the researcher to manipulate a subject's awareness of a legal entitlement. To illustrate, section 4 designs a field experiment involving child access laws in the voting context. Section 5 discusses results and interpretation. Section 6 concludes with substantive and methodological implications.

## 2 Limits to Randomizing Law

In one sense, the proposal to randomize law is ambitiously forward-looking. In another, it appears retrograde. In the 1960s and 1970s, government agencies directly participated in large-scale experiments that randomized individuals to divergent policies (Greenberg and Shroder, 1998). Major experiments included ones on the negative income tax, employment programs, electricity pricing, health insurance,

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<sup>1</sup> The findings are legion and far too numerous to cite, with canonical work by Macaulay (1963) and Ellickson (1991).

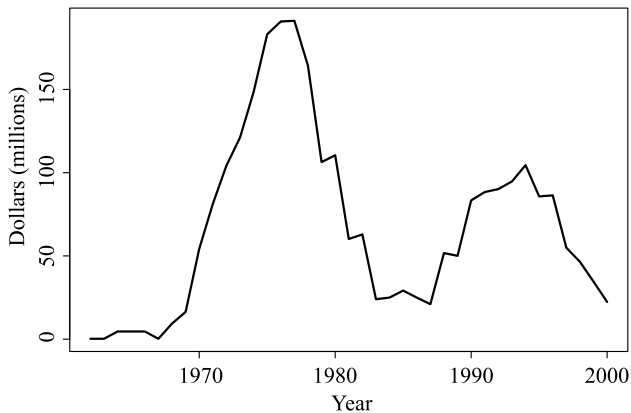
and housing allowances. It was the era of social experiments. Yet for numerous reasons, the era came to a sharp halt.

First and foremost was cost. Randomizing individuals to different policies proved to be extraordinarily expensive. Figure 1 displays estimates compiled from Greenberg and Shroder (1998) of (inflation-adjusted) expenditures on social experiments over time. RAND's Health Insurance Experiment cost roughly \$640 million in present dollars. Thirteen "Era I" experiments – those initiated between 1968 and 1975 – cost nearly \$2 billion in present dollars (Greenberg and Robins, 1986).

Second, randomization can be politically contentious. A coalition of activists opposed to welfare work requirements, for instance, brought suit against the Massachusetts Work Experience Program in 1978, disrupting treatment assignment and ultimately resulting in modification of eligibility requirements (Friedman, 1981). In another example, the Wisconsin Department of Health and Human Services contracted with the University of Wisconsin, Milwaukee, to provide an independent evaluation of a state program designed to boost school attendance. But upon learning of unfavorable results, the department demanded that key portions of the report be suppressed and canceled the contract (Quinn and Magill, 1994).

Third, substantial methodological challenges threaten internal validity, chiefly due to the difficulty of controlling the environment on a grand scale. One of the two principal subgroups in the Gary Income Maintenance Experiment, for instance, exhibited 35% attrition (Hausman and Wise, 1979, p. 462). In the National Supported Work Demonstration, 20–40% of participants were not available to complete final interviews (Manpower Demonstration Research Corporation, 1980, p. 46). More recently, Tennessee STAR randomized students in kindergarten through third grade to large and small classrooms in the 1980s. Though Mosteller (1995, p. 113) called it "[o]ne of the most important educational investigations ever carried out,"

Figure 1  
Expenditures on Social Experiments



Hanushek (2003, p. F89) notes that attrition rates of up to 50%, noncompliance, and nonresponse make the experiment “limited and flawed.” Hawthorne effects might have been acute when teachers in Tennessee knew that future budgetary allocations would turn on the results of the experiment.

Last, because settings amenable to randomization are highly nonrandom, field experiments pose substantial external validity problems. Effects in jurisdictions open to randomization may differ sharply from those in jurisdictions hostile to it. Experiments may also only provide partial-equilibrium effects. Qualified teachers, for instance, may be limited, so that making *all* classrooms smaller would yield effects quite different from the experiments.

The challenges that moderated grand social experiments equally plague the potential for randomizing law at a large scale. Indeed, most proponents recognize these limitations, with Ayres (2011) contemporaneously dubbing the proposal to randomize law “outlandish” (p. 1782). As a practical matter, researchers must find alternatives.

### 3 A Way Forward

We explore an alternative approach to learning about the causal effect of law by leveraging a simple insight: in many settings, individuals know shockingly little about their legal entitlements. If researchers can induce research subjects to be aware of an entitlement that they would otherwise think does not exist, we may be able to approximate (at an individual level) the ideal randomized experiment for a subset of the population. And even if individuals have some awareness of the law, randomly heightening its salience may still provide crucial insight into its effect.

To understand the informational intervention, it is helpful to place it in a formal framework of causal inference. Angrist, Imbens, and Rubin (1996) reinterpret conventional instrumental variables (IV) estimates in a potential outcomes framework. The best instrument is randomized and affects the probability of treatment, but the same framework can be applied to the informational context.

Let  $Z_i = 1$  if the *information* was randomized to subjects  $i = 1, \dots, N$ , and 0 otherwise. Let  $T_i = 1$  if the subject has *knowledge* of the legal entitlement, and 0 otherwise. Compliance behavior is characterized by the subject’s awareness of the legal entitlement in response to the information.  $D_i(z)$  equals 1 if subject  $i$  would be informed given a value of  $z$ , and 0 otherwise. Four types of subjects exist:

$$C_i = \begin{cases} c & \text{if } D_i(z) = z & \text{(compliers)} \\ n & \text{if } D_i(z) = 0 & \text{(never-takers, or disengaged)} \\ a & \text{if } D_i(z) = 1 & \text{(always-takers, or always-engaged)} \\ d & \text{if } D_i(z) = 1 - z & \text{(defiers).} \end{cases}$$

Define potential outcomes  $Y_i(z, D_i(z))$  under each assignment. The intention-to-treat effect (*ITT*) is then defined as a weighted average of  $ITT_i$  effects for each

subtype:

$$ITT = \sum_{t \in \{c, n, a, d\}} \frac{ITT_t \times N_t}{N},$$

$$ITT_t = \sum_{i \in \{i: C_i = t\}} [Y_i(1, D_i(1)) - Y_i(0, D_i(0))] / N_t,$$

where  $t \in \{c, n, a, d\}$ , and  $N_t$  denotes the number of respondents in each subtype.

In the IV setting, the inferential target is  $ITT_c$ , the causal effect on the (latent) subset of *compliers*: i.e., those subjects whose treatment exposure is determined by the randomized instrument.<sup>2</sup> In our information setting, the compliers are the subjects who would be aware of a legal entitlement solely because of the randomized information. The *disengaged* (or never-takers) are the subjects who remain unaware of the law regardless of  $z$ ; the *always-engaged* (or always-takers) are the subjects who are already aware of the legal entitlement (regardless of  $z$ ); and the *defiers* are individuals who would be aware only if randomized out.

One way of conceiving of the informational experiment is to use covariate information to identify, as closely as possible, the target population (the compliers) a priori: i.e., individuals who are unaware but would be informed about the legal entitlement by the intervention.<sup>3</sup> Although the interpretation of results is more complicated than in the ideal experiment, the approach has the virtue of potentially allowing the researcher to exogenously vary the perception of legal entitlements.<sup>4</sup>

There are numerous foundational questions in law for which it would be infeasible to randomize law itself, but feasible to randomize legal information. To study deterrence, we might inform would-be criminals about heightened sanctions. To study the influence of tort liability, we might randomly inform medical practitioners about the potential for malpractice liability for certain practices. To study the influence of contractual provisions, we might randomly heighten consumer awareness of specific provisions (e.g., mandatory arbitration). And to study intellectual property, we might randomly inform software startups about an existing patent infringement suit against a firm holding related technology.

<sup>2</sup> The identification assumptions in this framework are: (i) no interference among units ( $Y_i(z, D_i(z)) \perp\!\!\!\perp Z_{i'} \text{ for all } z = 0, 1, \text{ where } i' \neq i \text{ and } \perp\!\!\!\perp \text{ denotes independence}$ ); (ii) random assignment of instrument ( $Y_i(z, D_i(z)), D_i(z) \perp\!\!\!\perp Z_i \text{ for all } i \text{ and } z = 0, 1$ ); (iii) monotonicity ( $D_i(1) \geq D_i(0)$  for all  $i$ ); and (iv) exclusion restrictions on always- and never-takers ( $P(Y_i(1, D_i(1)) = 1 | X_i, C_i \in \{a, n\}) = P(Y_i(0, D_i(0)) = 1 | X_i, C_i \in \{a, n\})$ ).

<sup>3</sup> One could of course also design an intervention for the entire population and measure knowledge to stochastically identify compliers.

<sup>4</sup> Because the experiment below aims to construct a target population likely to be unaware, we do not distinguish between intention-to-treat effects and treatment effects in our application below. To be precise, even within the experiment's target population, there is still a subgroup of compliers. Engel's discussion of this article underscores that the intention-to-treat effects can deviate substantially from complier average causal effects.

While the approach has not necessarily been conceived of as an alternative (or complement) to randomizing law, prior studies have adopted similar designs. Rigotti et al. (1992) randomized information about a new no-smoking law to businesses in Brookline, MA. Chetty and Saez (2013) randomized personalized information about how the Earned Income Tax Credit works to 43,000 recipients at H&R Block. Fellner, Sausgruber, and Traxler (2013) randomly sent out letters about enforcement steps to Austrian households suspected of evading a TV license fee. Ridgeway et al. (2011) randomized a letter disclosing gun-ownership obligations to individuals in the waiting period to purchase a gun. Closest to our context are Gerber et al. (2013), which randomized letters informing first-time voters in Connecticut about secret-ballot protections, and Pang, Zeng, and Rozelle (2013), which randomized subjects to training about voting rights in Chinese village elections.

The information approach is also closely related to bringing randomization into the *implementation* of laws, for instance via randomized rollout into jurisdictions (see, e.g., King et al., 2007). Informational experiments can be conceived of as introducing randomization into the final implementation step of disseminating information about a legal change.<sup>5</sup>

#### 4 Field Experiment

Here we describe a field experiment that explores this general approach in the specific context of election law. As is the case for a wide variety of legal settings, Americans know little about voting rights and political institutions (Carpini and Keeter, 1996). Basic facts, such as the secret ballot, do not appear commonly known (Gerber et al., 2013). Even election officials are plagued by ignorance. Wood and Bloom (2008) document, for instance, that not a single one of 95 officials in Tennessee could list requirements for restoring voting rights to an individual with a criminal conviction. Section 4.1 provides background on little-known child access laws. Section 4.2 describes our experimental design, which builds on a long experimental tradition assessing political campaign tactics (Green, McGrath, and Aronow, 2013).

##### 4.1 Background on Child Access Laws

Georgia law, Ga. Code Ann. § 21-2-413, provides that:

“[A]ny elector shall be permitted to be accompanied into the enclosed area and into a voting compartment or voting machine booth while voting by such elector’s child or children under 18 years of age or any child who is 12 years of age or younger unless the poll manager or an assistant manager determines in his or her sole discretion that

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<sup>5</sup> The approach is also similar to the framework proposed by Greiner and Rubin (2011) for studying the effects of immutable characteristics (e.g., gender, race). Greiner and Rubin note that randomization of immutable characteristics is infeasible, but that randomizing the perception of immutable characteristics provides a much more credible experimental template.



such child or children are causing a disturbance or are interfering with the conduct of voting.”

Figure 2  
Child Access Laws across the Continental United States

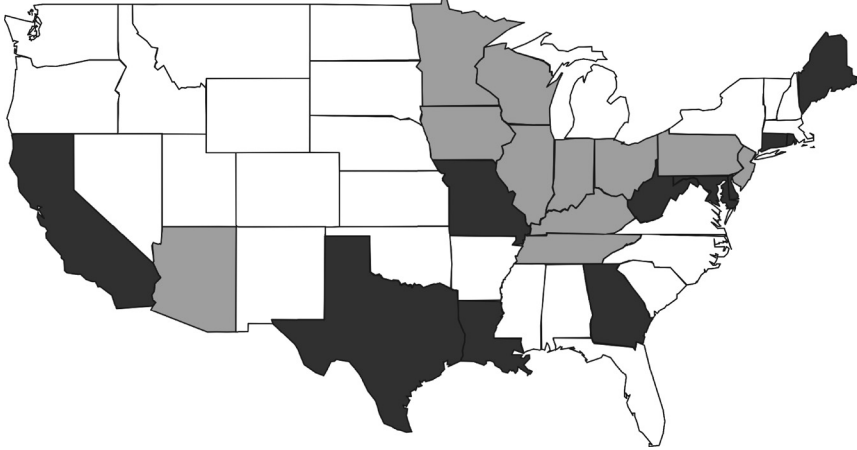


Figure 2 plots whether or not a state has a formal child access law. Shaded areas indicate states that affirmatively grant access to voters’ children into the voting booth. Dark shading indicates that the age cutoff upon initial enactment was below 17 years, and light shading indicates that the cutoff was 17 years. (Alaska and Hawaii do not have child access statutory provisions.) The lack of a formal statutory provision does not necessarily mean that children are affirmatively prohibited from the voting booth;<sup>6</sup> in many jurisdictions without formal provisions, discretion falls to the poll worker. The top panel of Table 1 shows which states have enacted child access laws. The second column indicates the maximum age of the child when enacted (or amended) against year on the x-axis (as denoted by the labels at the bottom of the panel). Maine, for instance, enacted a child access law in 1985, limiting the age of the child to 6 years, and increased it to 12 years in 1993 and 17 years in 1999. The third column indicates whether access is limited exclusively to a voter’s own child, and the fourth column indicates whether poll workers retain discretion to exclude the child, as in Georgia. The bottom panel lists the 25 jurisdictions that do not have formal child access provisions.

<sup>6</sup> For example, although Ohio has no formal provision, the state attorney general issued an advisory opinion (2012 Ohio Op. Atty. Gen. 2012-029) that authorizes voters to bring their minor children into the voting booth. Other states, such as Vermont (Vt. Stat. Ann. tit. 17 § 2569) have a general assistance provision that allows any voter to be assisted by “a person of the voter’s choice.”

Table 1  
Child Access Laws

State	Age limit (period of applicability in dashes --)		Exclusively family	Poll-worker discretion
	1977	2010		
Maryland	5	11	no	yes
Wisconsin	17	13	yes	no
California	13	17	no	no
Maine	6	17	no	no
Texas	10	17	yes	no
Missouri	12	17	no	no
Connecticut	10	15	yes	no
Louisiana	12		yes	no
Pennsylvania		17	yes	no
Indiana		17	no	no
Georgia		12	no	yes
Delaware		10	no	no
Rhode Island		13	no	no
Virginia		12	yes	no
New Jersey		17	yes	no
Tennessee		17	yes	no
New York		15	yes	yes
Kentucky		17	no	no
Michigan		17	no	no
West Virginia		14	yes	no
Arizona		17	no	no
North Carolina		17	no	no
Iowa		17	no	no
Minnesota		17	yes	no
Illinois		17	yes	yes

States without Child Access Laws

Alabama, Alaska, Arkansas, Colorado, D.C., Florida, Hawaii, Idaho, Kansas, Massachusetts, Mississippi, Montana, Nebraska, Nevada, New Hampshire, New Mexico, North Dakota, Ohio, Oklahoma, Oregon, South Carolina, South Dakota, Utah, Vermont, Washington, Wyoming

Notes: The top panel lists states granting access to children in the voting booth. "Age limit" indicates the cutoff at the year of enactment or amendment, ranging from 1977 to 2010, on the axis labeled at the bottom of the panel. "Exclusively family" indicates whether access is limited to the voter's own children (or, in one case, grandchildren). In other jurisdictions the voter may bring any child under the age limit into the voting booth. "Poll-worker discretion" indicates whether the statute provides for poll-worker discretion to deny a child entry to the voting booth. New York was coded as a "yes," although the statute does not explicitly grant the poll-worker discretion. It is implicit, though: "Any voting parent or guardian [...] shall provide appropriate supervision so as not to allow a child to interfere with the orderly process of voting." The bottom portion of the table lists states without statutory provisions for child access.

Sources: Ariz. Rev. Stat. Ann. § 16-515; West's Cal. Elec. Code § 14222; Conn. Gen. Stat. Ann. § 9-261; Del. Code. Ann. tit., 15 § 4937; Ga. Code Ann. § 21-2-413; 10 Ill. Comp. Stat. 5/17-8; Ind. Code § 3-11-11-8; Iowa Code § 49.88; Ky. Rev. Stat. Ann. § 117.235; La. Rev. Stat. Ann. § 18:563; Me. Rev. Stat. Ann. tit., 21-A § 681; Md. Code. Ann. Elec. Law § 10-308; Mich. Comp. Laws § 168.736a; Minn. Stat. § 204C.08; Mo. Stat. § 115.409; N.J. Stat. Ann. § 19:52-3; N.Y. Elec. Law § 8-106; N.C. Gen. Stat. § 163-166.3; 25 Pa. Cons. Stat. § 3531.1; R.I. Gen. Laws. § 17-19-53; Tenn. Code. Ann. § 2-7-103; Tex. Elec. Code Ann. § 64.002; Va. Code Ann. § 24.2-643; W. Va. Code § 3-1-37; Wis. Stat. § 6.80.

While locating detailed legislative history for these provisions is difficult, at least one justification of child access laws is to facilitate voting by parents. State representative Emory Morsberger introduced an amendment to Georgia's child access bill in 1992 in response to a constituent's complaints about having to leave her child unattended to cast a vote. Said Morsberger: "Nothing should keep a person from voting" (Vickers, 1992). Delaware state representative Deborah Hudson similarly sponsored a bill to increase the age cutoff after hearing complaints from mothers that

(a) poll workers had stopped them from bringing children into the voting booth, and (b) the need to find a babysitter would deter them from voting.<sup>7</sup> To be sure, another primary motivation for enacting child access laws, one which we do not address here, is civic education, namely, to instill democratic values in children.<sup>8</sup> However, age cutoffs *below* 17 years, which existed for roughly half of the jurisdictions with child access provisions at the time of enactment, underscore that civic education is not the exclusive justification for these laws.

Media reports corroborate the increased logistical difficulties of turning out to vote on Election Day with childcare responsibilities. One woman explained that “[as] a single parent mom [...] I can understand how some people don’t vote because it’s on the back burner” (Halpern, 2013). Aldrich (1993) notes that offering “an hour’s worth of child care” may alter the calculus for turnout (p. 264). More systematically, Wolfinger and Wolfinger (2008) show, using CPS data from 2000, that children are associated with a 4 to 11% reduction in turnout, depending on family structure.<sup>9</sup> CPS data from the 2008 election indicates that amongst nonvoters, parents are 5.5% more likely than nonparents to cite being “too busy” as the principal reason ( $p$ -value  $< 0.001$ ).

Evidence also suggests that people are generally unaware of the existence and content of child access laws. In Virginia, which has had a child access law on the books since 1993, a leader of a women’s rights groups nonetheless opined that female turnout was affected by the fact that “you can’t find a babysitter” (Lee, 2007). Given the considerable variability in age cutoffs and eligibility across jurisdictions and time, confusion is not surprising. In 2002, when one Maryland resident successfully brought his 11-year old son to the voting booth (despite Maryland’s prohibition of children then above the age of 10), he said, “I didn’t know it was a law on the books. Nobody said anything” (Thomas-Lester, 2002). Poll workers do not appear to have uniform views on child access either. One noted that “as a poll worker for the past 20 years, I cringe when I see a child entering the voting booth” (Donner, 2000). The Lawyers’ Committee for Civil Rights under Law (2008) reports that despite a child access provision in Pennsylvania, one voter’s child was denied access. Given these attitudes, not just by poll workers but possibly also by other voters, even if parents were loosely aware that they *could* bring a child to vote, *knowing* that a formal statute affirmatively grants them that right may increase their propensity to vote.

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<sup>7</sup> Telephone interview, Sep. 22, 2008.

<sup>8</sup> See N.Y. Elec. Law § 8-106: “[...] in order to facilitate education and foster early participation in the electoral process, any persons younger than the age of sixteen on Election Day shall be permitted to accompany a duly qualified voting parent or guardian into the appropriate polling place and voting booth for the specific purpose of observing that parent or guardian vote, and for the general purpose of observing the electoral process.”

<sup>9</sup> To be sure, not all scholars agree on the effect of children on turnout (Jennings, 1979).

## 4.2 Design

*Target Population.* Our target population consisted of the subpopulation of voters most likely to be affected by Georgia’s child access law, while simultaneously unaware of it. We procured data files maintained by MSHC Partners, which aggregates voting records, census information, and other public records, to develop selection criteria for our sample. We focused on recently registered mothers, with children under the age of 14, as they were least likely to be familiar with rules and norms at the precinct. Because the sample size was small for voters meeting all attributes, we defined three strata of voters that would yield a sample size with sufficient statistical power. Table 2 provides characteristics of the three strata defined by covariates. While stratum 2 is composed of mothers who voted before 2006, their children are sufficiently young to make awareness of child access rules unlikely.

Table 2  
Three Strata of Target Population

Stratum	Registration	Voted	Max. age mother	Max. age child	<i>N</i>
1	2004–2008	no	48	14	14,584
2	2004–2008	before 2006	–	4	1,266
3	2002–2004	no	48	14	4,221

*Notes:* Registration refers to earliest registration. All subjects were required to have valid, current addresses.

*Intervention.* To develop an intervention that would be effective at communication, we identified an organization that would be particularly credible to our target population. The League of Women Voters of Georgia happily participated in the study. We considered randomizing two versions of the postcard (one advertising the child access law and the other simply reminding individuals of the election date), but decided against it. The primary reason was practical: given a fixed budget, we could either increase the size of the treatment group or have two smaller treatment arms. Aiding that decision was a preliminary meta-analysis of the effect of mailers in existing field experiments, which might approximate the baseline postcard. (We conduct a formal meta-analysis and discuss interpretation below.) Figure 3 displays the postcard we designed and sent to 10,000 voters in Georgia around October 20. To verify delivery, one postcard was sent to the executive director of the League of Women Voters of Georgia, so the actual number of “treated” individuals is 9,999.


*Early-Voting Complication.* One considerable complication that arose in the middle of implementing the field experiment was an unexpectedly large expansion of early voting. Because Georgia’s outcome information does not draw a distinction, we use “early voting” to refer to any non-Election Day vote, including in-person

Figure 3  
Postcard Randomly Sent to 9,999 Voters in Georgia in October 2008

Back

You can bring your children  
into the voting booth.<sup>1</sup>

**If you have any questions about voting, please call the  
League of Women Voters at (678) 547-0755.**




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Remember to vote on November 4.

STA0801 |  

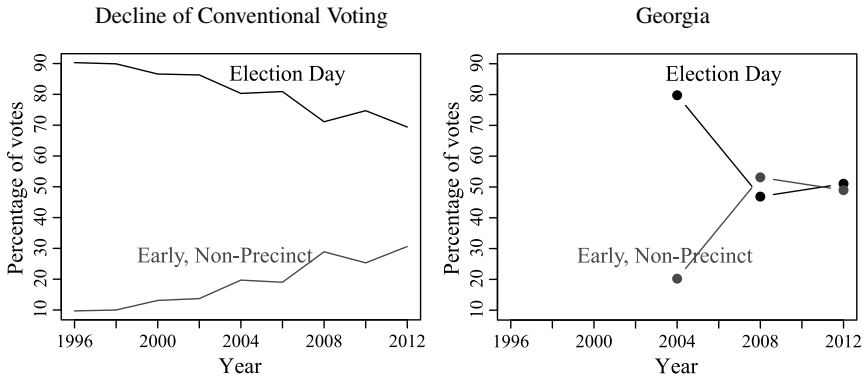


*To Do:*

1. get gas
2. pick up groceries
3. pick up babysitter

*(then go vote!!!)*

Figure 4  
Early Voting over Time



and absentee voting. The vast majority of early voting in Georgia in 2008 took place in person.<sup>10</sup>

In 2003, Georgia introduced early, in-person voting for any citizen to vote during the week before the election.<sup>11</sup> Absentee voting was “excuse-only” – applying, for instance, to the elderly, disabled, observant – and thereby very limited.<sup>12</sup> In 2004, 20% of voters cast early votes. In May 2008, the early, in-person voting period was extended from one week to 45 days before Election Day (i.e., beginning Sep. 23).<sup>13</sup> The number of citizens voting early and in person was unexpectedly high, with early voting jumping to 53.1% of voters. Early voting is generally known to be clustered at the beginning and end of the early-voting period.<sup>14</sup> Election Day was on November 4.

The left panel of Figure 4 uses data from the Early Voting Information Center/Current Population Survey to show that nationwide, the percentage of early voters has grown steadily since the mid-1990s. The right panel, however, shows the sharp, discontinuous jump for the 2008 election in Georgia, based on data from the Associated Press (2004) and Georgia’s Secretary of State Georgia Secretary of State Elections Division (2008). Because of this complication, we examine both Election Day and early-voting effects separately below.

<sup>10</sup> The Georgia Secretary of State’s Office tabulated the aggregate number of votes that were early in person and by mail. As of the day before the election, there were 2,020,839 votes cast, of which 1,778,317 were in person (88%) and 242,522 (12%) were by mail. See an archived version of the webpage here: <https://web.archive.org/web/20081119220428/http://sos.georgia.gov/elections/earlyvotingstats08.htm>.

<sup>11</sup> Act of June 2, 2003, Act 209, sec. 35, 2003 Ga. Laws 517 (codified at Ga. Code Ann. § 21-2-380).

<sup>12</sup> Ga. Code Ann. § 21-2-384 (1989).

<sup>13</sup> Act of May 12, 2008, Act 531, sec. 1, 2008 Ga. Laws 448 (codified at Ga. Code Ann. § 21-2-380).

<sup>14</sup> Email from Chris Mann, MSHC.

*Balance.* Table 3 presents randomization checks, confirming that the postcard was indeed randomly assigned. The first two columns present the means of covariates in treatment and control groups, with the pooled SD and  $p$ -value from a difference-in-means test in the third and fourth columns. There are no substantial differences between treatment and control groups.

Table 3  
Balance Statistics for Sample

	Covariate	Treated mean	Control mean	SD (pooled)	$p$ -value
	Age	30.19	30.12	7.36	0.51
	Median income (\$1000s)	66.00	66.00	62.87	0.97
	Bachelor's degree likelihood	0.41	0.41	0.21	0.40
	Recently moved	0.08	0.08	0.27	0.59
<i>Marriage</i>	Married	0.22	0.22	0.42	0.58
	Single	0.20	0.20	0.40	0.27
	Unknown	0.58	0.57	0.49	0.17
<i>Ethnicity</i>	White	0.67	0.66	0.47	0.44
	African-American	0.25	0.25	0.43	0.18
	Latino	0.04	0.04	0.20	0.55
<i>Religion</i>	Catholic	0.13	0.12	0.33	0.40
	Protestant	0.27	0.27	0.45	0.94
<i>Child</i>	0–3 years	0.59	0.60	0.49	0.39
	4–7 years	0.39	0.39	0.49	0.79
	8–12 years	0.19	0.19	0.39	0.91
<i>Voting</i>	Registered after 2004	0.79	0.79	0.41	0.92
	Registered after 2006	0.36	0.36	0.48	0.80
	Previous electoral participation	0.08	0.08	0.07	0.14
	Vote prediction (Catalist)	0.51	0.51	0.21	0.33

*Notes:* SD is the standard deviation pooled for both treatment and control groups.  $p$ -value is from a difference-in-means ( $t$ ) test. Electoral participation is the percentage of elections in which an adult voted. The Catalist vote prediction is a model-based estimate of the probability of voting. Georgia does not have partisan voter registration, so partisan affiliation is not an available covariate. Due to skewness in estimates, median income is reported.

## 5 Results

Table 4 presents basic results. The first row presents the count and percentage of voters turning out in the treatment and control groups. The last column presents the average treatment effect of roughly 1 percentage point ( $p$ -value = 0.14). Due to

Table 4  
Turnout Effects

	Count		Percentage		Effect of postcard
	Control	Treated	Control	Treated	
Overall turnout	5,575	5,640	55.4	56.4	1.04 (0.70)
Election day	3,919	3,887	38.9	38.9	-0.05 (0.69)
Early	1,656	1,753	16.4	17.5	1.09** (0.53)
<i>N</i>	10,069	9,999			

Notes: \*\* indicates statistical significance at the 0.05 level using Fisher's exact test, dichotomizing outcomes.  $p$ -value from  $\chi^2$  test for full contingency table is 0.09. Standard errors of treatment effect are in parentheses.

the unexpectedly sharp expansion of early voting, the middle rows break down the outcome by Election Day and early voting. While the postcard effect is effectively zero for Election Day turnout, we find some evidence that postcards increase early voting by roughly 1.1 percentage points ( $p$ -value = 0.04).

We offer three interpretations of the results.

*Lowering the Cost of Voting.* One possibility is that the postcard heightened the salience of the cost of voting to the target population. The intervention may thereby inadvertently have caused individuals to consider early voting as a way to avoid the cost on Election Day. This is consistent with the early-voting literature, which has found that "convenience [...] is the chief benefit" (Fortier, 2006, p. 49).

The inference is bolstered by three other pieces of evidence. First, Georgia lines were expected to be long, with unprecedented turnout for the Obama campaign. The average waiting time was about 40 minutes in 2008, a burden that would have been disproportionately difficult for parents (Stewart III, 2013, p. 455). Second, and closely related, a postelection state survey revealed that almost 90% of respondents cited convenience as either very or somewhat important for voting early (Bullock et al., 2009).

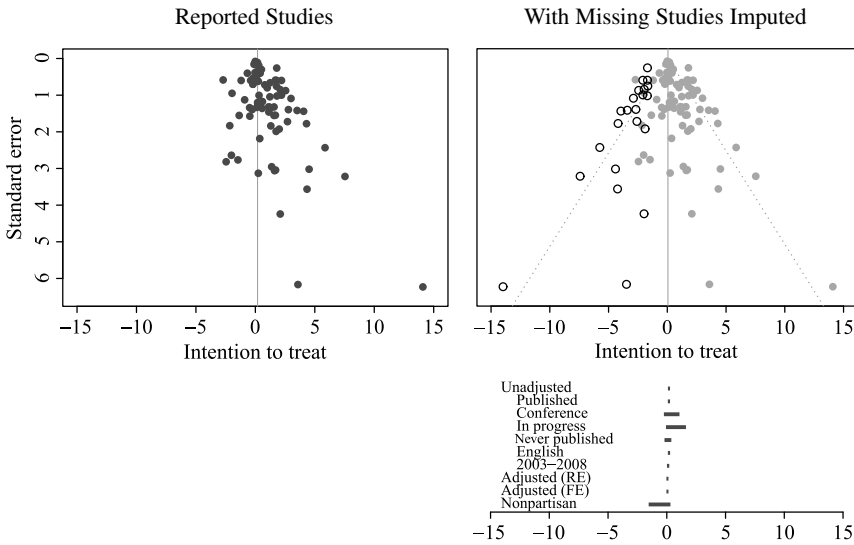
Third, in response to the postcard, the League of Women Voters received numerous phone calls from mothers following up with questions about the voting process. It is very well possible that by generating early interest with a phone number of a trusted organization, the postcard induced some mothers to become more informed about early voting. It is particularly telling that treatment and control groups had nearly identical Election Day turnout rates (38.9%), with the only difference being early voting.

*Baseline Effects.* An alternative explanation is that the treatment effects are purely a "get out the vote" (GOTV) effect, revealing little about the influence of child access



laws. Absent cost concerns, it would have been ideal to randomize a postcard from the League of Women Voters solely disclosing the date of the election *without* any reference to child access. Voters, however, also likely received an abundance of campaign information in the heavily covered 2008 election, making a pure GOTV effect unlikely. Moreover, when designing the experiment, our assessment of prior studies was that mailer effects were not strong and robust.

Figure 5  
Funnel Plot of Mailer Effects



To formalize this, we use data generously shared by Green, McGrath, and Aronow (2013) to conduct a meta-analysis of mailing effects. The left panel of Figure 5 provides a funnel plot, with the intention-to-treat effect from 77 studies on the *x*-axis and standard errors on the *y*-axis.<sup>15</sup> The vertical line represents the weighted average effect of 0.18 percentage points ( $SE = 0.05$ ). While this simple meta-analysis suggests that there are positive and statistically significant turnout effects of mailers, the effect is small. (Several studies aggregate effects over multiple mailers, thereby likely overstating the effect of a single mailer.) Moreover, the panel suggests pub-

<sup>15</sup> Like Green, McGrath, and Aronow (2013), we exclude studies that used social pressure or expressed gratitude for political involvement (i.e., we restricted ourselves to nonadvocacy, “conventional” mailers). Instead of relying in some instances on model-based parameter estimates, we re-collect the underlying turnout contingency table (when possible) to calculate effects. We also code an additional two mailers as expressing gratitude. As a result, there are small numerical differences between our results and those of Green et al.

lication bias.<sup>16</sup> If all studies were collected, we would expect effects to be arrayed in a funnel, with studies centered symmetrically around the true treatment effect, and with variability driven only by standard errors. Instead, the funnel plot is asymmetric. Using a regression test for funnel-plot asymmetry, we can reject the null of symmetry ( $p$ -value = 0.0006).

The right panel adjusts for publication bias using the trim-and-fill rank-based method by Duval and Tweedie (2000), with hollow dots representing imputed missing studies. The bottom panel presents 95% confidence intervals from several approaches. The first interval is simply the unadjusted one from studies in the left panel: (0.10, 0.27). The “adjusted (RE)” estimate uses a random-effects model with the trim-and-fill method, resulting in an interval of (-0.03, 0.14), containing the origin. Nearly all of these intervals are very close to 0.

Moreover, the 77 treatments included in the meta-analysis are almost surely over-inclusive. Many make partisan or civic appeals, and are therefore not substantively close to our hypothetical control postcard. Focusing on thirteen studies closest to our hypothetical control – i.e., strictly nonpartisan appeals – the interval is (-1.5, 0.31). In short, the meta-analysis confirms that our findings are unlikely to reflect a mere GOTV effect.

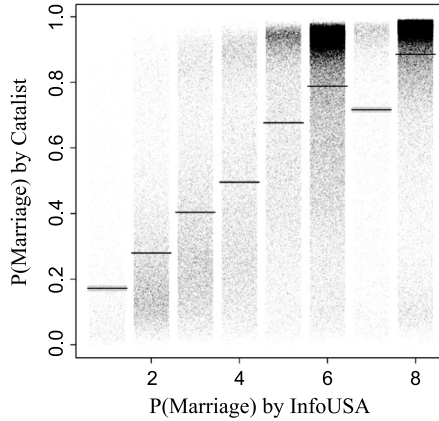
*Null Effect and Targeting.* It is also possible that, like other mailers, our postcard had no effect, with early voting being driven by type I error. Perhaps mothers were already fully informed about child access laws. (If so, that would of course be important for considering the effectiveness of such laws.) Postcards may have been ignored, although the calls to the League of Women Voters suggests that at least some voters paid attention.

We investigate one other explanation that has considerable implications for the design of informational experiments. Unlike conventional GOTV drives, the design of informational experiments depends importantly on using covariates to identify the subset of potential compliers: those likely to be subject to, but unaware of, the legal entitlement. Firms such as MSHC and proponents of “big data” tout the rich quality of individual voter data. Our data, for instance, purports to contain over 600 covariates about each individual, including whether an individual hunts, owns a pet, has a membership in an auto club, consumes sports drinks, likes arts and crafts, or enjoys gambling.

Yet further investigation suggests substantial measurement error may attenuate estimates. The reason is that many covariates appear to be model-based imputations, failing to account for measurement uncertainty. Figure 6, for instance, plots output from two models used to predict marital status, using a larger population of roughly 180,000 individuals from Georgia. The  $x$ -axis plots the value on the 9-point model by InfoUSA, and the  $y$ -axis plots the value of a model by Catalyst.

<sup>16</sup> This is despite the fact that Green, McGrath, and Aronow (2013) do exceptional work by gathering conference papers and contacting researchers in this field for unpublished studies. Evidence of the persistence of publication bias is consistent with Ho (2013) (finding publication bias even greater with conference papers).

Figure 6  
Measurement Uncertainty in Models of Marital Status



Each data point represents one individual, randomly jittered for visibility. While there is a high correlation between the two models, there is also substantial noise. Covariate information may hence be much less reliable than often assumed. There would of course be no reason to expect an effect if our postcards were sent, for instance, to individuals with no offspring. A null result may be an artifact of poor measurement.

## 6 Implications

Substantively, our findings suggest that one impetus for child access laws is well founded: to reduce the disproportionate burden of voting with childcare responsibilities. While our results do not provide strong evidence that turnout per se increased, the intervention appears to have caused a shift toward early voting to reduce the cost of Election Day voting. Our findings therefore highlight distributive implications of the expansion of early voting. Alternative voting methods may uniquely benefit parents with childcare responsibilities, an effect that may become increasingly important. Dual-income families, for instance, increased by 31% from 1996 to 2006,<sup>17</sup> and the proportion of single mothers with children (relative to all households with children) grew from 18.6% in 1990 to 25% in 2010 (Wang, Parker, and Taylor, 2013). Our results hence provide a previously unrecognized justification for early

<sup>17</sup> See *Monthly Labor Review*, November 2007, published by the U.S. Department of Labor, Bureau of Labor Statistics, Washington (DC).

voting.<sup>18</sup> Most interestingly, if early voting continues to expand, its availability may obviate the need for child access laws altogether.

Methodologically, our study demonstrates the feasibility of an alternative approach to learn about the potential effects of a law. The chief virtues are that informational experiments are substantially lower in cost and do not require government agency involvement, while potentially coming close to the ideal experiment for a subset of the population at the individual level with actual outcomes.

Our study also points to several considerations for the design of informational experiments. First, as Figure 6 demonstrates, accurate covariate measurements are critical. “Big data” may in fact have less informational content than touted. Second, one challenge to informational experiments is that effective information disclosures are difficult to design. The best evidence suggests that targeted, succinct disclosures work best (Fung, Graham, and Weil, 2007). Collecting information about knowledge of subjects after randomization would aid in isolating the causal mechanism, empirically pinpointing compliers (which would be uniquely helpful for implementation of the law), and verifying the efficacy of the disclosure. Third, interpretation of results is not necessarily as straightforward as an experiment directly randomizing individuals to different laws. In the extreme case, the informational effect could be the opposite of the effect of the law: the law might have maximum effect when subjects are fully informed, so that the informational experiment has no effect; conversely, the informational experiment might have maximum effect when subjects are underinformed, which may be precisely when the law itself has little effect. Prior information is crucial here. When subjects are underinformed about law, the former situation is unlikely; and an effect of the informational experiment is informative about the *potential* effect of law. Lastly, our experiment also illustrates how unanticipated events – here, the sharp, unexpected rise of early voting – can complicate even the simplest of field experiments.

Early voting also illustrates, however, the potential of informational experiments. It is extremely unlikely that Georgia would have randomly assigned early voting. Yet, Georgia did engage in an informational campaign to inform voters about early voting, many voters being unaware of it at the time of enactment. And that step could easily have been randomized. Doing so would have given us critical insight into the effects of early voting, and the approach may prove uniquely fruitful in empirical legal studies, where the ideal experiment often remains an elusive ideal.

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<sup>18</sup> Some evidence suggests that early voting does not increase turnout, and only causes existing voters to substitute toward early voting (Fortier, 2006; Gronke, 2008; Stein and Vonnahme, 2010).

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