

# TESTING THE MARKETPLACE OF IDEAS

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*Oliver Wendell Holmes’s notion of the marketplace of ideas—that the best test of truth is the power of an idea to get itself accepted in the competition of the market—is a central idea in free speech thought. Yet extant social science evidence provides at best mixed support for the metaphor’s veracity, and thus for the view that the truth of a proposition has substantial explanatory force in determining which propositions will be accepted and which not. But even if establishing an open marketplace for ideas is unlikely to produce a net gain in human knowledge, it may have other consequences. We illustrate how to empirically study the consequences of establishing or restricting a communicative domain. Our focus is on time, place, and manner restrictions, and we examine two potential natural experiments involving speech zones around polling places and health care facilities providing abortions. Using a regression discontinuity design with geocoded polling information for over 1.3 million voters in two high-density jurisdictions (Hudson County and Manhattan), we provide suggestive evidence that speech restrictions in Hudson County reduced turnout amongst voters in the buffer zone. By failing to cue voters of the election, speech restrictions may have unanticipated costs. And using difference-in-differences and synthetic control matching with state-level data from 1973 to 2011, we illustrate how one might study the impact of speech restrictions around health care facilities. Although the evidence is limited, Massachusetts’s restrictions were accompanied, if anything, by a decrease in the abortion rate. Buffer zones might channel speech toward more persuasive forms, belying the notion that the cure for bad speech is plainly more speech.*

INTRODUCTION .....	1161
I. LEAVING HOLMES BEHIND .....	1165
II. THE EMPIRICAL ARGUMENT FROM TRUTH .....	1167
A. <i>Truth as a Causal Variable</i> .....	1168
B. <i>Truth as the Outcome</i> .....	1171
C. <i>Groups vs. Individuals</i> .....	1172
III. TESTING THE IMPACT OF SPEECH RESTRICTIONS.....	1175
IV. VOTING .....	1178
A. <i>Background</i> .....	1178
B. <i>Research Design</i> .....	1181

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- C. *Data* ..... 1185
  - 1. *Jurisdictions*..... 1185
  - 2. *New Jersey and New York* ..... 1189
- D. *Results* ..... 1191
- E. *Explanations and Mechanisms* ..... 1195
- V. CLINICAL ACCESS ..... 1198
  - A. *Background*..... 1198
  - B. *Research Design* ..... 1203
  - C. *Data* ..... 1205
  - D. *Results* ..... 1208
  - E. *Explanations and Mechanisms* ..... 1214
- VI. IMPLICATIONS ..... 1219
- CONCLUSION ..... 1223
- APPENDIX:
  - A. *Sources for Voting Buffer Zone Jurisdictions* ..... 1224
  - B. *Sources for Abortion Buffer Zone Data*..... 1227

INTRODUCTION

Among the most enduring themes in the history, literature, and legal doctrine concerning freedom of speech is the view that speaking and writing deserve special legal, constitutional, and political protection because the unfettered exchange of ideas advances truth and knowledge.<sup>1</sup> Dissenting from the Supreme Court’s decision to uphold convictions for antiwar leafleting in *Abrams v. United States*, Justice Holmes famously opined that “the best test of truth is the power of the thought to get itself accepted in the competition of the market.”<sup>2</sup> In its most common invocation, the marketplace of ideas<sup>3</sup> purportedly

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<sup>1</sup> By referring to “special” protection we mean only to track existing American constitutional doctrine, which subjects restrictions on (some forms of) speech or communication to a higher degree of scrutiny than that applied to most non-speech conduct. Stated simply, we use the term “special” to mark the distinction between the baseline of rational basis scrutiny, *see, e.g.*, *Ferguson v. Skrupa*, 372 U.S. 726 (1963) (declining to apply heightened scrutiny to a statute restricting debt adjustment to lawyers); *Williamson v. Lee Optical of Okla.*, 348 U.S. 483, 487–88 (1955) (holding that only rational basis review applies to regulation of business and industrial conditions), and the various forms of heightened scrutiny applied to different categories of speech, *see, e.g.*, *Cent. Hudson Gas & Elec. Corp. v. Pub. Serv. Comm’n*, 447 U.S. 557, 563–64, 566 (1980) (applying intermediate scrutiny to regulation of commercial advertising); *Brandenburg v. Ohio*, 395 U.S. 444 (1969) (applying speech-specific version of strict scrutiny to restrictions on advocacy of unlawful conduct).

<sup>2</sup> 250 U.S. 616, 630 (1919) (Holmes, J., dissenting).

<sup>3</sup> The exact phrase “marketplace of ideas” became the accepted way of expressing the idea starting in the 1960s. *E.g.*, *Red Lion Broad. Co. v. FCC*, 395 U.S. 367, 390 (1969); *Time, Inc. v. Hill*, 385 U.S. 374, 406 (1967) (Harlan, J., concurring in part and dissenting in part); *Keyishian v. Bd. of Regents*, 385 U.S. 589, 603 (1967); *Lamont v. Postmaster Gen.*, 381 U.S. 301, 308 (1965). It has now become ubiquitous. *See, e.g.*, *United States v. Alvarez*,

distinguishes truth from falsity or is, at minimum, more reliable than official or expert selection of ideas thought to be true and suppression of ideas thought to be false.<sup>4</sup> Although theories justifying free speech on democratic governance or individual autonomy grounds rival the marketplace notion, it remains the case that, as one First Amendment scholar boldly put it, “[n]ever before or since has a Justice conceived a metaphor that has done so much to change the way that courts, lawyers, and the public understand an entire area of constitutional law.”<sup>5</sup>

Yet is the metaphor—or the claim that the metaphor has come to represent—accurate? The goal of this Symposium is to develop ways by which scholars might subject constitutional law to empirical scrutiny: i.e., to “test constitutional law.” Our contribution sets forth how one might—and might not—test the marketplace of ideas. Part I considers Holmes’s notion as political theory, namely as a normative claim about the virtues of democracy or as a conceptual claim about the definition and nature of democracy. From that perspective, in which the marketplace is *truth-defining*, Holmes’s statement has no empirical implications.<sup>6</sup> Part II, however, explains that the marketplace notion has taken on a very different meaning over the course of time, and we highlight three different ways of understanding Holmes’s conception as generating empirically verifiable propositions. Extant experimental research, however, provides no clear answer about the soundness of this interpretation of the marketplace of ideas claim. The principal difficulties with designing experimental tests lie in objectively measuring truth (when Holmes almost certainly contemplated normative, not factual, beliefs) and realistically modeling the marketplace (when Holmes likely conceived of a dynamic marketplace that

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132 S. Ct. 2537, 2541 (2012); *Davenport v. Wash. Educ. Ass’n*, 551 U.S. 177, 179 (2007); *Hustler Magazine, Inc. v. Falwell*, 485 U.S. 46, 52 (1988); *Cent. Hudson Gas*, 447 U.S. at 592 (Rehnquist, J., dissenting); Derek E. Bambauer, *Shopping Badly: Cognitive Biases, Communications, and the Fallacy of the Marketplace of Ideas*, 77 U. COLO. L. REV. 649 (2006); Vincent Blasi, *Holmes and the Marketplace of Ideas*, 2004 SUP. CT. REV. 1; Joseph Blocher, *Institutions in the Marketplace of Ideas*, 57 DUKE L.J. 821 (2008); Stanley Ingber, *The Marketplace of Ideas: A Legitimizing Myth*, 1984 DUKE L.J. 1; William P. Marshall, *In Defense of the Search for Truth as a First Amendment Justification*, 30 GA. L. REV. 1, 1 (1995); Eugene Volokh, *In Defense of the Marketplace of Ideas/Search for Truth as a Theory of Free Speech Protection*, 97 VA. L. REV. 595 (2011); Christopher T. Wonnell, *Truth and the Marketplace of Ideas*, 19 U.C. DAVIS L. REV. 669 (1986).

<sup>4</sup> See *Keyishian*, 385 U.S. at 603 (explaining that the marketplace of ideas provides “robust exchange of ideas which discovers truth out of a multitude of tongues, [rather] than through any kind of authoritative selection” (alteration in original) (internal quotation marks omitted)).

<sup>5</sup> Blocher, *supra* note 3, at 824–25.

<sup>6</sup> One could of course test empirically whether experts or the population actually agree with that notion of the marketplace as truth-defining, but that would be an entirely different project from the one we undertake here.

evolves over generations, rather than a one-shot interaction). Nevertheless, a considerable amount of existing empirical research can cast light on the question and tends, as we also explain in Part II, to justify skepticism about the causal efficacy of establishing an open marketplace of ideas in identifying true propositions and rejecting false ones.

It is a mistake, however, to think that the identification of truth is the only potential consequence of establishing an open marketplace for facts, ideas, opinion, and argument. When such a marketplace exists, its very existence may have a wealth of consequences on the behavior of those whose activities are affected by the existence or location of the marketplace. And thus Part III argues that it is critical to understand the full array of consequences that speech restrictions—against which the marketplace metaphor is most commonly trotted out—operate in practice. We focus on time, place, and manner restrictions, which in modern times frequently establish speech buffer zones of “enforced silence,”<sup>7</sup> thereby closing, in a very real sense and in discrete locations, the marketplace of ideas. Examining the effects of such buffer zones may enable us to identify their consequences, some of which may be independent of the question whether a marketplace for ideas is effective or ineffective in identifying or advancing truth.

Drawing on recent developments in the design of natural experiments,<sup>8</sup> we illustrate how one might study two such buffer zones, the apparent inconsistency of which featured prominently in the Supreme Court’s 2013 Term. Part IV studies the implications of *Burson v. Freeman*, the 1992 case upholding state restrictions on campaign activity within 100 feet of polling place entrances on election day.<sup>9</sup> By geocoding polling locations and over 1.3 million voters, we apply a regression discontinuity design that suggests that speech restriction, by removing cues to vote, may reduce turnout amongst people just inside the buffer zone. By potentially reducing turnout and impeding initiatives and exit polling, *Burson* may actually have undermined the

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<sup>7</sup> *Whitney v. California*, 274 U.S. 357, 377 (1927) (Brandeis, J., concurring).

<sup>8</sup> Our approach draws on the methodological contributions of a number of scholars and, in particular, Alberto Abadie, Alexis Diamond & Jens Hainmueller, *Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California’s Tobacco Control Program*, 105 J. AM. STAT. ASS’N 493 (2010); Joshua D. Angrist & Jörn-Steffen Pischke, *The Credibility Revolution in Empirical Economics: How Better Research Design Is Taking the Con out of Econometrics*, 24 J. ECON. PERSP. 3 (2010); Rajeev H. Dehejia & Sadek Wahba, *Propensity Score-Matching Methods for Nonexperimental Causal Studies*, 84 REV. ECON. & STAT. 151 (2002); Daniel E. Ho & Donald B. Rubin, *Credible Causal Inference for Empirical Legal Studies*, 7 ANN. REV. L. & SOC. SCI. 17 (2011); Guido W. Imbens & Thomas Lemieux, *Regression Discontinuity Designs: A Guide to Practice*, 142 J. ECONOMETRICS 615 (2008); David S. Lee & Thomas Lemieux, *Regression Discontinuity Designs in Economics*, 48 J. ECON. LITERATURE 281 (2010).

<sup>9</sup> 504 U.S. 191, 211 (1992).

franchise. Part V then studies the implications of *McCullen v. Coakley*, which in 2014 found unconstitutional a 35-foot fixed buffer zone around health care facilities providing abortions in Massachusetts.<sup>10</sup> Using synthetic control matching in a difference-in-differences framework, we show that the enactment of an earlier “floating” buffer zone in 2000 (restricting approaching individuals without consent), if anything, reduced the abortion rate. One mechanism for this reduction may be that the 2000 buffer zone channeled the type of speech, per the Court’s characterization, from “protest” to “persua[sive].”<sup>11</sup> In distinguishing *Burson*, however, the *McCullen* court factually botched state election law, leaving these cases more difficult to reconcile than acknowledged. Part VI briefly draws out implications for examining the full range of consequences of a marketplace of ideas, and, more speculatively, for empirical research in constitutional law generally.

A brief note about scope. We recognize that the marketplace of ideas is not the only possible basis for a distinct principle of freedom of speech. But even in the face of relatively longstanding skepticism,<sup>12</sup> the idea endures,<sup>13</sup> and thus seems ripe for empirical consideration.<sup>14</sup>

<sup>10</sup> 134 S. Ct. 2518, 2541 (2014).

<sup>11</sup> *Id.* at 2527 (distinguishing “protestors” from “sidewalk counseling,” whose aim is to “persuade [ . . . women to forgo abortions”); *see also* Hill v. Colorado, 530 U.S. 703, 708, 716 (2000) (noting that “[s]idewalk counseling consists of efforts to . . . persuade . . . passersby about abortion and abortion alternatives” and that “the right to attempt to persuade others . . . may not be curtailed simply because the speaker’s message may be offensive to his audience” (internal quotation marks omitted)).

<sup>12</sup> Skepticism about the epistemic reliability of the marketplace for ideas, or the search for truth as a free speech justification, can be found in, for example, FREDERICK SCHAUER, *FREE SPEECH: A PHILOSOPHICAL ENQUIRY* 15–30 (1982) (arguing the marketplace for ideas “suffers from crippling weaknesses” including failure “to demonstrate why open discussion leads to knowledge”); C. Edwin Baker, *Scope of the First Amendment Freedom of Speech*, 25 UCLA L. REV. 964, 965–66 (1978) (contending the disproportionate presence of certain viewpoints in mass media renders the marketplace of ideas incapable of generating objective truth); Bambauer, *supra* note 3, at 651 (asserting human biases undercut objective interpretation of data); Ingber, *supra* note 3, at 31 (describing the marketplace of ideas as a myth used to legitimize constraints on speech that clashes with accepted values); Alvin I. Goldman, *Epistemic Paternalism: Communication Control in Law and Society*, 88 J. PHIL. 113, 118–25 (1991) (contending that some situations call for epistemic paternalism); Alvin I. Goldman & James C. Cox, *Speech, Truth, and the Free Market for Ideas*, 2 LEGAL THEORY 1, 17 (1996) (arguing that economic theory of markets does not support the idea that a free marketplace of ideas will advance the search for truth); Frederick Schauer, *Social Epistemology, Holocaust Denial, and the Post-Millian Calculus*, in *THE CONTENT AND CONTEXT OF HATE SPEECH: RETHINKING REGULATION AND RESPONSES* 129, 138 (Michael Herz & Peter Molnar eds., 2012) (noting that allowing expression of false ideas may “increase the number of people who hold false beliefs”).

<sup>13</sup> The idea endures not only in the academic literature, as recounted in *supra* note 3, and not only in Supreme Court decisions, e.g., *McCullen v. Coakley*, 134 S. Ct. 2518, 2529 (2014); *United States v. Alvarez*, 132 S. Ct. 2537, 2541 (2012); *Sorrell v. IMS Health, Inc.*, 131 S. Ct. 2653, 2674 (2011); *Citizens United v. FEC*, 558 U.S. 310, 335 (2010); *Davenport*

Our goal here is to explore how to test the marketplace of ideas and to offer pathways that might be examined in further research. All of the existing scholarship on the marketplace of ideas is focused, whether by way of support or challenge, on the epistemic claims of the notion of the marketplace of ideas—i.e., the claims that it will facilitate the search for truth. In arguing that a marketplace of ideas may have consequences other than the epistemic, we aim to open up a new domain of research about the marketplace of ideas. We do not purport to resolve conclusively questions about voting or abortion buffer zones, or about speech restrictions generally.<sup>15</sup> Our pilot findings are tentative (and sensitive in ways we spell out below), but suggestive, and aimed at furthering the goal of this Symposium by suggesting paths forward for empirical constitutional law.<sup>16</sup>

## I

### LEAVING HOLMES BEHIND

However enduring Holmes's phrase may be, his reference to the marketplace of ideas in terms of "the competition of the

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v. Wash. Educ. Ass'n, 551 U.S. 177, 188 (2007), but also in the everyday rhetoric of countless free speech and anti-censorship advocates, whether institutional or individual, see, e.g., Tom Shales, *Michael Powell and the FCC: Giving Away the Marketplace of Ideas*, WASH. POST, June 2, 2003, at C1; Robert Shibley, *Censorship Can't Cure Oklahoma Frat Racism: A Marketplace of Ideas is the Answer*, USA TODAY, Mar. 12, 2015, at 7A.

<sup>14</sup> Moreover, if we understand the marketplace of ideas not as the basis for a principle of freedom of speech, but rather as the institution that exists as a *consequence* of any principle of freedom of speech, then even more empirical questions arise.

<sup>15</sup> The charge for this Symposium was principally to "explore . . . how one might go about testing . . . empirical assertion[s] about constitutional law]," although "[i]deally, we might even be able to take the inquiry to the next level and get some actual empirical results." Invitation to Participate in "Testing the Constitution: The Empirics of Constitutional Doctrine" from Barry Friedman, Professor, N.Y.U. Sch. of Law, et al. to Daniel Ho, Professor of Law, Stanford Law Sch., et al. (July 9, 2013) (on file with the New York University Law Review) (emphasis added).

<sup>16</sup> We also do not examine the marketplace of ideas when it comes to antitrust or media regulation, domains in which Holmes is also often invoked. While these contexts often have First Amendment issues lurking in the background, the primary legal questions occur under antitrust, administrative, and communications law. Going back at least to Peter O. Steiner, *Program Patterns and Preferences, and the Workability of Competition in Radio Broadcasting*, 66 Q.J. ECON. 194, 196, 206–07 (1952), economic theory has been unclear about the impact of antitrust or media regulations on the diversity of ideas, and empirical results have been similarly mixed. See, e.g., Daniel E. Ho & Kevin M. Quinn, *Viewpoint Diversity and Media Consolidation: An Empirical Study*, 61 STAN. L. REV. 781 (2009) (examining empirically the conditions under which media consolidation does or does not produce viewpoint diversity). That said, however, it is worth noting that there is a long history of taking the metaphor of the marketplace of ideas somewhat literally, and thus attempting to apply ideas such as market failure to the basic marketplace of ideas claim. See, e.g., Goldman & Cox, *supra* note 12, at 3–4 (analyzing the marketplace of ideas literally, considering the idea that regulation purely by market mechanisms achieves more total truth possession than state and other regulations, and rejecting this thesis).

market”<sup>17</sup> may simply be a normative claim about the value of democracy, or alternatively merely a conceptual claim about the meaning of democracy. It is possible, of course, that when Holmes described the competition of the market as the “best test of truth,”<sup>18</sup> he might have been phrasing in different language the fundamentally epistemic claim that John Milton had in mind in *Areopagitica* when he asked, rhetorically, “who ever knew Truth put to the worse, in a free and open encounter?”<sup>19</sup> Or he might have been tracing the similar but better-developed arguments of John Stuart Mill in *On Liberty*,<sup>20</sup> which maintained that providing individuals with the liberty to express whatever propositions they wished to express would make it easier for a population to locate truth, expose error, and refine even those truths already known.

But there is little reason to believe that either of these approaches was what Holmes had in mind. For one thing, his statements were made in the context of discussing a political pamphlet with little factual content but strong advocacy of a particular political position. In addition, Holmes’s statement, when seen in the context of the other free speech issues and cases of the time,<sup>21</sup> almost all of which involved antigovernmental advocacy, is best understood as advocating the legal and constitutional standards most appropriately suited for dealing with the problem of potentially dangerous advocacy. Finally, Holmes’s own epistemological skepticism would have made him at least a bit uncomfortable with the notion of truth defined independently of the political or deliberative process.<sup>22</sup>

Instead, Holmes was likely thinking of the competition not among factual propositions, nor even among normative ones whose soundness was subject to widespread agreement, but rather among normative moral, ideological, or political programs (e.g., socialism versus capitalism, democracy versus monarchy, regulation versus

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<sup>17</sup> *Abrams v. United States*, 250 U.S. 616, 630 (1919) (Holmes, J., dissenting).

<sup>18</sup> *Id.*

<sup>19</sup> JOHN MILTON, *AREOPAGITICA: A SPEECH OF MR. JOHN MILTON FOR THE LIBERTY OF UNLICENSED PRINTING, TO THE PARLIAMENT OF ENGLAND* 35 (Payson & Clarke Ltd. 1927) (1644).

<sup>20</sup> JOHN STUART MILL, *ON LIBERTY* 17–52 (David Spitz ed., W. W. Norton 1975) (1859).

<sup>21</sup> *See, e.g.*, *Schenck v. United States*, 249 U.S. 47, 48–49 (1919) (involving advocacy against military recruitment and conscription); *Debs v. United States*, 249 U.S. 211, 212 (1919) (same); *Frohwerk v. United States*, 249 U.S. 204, 205 (1919) (same); *Masses Publ’g Co. v. Patten*, 244 F. 535, 536 (S.D.N.Y. 1917) (same). The cases and their political setting are described and analyzed in GEOFFREY R. STONE, *PERILOUS TIMES: FREE SPEECH IN WARTIME: FROM THE SEDITION ACT OF 1798 TO THE WAR ON TERRORISM* 125–225 (2004).

<sup>22</sup> *See* MAX LERNER, *THE MIND AND FAITH OF JUSTICE HOLMES* 290 (1943) (describing Holmes’s skepticism).

laissez-faire).<sup>23</sup> Seen this way, the reference to the marketplace makes sense. Just as, more or less, pure free market theory *defines* value in terms of what succeeds in the competition of the market—the value of a product or service is defined by what the market is willing to pay for it, even if it seems as if the market is willing to pay a great deal for things that others find silly—so too might Holmes have believed that the value of a political idea or ideological program was simply a function of which ideas were accepted and which were rejected.<sup>24</sup> Ideas were good or bad insofar as they were accepted or rejected in the competition of the market. And that is because the market for political ideas is, or at least may well have been for Holmes, coextensive with the idea of democracy itself,<sup>25</sup> such that democratic political truth is determined by, and, indeed, defined by, the market.

For purposes of this Symposium, questions about whether this is the best reading of Holmes or whether these normative claims are sound are beyond our scope. To the extent that the marketplace *defines* political truth, it is hardly falsifiable or testable.

## II

### THE EMPIRICAL ARGUMENT FROM TRUTH

While Holmes may well have considered the marketplace to be truth-defining, it is now common to invoke the metaphor in quite a different fashion. The more common argument from truth—the argument from the marketplace of ideas—is that unrestricted speech, as a matter of institutional design, is epistemically superior to the most common alternatives.<sup>26</sup> One canonical formulation comes from Justice Brandeis, who in his memorable concurring opinion in *Whitney v. California* opined that where there is speech that is false, fallacious, or evil, “the remedy to be applied is more speech, not enforced silence.”<sup>27</sup> Unlike the claim that the marketplace is truth-defining,

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<sup>23</sup> This understanding of Holmes’s views is largely consistent with that in Blasi, *supra* note 3 (arguing that Holmes’s “marketplace of ideas” was not envisioned as a system for determining absolute truth but, rather, one for perpetual opposition to and reevaluation of normative ideas).

<sup>24</sup> It is for this reason that Holmes is described as having a “survival theory of truth.” Carl A. Auerbach, *The Communist Control Act of 1954: A Proposed Legal-Political Theory of Free Speech*, 23 U. CHI. L. REV. 173, 187 n.46 (1956) (internal quotation marks omitted) (citing LERNER, *supra* note 22, at 290).

<sup>25</sup> See MARTIN H. REDISH, *THE LOGIC OF PERSECUTION: FREE EXPRESSION AND THE MCCARTHY ERA* 8–9 (2005) (making this argument).

<sup>26</sup> This argument is summarized and criticized in SCHAUER, *supra* note 12, at 15–34.

<sup>27</sup> 247 U.S. 357, 377 (1927) (Brandeis, J., concurring). Given the context in which he was writing, and the context in which the free speech cases of the early part of the twentieth century arose, it is possible that Brandeis, like Holmes before him, was making a claim that sounded more in democracy than epistemology. But whatever Brandeis’s



whether the marketplace is epistemically superior to other methods of locating truth is indeed an empirical, and not a moral or normative, claim.

We might interpret the marketplace of ideas to stand for three distinct empirical propositions.

### A. *Truth as a Causal Variable*

It is possible that the truth of a proposition will have a positive causal effect on whether the idea will be accepted. In *United States v. Alvarez*,<sup>28</sup> for example, the case in which the Supreme Court struck down a penalty for falsely claiming to have been awarded the Congressional Medal of Honor, the question whether Alvarez was awarded the Medal was not a contested matter of social policy. He won it, or not.<sup>29</sup> Adherents of the marketplace of ideas justification for a principle of freedom of speech might be taken to imply that a true statement about winning the medal (by an actual medal winner) would be more likely to be accepted than a false statement. The question, prominent since Milton's *Areopagitica*<sup>30</sup> and arguably at the heart of John Stuart Mill's *On Liberty*,<sup>31</sup> is what mechanism a society should use to determine these matters of fact, including, for example, scientific facts such as the extent of climate change, the relationship between smoking and lung cancer, or the dangers, if any, arising from childhood vaccination or genetically modified foods.

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immediate aims, his statement has endured as much as has that of Holmes, and is scarcely restricted to questions of government policy for which there may be no clear indication of which policy is right and which wrong. Indeed, the very fact that Brandeis made reference to falsehoods, fallacies, and evils suggests that he was plainly thinking about matters as to which there actually was a right and a wrong, and one defined independently of the deliberative process. Understood in this way, Brandeis is best interpreted as making an epistemic claim, the claim being that in the process of countering false speech with true speech, a population of listeners will more often than not (or more often than under alternative epistemic institutional designs) choose the true over the false, the sound over the fallacious, and the good over the evil. And thus the claim is that the truth of a true proposition is more likely to produce its acceptance than is the identification of the proposition as true by some official or government, and sufficient to tend towards acceptance even in an environment in which (erroneous) denials of its truth are permitted. And so too, in reverse, for false propositions, which Brandeis, Mill, and countless others believed would fall in the face of public expressions of or demonstrations of their falsity.

<sup>28</sup> 132 S. Ct. 2537 (2012).

<sup>29</sup> On the full array of free speech issues presented by verifiable factual claims, see Frederick Schauer, *Facts and the First Amendment*, 57 UCLA L. REV. 897 (2010).

<sup>30</sup> See MILTON, *supra* note 19. Or perhaps much earlier, for in the First Book of Esdras it is said that "Great is Truth, and mighty above all things." 1 *Esdras* 4:41. And Walter Bagehot insisted that "in discussion truth has an advantage." 3 WALTER BAGEHOT, *The Metaphysical Basis of Toleration*, in LITERARY STUDIES 204, 208 (Richard Holt Hutton ed., 1898).

<sup>31</sup> See MILL, *supra* note 20.

While falsifiable in principle, meaningfully testing the proposition that truth is a causal (or explanatory/independent) variable proves difficult. How would we randomize the truthfulness of a statement about winning the Congressional Medal of Honor, without necessarily randomizing the individual making the statement (thereby making it difficult to distinguish the effect of truthfulness from the effect of the individual)? What true-or-false statements (e.g., “I have a college degree,” “the unemployment rate has decreased under President X”) would provide meaningful tests of the marketplace of ideas? The conceptual difficulty of even conceiving of a meaningful experimental test of truth as the causal variable suggests that this avenue is not particularly fruitful as the focus of rigorous empirical research.<sup>32</sup>

Decades of social psychology research also reveal how highly contingent the answers can be. The degree of confidence with which a speaker articulates a proposition correlates significantly with whether the proposition will be believed by an audience, but only poorly with whether the proposition is true.<sup>33</sup> The acceptability of an idea varies with what social psychologists call “peripheral cues,” which include, among others, the identity, authority, and charisma of the agent expressing the proposition (the speaker),<sup>34</sup> the frequency with which the proposition is expressed,<sup>35</sup> the manner or style with which the proposition is expressed,<sup>36</sup> the medium through which the proposition is

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<sup>32</sup> For a discussion of the critical role of a hypothetical experiment in causal inference, see Donald B. Rubin, Comment, *Which Ifs Have Causal Answers*, 81 J. AM. STAT. ASS'N 961 (1986).

<sup>33</sup> See Barbara A. Spellman & Elizabeth R. Tenney, *Credible Testimony In and Out of Court*, 17 PSYCHONOMIC BULL. & REV. 168, 169, 171 (2010) (noting that “[t]he huge research literature on confidence agrees that confidence is usually the most important factor in assessing credibility,” but that “literature about the confidence-accuracy relationship for eyewitnesses has mixed findings”); see also Elizabeth R. Tenney et al., *Calibration Trumps Confidence as a Basis for Witness Credibility*, 18 PSYCHOL. SCI. 46, 46–47 (2007) (discussing experiments in which “confidence significantly affected judgments of credibility,” but indicating “no interaction between confidence and error”); Elizabeth R. Tenney, Barbara A. Spellman & Robert J. MacCoun, *The Benefits of Knowing What You Know (and What You Don't): How Calibration Affects Credibility*, 44 J. EXPERIMENTAL SOC. PSYCHOL. 1368, 1368 (2008) (“People who are extremely confident . . . are believed more often than people who express low confidence.”).

<sup>34</sup> The classic study is ROBERT K. MERTON, MARJORIE FISKE & ALBERTA CURTIS, *MASS PERSUASION: THE SOCIAL PSYCHOLOGY OF A WAR BOND DRIVE* (1946). Much of the research is summarized in RICHARD E. PETTY & JOHN T. CACIOPPO, *ATTITUDES AND PERSUASION: CLASSIC AND CONTEMPORARY APPROACHES* (1996).

<sup>35</sup> See Ian Maynard Begg, Ann Anas & Suzanne Farinacci, *Dissociation of Processes in Belief: Source Recollection, Statement Familiarity, and the Illusion of Truth*, 121 J. EXPERIMENTAL PSYCHOL. 446 (1992) (investigating the mechanisms through which repetition positively influences assessments of credibility).

<sup>36</sup> See e.g., ROBERT B. CIALDINI, *INFLUENCE: THE PSYCHOLOGY OF PERSUASION* 4–14 (rev. ed. 2007) (discussing trigger features that consistently elicit certain behavioral responses).

expressed,<sup>37</sup> the recipients' stake or interest in the outcome,<sup>38</sup> and the prior beliefs and allegiances of the recipients.<sup>39</sup>

Furthermore, the marketplace is not a static concept, easily tested in a one-shot laboratory setting. Holmes and others almost surely had a dynamic marketplace in mind, one that determines which propositions are accepted by the interaction of marketplace participants over generations.<sup>40</sup> In *Alvarez*, for instance, if false statements about winning a Congressional Medal of Honor increase, then (a) skepticism about purported medal winners, (b) attempts to verify claims, and (c) incentives for the government or third parties to provide a database of actual winners each will likely increase.<sup>41</sup> Without speech restrictions, a static experiment might reveal the acceptance of false claims, but such false claims may hence not survive in a dynamic equilibrium. Conversely, the Stolen Valor Act—criminalizing false statements about winning the medal<sup>42</sup>—might actually increase the number of false claims *accepted* by the marketplace in equilibrium, by reducing the incentive to develop ways to verify claims.

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<sup>37</sup> See, e.g., B.J. FOGG, PERSUASIVE TECHNOLOGY: USING COMPUTERS TO CHANGE WHAT WE THINK AND DO 1–2 (2002) (arguing that the internet has developed into an effective medium of persuasion).

<sup>38</sup> Richard E. Petty & John T. Cacioppo, *Personal Involvement as a Determinant of Argument-Based Persuasion*, 41 J. PERSONALITY & SOC. PSYCHOL. 847, 847 (1981).

<sup>39</sup> CIALDINI, *supra* note 36, at 5–6. For important analysis of the multiple content-independent factors that influence which ideas are accepted and which are not, see generally CHIP HEATH & DAN HEATH, MADE TO STICK: WHY SOME IDEAS SURVIVE AND OTHERS DIE (2008) (discussing how recipient understanding and memory of ideas are improved when such ideas are conveyed according to six factors), and Dan M. Kahan & Donald Braman, *Cultural Cognition and Public Policy*, 24 YALE L. & POL'Y REV. 149, 149–60 (2006) (arguing that multiple cultural factors strongly influence one's acceptance of ideas). A formal model challenging Brandeis's notion is provided in Edward Glaeser & Cass R. Sunstein, *Does More Speech Correct Falsehoods?*, 43 J. LEGAL STUD. 65, 65 (2014). The boldest implication of this formulation of the marketplace of ideas is that truth would be more important than any other factor in determining whether an idea is accepted. Yet it is important to recognize that truth might have some causal effect even when other factors can swamp its effect in some settings.

<sup>40</sup> See Blasi, *supra* note 3, at 26 (discussing the Holmesian evolution of ideas over generations of the population); Blocher, *supra* note 3, at 829–32 n.24 (discussing slow evolution of ideas in conceptualization by Milton, Mill, and Holmes); Ingber, *supra* note 3, at 86 (noting the slow evolution of ideas over time); see also Richard A. Posner, *Free Speech in an Economic Perspective*, 20 SUFFOLK U. L. REV. 1, 27 (1986) (“[T]ruth (defined for this purpose as the agreement of all persons deemed rational) will be resolved (if ever) only in a ‘marketplace of ideas’ unfolding through decades, perhaps even centuries.”).

<sup>41</sup> The plurality and dissent split on exactly this prediction. *Compare* 132 S. Ct. 2537, 2551 (2012) (plurality opinion) (predicting that an online searchable database would allow easy verification of false claims), *with id.* at 2559–60 (Alito, J., dissenting) (describing the impracticability of creating such a database).

<sup>42</sup> Stolen Valor Act, 18 U.S.C. § 704(c)(1) (2012) (struck down as unconstitutional by *Alvarez*).

Given these dynamics, testing truth as a causal variable—as contemplated by the notion of the marketplace of ideas—is at minimum very challenging. The existing psychological research suggests that truth is less of a causal variable than Milton and others supposed, but the methodological difficulties of going much further than this are severe.

### B. *Truth as the Outcome*

A different way to understand the marketplace is as *truth-locating* (in the Millian sense). Rather than thinking of truth as the causal (or explanatory/independent) variable, the marketplace of ideas principle may be contemplating truth as the outcome (or dependent variable): i.e., the likelihood that society arrives at the truth. One mechanism may be that the marketplace is *truth-eliciting*, encouraging those in possession of truthful propositions to offer them. The empirical question then is whether the marketplace, compared to other approaches to institutional or decisional design, will increase or decrease the likelihood of arriving at the truth or the supply of truthful propositions.

The experimental work that comes closest to examining truth as the outcome in a dynamic setting, however, paints a mixed picture. MacNeil and Sherif, building on Jacobs and Campbell's classic optical illusion experiment that approximates the intergenerational dynamic of the marketplace,<sup>43</sup> introduced planted subjects that offered wrong answers differing in arbitrariness.<sup>44</sup> Examining the transmission of ideas over generations of experimental subjects (after the plant was removed), they found that group norms are influenced by plausible (but false) answers and over time revert to natural (internally generated or pre-existing) norms.<sup>45</sup> The reversion might provide some credence to the marketplace of ideas as a facilitator of truthful propositions, but the natural norms were also objectively wrong. Heath, Bell and Sternberg examined the likelihood that ideas are transmitted by research subjects, and show that emotional content matters a great deal.<sup>46</sup> Plausible, but false, ideas have long shelf lives.<sup>47</sup> Even more

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<sup>43</sup> Robert C. Jacobs & Donald T. Campbell, *The Perpetuation of an Arbitrary Tradition Through Several Generations of a Laboratory Microculture*, 62 J. ABNORMAL & SOC. PSYCHOL. 649 (1961).

<sup>44</sup> Mark K. MacNeil & Muzafer Sherif, *Norm Change over Subject Generations as a Function of Arbitrariness of Prescribed Norms*, 34 J. PERSONALITY & SOC. PSYCHOL. 762, 763, 766 (1972).

<sup>45</sup> *Id.* at 772–73.

<sup>46</sup> Chip Heath, Chris Bell & Emily Sternberg, *Emotional Selection in Memes: The Case of Urban Legends*, 81 J. PERSONALITY & SOC. PSYCH. 1028 (2001) (reporting three studies indicating higher rates of passing along urban legend stories when disgust and other emotional content was higher).

pernicious is the finding by Fragle and Heath that while people are more likely to believe information from a credible source, they are also likely to misattribute plausible, but false, information to a credible source.<sup>48</sup>

Experimental work on truth as the outcome in a dynamic setting might thus cause us to question whether the marketplace reliably converges upon truth. But the marketplace of ideas claim is also a comparative one. The question is not whether the marketplace of ideas reliably produces or elicits truth; rather it is how the marketplace performs in locating truth relative to some alternative that restricts speech in some way. Traditionally, the alternative is government selection,<sup>49</sup> which, of course, has its own pathologies. It is this comparative question—the causal effect of speech restrictions (or government selection) versus an unfettered marketplace—that becomes even more difficult to test meaningfully in a laboratory setting, and which animates our attempts below to identify natural experiments that expose individuals to speech restrictions in real-world settings.

### C. Groups vs. Individuals

A third empirical hypothesis is that aggregate decisionmaking may be more epistemically reliable than individual, even expert, decisionmaking. In short, the hypothesis is that increasing the number of decisionmakers may increase reliability.

Evidence from decades of important social psychology research is mixed on the question.<sup>50</sup> First, groups do tend to perform better than individuals on “intellective” tasks, with factually correct solutions, like

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<sup>47</sup> See *id.* at 1033–34 (finding stories were more likely to be passed along when people said they were plausible and also when they evoked greater disgust); see also Chip Heath, *Do People Prefer to Pass Along Good or Bad News? Valence and Relevance of News as Predictors of Transmission Propensity*, 68 *ORG. BEHAV. & HUM. DECISION PROC.* 79, 91 (1996) (showing that research subjects can be prone to share exaggeratedly (false) bad news in negative emotional environments and exaggeratedly (false) good news in positive environments).

<sup>48</sup> Alison R. Fragle & Chip Heath, *Evolving Informational Credentials: The (Mis)Attribution of Believable Facts to Credible Sources*, 30 *PERSONALITY & SOC. PSYCHOL. BULL.* 225, 225 (2004).

<sup>49</sup> Or, earlier, selection by an established church.

<sup>50</sup> Compare JAMES SUROWIECKI, *THE WISDOM OF CROWDS* (2004) (supporting the epistemic reliability of aggregate decisionmaking), with IRVING L. JANIS, *VICTIMS OF GROUPTHINK: A PSYCHOLOGICAL STUDY OF FOREIGN-POLICY DECISIONS AND FIASCOES* (1972) (illustrating the frequent epistemic disadvantages in aggregate decisionmaking), and Jan Lorenz et al., *How Social Influence Can Undermine the Wisdom of Crowd Effect*, 108 *PROC. NAT'L ACAD. SCI.* 9020, 9020 (2011) (demonstrating that the influence of other group members on the opinions of individuals within the group can negatively influence group accuracy for both statistical and psychological reasons).

math problems.<sup>51</sup> Monetary compensation and time also influence individual ability to accurately recall facts.<sup>52</sup> Prior group allegiances (i.e., ideology) can skew seemingly factual assessments, such as whether demonstrators were obstructing pedestrian traffic.<sup>53</sup> As we noted above, however, at least Holmes and perhaps Mill and others likely did not have factual propositions in mind. Nor do their successors, even those who do not view the market for ideas as truth-defining. On “judgmental” tasks that do not have factual answers, whether groups perform better depends on a wide variety of circumstances.<sup>54</sup> Groups, for instance, perform well (in the sense of reduction in heuristics and biases) when information is widely shared, but perform poorly in incorporating truthful information when it is not widely shared.<sup>55</sup> In a beauty contest game, for example, groups are not smarter, but learn faster than individuals.<sup>56</sup> Further, strong leaders can manipulate groups<sup>57</sup> and group deliberation can often polarize opinions.<sup>58</sup>

The import of the experimental evidence on the epistemic reliability or decision-making soundness of the marketplace of ideas in practice is thus unclear. Consider the debate about whether the heuristics and biases documented in a laboratory setting can be gener-

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<sup>51</sup> PATRICK R. LAUGHLIN, *GROUP PROBLEM SOLVING* 113 (2011).

<sup>52</sup> Markus Prior & Arthur Lupia, *Money, Time, and Political Knowledge: Distinguishing Quick Recall and Political Learning Skills*, 52 AM. J. POL. SCI. 169, 174, 176 (2008) (finding monetary incentives increase the rate of correct answers to political knowledge questions, as does giving extra time, which in the study was twenty-four hours).

<sup>53</sup> See Dan M. Kahan et al., “*They Saw a Protest*”: *Cognitive Illiberalism and the Speech-Conduct Distinction*, 64 STAN. L. REV. 851, 851 (2012) (randomizing whether protests were outside of an abortion clinic or military recruitment center and finding that “cultural cognition” had a substantial impact on factual assessments pertaining to the speech-conduct distinction).

<sup>54</sup> See Norbert L. Kerr, Robert J. MacCoun & Geoffrey P. Kramer, *Bias in Judgment: Comparing Individuals and Groups*, 103 PSYCHOL. REV. 687, 687 (1996) (reviewing studies comparing “individual and group susceptibility to particular types of bias” and demonstrating “that there is no simple and general pattern”).

<sup>55</sup> See Garold Stasser & William Titus, *Pooling of Unshared Information in Group Decision Making: Biased Information Sampling During Discussion*, 48 J. PERSONALITY & SOC. PSYCHOL. 1467, 1467, 1478 (1985) (noting that group discussion can serve a corrective function for incomplete and biased information held by individuals, but that group discussion can fall short of its potential).

<sup>56</sup> Martin G. Kocher & Matthias Sutter, *The Decision Maker Matters: Individual Versus Group Behavior in Experimental Beauty-Contest Games*, 115 ECON. J. 200, 200 (2005).

<sup>57</sup> See Michael G. Cruz, David Dryden Henningsen & Brian A. Smith, *The Impact of Directive Leadership on Group Information Sampling, Decisions, and Perceptions of the Leader*, 26 COMM. RES. 349, 363 (1999) (showing that “when the leader supported a low quality decision to groups with a hidden profile, group decisions reflected leader preferences”).

<sup>58</sup> Cass R. Sunstein, *Deliberative Trouble? Why Groups Go to Extremes*, 110 YALE L.J. 71, 74 (2000).

alized to actual environments. Cognitive shortcuts—such as deference to charismatic individuals—may perform poorly in laboratory settings but, some argue, may exhibit contextual rationality if charisma is correlated with knowledge in the real world.<sup>59</sup>

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The fact that prior evidence has not provided a solid empirical grounding for Holmes's (or anyone else's) marketplace should not be surprising. These studies have largely not been conducted with Holmes, Mill, or free speech in mind.

One common difficulty in testing each of the three empirical propositions associated with the marketplace of ideas—truth as a cause, truth as an outcome, and groups versus individuals—is that for many realistic speech restrictions, truth is not easily defined.<sup>60</sup> Consider how we might formalize the conjectures in *Alvarez* about the likelihood that comprehensive information disclosure (in that case about who won a Congressional Medal of Honor) would cure false statements. To explore this possibility, we looked into the historical availability of campaign finance records and identified one state that went one year from having virtually no publicly available campaign finance information online to a comprehensive, searchable database. We thus hypothesized that, per *Alvarez*, such comprehensive disclosure might

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<sup>59</sup> For an overview of the debates about the rationality and soundness of various decision-making heuristics, see MARK KELMAN, *THE HEURISTICS DEBATE* 71 (2011).

<sup>60</sup> There are, however, important domains of free speech discussion and litigation in which the identification of truth is often more or less straightforward. Defamation is one obvious example: although the very existence of defamation law creates major free speech and free press issues, see, e.g., *N.Y. Times Co. v. Sullivan*, 376 U.S. 254, 300 (1964) (discussing general curtailment on public speech implicated by defamation liability), in many or even most modern defamation controversies the falsity of what was published is not an issue, only the publisher's knowledge of it. See, e.g., *Masson v. New Yorker Magazine, Inc.*, 501 U.S. 496, 496 (1991) (misattributed quotations); *Dun & Bradstreet, Inc. v. Greenmoss Builders, Inc.*, 472 U.S. 749, 749 (1985) (existence of a bankruptcy filing); *Gertz v. Robert Welch, Inc.*, 418 U.S. 323 (1974) (existence of a criminal record). Similarly, existing "commercial speech" doctrine excludes factual falsity from First Amendment protection. *Lorillard Tobacco Co. v. Reilly*, 533 U.S. 525, 554 (2001) ("For commercial speech to come within [the First Amendment], it at least must . . . not be misleading."); *Cent. Hudson Gas & Elec. Corp. v. Pub. Serv. Comm'n of New York*, 447 U.S. 557, 563–66 (1980) ("[T]here can be no constitutional objection to the suppression of commercial messages that do not accurately inform the public . . ."). The question whether even demonstrably false advertising statements are protected remains debated. See Chester S. Galloway, Herbert Jack Rotfeld & Jef I. Richards, *Holding Media Responsible for Deceptive Weight-Loss Advertising*, 107 W. VA. L. REV. 353, 353 (2005) (supporting liability for deceptive advertising); Robert L. Kerr, *From Sullivan to Nike: Will the Noble Purpose of the Landmark Free Speech Case Be Subverted to Immunize False Advertising?*, 9 COMM. L. & POL'Y 525, 566 (2004) (noting that whether First Amendment protection will be extended to any false commercial speech is for a future Court to decide).

increase the veracity of campaign speech. (There are of course reasons to think the contrary, as more campaign finance information might simply provide more ammunition for false assertions.) Yet fact-checking campaign advertisements—when campaign speech may embellish, but not outright lie—proved difficult. And unlike *Alvarez*, most actual speech restrictions involve normative (judgmental) propositions, where the ground truth is not ascertainable and thus the veracity of the statements is typically not readily confirmed.

At a minimum, existing research suggests great caution before accepting the irreducibly empirical propositions that the falsity of a statement will reliably produce rejection of that statement, that the marketplace will tend towards the location of truth, and that decentralized decisionmaking is necessarily better at identifying “truth” either in the Holmesian policy sense or for more straightforwardly factual propositions.

But perhaps the traditional fixation on truth is too narrow. When a marketplace of ideas—a free speech zone, in either the literal or figurative sense—is established, there may well be a range of consequences far broader than just the possibility that truths will be located—or not. Marketplaces of ideas have other consequences, including effects on who participates and who does not. A marketplace for ideas is a location, whether physical or virtual, and thus the decision to establish a marketplace of ideas, whether voluntarily or as a result of the First Amendment’s commands, may well have consequences for who enters that location, who leaves that location, and what the entrants do once they are there. In other words, understanding the effects of a marketplace for ideas solely in epistemic terms seems far too constricted an understanding of just what happens when an institution—governmental or otherwise—opens or closes the marketplace of ideas.

Guided by this insight and its consequent broadening of the empirical questions about the effects of a marketplace of ideas, we argue that one productive way forward consists not, or at least not solely, in attempting to design a test for possibly elusive truth, but rather to directly test for observable (but not necessarily epistemic) effects that result when speech restrictions close a marketplace of ideas.

### III

#### TESTING THE IMPACT OF SPEECH RESTRICTIONS

In light of the considerations sketched above, we set out to study the impact, whether epistemic or not, of the marketplace of ideas in a



context where it is commonly invoked: “time, place, and manner” restrictions on speech.

First Amendment time, place, and manner jurisprudence permits states to establish content-neutral “buffer zones,” that is distinct geographic areas of “enforced silence.” These buffer zones, which might prohibit speech within a zone or relegate speech to outside of a zone, exist across a wide array of institutions. State fairs and airports can limit the distribution of materials to fixed locations.<sup>61</sup> The National Labor Relations Board can limit electioneering “at or near the polls.”<sup>62</sup> Political conventions can relegate protests to a fixed demonstration zone outside a perimeter area.<sup>63</sup> Public universities can confine speech to a particular area on campus.<sup>64</sup> States may prohibit any person from knowingly approaching a person without consent within 100 feet of a health care facility.<sup>65</sup> Cities might prohibit picketing within 150 feet of a school.<sup>66</sup> States might ban disorderly conduct within 300 feet of a funeral.<sup>67</sup> The District of Columbia can in a content-neutral way prohibit signs and congregating within 500 feet of foreign embassies.<sup>68</sup> And cities may prevent the location of an adult theater within 1,000 feet of any residential zone, church, park, or school.<sup>69</sup>

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<sup>61</sup> See, e.g., *Int'l Soc'y for Krishna Consciousness, Inc. v. Lee*, 505 U.S. 672, 684–85 (1992) (holding the port authority may confine solicitation to sidewalks outside terminals); *Heffron v. Int'l Soc'y for Krishna Consciousness, Inc.*, 452 U.S. 640, 654 (1981) (upholding Minnesota's restrictions of solicitation on fairgrounds).

<sup>62</sup> See, e.g., *NLRB v. Carroll Contracting and Ready-Mix, Inc.*, 636 F.2d 111, 113 (5th Cir. Unit B Feb. 1981) (quoting *Claussen Baking Co.*, 134 NLRB 111, 112 (1961)).

<sup>63</sup> See, e.g., *Am. Civil Liberties Union of Colo. v. City and County of Denver*, 569 F. Supp. 2d 1142, 1178 (D. Colo. 2008) (finding a designated demonstration zone reasonable based on the high likelihood of violent protest).

<sup>64</sup> See, e.g., *Bloedorn v. Grube*, 631 F.3d 1218, 1237 (11th Cir. 2011) (upholding a designated free-speech zone policy that was consistently applied and was not highly discretionary).

<sup>65</sup> See, e.g., *Hill v. Colorado*, 530 U.S. 703, 734–35 (2000) (upholding a buffer zone law that limits speakers' rights to approach pedestrians within 100 feet of a health care facility).

<sup>66</sup> See, e.g., *Police Dep't of Chi. v. Mosley*, 408 U.S. 92, 92 (1972) (finding restriction unconstitutional because it impermissibly distinguished between labor picketing and other picketing).

<sup>67</sup> See, e.g., *Phelps-Roper v. Koster*, 734 F. Supp. 2d 870, 872 (W.D. Mo. 2010) (prohibiting protest activities in front of and near a funeral). The larger issue of what restriction might and might not be permissible at or near funerals was at the center of *Snyder v. Phelps*, 562 U.S. 443, 448–49, 460–61 (2011), where the Supreme Court rejected an intentional infliction of emotional distress action against religiously and politically inspired funeral protesters.

<sup>68</sup> See, e.g., *Boos v. Barry*, 485 U.S. 312, 316, 334 (1988) (finding prohibition on congregating within 500 feet of embassies constitutional).

<sup>69</sup> See, e.g., *City of Renton v. Playtime Theatres, Inc.*, 475 U.S. 41, 43 (1986) (finding such city provision constitutional).

How might such buffer zones affect the transmission of ideas? Are fewer or different kinds of individuals reached as a result? Do buffer zones affect the amount, type, or location of speech? Do buffer zones actually preserve, facilitate, or affect access to institutions around which they are implemented?

The approach we explore has several virtues. Most importantly, our approach allows for the direct assessment of the impact of speech restrictions on observable outcomes in real settings. We can thereby address the implicit counterfactual in most invocations of the marketplace and at the core of the second empirical understanding of the Holmes quotation: that unfettered speech, compared to some alternative institutional design that imposes speech restrictions, has consequences. Moreover, our results can concretely inform the policy considerations and First Amendment analysis in the applied context.<sup>70</sup> What matters in these cases is not whether wholesale speech restrictions *generally* impede truth-finding (as explained above, the literature evades such generalizations), but what effect the *specific* time, place, and manner restrictions being litigated might have. As we show below, our results have considerable implications for understanding the state interest in and speech effects of each buffer zone.

That said, this approach has limitations. While we examine the consequences of speech restrictions, we can say little about the impact on “truth” per se. In addition, as we explain below, identifying and designing credible natural experiments can be challenging, limiting the number of questions that can be answered. Natural experiments gain external validity relative to laboratory experiments, but the lack of control over the environment can pose greater challenges with internal validity.<sup>71</sup> Our pilot studies provide suggestions below for how to adapt and improve our research designs to better understand the impact of speech restrictions.

By examining natural experiments in “enforced silence,” we do not mean to disparage laboratory experimental work, some of which we hope to pursue ourselves. To the contrary, we hope our thoughts above will help researchers prospectively design experiments that more closely approximate the dynamic marketplace that many of the classic free speech thinkers (perhaps especially Mill) had in mind, as well as the institutional settings with which First Amendment law

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<sup>70</sup> On the value of assessing the effects of particular decisions and doctrines, see Richard A. Posner, *Against Constitutional Theory*, 73 N.Y.U. L. REV. 1, 11 (1998).

<sup>71</sup> Internal validity focuses on minimizing bias (in sample), while external validity “refers to the capability of the results of an experiment to be generalized” to other scenarios. WAYNE W. DANIEL, *INTRODUCTORY STATISTICS WITH APPLICATIONS* 240 (1977).

grapples. We focus on buffer zone natural experiments primarily because it struck us as uncharted territory, with many potential research designs to exploit. Indeed, to our knowledge, no prior empirical work has systematically examined the impact of speech restrictions in this fashion.

#### IV VOTING

##### A. *Background*

We first examine the case of voting, for it is here that the existence of buffer zones—prohibitions on speech at or near polling places—seemed most clearly to close off certain locations as marketplaces for ideas. In *Burson v. Freeman*,<sup>72</sup> the Supreme Court considered the constitutionality of a Tennessee election statute that prohibited “the display of campaign posters, signs or other campaign materials, distribution of campaign materials, and solicitation of votes for or against any person, political party, or position on a question”<sup>73</sup> within 100 feet of entrances to polling places. Freeman, a local campaign worker and once-candidate, brought a challenge to the buffer zone, arguing that it restricted her ability to communicate with voters in violation of the First Amendment. Writing for the plurality, Justice Blackmun found the statute to be content-based, but nonetheless upheld the buffer zone.<sup>74</sup> In light of the long history of intimidation and fraud near polling places, he concluded states have a compelling interest in “protecting voters against confusion and undue influence,” as well as “in ensuring that an individual’s right to vote is not undermined by fraud in the election process.”<sup>75</sup>

Addressing the argument that the statute was overinclusive—as the state arguably could have relied on narrowed statutes criminalizing specifically voter intimidation and interference—Justice Blackmun cited a Tennessee provision that affirmatively barred police officers from coming within ten feet of the entrance to polling locations. Because under that statute “law enforcement officers generally are barred from the vicinity of the polls,” Justice Blackmun reasoned

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<sup>72</sup> 504 U.S. 191 (1992).

<sup>73</sup> TENN. CODE ANN. § 2-7-111(b) (2014).

<sup>74</sup> The plurality held that the buffer zone was not content-neutral because it prohibited campaign speech but not other categories of speech (such as commercial solicitation). *Burson*, 504 U.S. at 197. But while the restriction drew distinctions on the basis of subject matter, it did not restrict on the basis of viewpoint. For example, it was not a restriction that limited Republicans more than Democrats, or one that imposed greater burdens on the opponents of some ballot initiative than on the supporters.

<sup>75</sup> *Id.* at 199.

that many acts of election interference would go undetected absent the buffer zone.<sup>76</sup> (The court cited no other state law, so “generally are barred” almost surely referred to the fact that the same Tennessee provision allowed police officers to enter the polling place at the request of the poll worker or for the officer to vote.) “The real question then is *how large* a restricted zone is permissible or sufficiently tailored.”<sup>77</sup> Justice Blackmun noted that the state did not need to provide empirical evidence of the effect of the buffer zone on political stability.<sup>78</sup> “[T]he long, uninterrupted, and prevalent use of these statutes” would make it “difficult to make specific findings about the effects of a voting regulation.”<sup>79</sup> The buffer zone was in that sense prophylactic, given that the remedy of “[r]erunning an election would have a negative impact on voter turnout.”<sup>80</sup>

Justice Stevens, joined by Justices O’Connor and Souter, dissented, noting that “[t]he First Amendment affords the broadest protection to such political expression in order to assure the unfettered interchange of ideas for the bringing about of political and social changes desired by the people.”<sup>81</sup> The dissent calculated that the buffer zones collectively encompassed a large area of the state: over 30,000 square feet.<sup>82</sup> Under state election law, 12 of 95 counties also had expanded buffer zones of 300 feet.<sup>83</sup> Justice Stevens reasoned that the advent of the secret ballot and the reduction in election corruption over the course of a century obviated the need for buffer zones.<sup>84</sup> The buffer zone was content-based, systematically disadvantaging “[c]andidates with fewer resources, candidates for lower visibility

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<sup>76</sup> *Id.* at 207.

<sup>77</sup> *Id.* at 208.

<sup>78</sup> *Id.* at 208–09.

<sup>79</sup> *Id.*

<sup>80</sup> *Id.* at 209.

<sup>81</sup> *Id.* at 217 (Stevens, J., dissenting) (citations omitted) (internal quotations omitted).

<sup>82</sup> *Id.* at 218.

<sup>83</sup> Section 1 of chapter 362 of the public acts of 1987 amended TENN. CODE ANN. § 2-7-111(a) to assign a 300-foot buffer zone to Bradley, Blount, Carter, Greene, Loudon, McMinn, Monroe, Polk, Sumner, Unicoi, and Wilson counties after Bradley County attempted to adopt a 300-foot buffer zone by referendum (which passed by 84%). 1987 Tenn. Pub. Acts 723; The Three Hundred Foot (300’) Election Boundary, 87-185 Op. Att’y Gen. of Tenn. (1987) (discussing the law, noting which counties were affected, and providing a summary of legislative history). An opinion of the Tennessee Attorney General confirms that in 2002, Blount, Bradley, Carter, Claiborne, Greene, McMinn, Monroe, Polk, Sumner, Unicoi, and Wilson counties still had a 300-foot buffer zone. Tenn. Code Ann. § 2-7-111 – 100 Foot Boundary – Campaign Free Zone – Applicability to Adjacent Private Property, 02-118 Op. Att’y Gen. of Tenn. (2002). However, TENN. CODE ANN. § 2-7-111(a) currently no longer includes this provision for separate 300-foot buffer zones. TENN. CODE ANN. § 2-7-111(a) (2014).

<sup>84</sup> *Burson*, 504 U.S. at 220–22 (Stevens, J., dissenting).

offices, and ‘grassroots’ candidates.”<sup>85</sup> To be effective, it would have been counterproductive for Freeman to intimidate, harass, or interfere with voters, as her goal was to persuade voters. Lower courts have found that buffer zones can also hamper exit polls,<sup>86</sup> which some believe to be an “indispensable part of democracy”<sup>87</sup> and which are an important tool for researchers, politicians, and the media to understand and participate in the democratic process.<sup>88</sup>

*Burson* is the antithesis of most understandings of a free speech principle, and certainly of those understandings relying on some conception of the marketplace of ideas. For “Freeman and other advocates [who aimed to persuade voters on their way to the voting booth] . . . the environment of the polling place was the truest sort of ‘marketplace of ideas.’”<sup>89</sup> While in the classic marketplace theory, as Justice Brandeis observed in *Whitney*, the remedy for confusion and fraud is counter-speech,<sup>90</sup> the *Burson* Court affirmed the speech restriction.<sup>91</sup> In any of the epistemic interpretations of the marketplace of ideas, such restrictions should ultimately impede the search for truth.

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<sup>85</sup> *Id.* at 224.

<sup>86</sup> Prior to *Burson*, several lower courts invalidated buffer zones with respect to exit polling. *See, e.g.*, *Daily Herald Co. v. Munro*, 838 F.2d 380, 389 (9th Cir. 1988); *CBS Inc. v. Smith*, 681 F. Supp. 794, 806 (S.D. Fla. 1988); *Nat’l Broad. Co. v. Cleland*, 697 F. Supp. 1204, 1215 (N.D. Ga. 1988) (adopting a “narrowing construction” of Georgia statute and enjoining the enforcement of buffer zones beyond twenty five feet from a polling place). Post-*Burson*, as Table 1 shows, numerous states prohibit entering buffer zones, seemingly impeding exit polling, while other states were uncertain about whether anti-electioneering statutes covered exit polling, which led to protracted litigation about applicability to exit polling.

<sup>87</sup> Richard Hilmer, *Exit Polls—A Lot More than Just a Tool for Election Forecasts*, in *PUBLIC OPINION POLLING IN A GLOBALIZED WORLD* 93, 107 (Marita Carballo & Ulf Hjelmar eds., 2008). Whether exit polls also have negative effects depends in part on when and how the results of those polls are publicized. When exit poll results are made available prior to the close of polls, some voters may be dissuaded from voting at all, and various other deleterious consequences may occur. *See, e.g.*, Michael X. Delli Carpini, *Scooping the Voters? The Consequences of the Networks’ Early Call of the 1980 Presidential Race*, 46 J. POL. 866 (1984) (examining the effects of publicized exit poll results on voter turnout).

<sup>88</sup> *See* DAVID R. TARR & BOB BENENSON, *ELECTIONS A TO Z* 196 (4th ed. 2012) (“Few aspects of modern American politics are as inherently useful to political scientists, party strategists, and the news media as exit polls.”).

<sup>89</sup> BRIAN K. PINAIRE, *THE CONSTITUTION OF ELECTORAL SPEECH LAW: THE SUPREME COURT AND FREEDOM OF EXPRESSION IN CAMPAIGNS AND ELECTIONS* 134 (2008).

<sup>90</sup> *Whitney v. California*, 274 U.S. 357, 377 (1927) (Brandeis, J., concurring). The marketplace of ideas and Justice Brandeis’s maxim are often taken in tandem, *see, e.g.*, *United States v. Alvarez*, 132 S. Ct. 2537, 2550 (2012) (quoting Justice Brandeis in *Whitney* together with Justice Holmes in *Abrams*), even if, as we explore above, they may well be importantly different.

<sup>91</sup> 504 U.S. 191, 211 (1992) (plurality opinion).

### B. Research Design

To evaluate the effects of the speech restriction, consider the ideal hypothetical experiment. A researcher might randomize buffer zones across polling locations or elections and measure various outcomes, including reports of voting fraud and voter intimidation (to capture state interests), as well as campaign, voter turnout, deliberation, and election outcomes (to capture speech effects).

As the *Burson* plurality notes, because nearly all states have long-standing buffer zones, it would be difficult to replicate this experiment with state-level variation. Yet with respect to at least one outcome, one possible natural experiment exists, capitalizing on the fact that the buffer zone radius is sharply and arbitrarily defined, ranging anywhere from 10 to 600 feet. (Until 1994, Hawaii's was 1,000 feet.<sup>92</sup>) Whether a voter lives just inside or just outside of the buffer zone is plausibly random. Voters just outside of the buffer zone are subject to the "treatment" of campaign information (buttons, campaign posters, canvassing) and voters just inside of the buffer zone are not, yet because of the arbitrariness of the boundary, the two groups close to the threshold are plausibly comparable along all other dimensions.<sup>93</sup>

We hypothesize that one beneficial effect of same-day campaigning is that it may *cue* or remind the voter of the existence of the election. Prior research has established that many voters cite inconvenience as an impediment to voting, or simply forget to vote on election day.<sup>94</sup> As Dale and Strauss put it: "some registered voters are

<sup>92</sup> HAW. REV. STAT. § 11-132 (1993).

<sup>93</sup> Because polling locations are not randomly assigned, where voters live matters. A simple comparison of voters inside and outside of a buffer zone would almost surely be confounded. For instance, depending on the jurisdiction, proximity to a school (a common polling place location) can increase home value. As a result, voters close to the school may also earn higher incomes than those living far away from the school, so that one could not attribute turnout differences to the buffer zone. The critical assumption in a regression discontinuity design is that all pretreatment covariates are *smooth* at the threshold, so that the treatment effect can be estimated at the threshold  $c$  as:

where  $Y$  represents the outcome of interest and  $Z$  represents the running variable. Jinyong Hahn, Petra Todd & Wilbert Van der Klaauw, *Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design*, 69 *ECONOMETRICA* 201, 204 (2001). Most voters, of course, travel some distance to the voting booth, but our approach uses only voters sufficiently close to the polling place to identify turnout effects. Turnout has been shown to decline monotonically in travel distance to the voting booth. See *infra* note 98 (providing sources with prior research that supports this point).

<sup>94</sup> See, e.g., CHARLES S. BULLOCK III ET AL., A SURVEY OF GEORGIA VOTERS IN THE 2008 GENERAL ELECTION 7, 10, tbl.3 (2009), available at [http://www.pewtrusts.org/~media/legacy/uploadedfiles/pes\\_assets/2009/GAvoterspdf.pdf](http://www.pewtrusts.org/~media/legacy/uploadedfiles/pes_assets/2009/GAvoterspdf.pdf) (finding that 16% of Georgia voters vote absentee because they worry they might forget to vote on election day); Allison Dale & Aaron Strauss, *Don't Forget to Vote: Text Message Reminders as a Mobilization*

less in need of persuasion to participate than they are in need of a reminder to make time in their busy schedules to go to the polls.”<sup>95</sup> Even if voters are exposed to election day activity when away from home, campaign activity close to (or visible from) home may more concretely remind them to head to the polling place.

The *Burson* majority considered higher turnout itself a desirable end,<sup>96</sup> presumably because it may facilitate more accurate aggregation of voter preferences, although that proposition is contested in political science.<sup>97</sup> We test for the effect of prohibitions on voter turnout using a regression discontinuity design. Figure 1 displays the intuition of this design, plotting the distance from a polling place on the *x*-axis, with the vertical line representing the buffer zone threshold, and the probability of turnout on the *y*-axis with simulated, hypothetical data. The left panel displays the probability curve under no speech effect: turnout monotonically declines in distance to the voting booth, which is consistent with prior research.<sup>98</sup> The right panel plots what we would expect under cueing: while turnout decreases in distance, there is a discontinuous jump right outside of the buffer zone, representing the individuals exposed to same-day campaigning that cued them to

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*Tool*, 53 AM. J. POL. SCI. 787, 787 (2009) (finding in a field experiment that some voters simply need a noticeable reminder to vote).

<sup>95</sup> Dale & Strauss, *supra* note 94, at 787.

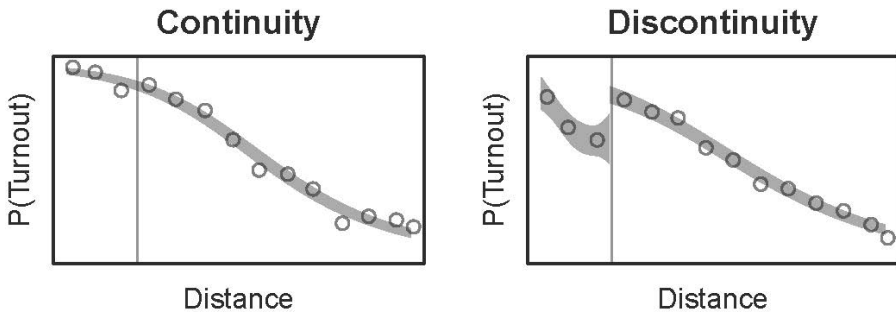
<sup>96</sup> See *Burson v. Freeman*, 504 U.S. 191, 209 (1992) (rejecting rerunning an election as an “imperfect” remedy, because it would lower voter turnout).

<sup>97</sup> The literature here is voluminous. Among the important contributions are: Larry M. Bartels, *Uninformed Votes: Information Effects in Presidential Elections*, 40 AM. J. POL. SCI. 194, 218 (1996) (rejecting the hypothesis that a mass electorate produces the same aggregate outcome as would occur if voters were fully informed); see also ARTHUR LUPIA & MATHEW D. MCCUBBINS, *THE DEMOCRATIC DILEMMA: CAN CITIZENS LEARN WHAT THEY NEED TO KNOW?* (1998) (arguing that limited information doesn’t necessarily stop people from making reasoned choices, especially under certain conditions); Samuel L. Popkin & Michael A. Dimock, *Political Knowledge and Citizen Competence*, in *CITIZEN COMPETENCE AND DEMOCRATIC INSTITUTIONS* 117 (Stephen L. Elkin & Karol Edward Softan eds., 1999) (arguing that voters use information shortcuts in the absence of factual information that allows them to make reasoned choices, despite being uninformed).

<sup>98</sup> See Joshua J. Dyck & James G. Gimpel, *Distance, Turnout, and the Convenience of Voting*, 86 SOC. SCI. Q. 531, 533 (2005) (finding distance strongly affects voting behavior); J.G. Gimpel & J.E. Schuknecht, *Political Participation and the Accessibility of the Ballot Box*, 22 POL. GEO. 471, 481–84 (2003) (concluding that distance imposes costs that decrease voter turnout from short to middling ranges of distances); Moshe Haspel & H. Gibbs Knotts, *Location, Location, Location: Precinct Placement and the Costs of Voting*, 67 J. POL. 560, 560–61 (2005). But see Henry E. Brady & John E. McNulty, *Turning Out to Vote: The Costs of Finding and Getting to the Polling Place*, 105 AM. POL. SCI. REV. 115, 116 (2011) (finding that changing polling locations so that they were further away led to a decrease in overall turnout largely due to “a search effect resulting from the costs of finding and going to a new polling place” and not transportation costs “because the decision not to vote appears to be essentially unaffected by the distance to the polling place”).

vote. In a regression discontinuity (RD) design, the crucial comparison is the set of voters close to the threshold of the buffer zone. Individuals who live in the same location of the polling place, for instance, are actually likely to have higher turnout levels,<sup>99</sup> even without exposure to campaign materials, as the travel distance is effectively zero and passing by the polling location (e.g., in the lobby of their building) may have the same cueing effect as campaign activity.<sup>100</sup>

FIGURE 1



*Notes:* Intuition of regression-discontinuity design. The left panel displays simulated data when the probability of turnout is a monotonic function of distance to the voting booth. The right panel displays simulated data when the probability of turnout discontinuously decreases within the buffer zone, marked by the grey vertical line.

By virtue of the First Amendment, and thus by virtue of the unavailability in practice of a range of restrictions that could not constitutionally be implemented, conceiving of research designs to test the causal effects of speech restrictions is inherently difficult. Our primary purpose, given the Symposium’s goals, is to explore how the RD design provides a way forward to assess the impact of any arbitrary cutoff. The design, however, could still be improved in several respects. First, the biggest challenge is that buffer zones are conven-

<sup>99</sup> See Haspel & Knotts, *supra* note 98, at 567–69 (finding that the likelihood of voting decreased dramatically when the polling place was located 0.69 miles away from voters as opposed to 0.01 miles away from their residence).

<sup>100</sup> There are strong reasons to believe that such “residence effects” are likely to exist. Roughly 37% and 22% of polling places in Manhattan and Hudson County, respectively, are located in residential buildings—including government-owned or low-income housing, senior centers, university housing, and privately owned apartment buildings. Further, when public buildings are scarce, private buildings may be used as polling places at the discretion of owners. Owners granting such permission are likely to differ considerably from owners refusing to host a polling place in terms of political consciousness. As well, some jurisdictions expressly provide for polling places to be established in public and private nursing homes to facilitate voting amongst the elderly. See, e.g., MICH. COMP. LAWS ANN. § 168.662(3) (West 2014) (allowing the establishment of a polling place where elderly reside with certain conditions); NEV. REV. STAT. § 293.2735 (2013) (same); R.I. GEN. LAWS § 17-11-1 (2013) (same).



tionally relatively small. Depending on the jurisdiction, however, cueing effects are nonetheless possible.

Figure 2, for instance, displays an example of a 100-foot buffer zone surrounding a polling place in Hudson County, New Jersey. The right panel displays the location of the entrance to the polling place (marked by the oval). While individuals living specifically at the polling location may be cued to the existence of the election, voters living farther away within the buffer zone (from the vantage point of the right panel) may not observe as many campaign cues as voters living outside of the buffer zone. (Poll workers are instructed solely to place one unobtrusive “vote here” sign at the main entrance to polling places.<sup>101</sup>) More importantly, for our purposes, to the extent that the size of the buffer zone is a concern, a similar design could be adapted to study (a) high-density municipalities in states with larger buffer zones, such as Louisiana’s 600-foot radius,<sup>102</sup> (b) counties in Tennessee that were assigned an expanded buffer zone of 300 feet,<sup>103</sup> (c) a 1000 foot radius around adult theaters,<sup>104</sup> (d) the impact of free speech zones (e.g., on college campuses) given the arbitrariness of residential proximity to such zones, or (e) the impact of geographic restrictions of access to various media, such as locational restrictions on newspaper boxes, newsstands, billboards, and the like.

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<sup>101</sup> BD. OF ELECTIONS IN THE CITY OF N.Y., POLL WORKER’S MANUAL 38 (2012), available at <http://vote.nyc.ny.us/downloads/pdf/documents/boe/pollworkers/pollworkersmanual.pdf>; N.J. DIV. OF ELECTIONS, STATE OF N.J., DISTRICT BOARD MEMBER TRAINING MANUAL 7 (2011), available at <http://www.state.nj.us/state/elections/publications/boardworkers-manual-080912.pdf>.

<sup>102</sup> LA. REV. STAT. ANN. § 18:1462(A) (2013).

<sup>103</sup> See *supra* note 83 (discussing twelve out of ninety-five counties in Tennessee having adopted a 300-foot buffer zone).

<sup>104</sup> See *City of Renton v. Playtime Theatres*, 475 U.S. 41, 43 (1986) (upholding zoning ordinance prohibiting adult theaters from being located within 1000 feet of a church, school, or park).

FIGURE 2



Notes: Example of buffer zone in left panel and street view from residence within buffer zone, with polling entrance marked with a white-lined oval.

Second, focusing on turnout is obviously only one component of the calculus in *Burson*. We cannot easily measure the effect on substantive voting outcomes (due to the secret ballot), the ability of independent candidates to get out their message, or congestion at the voting booth.<sup>105</sup>

### C. Data

#### 1. Jurisdictions

To illustrate how the design might be carried out, we have selected jurisdictions based on several criteria. First, we have focused on jurisdictions with high population density relative to the size of the buffer zone, as statistical power depends considerably on the number of units around the threshold. Second, we have selected jurisdictions with available voter and polling place addresses to geocode each voter's distance to the closest polling place.<sup>106</sup> Third, we have focused on jurisdictions with low rates of absentee/mail voting, as buffer zones are typically only effective on election day.

<sup>105</sup> Absent the buffer zone, if the *Burson* plurality is right, we should expect long lines or crowds of individuals around the polling place, and a general reduction in turnout.

<sup>106</sup> Formal costs and availability of the voter file vary considerably, as does actual practice. According to election regulations, N.Y. ELEC. LAW § 3-103 (McKinney 2007) (providing that “[t]he information contained in the statewide voter registration list shall not be used for non-election purposes”), the New York voter file is not available for academic research, but we secured it easily. With a buffer zone of 600 feet, New Orleans might have been an ideal jurisdiction to study, but obtaining its voter file turned out to be relatively expensive (\$5000).

TABLE 1

State	(A) Voting			(B) Polling Place		(C) Buffer Zone			(D) Police Presence			(E)		(F) Demographics				
	2008 Early Voting	Voter File Cost (\$)	Voter File Available	Preference for Public	No Private Building	Radius (feet)	Campaign Activity	Person	Def. Barred	Def. Allowed	Unrestricted	Densest Jurisdiction %City = County; Mun. = Municipality	Pop. Density	Poverty Rate	Median Income (\$1000)	% College Degree	% Renter	% Non-white Pop.
NY	4	0		✓		100	✓		✓	✓	New York County	69,468	18	68	58	77	43	
CA	45	30	✓			100	✓		✓		San Francisco Cty.	17,179	13	74	52	64	52	
NJ	7	3	✓			100	✓	+	✓	✓	Hudson County	13,731	16	59	36	68	46	
MA	7	0				150	✓		✓	✓	Suffolk County	12,416	21	53	40	65	44	
PA	4	20	✓	✓	✓	0/10		✓	✓		Philadelphia Cty.	11,380	26	37	23	46	59	
VA	14	4,330		✓		40	✓	+		✓	Alexandria City	9,314	8	84	61	57	39	
MD	11	125	✓	✓		100	✓		✓	✓	Baltimore City	7,672	23	41	26	52	70	
IL	9	2,000	✓	✓		100	✓		✓	✓	Cook County	5,495	16	55	34	42	45	
MO	11	127	✓	✓		25	✓		✓	✓	St. Louis City	5,158	27	34	29	55	56	
WI	21	12,500	✓	✓		100	✓		✓	✓	Milwaukee County	3,926	21	44	28	49	39	
CO	78	500	✓	✓		100	✓			✓	Denver County	3,923	19	49	42	50	31	
FL	54	10	✓			100	✓			✓	Pinellas County	3,348	13	46	27	33	18	
MN	10	46				100	✓	✓	✓		Ramsey County	3,342	17	53	39	39	30	
MI	20	22	✓	✓		100	✓		✓	✓	Wayne County	2,974	24	42	21	36	48	
OH	30	0	✓	✓		100	✓	+	✓	✓	Cuyahoga County	2,800	18	44	29	39	36	
TX	67	1,100	✓	✓		100	✓	+	✓	✓	Dallas County	2,718	19	49	28	47	47	
GA	53	500	✓	✓	✓	150	✓		✓	✓	DeKalb County	2,586	19	51	39	43	67	
IN	24	500				50	✓	✓			Marion County	2,280	19	43	28	44	37	
RI	5	50	✓			50	✓		✓	✓	Bristol County	2,064	8	71	42	29	4	
LA	15	5,000	✓	✓		600	✓			✓	Orleans Parish	2,029	27	37	33	52	67	
KY	6	450				300	✓		✓	✓	Jefferson County	1,948	17	47	30	37	27	
NC	60	0	✓			50	✓			✓	Mecklenburg Cty.	1,756	15	56	40	39	45	
OR	100	500	✓			100	✓			✓	Multnomah County	1,705	17	52	39	45	24	
HI	39	250	✓	✓		200	✓	✓			Honolulu County	1,587	10	72	32	44	79	
NE	22	500	✓			200	✓		✓	✓	Douglas County	1,574	14	53	36	37	24	
CT	10	300	✓			75	✓	+	✓	✓	Fairfield County	1,467	9	83	45	31	25	
UT	37	1,050	✓			150	✓		✓	✓	Salt Lake County	1,387	12	60	31	33	19	
DE	5	250	✓	✓		50	✓	✓			New Castle County	1,263	11	65	34	30	35	
TN	59	2,500	✓			100	✓		✓		Davidson County	1,243	19	47	35	44	39	
KS	35	200	✓			250	✓			✓	Johnson County	1,150	6	75	52	29	14	
OK	13	150	✓			300/50	✓	✓			Tulsa County	1,058	15	48	30	39	31	
WA	89	30				0	✓		✓	✓	King County	913	11	71	46	41	31	
IA	36	1,000	✓	✓		300	✓	+			Polk County	751	11	58	34	31	15	
AL	4	27,000	✓	✓		30	✓	✓	✓	✓	Jefferson County	593	17	45	29	35	47	
SC	17	1,795				200	✓		✓	✓	Greenville County	575	15	48	31	33	26	
NM	62	4,000	✓	✓		100	✓			✓	Bernalillo County	571	17	48	32	37	31	
AR	37	3	✓			100	✓		✓	✓	Pulaski County	504	17	46	32	40	43	
NH	10	17,838				10	✓	✓	✓	✓	Hillsborough Cty.	457	8	70	35	33	10	
WV	24	14,025	✓			300	✓	✓			Ohio County	420	15	31	27	33	7	
AZ	53					75	✓	✓			Maricopa County	415	16	54	30	36	27	

State	(A) Voting			(B) Polling Place		(C) Buffer Zone			(D) Police Presence			Densest Jurisdiction *(City = County; Mun. = Municipality)	Pop. Density	(F) Demographics				
	2008 Early Voting	Voter File Cost (\$)	Voter File Available	Preference for Public	No Private Building	Radius (feet)	Campaign Activity	Person	Def. Barred	Def. Allowed	Unrestricted			Poverty Rate	Median Income (\$1000)	% College Degree	% Renter	% Non-white Pop.
NV	67	107	✓	✓		100	✓				✓	Carson City	382	15	54	21	41	20
ID	30	20	✓			100	✓		✓	✓		Ada County	373	12	55	35	32	10
MS	10	2,100	✓	✓		150/30	✓	✓				DeSoto County	339	10	59	21	24	18
ME	31	2,000	✓			250	✓			✓		Cumberland Cty.	337	11	57	40	33	7
VT	29	0	✓	✓	✓	0	✓				✓	Chittenden County	292	12	64	46	35	8
SD	25	2,500				100	✓		✓			Minnehaha County	210	11	52	29	35	12
AK	30	178	✓			200	✓				✓	Anchorage Mun.	171	8	76	33	40	34
ND	37					100	✓				✓	Cass County	85	13	51	37	46	8
MT	40	1,000	✓			100	✓		✓	✓		Yellowstone Cty.	56	12	51	29	32	9
WY	25	50				300	✓				✓	Laramie County	34	10	55	24	32	12

Notes: 2008 Early Voting includes early in-person, absentee, or mail voting. Voter File Cost indicates the estimated cost of securing the voter file and Voter File Available indicates whether the voter file is available based on registrar website or phone call. Location of polling place information was collected from each state’s election code. Preference for Public indicates that state election law states a preference for polling places in public or tax exempt buildings. No Private Building means that a state’s code does not provide any exceptions to polling places being located in public buildings. Information about polling place buffer zones and statutory provisions pertaining to police presence inside the buffer zone is provided at the state level and was collected from each state’s election code. Radius of the buffer zone for Vermont and Washington are coded as 0, as these states only have a restriction on electioneering within the building of the polling place. Oregon has a 100-foot buffer zone around ballot drop-off locations located in state or local government elections offices, but not around other drop-off locations such as libraries. Because Oregon and Washington mail ballots in advance and ballots can be turned in any time after received and before the end of election day, these buffer zones run for longer periods. See OR. REV. STAT. § 260.695 (2013) (providing buffer zone “beginning on the date that ballots are mailed to electors . . . and ending on election day”); WASH. REV. CODE ANN. § 29A.84.510 (West 2014) (providing that the buffer zone is in effect “[d]uring the voting period that begins eighteen days before and ends the day of” any election). Pennsylvania has a hybrid buffer zone of barring individuals ten feet within the polling place, but barring campaign materials only in the polling place itself. 25 PA. CONS. STAT. ANN. § 3060(c), (d) (West 2007). It is the only state with a police buffer zone larger (100 feet) than the electioneering buffer zone. 25 PA. CONS. STAT. ANN. § 3047 (West Supp. 2014). Mississippi has a hybrid buffer zone, with a 150-foot non-electioneering zone and a thirty-foot buffer banning persons other than election officers. MISS. CODE ANN. § 23-15-245 (West 2014); MISS. CODE ANN. § 23-15-895 (West 2014). Campaign Activity indicates whether the buffer zone prohibits campaign-related activities, such as the solicitation of votes, posting of signs, or distribution of literature. Person indicates whether the buffer zone prohibits all persons (save for individuals like election officials and persons in the act of voting). Sometimes this prohibition is subject to a smaller radius. A (+) sign indicates that the buffer zone prohibits loitering, but not entering per se. For police presence, an asterisk (\*) indicates that police are not generally restricted from the buffer zone, but are restricted from the polling room. See, e.g., FLA. STAT. ANN. § 102.031 (West 2013). Def. Barred means that the election law specifically mentions that police are not allowed in the buffer zone, sometimes within a smaller radius. See, e.g., MINN. STAT. ANN. § 204C.06 (West 2009). Def. Allowed indicates that state election law contemplates or requires police presence or the state election division confirmed police presence. Unrestricted indicates that there are no restrictions on police presence, which occurs when (i) election law allows individual access without mentioning police, (ii) election law disallows individual access, but exempts police, or (iii) election law restricts individual access but election officials confirmed police presence inside the buffer zone. In all cases where police are generally restricted, police would be allowed in the buffer zone if called by election officials. Densest Jurisdictions represent the county or independent city, depending on the relevant voting authority, with the highest population density. Pop. density is the population per square mile of the voting authority according to the 2010 census. Demographic information is from the United States

Census Bureau's American Fact Finder website. From the 2008–2012 American Community Survey 5-Year Estimates, *Poverty Rate* is the proportion of individuals below the poverty line; *Median Income* is the median household income in \$1000s; and *% College Degree* is the percentage of the population 25 years and over holding at least a Bachelor's degree. *% Renter* is the percentage of occupied housing units that are renter occupied according to the General Housing Characteristics section of the 2010 Census Summary File 1. *% Non-white Pop.* is calculated from the percentage of white residents according to the Race and Hispanic or Latino Origin section of the 2010 Census Summary File 1. Appendix A lists the sources in more detail.

Table 1 displays attributes of the state and county relevant to selecting the jurisdiction. The rows are sorted by decreasing population density of the densest county (or independent city) in each state. The first column of group (A) indicates the rate of early voting in 2008, including in-person, absentee, and mail voting. The radius column in group (C) indicates the size of the buffer zone by feet of the radius. Most states prohibit campaign activity,<sup>107</sup> but some states ban the presence of any person not in the act of voting (with the exception of election officials).<sup>108</sup> In all but three states, polling places may be located in private buildings, although most election laws state a preference for the use of public buildings.<sup>109</sup>

Group (F) presents basic demographic information about the fifty jurisdictions (typically at the county level). Because population density will greatly affect the number of voters living inside a buffer zone, the potential distributive dynamics of who is affected by the buffer zone become quickly apparent. Higher density jurisdictions are more likely to be urban. Measures of poverty, education, renters (versus owners), and the minority population are each statistically significantly correlated with population density (logged for skewness).

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<sup>107</sup> See, e.g., ALASKA STAT. § 15.15.170 (2014) (prohibiting individuals from persuading a person to vote for or against a candidate, proposition, or question within 200 feet of a polling place); ARIZ. REV. STAT. ANN. § 16-515(A), (F)–(G) (2015) (forbidding electioneering within 75 feet of a polling place); ARK. CODE ANN. § 7-1-103(9)(A) (Supp. 2013) (banning the distribution of campaign literature, solicitation of signatures or contributions, and electioneering of any kind within 100 feet of a polling place); COLO. REV. STAT. § 1-13-714 (2014) (prohibiting electioneering within 100 feet of a polling place).

<sup>108</sup> See, e.g., MINN. STAT. ANN. § 204C.06 (West 2009) (providing that “[n]o one except an election official or an individual who is waiting to register or to vote shall stand within 100 feet of the building in which a polling place is located” and that “[e]xcept when summoned by an election judge to restore the peace or when voting or registering to vote, no peace officer shall enter or remain in a polling place or stand within 50 feet of the entrance of a polling place”).

<sup>109</sup> See, e.g., N.J. STAT. ANN. § 19:8-3 (West 2014) (stating a preference for schoolhouses or public buildings); N.Y. ELEC. LAW § 4-104 (McKinney Supp. 2015) (stating a preference for tax-exempt buildings); TEX. ELEC. CODE ANN. § 43.031 (West 2010) (stating a preference for public buildings).

## 2. *New Jersey and New York*

Based on this jurisdictional information, our pilot study focused on the general election of 2012<sup>110</sup> in Hudson County (New Jersey)<sup>111</sup> and Manhattan (coextensive with New York County).<sup>112</sup> Each of these has (a) very high population density relative to a 100 foot buffer zone, (b) accessible statewide voter files, (c) historical polling place locations, (d) low rates of absentee or early voting, and (e) buildings with private residences serving as polling locations.<sup>113</sup>

We proceeded by geocoding locations of roughly 312,000 Hudson County and 1.05 million Manhattan registered voters active in 2012, as well as 450 Hudson County and 1,206 New York polling place locations in 2012.<sup>114</sup> Based on these latitudes and longitudes, we calculated the closest polling location (geodesic distance) for each registered voter.<sup>115</sup>

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<sup>110</sup> The focus on the general election of 2012 trades off two potential considerations. While we might expect voters to be less aware of the existence of a local, primary, or off-cycle election, the likelihood of common campaign materials (lawn signs, window placards) is also much lower in those elections. To ensure accuracy of voter addresses and polling locations, we focus on a relatively recent general election.

<sup>111</sup> Hudson County contains several cities with high population density, including Jersey City, Union City, Bayonne, and Hoboken.

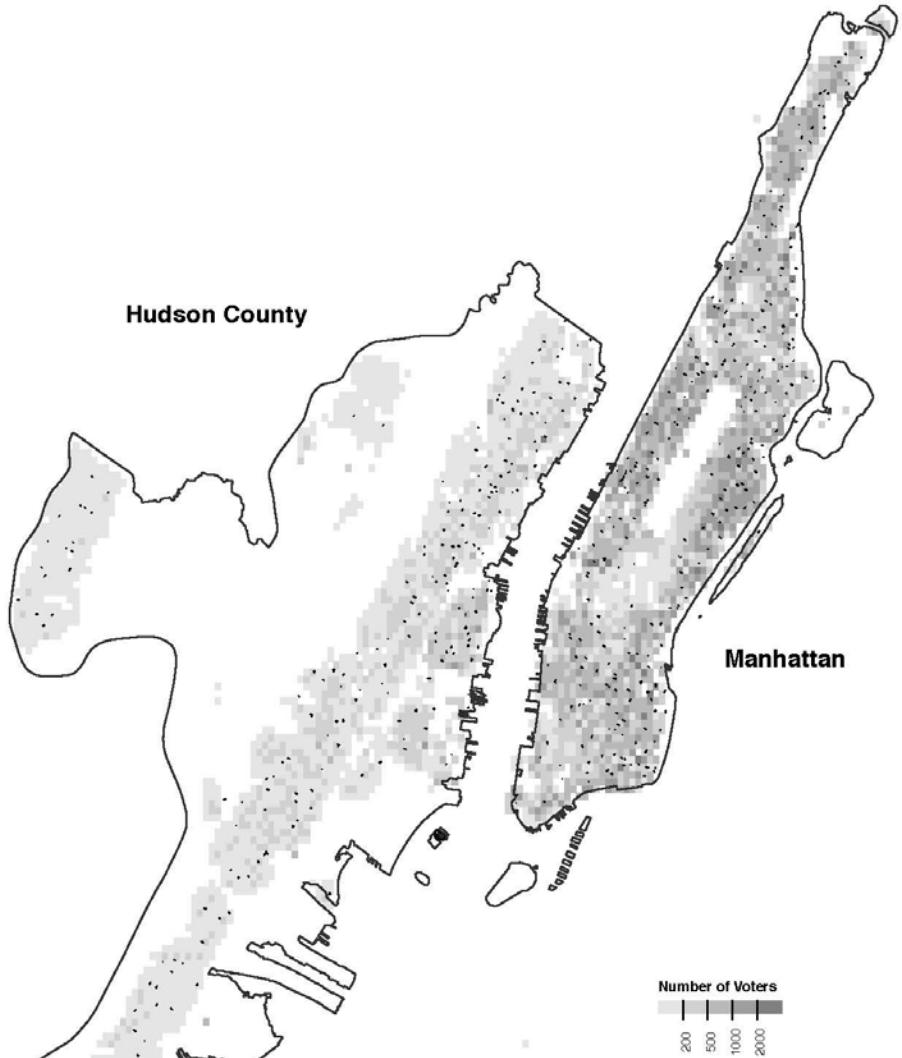
<sup>112</sup> In San Francisco, the second densest jurisdiction, the majority of voting is now absentee, making it a poor test case.

<sup>113</sup> See N.Y. ELEC. LAW § 4-104 (McKinney Supp. 2015) (indicating preference for public buildings, but not affirmatively excluding private buildings); N.J. STAT. ANN. § 19:8-3 (West 2014) (affirmatively providing for the use of private buildings). Two examples are 505 LaGuardia Place in New York and 411 Marshall Drive in Hoboken, New Jersey.

<sup>114</sup> Our data has turnout and registration rates that are consistent with reported statistics. 199,061 out of 341,253 registered voters in Hudson County and 603,336 out of 974,855 registered voters in Manhattan turned out to vote for the 2012 general election. *Number of Registered Voters and Ballots Cast, General Election Results, November 6, 2012, Hudson County*, DEPT. OF STATE, STATE OF N.J. (Nov. 6, 2012), <http://www.state.nj.us/state/elections/2012-results/2012-ballotscast-hudson.pdf>; BD. OF ELECTIONS, CITY OF N.Y. ANNUAL REPORT 2012, at 22 (2012), available at <http://vote.nyc.ny.us/downloads/pdf/documents/boe/AnnualReports/BOEAnnualReport12.pdf>.

<sup>115</sup> We used the closest polling location regardless of whether it was in fact the voter's registered polling location. This is because voters would still be subject to the treatment of the buffer zone, regardless of which polling location is the cause. Geocoding rates (the percentage of addresses, from the voter file and the list of polling places, that we were able to match with actual locations) were 97% and 99% for voters and 91% and 100% for polling places in Hudson County and Manhattan, respectively.

FIGURE 3

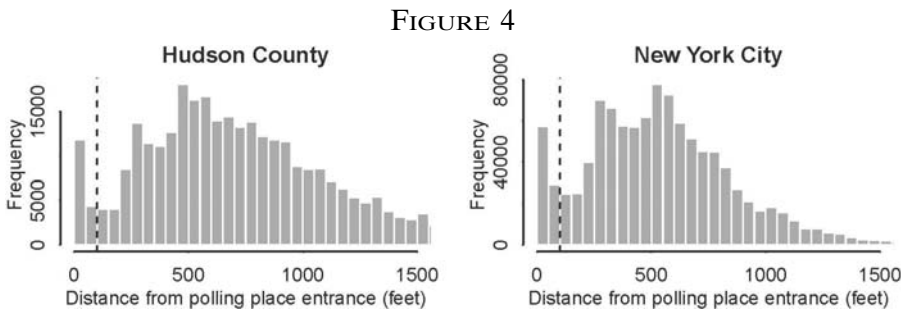


Notes: Map of voting data. The grey shading indicates the density of the registered voter population with darker shades indicating more voters. The black dots plot voters within 100 feet of a polling place.

Figure 3 displays this geocoded data, with grey shading representing the density of register voters, and black dots indicating voters within the 100-foot buffer zone. This figure gives some credence to Justice Stevens’s concern about the sheer scope of buffer zones: Over 16,000 voters in Hudson County and 86,000 voters in Manhattan live in buffer zones. On the other hand, 75% of polling places in Manhattan and 55% of polling places in Hudson County—typically polling

places with large footprints (e.g., public schools)—have no residents living within 100 feet. Our approach uses information exclusively from voters living sufficiently close to the buffer zone.

Figure 4 displays the density of voter locations relative to polling places. One critical assumption in an RD design is that covariate distributions are smooth at the threshold of 100 feet, which would be violated if voters could precisely manipulate the threshold. While the figure shows that there is a spike of voters living in polling places (e.g., the lobby of a residence), there is no evidence of a discontinuity at the threshold.



*Notes:* Voter distribution by distance to closest voting booth. The dashed vertical line indicates the radius of the buffer zone. For visibility  $x$ -axis is truncated at 1,500 feet.

From voter files, we were able to observe gender, age, whether the voter lives in an apartment, and, in the case of New York, registered party. To verify balance along a wider array of demographic characteristics (e.g., race, income, marital status, education), we purchased and merged information from Catalist, a data-aggregation firm specializing in voter analytics. This Catalist data covers the full sample of voters within the bandwidth in Hudson County, a random sample of voters within the bandwidth in New York, and a random sample from the population in each jurisdiction.

#### D. Results

In an ideal RD design, the researcher simply determines the bandwidth (or bin size) around the threshold and examines whether there is a difference in outcomes between treatment and control units. We used cross-validation to determine the optimal bin size of roughly forty feet.<sup>116</sup>

<sup>116</sup> The methodology is that set out in Imbens & Lemieux, *supra* note 8. Given the asymmetry in the running variable, we modified the algorithm to examine divergent optimal bandwidths below and above the threshold, but results suggested that the optimum was comparable. We use observations plus or minus forty feet from the threshold as the



TABLE 2

	<u>Within bandwidth</u>		<u>All</u>
	Treated Mean	Control Mean	Mean
<b>Hudson County</b>			
Female*	0.55	0.55	0.54
Age*	46.75	46.97	45.85
Apartment*	0.73	0.75	0.66
Caucasian	0.40	0.39	0.40
African-American	0.11	0.14	0.14
Hispanic	0.43	0.41	0.35
Democratic	0.56	0.54	0.50
Catholic	0.57	0.54	0.48
Married	0.24	0.23	0.27
Income < \$50k	0.47	0.42	0.38
Income \$50–100k	0.28	0.31	0.29
Income > \$100k	0.28	0.25	0.33
College	0.29	0.31	0.28
<i>N</i>	3,233	3,276	296,244
Catalist	3,233	3,276	2,500
<b>Manhattan</b>			
Female*	0.59	0.58	0.57
Age*	49.00	48.41	48.72
Apartment*	0.97	0.96	0.96
Caucasian	0.46	0.46	0.55
African-American	0.20	0.21	0.18
Hispanic	0.23	0.24	0.19
Democratic*	0.70	0.70	0.68
Catholic	0.32	0.33	0.30
Married	0.17	0.15	0.18
Income < \$50k	0.48	0.54	0.46
Income \$50–100k	0.25	0.24	0.24
Income > \$100k	0.27	0.22	0.30
College	0.51	0.49	0.57
<i>N</i>	23,599	21,168	953,923
Catalist	3,000	3,000	2,500

*Notes:* Balance statistics for sample. The left panel presents balance statistics within bins, determined by cross-validation, of “treated” voters within 60 to 100 feet from the polling place and “control” voters 100 to 140 feet away from the polling place. The right panel presents comparable statistics for the population of voters living outside of the buffer zone, showing that voters living near polling places differ in some respects. Asterisk indicates that statistics are calculated for the full sample; otherwise, information comes from Catalist sample.

Table 2 presents balance statistics of voters that fall within this bandwidth, with treated voters that live 60 to 100 feet from a polling

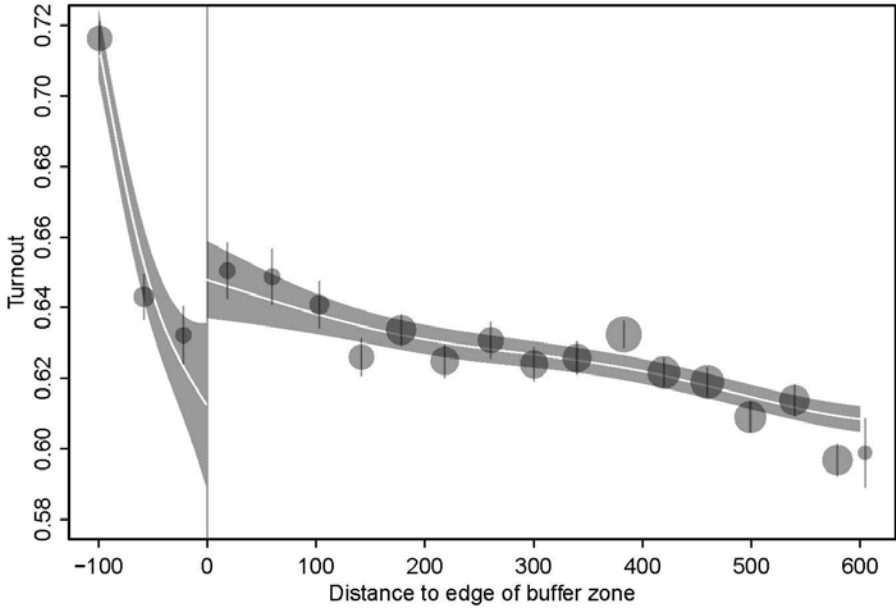
testing sample, but the bandwidth results exhibit some sensitivity to the size of the testing sample. Another way to intuitively think about bandwidth choice is to examine whether the trends away from the bandwidth make substantive sense (i.e., whether they correctly trade off bias and variance). In our application, that means that the bandwidth has to be small enough to allow for a sharp rise in turnout within the buffer zone, but large enough to exhibit a relatively smooth decline of turnout probability in distance. Because our ultimate interpretation below is that there may not be a clean discontinuity, we do not investigate this further.

booth in the first column and control voters that live 100 to 140 feet from a polling booth in the second column. Given that polling locations are dispersed across each of the counties, we should not expect there to be large differences between voters on either side of the buffer zone.

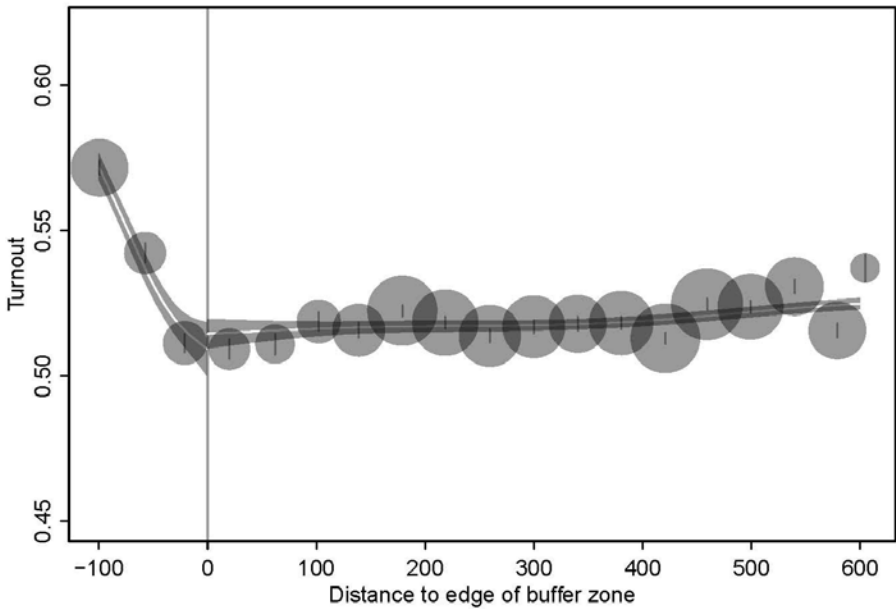
The right column compares population characteristics and shows that the populations of voters differ in some respects. In Hudson County, the population of voters is less likely to live in an apartment, less Democratic, less likely to be Catholic, and more likely to be higher income ( $p$ -values  $< 0.05$ ). In Manhattan, the population of voters is less likely to live in an apartment, more likely to be white, less likely to be Hispanic, and less likely to be Democratic ( $p$ -values  $< 0.05$ ). This shows both that naïve comparisons of turnout rates would be misleading and confirms that to the extent there is a buffer zone turnout effect, it is likely to have particular distributive implications even within a jurisdiction.

Figure 5 plots the basic RD results. The  $x$ -axis represents the distance to the edge of the buffer zone, rescaled so that 0 represents the threshold (with negative values corresponding to voters living inside the buffer zone and positive values corresponding to voters living outside of the buffer zone), and the  $y$ -axis represents turnout. Each dot represents the turnout rate within a bin of forty feet, weighted by the number of voters. Vertical lines indicate 95% confidence intervals, and the grey bands plot 95% confidence intervals from a (generalized additive) model, estimated separately for the sample below and above the threshold. The top panel shows that Hudson County exhibits evidence of a drop in turnout just inside of the buffer zone, with turnout monotonically declining in distance from the voting booth. This pattern is what we would expect under our cueing hypothesis. The lower panel shows that there is no such comparable pattern in Manhattan. Curiously, in Manhattan distance does not appear to decrease turnout and there is a kink around the 100-foot threshold.

FIGURE 5  
Hudson County



New York County

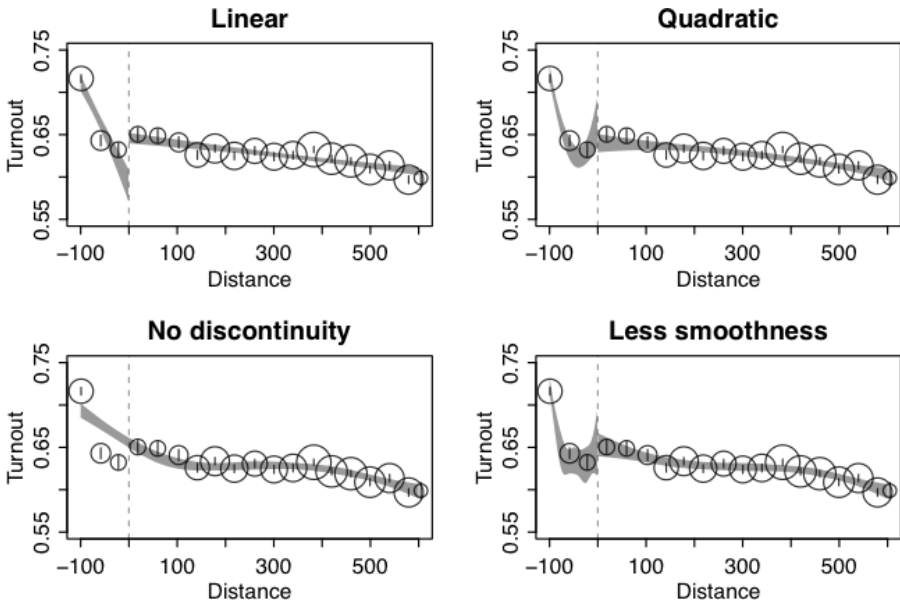


Notes: Regression discontinuity results from Hudson County and Manhattan. Optimal bin size was determined based on cross-validation. The *x*-axis represents distance (in feet) to the edge of the buffer zone, with 0 representing the edge and individuals to the left being inside and individuals to the right being outside of the buffer zone. Vertical lines indicate 95% confidence intervals. Light grey curves plot 95% confidence intervals from generalized additive models.

### E. Explanations and Mechanisms

*Model Sensitivity.* RD designs can be quite sensitive to modeling assumptions. Figure 6 displays comparable results from Hudson County under alternative specifications, showing that turnout difference at the threshold is model dependent. Under a linearity assumption (in a generalized linear model with a logistic link), the top left panel exhibits a dramatic causal effect. Adding a quadratic term in the top right panel, however, suggests no difference at the threshold. In a conventional RD design, where treatment is a sharp function of the cutoff, that would lead us to think that the inferences are not very robust about the causal effect of speech restrictions.

FIGURE 6



*Notes:* Model sensitivity. The grey bands in the top left panel present 95% confidence bands from generalized linear models, fit separately to treatment and control groups, with distance as the explanatory variable. The top right panel presents results adding quadratic terms to the models. The bottom right panel presents results from generalized additive model with less smoothness (using tensor product smooth).

In our application, however, the turnout drop inside the buffer zone in Hudson County persists across parameterizations. The quadratic model, for instance, still exhibits the drop around thirty to eighty feet from the polls. The same goes for a (generalized additive) model with less smoothing in the bottom right panel. If, on the other hand, we assume that turnout was a smooth function of distance to the voting booth (as in the bottom left panel), the model would misclassify voters within the radius, both at the voting location and beyond it.

One possible explanation for these findings is that the treatment is not a sharp function of the cutoff. At ninety-five feet, voters might still be able to see campaign advertisements by looking outside the buffer zone. Geocoding imputes voter locations to the centroid of a parcel or building, which may not correspond with the buffer zone. And enforcement likely declines in the outer reaches of the radius. Consistent with this “fuzzier” boundary is the considerable confusion over how long thirty-five feet was in the oral argument in *McCullen v. Coakley*.<sup>117</sup> While the RD design may not be as clean, the turnout-distance pattern (with an unexpected drop within the radius, but not quite at the threshold) hence still provides suggestive evidence of buffer zone effects.

*Imbalance / Clustering.* In typical RD designs, the bandwidth selection presents a bias-variance tradeoff. Narrowing the bandwidth should reduce bias (by making voters arbitrarily similar) but increase variance given the smaller sample size. We initially determined the bandwidth via cross-validation, verifying balance with covariates observable from voter files. After securing Catalist data, however, we discovered that narrowing the bandwidth, puzzlingly, does not always increase balance, and that conventional statistical tests reject the null of no differences at high rates. Upon sampling addresses, the likely reason appeared to be clustering at the building and household levels. Individual members of the same household or same apartment building are likely to be highly similar in demographic characteristics, so that conventional difference-in-means tests overreject.<sup>118</sup> In an ideal world, we would have all covariate information at our disposal to determine the bandwidth and to fully assess the sensitivity of results. Unfortunately, Catalist data can be expensive (with a quote exceeding \$43,000 to obtain covariates for the full dataset), making this approach infeasible. Clustering may affect a wide variety of methods using geographic matching or distance as a running variable in a RD design.<sup>119</sup>

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<sup>117</sup> See Transcript of Oral Argument at 21, *McCullen v. Coakley*, 134 S. Ct. 2518 (2014) (No. 12-1168), 2014 WL 144977, at \*21 (Justice Ginsburg: “How much is—how much is restricted?”); *id.* at 29–30 (Justice Kagan: “You know, 35 feet is a ways. It’s from this bench to the end of the court. And if you imagine the Chief Justice as sort of where the door would be, it’s most of the width of this courtroom as well. . . . That’s a lot of space.”); *id.* at 30 (Justice Sotomayor: “I thought [35 feet] was two car lengths.”).

<sup>118</sup> It is not easy to adjust for clustering given the available data, as address information in the voter rolls can be quite noisy.

<sup>119</sup> The methodological issues are discussed in Sandra E. Black, *Do Better Schools Matter? Parental Valuation of Elementary School Education*, 114 Q.J. ECON. 577, 578 (1999) and Brady & McNulty, *supra* note 98, at 119–20. For a useful discussion of similar problems with geographic RD designs that are acute when jurisdictional boundaries themselves are the cutoff, see Luke J. Keele & Rocío Titiunik, *Geographic Boundaries as Regression Discontinuities*, 23 POL. ANALYSIS 127 (2015).

*Reconciling Manhattan and Hudson County.* Why does some evidence of cueing exist in Hudson County and not Manhattan? While the jurisdictions are adjacent, voters differ substantially. Manhattan voters, for instance, are generally more educated,<sup>120</sup> meaning that they may be less likely to be in need of cueing when the election is taking place. Manhattan voters may also be less likely than Hudson County voters to stay home on election day. Another plausible explanation centers on enforcement. Although the buffer zones are largely identical in substantive scope, New Jersey poll workers are instructed to tell residents to remove campaign information within the buffer zone, while New York only instructs poll workers to put up a sign at the voting location disclosing the electioneering ban.<sup>121</sup> More proactive enforcement in New Jersey may explain that, as enforced, the buffer zone in New Jersey looks quite different from the statutorily equivalent one in New York. Leveraging differences in enforcement strategy across precincts might be an alternative way to examine the impact of buffer zones. Enforcement differences also suggest experimental methods to test cueing: Jurisdictions could randomize the scope of enforcement by randomizing poll workers into different training sessions (which vary the emphasis of buffer zone enforcement responsibilities); or researchers could randomize the extent of signage indicating the location of voting booths (which would presumably not be considered electioneering).<sup>122</sup>

*Distributive Implications.* Our data also reveal that the types of voters within the buffer zone can be quite different from the population. In both jurisdictions, such voters are more likely to be minority voters and Democrats. Comparing the largest counties in the fifty states from Table 1, the distributive dynamic becomes quite obvious: To the extent that there's a turnout effect, it disproportionately impacts communities in high-density urban regions. Understanding polling place location decisions becomes crucial here. If Justice Stevens is right as to the effects on third party candidates, buffer zones

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<sup>120</sup> See *supra* Table 1.

<sup>121</sup> BD. OF ELECTIONS IN THE CITY OF NEW YORK, POLL WORKER'S MANUAL 42 (2012), available at <http://vote.nyc.ny.us/downloads/pdf/documents/boe/pollworkers/pollworkersmanual.pdf>; N.J. DIVISION OF ELECTIONS, STATE OF N.J., DISTRICT BOARD MEMBER TRAINING MANUAL 12 (2011), available at <http://www.state.nj.us/state/elections/publications/boardworkers-manual-080912.pdf>.

<sup>122</sup> For a similar idea, namely to randomize *information* about legal entitlements when randomizing law is infeasible, see Daniel E. Ho, *Randomizing . . . What? A Field Experiment of Child Access Voting Laws*, 171 J. INST'L & THEOR. ECON. 150 (2015).

may be another feature of election law that is explained in part by partisan entrenchment.<sup>123</sup>

\* \* \* \*

The RD design provides suggestive evidence of our cueing hypothesis: Turnout drops unexpectedly amongst voters just inside the buffer zone in Hudson County. If this is correct, the finding vindicates the invocation by Freeman of the marketplace of ideas: If, as is commonly assumed, higher turnout improves democratic decisionmaking (a contested assumption to be sure<sup>124</sup>), speech restrictions may impede one possible search for truth.

## V

### CLINICAL ACCESS

#### A. Background

Just as buffer zones close off the marketplace of ideas within certain locations in the vicinity of polling places, so too do the somewhat more recent buffer zones around abortion clinics, which are designed to allow women unimpeded access to such clinics in the face of widespread, and potentially intimidating or obstructing, protests by those opposed to abortion. These buffer zones around abortion clinics have generated great political controversy as well as fierce litigation, principally on First Amendment grounds. In *Madsen v. Women's Health Center, Inc.*, the Supreme Court upheld a state court injunction requiring a thirty-six-foot "fixed" buffer zone around entrances and driveways of abortion clinics in Florida, while striking down the buffer zone as applied to private property.<sup>125</sup> In *Schenck v. Pro-Choice Network of Western New York*, the Court upheld a federal district court injunction establishing a fifteen-foot fixed buffer zone around entrances and driveways of clinics, but struck down a fifteen-foot "floating" buffer zone around a person seeking to enter clinics.<sup>126</sup>

Three states (Massachusetts, Colorado, and Montana) have enacted statewide buffer zones. In Massachusetts, for instance, state legislators introduced a proposal for a fixed twenty-five-foot buffer zone in 1997, in reaction to a violent shooting by John Salvi, who killed two clinic workers and injured five others, at a Brookline

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<sup>123</sup> See generally Samuel Issacharoff & Richard H. Pildes, *Politics as Markets: Partisan Lockups of the Democratic Process*, 50 STAN. L. REV. 643 (1998) (discussing partisan entrenchment generally).

<sup>124</sup> See *infra* notes 213–14 and accompanying text for a discussion of this debate.

<sup>125</sup> 512 U.S. 753, 776 (1994).

<sup>126</sup> 519 U.S. 357, 377–81 (1997).

Planned Parenthood in 1994.<sup>127</sup> The bill stalled for years, in part because of a budget impasse, opposition by numerous groups (including the ACLU, the AFL-CIO, and anti-abortion “sidewalk counselors”), and opposition by House Speaker Thomas Finneran.<sup>128</sup> In June 2000, the Supreme Court decided *Hill v. Colorado*, upholding Colorado’s floating eight-foot buffer zone around individuals within a 100-foot buffer zone.<sup>129</sup> Massachusetts legislators then reached a compromise, scaling back the buffer zone to a floating buffer zone modeled on Colorado’s. Signed by the governor in August, the law provided that individuals could not knowingly approach within six feet of another person for the purpose of engaging in “protest, education, or counseling” without consent within eighteen feet of a clinic entrance.<sup>130</sup> In November 2000, a federal district court (distinguishing *Hill* on the basis of content neutrality) issued a preliminary injunction in Massachusetts.<sup>131</sup> The injunction was stayed a month later by the First Circuit, which upheld the statute the subsequent year.<sup>132</sup>

From 2000 to 2007, various elements of the floating buffer zone proved difficult to enforce.<sup>133</sup> The primary difficulty was in determining whether an individual had consented to being approached, an especially important issue given the central First Amendment principle of allowing speakers to reach willing listeners. In response, Massachusetts changed the buffer zone from a floating to a fixed buffer in 2007, prohibiting knowingly standing on a public way or sidewalk within thirty-five feet of a clinic entrance or driveway.<sup>134</sup>

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<sup>127</sup> It was generally recognized that the buffer zone would not prevent such a shooting, but legislators nonetheless framed the proposal as a response to the Salvi shooting. *In re Op. of the Justices to the Senate*, 723 N.E.2d 1, 5 (Mass. 2000) (noting the bill’s reference to the shooting).

<sup>128</sup> Brief for Planned Parenthood League of Massachusetts & Planned Parenthood Federation of America as Amici Curiae Supporting Respondents, *McCullen v. Coakley*, 134 S. Ct. 2518 (2014) (No. 12-1168), 2013 WL 6140516; Bronislaus B. Kush, *Anti-Abortion Group Protests Bill*, WORCESTER TELEGRAM & GAZETTE, July 15, 2000, at A1 (describing an opposition coalition including the ACLU, the AFL-CIO, and anti-abortion advocates); Editorial, *Tolerable Compromise Cellucci Should OK Diminished Clinic Buffer Zone*, WORCESTER TELEGRAM & GAZETTE, Aug. 9, 2000, at A10 (discussing Finneran’s opposition); *Mass. Lawmakers Want Wider Buffer Zones at Abortion Clinics*, FIRST AMENDMENT CENTER (Jan. 1, 2006), <http://www.firstamendmentcenter.org/mass-lawmakers-want-wider-buffer-zones-at-abortion-clinics> (same).

<sup>129</sup> 530 U.S. 703, 726–27 (2000).

<sup>130</sup> An Act Relative to Reproductive Health Care Facilities, 2000 Mass. Acts 1030.

<sup>131</sup> *McGuire v. Reilly*, 122 F. Supp. 2d 97, 101, 104 (D. Mass. 2000), *rev’d*, 260 F.3d 36 (1st Cir. 2001).

<sup>132</sup> *McGuire v. Reilly*, 260 F.3d 36, 51 (1st Cir. 2001).

<sup>133</sup> *McCullen v. Coakley*, 134 S. Ct. 2518, 2525–26 (2014).

<sup>134</sup> *Id.* at 2526.



In *McCullen v. Coakley*, the Supreme Court invalidated Massachusetts's fixed buffer zone.<sup>135</sup> The Court emphasized the historic role of public streets and sidewalks as venues for the exchange of ideas, given the First Amendment's purpose "to preserve an uninhibited marketplace of ideas in which truth will ultimately prevail."<sup>136</sup> While the Court found the act to be content neutral, it was still not sufficiently narrowly tailored to promote Massachusetts's significant interest in "protecting a woman's freedom to seek pregnancy-related services."<sup>137</sup> In particular, the Court emphasized that while protest speech could be relegated to areas outside the buffer zone, the plaintiffs in the case (Eleanor McCullen, Jean Zarrella, and Eric Cadin) deployed a kind of "persuasive," not protest, speech.<sup>138</sup> They engaged, according to the Court, in "personal, caring, consensual conversations,"<sup>139</sup> because that approach was, in their assessment, more effective in reaching women. The three named plaintiffs alone claimed to have dissuaded hundreds of women from seeking abortions from 2000 to 2007. The Court noted that a more narrowly tailored statute—permitting this kind of persuasive speech—could focus more on intimidation and interference with securing reproductive health services, comparable to the federal Freedom of Access to Clinic Entrances Act.<sup>140</sup> (Massachusetts already had a state provision prohibiting obstruction of entering and existing clinics.)<sup>141</sup>

Massachusetts relied heavily in its brief on *Burson*, with which *McCullen* shares many commonalities.<sup>142</sup> Freeman, like McCullen, sought to engage in persuasive speech on important issues of public policies;<sup>143</sup> Massachusetts, like Tennessee, argued that a fixed buffer zone on a public street was prophylactic given the difficulties of

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<sup>135</sup> *Id.* at 2541.

<sup>136</sup> *Id.* at 2529 (citation and internal quotation marks omitted).

<sup>137</sup> *Id.* at 2535.

<sup>138</sup> *Id.* at 2527 (noting the claim by petitioners that they have "collectively persuaded hundreds of women to forgo abortions"); *id.* at 2536 ("Petitioners are not protestors. They seek . . . to inform women of various alternatives and to provide help in pursuing them.").

<sup>139</sup> *Id.*

<sup>140</sup> *Id.* at 2537.

<sup>141</sup> *Id.*

<sup>142</sup> Respondents' Brief on the Merits, *McCullen v. Coakley*, 134 S. Ct. 2518 (2014) (No. 12-1168), 2013 WL 6091500; *see also* Brief for the Defendants-Appellees at 40, *McCullen v. Coakley*, 708 F.3d 1 (1st Cir. 2013) (No. 12-1334), 2012 WL 2872265, at \*40 (citing *Burson*, in the First Circuit case, for the proposition that protestors "have no constitutional right to position themselves so that they can force unwilling listeners to interact with them").

<sup>143</sup> Freeman, an active figure in Tennessee politics, wished to communicate with the voters prior to their casting their votes. *Burson v. Freeman*, 504 U.S. 191, 194 (1992).

enforcement;<sup>144</sup> and in both cases other laws existed specifically addressing interference or obstruction.<sup>145</sup> The majority, however, distinguished *Burson* on enforcement grounds. Voter intimidation and fraud, the Court concluded, are harder to detect than obstruction and harassment at abortion clinics.<sup>146</sup> This was in large part because of the difference in police presence: “[U]nder state law, ‘law enforcement officers generally are barred from the vicinity of the polls to avoid any appearance of coercion in the electoral process,’ with the result that ‘many acts of interference would go undetected.’ Not so here. . . . [T]he police maintain a significant presence outside Massachusetts abortion clinics.”<sup>147</sup> The distinction is misleading at best. As noted above, *Burson* contemplated only the exclusion of police officers under Tennessee law, not generally “under state law.”<sup>148</sup> Group (D) of Table 1 presents actual restrictions on police presence based on researching each state election law. Other than Tennessee, only three states affirmatively ban police or peace officers from within the buffer zone.<sup>149</sup> Some twenty states affirmatively *allow* or even *require* a police presence. In Manhattan, for instance, “at least one police officer or peace officer . . . shall be assigned for duty from the opening until the closing of the polls.”<sup>150</sup> Numerous states restrict nearly all persons from the buffer zone without any reference to police officers,<sup>151</sup> which one might classify as a police restriction in the last column of group (D). Some states define prohibited activities, with no

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<sup>144</sup> See *McCullen*, 134 S. Ct. at 2525–26, 2540–41 (noting the state’s arguments regarding the difficulties of policing a floating buffer zone and the state’s misplaced attempt to rely on *Freeman*).

<sup>145</sup> See MASS. GEN. LAWS ANN. ch. 266, § 120E (West 2008) (prohibiting obstructing entry to, or departure from, medical facilities); TENN. CODE ANN. § 2-19-115 (Supp. 1991) (prohibiting violence or intimidation to prevent voting).

<sup>146</sup> *McCullen*, 134 S. Ct. at 2540–41.

<sup>147</sup> *McCullen*, 134 S. Ct. at 2540 (quoting *Burson v. Freeman*, 504 U.S. 191, 207 (1992)).

<sup>148</sup> *Id.* This is not an error attributable to poor briefing. Petitioners distinguished *Burson* on the basis that “law enforcement officers were ‘barred from the vicinity of the polls,’” Reply Brief for Petitioners at 20–21, *McCullen v. Coakley*, 134 S. Ct. 2518 (2014) (No. 12-1168), 2013 WL 6805692, at \*20–21, without implying that state laws generally bar police officers from polling places.

<sup>149</sup> These provisions include exceptions that allow police officers to vote and respond to problems that arise in the polling place. CAL. ELEC. CODE § 18544 (West 2014); MINN. STAT. ANN. § 204C.06 (West 2009); 25 PA. CONS. STAT. ANN. § 3060(a), (d) (2007).

<sup>150</sup> N.Y. ELEC. LAW § 8-104 (McKinney 2014).

<sup>151</sup> Delaware statute provides, for instance, that “[n]o other person except [media and persons conducting exit polls] shall be permitted within 50 feet of any entrance to the building used by voters.” DEL. CODE ANN. tit. 15, § 4933(b), (c) (2007). Minnesota provides that “[n]o one except an election official or an individual who is waiting to register or to vote or an individual who is conducting exit polling shall stand within 100 feet of the building in which a polling place is located.” MINN. STAT. § 204C.06(1) (West 2009). These statutes typically allow for a narrow set of exceptions not including the police.

mention of police, which Table 1 does not treat as a police restriction.<sup>152</sup> In short, state law, if anything, contradicts the purported distinction between *Burson* and *McCullen* on enforcement grounds.<sup>153</sup>

In response to *McCullen*, Massachusetts revised its law, establishing a twenty-five-foot buffer zone applicable only to individuals who had been issued a withdrawal order because they “substantially impeded access” to the clinic, effective for a limited time period.<sup>154</sup>

Anecdotal claims about the effects of buffer zones abound. Clinic employees in Massachusetts testified that “prospective patients *occasionally retreated* from the clinics” due to protest activity around clinics.<sup>155</sup> The Supreme Court has noted in another context that “demonstrations in front of abortion clinics *impeded* access to those clinics.”<sup>156</sup> Upon the most recent amendment of Massachusetts’s buffer zone law, the *Boston Globe* noted, “violence and harassment is often the most traumatic part of the abortion experience and *may discourage* women from receiving proper reproductive care.”<sup>157</sup> The *New York Times* editorialized in the context of *McCullen* that “the broader issue at stake is protecting women’s *access* to abortion.”<sup>158</sup> On the other hand, a *Boston Globe* op-ed assessing the 2007 law argued that sidewalk counselors became much less effective, which “in turn, means more abortions.”<sup>159</sup>

No academic research has effectively put these conjectures to the test. The only related study is by Pridemore and Freilich, who

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<sup>152</sup> Virginia, for instance, provides that “it shall be unlawful for any person . . . to loiter or congregate within 40 feet of any entrance of any polling place.” VA. CODE ANN. § 24.2-604(A) (2012).

<sup>153</sup> It is also at least questionable whether police presence around abortion clinics is as significant as the *McCullen* majority suggests. See Tara A. Kelly, *Silencing the Lambs: Restricting the First Amendment Rights of Abortion Clinic Protestors in Madsen v. Women’s Health Center*, 68 S. CAL. L. REV. 427, 440–41 (1994) (noting that protestors routinely violate injunctions designed to ensure clinic access, and that local law enforcement is often ineffective at enforcing these court orders).

<sup>154</sup> Act of July 30, 2014, ch. 197, sec. 2, § 102E½(b), 2014 Mass. Legis. Serv. 676, 677 (West) (codified at MASS. GEN. LAWS ANN. ch. 266, § 120E½(b) (West Supp. 2015)).

<sup>155</sup> *McCullen*, 134 S. Ct. 2518, 2526 (2014) (emphasis added).

<sup>156</sup> *Hill v. Colorado*, 530 U.S. 703, 709 (2000) (emphasis added).

<sup>157</sup> Denali Tietjen, *Gov. Patrick Signs Mass. Abortion Buffer Zone Bill*, BOS. GLOBE (July 30, 2014, 2:29 PM), <http://www.boston.com/health/2014/07/30/gov-patrick-signs-mass-buffer-zone-bill-into-law/ZoLyawzwsQMtc9J53IilisRL/story.html> (emphasis added).

<sup>158</sup> Editorial, *Abortion Rights Before the Court*, N.Y. TIMES, Jan. 14, 2014, at A26 (emphasis added).

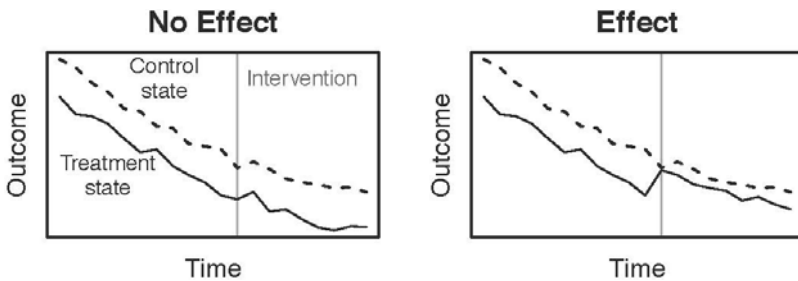
<sup>159</sup> Jeff Jacoby, *Buffering Out the Right to Hear*, BOS. GLOBE (Jan. 5, 2014), <http://www.bostonglobe.com/opinion/2014/01/05/abortion-buffer-zones-and-right-hear/PHcFhtX6RVD9caugUNdDil/story.html>.

examined the effect of buffer zone laws on abortion-related crimes, finding no effect.<sup>160</sup>

*B. Research Design*

One ideal hypothetical experiment would randomly assign speech buffer zones (and buffer zone attributes—e.g., fixed or floating, radius, exemptions) to clinics and measure outcomes of violence, harassment, and abortion. A “difference-in-differences” (DID) design attempts to replicate this experiment with observational data, using changes in buffer zones over time and across jurisdictions. Using jurisdiction-year panel data allows the researcher to adjust for (state-invariant) time effects and (time-invariant) jurisdiction effects, identifying the causal effect of the buffer zone by changes in outcomes in jurisdictions adopting a buffer zone, relative to control jurisdictions. Figure 7 plots the intuition of the identification approach. The left panel plots simulated data where an intervention, marked by the vertical line, has no apparent effect on the treatment state (solid line), relative to the control state (dashed line). The right panel plots simulated data with an apparent increase in outcomes only in the treatment state postintervention.

FIGURE 7



*Notes:* Intuition of difference-in-differences design. The left panel displays simulated data for a treatment state (solid line) and a control state (dashed line), where the treatment state enacts an intervention marked with the vertical line. The preintervention trends are parallel and there is no change with the intervention. The right panel displays simulated data when outcomes change for the treatment state at the time of enactment. The difference across time (before and after the intervention) and across treatment and control groups is the DID estimate of the causal effect.

One important DID assumption is that the treatment is exogenous. The assumption would be violated, for instance, if Massachusetts had immediately enacted a buffer zone in reaction to the 1994 clinic shootings. Violence alone may well have deterred

<sup>160</sup> William Alex Pridemore & Joshua D. Freilich, *The Impact of State Laws Protecting Abortion Clinics and Reproductive Rights on Crimes Against Abortion Providers: Deterrence, Backlash, or Neither?*, 31 J.L. & HUM. BEHAV. 611, 622 (2007).

women from visiting abortion clinics in Massachusetts, so that attempts to estimate the effect of the buffer zone would be confounded. The fact that the buffer zone in Massachusetts was delayed for some six years owing to plausibly exogenous reasons (such as the budget impasse<sup>161</sup>) gives some credence to the DID approach. While we control for obtainable demographic and policy variables, we assess what other confounding factors may have changed acutely around 2000 in Massachusetts or control states. Another important assumption is that the treatment and control states follow the same (parallel) time trends. If the treatment state in the right panel of Figure 7, for instance, exhibited a state-specific “shock” around the time of the intervention, DID inferences would be biased. Long pretreatment time series that are parallel make DID inferences more credible, as the post-treatment difference is more plausibly attributed to the intervention.

If DID assumptions are met and if buffer zones facilitate access and reduce the deterrent effect of protests, we should expect that abortion rates increase in treatment jurisdictions around enactment (relative to control jurisdictions). On the other hand, the buffer zone may also affect the type of speech surrounding the clinic. *McCullen*, in particular, focused on differences between protest and persuasive speech.<sup>162</sup> Enforcement difficulties of the floating buffer zone centered on who had consented to hear persuasive speech, as protests were more clearly prohibited within the floating buffer. If the buffer zone shifted protest speech away from the entrances, it may, counter-intuitively, have enhanced the ability of “sidewalk counselors” to reach their intended audience, thereby potentially reducing the abortion rate.

While DID approaches are promising, there is an increasing recognition that such models can perform poorly because (a) control states can differ substantially from treatment states, making regression adjustments fragile, and (b) models are falsely precise (underestimating standard errors). We therefore illustrate a “synthetic control” approach with DID.<sup>163</sup> The intuition of the approach is that while no single state may look exactly like Massachusetts, we can potentially

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<sup>161</sup> The budget impasse, however, might also have directly affected abortion rates. For instance, if the budget impasse reduced state funding to abortion clinics, exogeneity would be violated.

<sup>162</sup> Because Supreme Court opinions treat the speech as “persuasive,” we will use that terminology, but one might disagree on normative or conceptual grounds as to whether “sidewalk counseling” is persuasive, coercive, informative, intrusive, or something else.

<sup>163</sup> These methods were pioneered in Alberto Abadie & Javier Gardeazabal, *The Economic Costs of Conflict: A Case Study of the Basque Country*, 93 AM. ECON. REV. 113 (2003). See also Abadie, Diamond & Hainmueller, *supra* note 8 (applying the synthetic

create a synthetic control state (essentially a weighted average of control states) that looks like Massachusetts in all principal respects, except for the buffer zone.

### C. Data

We compile abortion outcomes from the Centers for Disease Control (CDC) and the Alan Guttmacher Institute (AGI).<sup>164</sup> Most researchers consider AGI data to be the most reliable measure,<sup>165</sup> and we focus on the abortion rate (i.e., the number of abortions per 1,000 women between 15 and 44 years). We merge this information with twenty demographic variables, aggregated at the state-year level, from the Department of Commerce's Bureau of Economic Analysis (population, income), the Bureau of Labor Statistics (unemployment), and the Current Population Survey (e.g., gender, women of child-bearing age, ethnicity, rural background, poverty level, unemployment rate, education level, marital status, and recipients of government assistance) as well as twenty measures of abortion<sup>166</sup> and contraceptive<sup>167</sup> policy (e.g., required parental notification for minors, mandatory counseling, funding restrictions, Medicaid expanded coverage). Our covariate set hence includes most conventional predictors of the abortion rate.<sup>168</sup> Appendix B provides a full description of variables.

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control approach developed by Abadie and Gardeazabal to study the effects of a tobacco-control program adopted by California Proposition 99).

<sup>164</sup> CDC's Abortion Surveillance reports are available from 1990 to 2010, and AGI data is available from 1973 to 2011, albeit with some missing years. Frequencies (the total number of reported abortions) and rates (the number of abortions per 1000 women between 15 to 44 years) exhibit high correlation ( $R^2=0.96$  and  $0.72$ , respectively), but AGI generally shows higher frequencies and rates.

<sup>165</sup> See, e.g., PHILIP B. LEVINE, *SEX AND CONSEQUENCES: ABORTION, PUBLIC POLICY, AND THE ECONOMICS OF FERTILITY* 20 (2004) ("Because AGI has an extensive list of abortion providers, including those who perform a very small number of abortions per year, these data are generally recognized as the most accurate available."); Theodore Joyce & Robert Kaestner, *The Effect of Expansions in Medicaid Income Eligibility on Abortion*, 33 *DEMOGRAPHY* 181, 185 nn.10–11 (1996) (using AGI data). AGI compiles the population of all providers in each state and directly surveys each provider using a uniform questionnaire and nonresponse follow-up. CDC obtains the data through voluntary reports by each state's central health authority; reporting by providers to state health departments and state health departments to CDC varies considerably across jurisdictions and time. For completeness, we present results with AGI and CDC data.

<sup>166</sup> Many thanks to Rebecca Kreitzer for sharing her data on abortion-related state laws, collected originally from annual reports issued by the National Abortion Rights Action League (now known as NARAL Pro-Choice America).

<sup>167</sup> We hand-collected information about the mandated contraceptive insurance coverage and Medicaid expansion of eligibility for family planning.

<sup>168</sup> The literature has shown that public funding for abortions and parental involvement laws in particular are important predictors of the abortion rate. See Rebecca M. Blank, Christine C. George & Rebecca A. London, *State Abortion Rates: The Impact of Policies, Providers, Politics, Demographics, and Economic Environment*, 15 *J. HEALTH ECON.* 513,

TABLE 3

Year	Massachusetts		New York	
	Number	Rate	Number	Rate
1996	41,160	28.8	167,600	39.6
2000	30,410	21.4	164,630	39.1

Notes: Excerpt of AGI data for two years when abortion provider surveys were conducted.

Table 3 displays an excerpt from AGI data, foreshadowing the basic trend from Massachusetts. According to AGI, the number and rate of abortions dropped significantly from 1996 to 2000 in Massachusetts, while staying roughly stable in New York. As we discuss below, AGI shifted in recent years toward biennial surveys, so the 2001 survey collected information about both 1999 and 2000. Standard practice, both by AGI and by researchers, is to interpolate missing years.<sup>169</sup> We examine sensitivity to this interpolation below.

514 (1996) (finding that 19–25% of abortions are dependent on public funding); Deborah Haas-Wilson, *The Economic Impact of State Restrictions on Abortion: Parental Consent and Notification Laws and Medicaid Funding Restrictions*, 12 J. POL'Y ANALYSIS & MGMT. 498, 505–06 (1993) (finding that consent laws lower the abortion rate and public funding increases it); Stephen Matthews, David Ribar & Mark Wilhelm, *The Effect of Economic Conditions and Access to Reproductive Health Services on State Abortion Rates and Birthrates*, 29 FAMILY PLAN. PERSP. 52, 55–58 (1997) (finding that restrictive access to providers lowers abortion rate); Annette Tomal, *Parental Involvement Laws and Minor and Non-Minor Teen Abortion and Birth Rates*, 20 J. FAMILY & ECON. ISSUES 149, 157–58 (1999) (finding that parental consent laws lower abortion rates and raise birth rates). Recent work finds that waiting periods and informed consent laws are also predictive, and we include several measures of the restrictions on access to abortions by state, (e.g., informed consent, postviability ban, or mandatory viability tests). See Michael J. New, *Analyzing the Effect of Anti-Abortion U.S. State Legislation in the Post-Casey Era*, 11 ST. POL. & POL'Y Q. 28, 37–42 (2011) (finding that various forms of abortion restrictions lower incidence and ratio of abortions). For common demographic and economic factors, see Blank, George & London, *supra*, at 525 (marriage rates, birth rates, proportion of teenagers and older women among all fertile women, black proportion of population, nonmetropolitan proportion of population, unemployment rates, per capita income, labor force participation among women); Lawrence B. Finer & Stanley K. Henshaw, *Disparities in Rates of Unintended Pregnancy in the United States, 1994 and 2001*, 38 PERSP. SEXUAL & REPROD. HEALTH 90, 93 (2006) (age, marital status, marital history, cohabitation status, income, education, and race/ethnicity); Tomal, *supra*, at 153–56 (church membership, education, population density, income, unemployment rate, marital status, race). For contraceptive access, see Rachel K. Jones, Mia R.S. Zolna, Stanley K. Henshaw & Lawrence B. Finer, *Abortion in the United States: Incidence and Access to Services, 2005*, 40 PERSP. ON SEXUAL REPROD. HEALTH 6, 15 (2008). Evidence on the effect of AFDC benefits (welfare) on the abortion rate is mixed at best, but we include a measure of the extent of public assistance. See Gregory Acs, *The Impact of Welfare on Young Mothers' Subsequent Childbearing Decisions*, 31 J. HUM. RESOURCES 898 (1996) (finding the level of welfare benefits does not predict childbearing decisions); Matthews, Ribar & Wilhelm, *supra*, at 55, 58 (finding that AFDC benefits do not have a large effect on the abortion rate). There is little evidence that “political climate” variables, such as the party of a state’s governor, are statistically significant predictors, so these are not included. See Blank, George & London, *supra*, at 540.

<sup>169</sup> See, e.g., *infra* note 182 and accompanying text.

While we use this data to illustrate the research design, further data collection would improve the reliability of inferences in critical ways. First, designs should go beyond state-level data. There is in fact rich variation in municipally and judicially established buffer zones, which may be more plausibly exogenous and provide more credible comparison groups that hold state policies constant. While AGI possesses county-level abortion information (as all of their surveys are conducted at the provider level), it did not share the data with us for research purposes. Second, there may be particular differences within a jurisdiction as to the speech that types of providers are exposed to. For instance, general hospitals may be less likely to be affected by protests than specific family planning clinics. Provider differences may lend themselves to a “triple difference” design, leveraging differences across jurisdictions, time, and types of providers.<sup>170</sup> Third, within a jurisdiction, there may be sharp temporal differences in speech activity. According to some of the record evidence in *McCullen*, for instance, protest activity focused largely on one clinic on specific days of the week.<sup>171</sup> With granular (time-stamped) data, it may be possible to estimate deterrence/substitution effects. Last, the outcomes we study are limited to the abortion rate. Not only is the abortion rate difficult to measure, but focusing on that outcome does not capture many other elements relevant to the First Amendment analysis, such as the emotional harm to women visiting these clinics or employees working at these clinics or the ability by interest groups to mobilize broader societal support.<sup>172</sup> Conducting a survey of individuals before and after the enactment of a buffer zone may permit the use of DID designs to study these dimensions.

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<sup>170</sup> Some buffer zones apply only to clinics and others to both hospitals and clinics.

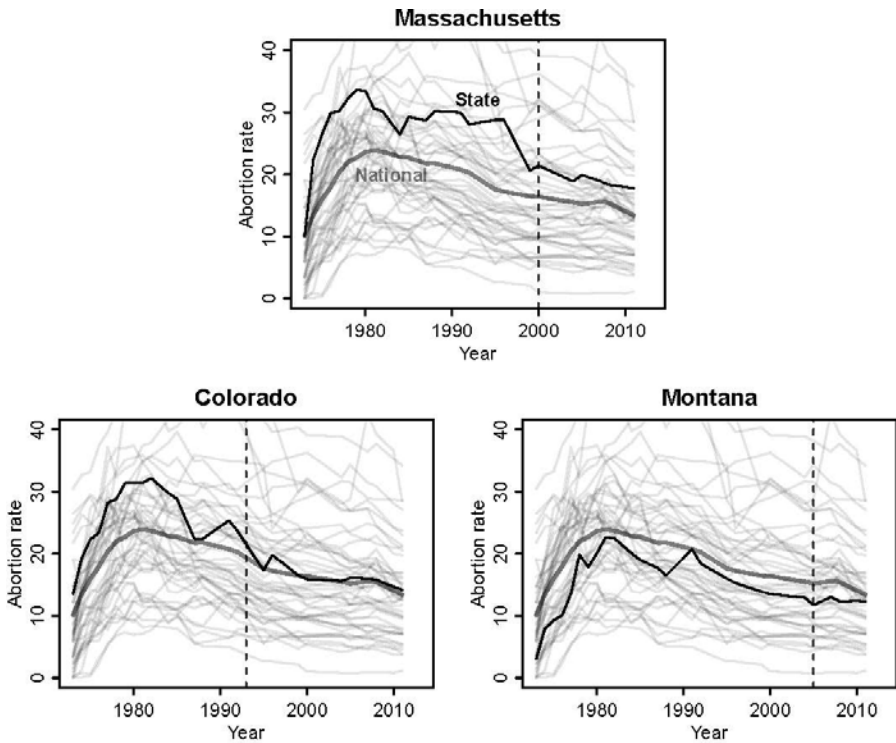
<sup>171</sup> *McCullen v. Coakley*, 134 S. Ct. 2518, 2524 (2014).

<sup>172</sup> See Richard A. Posner, *Supreme Court Breakfast Table*, SLATE (June 26, 2014, 6:56 PM), [http://www.slate.com/articles/news\\_and\\_politics/the\\_breakfast\\_table/features/2014/scotus\\_roundup/scotus\\_end\\_of\\_term\\_remembering\\_town\\_of\\_greece\\_and\\_more\\_on\\_cellphones\\_buffer.html](http://www.slate.com/articles/news_and_politics/the_breakfast_table/features/2014/scotus_roundup/scotus_end_of_term_remembering_town_of_greece_and_more_on_cellphones_buffer.html) (“The issue is the privacy, anxiety, and embarrassment of the abortion clinic’s patients—interests that outweigh, in my judgment anyway, the negligible contribution that abortion protesters make to the marketplace of ideas and opinions.”).



## D. Results

FIGURE 8



*Notes:* Outcome information for three states enacting statewide buffer zones (indicated by vertical dashed line). Black lines indicate the state, bold grey lines indicate the national average, and thin grey lines plot all other states.

Figure 8 plots AGI abortion rate time series for the three states enacting buffer zones during our observation period. The black lines plot the time trends for these three treatment states, thin grey lines plot time trends of the control states, and the thick grey lines plot the national average. The dashed vertical lines indicate the years of enactment—2000 for Massachusetts, 1993 for Colorado, and 2005 in Montana. For Massachusetts and Colorado, we observe a drop around the time of the enactment of the buffer zones relative to the national average, while the trends appear roughly parallel for Montana.

Figure 8 also shows, however, that abortion rates have been dropping nationally since 1980, and the question is whether the drops in Massachusetts and Colorado are disproportionate given the national trends.

TABLE 4

	AGI					CDC	
	A	B	C	D	E	F	G
Buffer zone effect	-3.25** (1.28)	-4.00*** (1.00)	-2.88** (1.16)	-4.15*** (1.40)	-2.87** (1.39)	-1.48** (0.69)	-0.89 (0.69)
Demographic controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Policy controls	No	No	Yes	No	Yes	No	Yes
Interpolation	Yes	Yes	Yes	No	No	No	No
Parameters	89	103	123	90	110	91	111
R <sup>2</sup>	0.98	0.99	0.99	0.99	0.99	0.99	0.99
N	1,950	1,584	1,566	947	937	994	999
Years	1973– 2011	1979– 2011	1979– 2011	1979– 2011	1979– 2011	1990– 2010	1990– 2010

Notes: Difference-in-differences linear regression estimates. Standard errors, clustered by state, are presented in parentheses below coefficient estimates. All models control for state and year fixed effects. *Parameters* indicates the number of parameters estimated in the regression. *N* indicates the sample size. Two asterisks (\*\*) and three asterisks (\*\*\*) denote statistical significance at  $\alpha$ -levels of 0.05 and 0.01, respectively.

Table 4 presents results from DID regression models.<sup>173</sup> Each AGI model finds that the abortion rate in treatment states dropped substantially (and statistically significantly) more than in control states around the implementation of the buffer zone. The abortion rate on average drops by 3–4% after a state has enacted the buffer zone. These results appear to be robust to the addition of demographic covariates, policy covariates,<sup>174</sup> and linear interpolation, which provides suggestive evidence that the buffer zone, if anything, reduced the abortion rate. Using CDC data, the results remain robust when controlling for demographic attributes, but are sensitive to controlling for policy variables (more on that below).

How much should we trust these DID estimates? If states enacting buffer zone laws differ substantially from those that do not, standard linear regression estimates may extrapolate substantially. Conventional standard errors represent only the uncertainty due to sampling,<sup>175</sup> but the real source of uncertainty comes from inferring

<sup>173</sup> To test for whether the change is statistically distinguishable in the treatment states, we estimate a series of DID regressions of the following form:  $E(Y_{s,t}) = \tau T_{s,t} + \alpha_s + \eta_t + X'_{i,t}\beta$ , where  $Y_{s,t}$  indicates the abortion rate (per 1000 women ages fifteen to forty-four) in state  $s$  at year  $t$ ,  $T_{s,t}$  indicates whether a state has enacted a buffer zone (e.g., 1 for Massachusetts post-2000, and 0 pre-2000),  $X_{i,t}$  represents state time-varying covariates, and  $\alpha$  and  $\eta$  represent state and year fixed effects. The parameter of interest is  $\tau$ , and standard errors are clustered by state.  $Y_{s,t}$  is the standard outcome used in the literature, despite the fact that the number of women may also be endogenous.

<sup>174</sup> The policy variables may well induce post-treatment bias if the buffer zone changes the political dynamics around enactment of other policies.

<sup>175</sup> And, without clustering, standard errors exhibit poor properties. See Marianne Bertrand, Esther Duflo & Sendhil Mullanaithan, *How Much Should We Trust Difference-in-Differences Estimates?*, 119 Q.J. ECON. 249, 273–74 (2004) (identifying that serial

the *counterfactual* abortion rate in Colorado, Montana, and Massachusetts had each of these states *not* enacted a buffer zone.

TABLE 5

	Mass.	Synthetic Control	Control w/out Nevada	All states
<b>Demographic covariates</b>				
Population (millions)	6.0	5.1	13.0	5.1
Income per capita (1000s)	22.2	19.7	20.2	17.9
Female	51.6	50.5	51.8	51.2
Female (age 15–44)	23.8	23.3	23.1	23.3
White	93.0	88.3	84.7	85.4
White female	93.0	87.8	84.2	85
African American	4.8	<b>8.4</b>	12.7	10.3
African American female	4.8	8.8	13.2	10.6
Hispanic	3.9	6.4	6.4	4.9
Hispanic female	4.0	6.0	6.3	4.8
Rural	9.5	18.9	17.0	36.0
Poverty	9.7	11.2	12.8	13.3
Poverty female	11.1	12.6	14.5	14.8
Unemployment	5.8	6.5	7.2	6.7
Unemployment female	4.7	5.7	6.2	6.2
High school	57.0	<b>55.2</b>	52.5	53.0
College	23.7	17.3	18.3	17.1
Married	51.5	55.9	<b>54.3</b>	57.6
Welfare	7.7	7.0	8.5	7.7
Medicaid	10.0	7.8	<b>10.7</b>	9.0
<b>Policy covariates</b>				
Public funding restricted to maternal health	95.5	63.5	71.6	62.3
Post-viability ban	0.0	64.9	85.9	53.9
Legality of refusal to perform	81.8	<b>65.1</b>	<b>79.6</b>	52.6
Parental consent	100.0	<b>68.8</b>	<b>48.8</b>	48.8
TRAP hospital requirement	100.0	74.1	37.6	45.8
Informed consent	54.5	45.2	24.6	35.5
TRAP licensing requirement	0.0	57.0	37.7	26.7
Post 20-week allowed	100.0	35.0	24.9	19.1
Waiting period	50.0	0.0	15.0	18.4
Fetal disposal law	0.0	12.9	<b>3.6</b>	16.4
Public insurance restricted	22.7	33.1	55.4	15.7
Private insurance restricted	0.0	7.7	24.9	13.3
IDE Ban	0.0	5.5	5.6	10.3
Restricted at public facilities	0.0	4.6	24.7	9.9
Gag rule	0.0	8.8	11.7	6.3
Restriction up to 20-week pregnancy	0.0	4.7	5.6	5.6
Medicaid expansion	0.0	<b>0.2</b>	<b>5.9</b>	5.2
Mandatory viability test	0.0	4.2	2.8	4.7
Insurance waiver required	0.0	0.0	0.0	3.9
Contraceptive insurance coverage	0.0	1.8	<b>0.1</b>	2.1

*Notes:* Balance statistics for synthetic matching for 1979 to 2000 pretreatment period, excluding Colorado and Montana. Bolded numbers indicate that matching method placed a high weight (greater than 4%) on these variables. Synthetic controls can worsen balance on variables that have close to zero weight.

correlation in the treatment and outcome variables can mean that standard errors are too small and suggesting methodological refinements for difference-in-differences analysis).

Recent developments in matching methods attempt to address these inferential challenges.<sup>176</sup> In the DID context, the development of “synthetic control” matching is particularly promising.<sup>177</sup> The chief idea is to create a synthetic control state that looks as close as possible to the treatment state prior to the buffer zone (at least in measures predictive of the abortion rate), so that the difference in the abortion rates post-enactment can be attributed to the buffer zone. To illustrate this approach, we perform synthetic control matching, using all pre-2000 (i.e., pretreatment) demographic, policy, and outcome variables, with Massachusetts as the treatment state (excluding Colorado and Montana from the donor control pool).<sup>178</sup>

Table 5 presents balance statistics for covariates. The columns present covariate means from Massachusetts, the synthetic control, and the full control pool. The synthetic control appears closer to Massachusetts, but imbalances remain. Bolded numbers indicate variables that receive high weight in matching, as these are variables that have high predictive value for outcomes during the pretreatment period.

The left panel of Figure 9 plots results. The solid line indicates the abortion rate over time for Massachusetts. The dashed line indicates the abortion rate over time for the synthetic control group. After 2000, the dashed line represents the estimate of the *counterfactual* abortion rate had Massachusetts not enacted a buffer zone. Contrary to the standard DID estimates, states are largely indistinguishable in the post-treatment period, suggesting that the buffer zone had no effect.

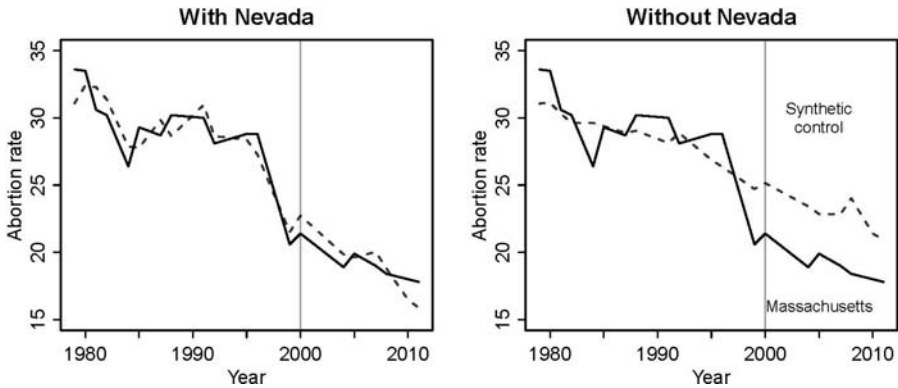
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<sup>176</sup> See, e.g., Daniel E. Ho, Kosuke Imai, Gary King & Elizabeth A. Stuart, *Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference*, 15 POL. ANALYSIS 199 (2007) (discussing the problem of model dependence in causal effect estimates and offering a method to reduce the problem); Guido W. Imbens, *Nonparametric Estimation of Average Treatment Effects Under Exogeneity: A Review*, 86 REV. ECON. STAT. 4 (reviewing the current literature on “inference for average treatment effects under the assumption of unconfoundedness”) (2004); Elizabeth A. Stuart, *Matching Methods for Causal Inference: A Review and a Look Forward*, 25 STAT. SCI. 1 (2010) (providing an overview of current techniques used in matching methods to reduce bias from covariates).

<sup>177</sup> See Abadie & Gardeazabal, *supra* note 163; Abadie, Diamond & Hainmueller, *supra* note 8.

<sup>178</sup> We use the implementation by Alberto Abadie, Alexis Diamond & Jens Hainmueller, *Synth: An R Package for Synthetic Control Methods in Comparative Case Studies*, 42 J. STAT. SOFTWARE 1 (2011).

FIGURE 9



Notes: Synthetic control matching. The solid line plots the abortion rate time series for Massachusetts and the dashed line plots that of the synthetic control state. The left panel presents results including Nevada, which enacted mandatory contraceptive insurance coverage in 2000, and the right panel presents results excluding Nevada.

TABLE 6

<u>With Nevada</u>		<u>Without Nevada</u>	
State	Weight	State	Weight
Nevada	39.0	Illinois	30.5
Minnesota	25.8	New York	30.4
Illinois	25.4	Pennsylvania	30.2
Missouri	9.1	Minnesota	7.2
Maryland	0.3	Delaware	1.2
Rhode Island	0.3	Maryland	0.3
New York	0.1	Rhode Island	0.2

Notes: Synthetic weights. The left columns indicate weights (summing up to 100, rounded) with Nevada and right columns indicate weights without Nevada.

To understand what is driving these results, Table 6 shows the weights placed on each of the states for the two models. Nevada receives nearly 40% of the weight when included. Nevada, however, also enacted a requirement in 1999 that health insurance plans provide contraceptive prescription coverage, and its abortion rate dropped over 10% between the 1997 and 2001 AGI survey periods. For this reason, including Nevada appears problematic—it confounds inferences because both Massachusetts and Nevada enacted policy changes that are likely to affect the abortion rate at around the same time.

The right panel of Figure 9 presents results from a model excluding Nevada from the control pool.<sup>179</sup> Again, the dashed line after 2000 represents the estimated counterfactual abortion rate for Massachusetts without a buffer zone. The synthetic control state has a comparable time series until 1999/2000, but after that, Massachusetts drops sharply relative to the control state. (Using permutation inference, that drop is sharper than might be expected due to chance.<sup>180</sup>)

These weights and the approach illustrate how synthetic control methods help to formalize and inform qualitative case studies. How reasonable are these as control states (i.e., should Nevada be included)? Given rapidly changing state laws affecting abortion access and the imbalance across treatment and control states, does the data support drawing an inference about the causal effect of the buffer zone at all? The “curse of dimensionality” looms large here: with twenty binary policy variables, there are 1,048,576 possible combinations of state policies, and only fifty states at the researcher’s disposal. Yet in empirical legal studies, where laws cannot be easily randomized on a large scale, researchers often have few other options than to address these questions with observational data.

\* \* \* \*

Whether the buffer zone caused the abortion rate to drop or not, the fact that there is no evidence that the buffer zone *increased* the abortion rate challenges one conventional view of buffer zones as preserving direct access to clinics.

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<sup>179</sup> The principal synthetic control states that comprise 98% of the basket did not enact mandatory contraceptive coverage around 1999–2000. Maryland, however, enacted such a requirement effective in October 1998. See 1998 Md. Laws 1192, 1193 (codified as amended at MD. CODE ANN., INS. § 15-826 (LexisNexis 2011)) (mandating coverage of FDA-approved contraceptives obtained by prescription). The general challenge here is that due to a confluence of factors (e.g., changes in the Federal Employees Health Benefits Program and a decision by the U.S. Equal Employment Opportunity Commission), many other control states enacted mandatory contraceptive coverage in the late 1990s and early 2000s. Cynthia Dailard, *Contraceptive Coverage: A 10-Year Retrospective*, GUTTMACHER REP. ON PUB. POL’Y, June 2004, at 6, 7–8; Adam Sonfield et al., *U.S. Insurance Coverage of Contraceptives and the Impact of Contraceptive Coverage Mandates, 2002*, 36 PERSP. ON SEXUAL & REPROD. HEALTH 72, 73 (2004).

<sup>180</sup> Using a one-tailed test statistic, namely the difference between the mean post-treatment prediction error (i.e., the difference between treatment and synthetic control outcomes) and the pretreatment prediction error, the *p*-value is 0.04. Using a two-tailed test statistic, namely the ratio of post-treatment mean squared prediction error (MSPE) to the pretreatment MSPE, the *p*-value is 0.20. This is because placebo treatments generate an unusual number of upward deviations for control states in the post-treatment period.

### E. Explanations and Mechanisms

*Model sensitivity.* The DID estimates stand in contrast to the sensitivity of the synthetic control results. The latter, however, arguably represents our uncertainty about the counterfactual outcomes in Massachusetts post-2000 more accurately. The key substantive questions are how much weight we want to put on the drop around 1999 and how meaningful the comparison to Nevada and other states is. The remaining imbalances should also give some caution to any strong inference about the causal effect of the buffer zone from state-level data.<sup>181</sup>

*Timing.* A critical reason to question whether the drop in the abortion rate is attributable to the buffer zone is that the Massachusetts data exhibit a drop in the abortion rate one year *prior* to enactment. (Unfortunately, AGI did not conduct its survey in 1997 and 1998, with standard practice being to interpolate missing years.<sup>182</sup>) The 1999 data, however, stem from a survey conducted in 2001, asking providers about their rates in 1999 and 2000. These biennial reports are highly correlated, so it is likely that survey responses as to 1999 reflect 2000 trends. AGI itself, for instance, does not make much of the 1999 data in its own publications.<sup>183</sup> To test this, we compare the pairwise correlations between the reported abortion rate in 1999 and 2000 by AGI to those in CDC, which are conducted annually. We confirm that the AGI pairwise correlation is indeed statistically signifi-

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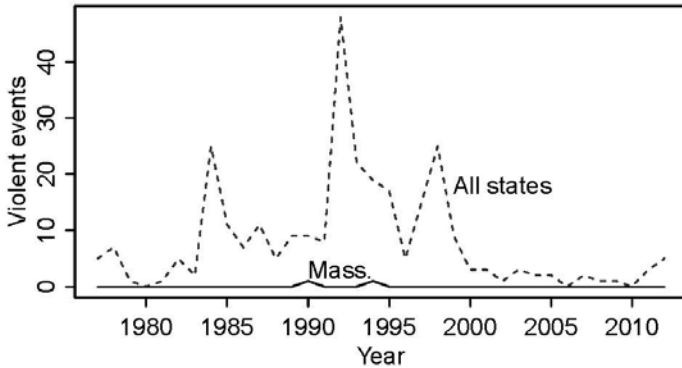
<sup>181</sup> There are many other methodological considerations with synthetic control matching. First, some covariates are only observed for more limited time periods (e.g., CPS data on a state-identified level is available from 1977 onward). The current implementation of synthetic control matching balances *averages* of covariates during the pretreatment period (omitting missing years), so these covariates are still included, but ideally one would observe these covariates for the full matching window. (Conventional DID regressions perform list-wise deletion.) Second, taking the average of covariates during the pretreatment period may obscure important differences between states. For example, a state that has policy A for the first half of the pretreatment window, but repeals that policy for the second half of the period, is treated indistinguishably from a state that has no policy for the first half of the pretreatment window but then enacts policy A. Third, synthetic control matching ignores all covariates on the post-treatment period, on the assumption that these are plausibly affected by the treatment. A control state, however, might enact a policy in the post-treatment period that is unaffected by the absence or presence of a buffer zone, which could confound estimates. One way to approach this would be to trim the control pool of any such control states, but that underscores how little support there is in the data for the inference about buffer zones.

<sup>182</sup> STANLEY K. HENSHAW & KATHRYN KOST, TRENDS IN THE CHARACTERISTICS OF WOMEN OBTAINING ABORTIONS, 1974 TO 2004, at 4 (2008); see also John J. Donohue III & Steven D. Levitt, *The Impact of Legalized Abortion on Crime*, 116 Q.J. ECON. 379, 415–16 (2001) (describing annual data from AGI).

<sup>183</sup> See Lawrence B. Finer & Stanley K. Henshaw, *Abortion Incidence and Services in the United States in 2000*, 35 PERSP. ON SEXUAL & REPROD. HEALTH 6, 9 tbl.2 (2003) (providing data from 1992, 1996, and 2000, but omitting data from 1999).

cantly higher in that year, suggesting that Massachusetts’s lead drop may not be a lead at all. Colorado does not exhibit a lead drop.

FIGURE 10  
Clinic Violence



Notes: Violence against abortion clinics over time. Incidents are collected by the National Abortion Federation, and include incidents of arson, murder, shooting, bombing, and acid attacks.

*Intimidation.* To the extent that there is a drop around the enactment of the buffer zone, it is also possible that it has less to do with the buffer zone per se than the reasons for enacting it: intimidation of women seeking abortions and clinic employees. The impetus for the 2000 Massachusetts buffer zone was “unduly aggressive behaviors”<sup>184</sup> by protesters, with the 1994 shooting being the most egregious example. Violence and intimidation may well have deterred women from visiting clinics, and would suggest that the DID exogeneity assumption is violated. The challenge with this explanation, however, is that the level of harassment or intimidation must be (a) specific to the enacting states, and (b) increase sharply around the time of enactment. As Figure 10 shows, direct violence was decreasing nationwide in the late 1990s, and this kind of protest activity did not appear to increase sharply around the time of enactment. A more plausible mechanism is that violence and intimidation caused certain clinics to shut down, with some lag. Applying a similar DID model using the number of providers as the outcome, we find evidence that the per capita number of clinics declined around the time of the buffer zone enactment. This corroborates the state interest in enacting the buffer of protecting clinic employees. On the other hand, the number of providers alone does not account for the decline in the abortion rate.

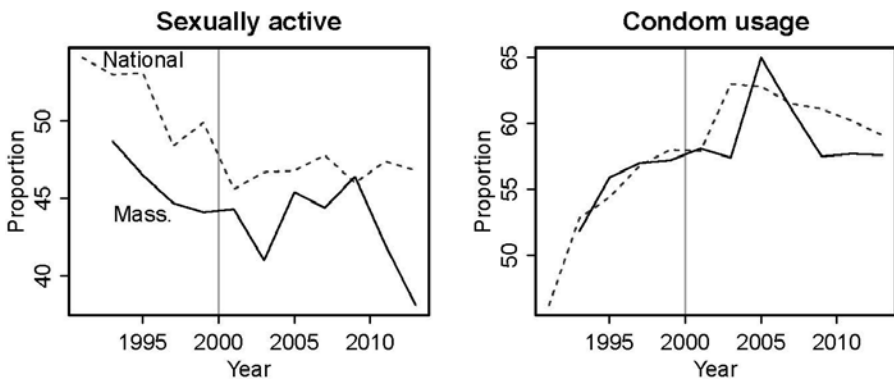
<sup>184</sup> McCullen v. Coakley, 571 F.3d 167, 172 (1st Cir. 2009).



Closures typically occur for the smallest providers<sup>185</sup> and are thereby unlikely to explain the large and sharp declines in Massachusetts and Colorado. Indeed, even though the number of providers is almost surely post-treatment, the DID findings on abortion rate persist even after controlling for the provider rate, using the most saturated AGI model.<sup>186</sup>

*Salience of Violence.* Legislative discussion of the proposed buffer zone might itself have highlighted the existence of abortion clinic protests. Even if protest levels were constant, the heightened salience associated with the legislative discussion might then have deterred women. There is, however, limited evidence that the legislative discussion sparked greater public consciousness of clinic protests.<sup>187</sup>

FIGURE 11



Notes: Sexual activity and condom usage amongst high school students from the CDC Youth Risk Behavior Survey, conducted biannually. The solid and dashed lines indicate Massachusetts and national trends, respectively.

*Contraception.* One other possibility, given that clinics also often provide contraception, is that the buffer zone facilitated access to contraception, which might explain the drop in the abortion rate.<sup>188</sup> If true, this would suggest that abortion protesters are themselves undermining the role of clinics in reducing the number of abortions. Aside

<sup>185</sup> *Finer & Henshaw, supra* note 183, at 14.

<sup>186</sup> The number of providers is only available for a limited number of years.

<sup>187</sup> As a suggestion for further research, we note that a comprehensive content analysis of mass media could provide some evidence of the extent to which legislative discussion on this or other topics increased public awareness of this and other problems.

<sup>188</sup> See GUTTMACHER INSTITUTE, *STATE FACTS ABOUT ABORTION: MASSACHUSETTS* (2014), available at <http://www.guttmacher.org/pubs/sfaa/massachusetts.pdf> (“The very small group of American women who are at risk of experiencing an unintended pregnancy but are not using contraceptives account for more than half of all abortions.”).

from anecdotal accounts,<sup>189</sup> however, there is not much qualitative evidence to support this explanation. Only three of seven Planned Parenthood chapters in Massachusetts offer abortion services,<sup>190</sup> and, at the time, many other substitute establishments offered free or low-cost contraceptive care.<sup>191</sup> Data on sexual activity and contraceptive usage from a random sample of national and Massachusetts high school students, depicted in Figure 11, do not suggest that Massachusetts exhibited disproportionately greater access to contraception (or decreased sexual activity) around 2000.<sup>192</sup>

*Channeling.* Another possibility, particularly interesting from the vantage point of the marketplace of ideas, is that the buffer zone channeled the type of speech around abortion clinics. In *Hill v. Colorado*, the Court noted that a buffer zone “might encourage the most aggressive and vociferous protesters to moderate their confrontational and harassing conduct, and thereby make it easier for thoughtful and law-abiding sidewalk counselors . . . to make themselves heard.”<sup>193</sup> It is possible then that a restriction on speech can actually allow more persuasive speech to be heard.<sup>194</sup> The litigants in *McCullen* corroborate this mechanism. While McCullen reported persuading roughly eighty women from 2000 to 2007, she claimed to have reached “far fewer people” since 2007; Zarella claimed she had 100 successful interactions from 2000 to 2007, but none since.<sup>195</sup> The scope of “side-

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<sup>189</sup> E.g., Sarah Betancourt, *Massachusetts Gets the Final Word After Explosive Supreme Court Decision*, LIBERALAND (Aug. 1, 2014), <http://www.alan.com/2014/07/31/massachusetts-gets-the-final-word-after-explosive-supreme-court-decision/>.

<sup>190</sup> *Health Center Search Results: Massachusetts*, PLANNED PARENTHOOD, <http://www.plannedparenthood.org/health-center/MA> (last visited Mar. 24, 2015).

<sup>191</sup> See MASS. LEAGUE OF COMMUNITY HEALTH CENTERS, *WHY MASSACHUSETTS COMMUNITY HEALTH CENTERS?* 5, 10 (2004), available at <http://www.massleague.org/About/WhyMACHCs.pdf> (explaining how fifty community health centers in Massachusetts provide a wide range of services for low-income people, including family planning); *Family Planning Site List by Region*, MASS.GOV, <http://www.mass.gov/eohhs/docs/dph/com-health/family-plan/fpp-site-list-region.pdf> (last visited Mar. 24, 2015) (listing ninety family-planning sites in the state, within the ABCD Family Planning network, which has operated for fifty years, offering thirty-two sites in the Greater Boston area alone, with free and low-cost family planning, birth control counseling, and contraception for the uninsured).

<sup>192</sup> There is a sharp increase in 2005 in condom usage in Massachusetts, but attributing this increase to the buffer zone is inconsistent with the timing around 2000 and the preceding sharp increase in the national trend.

<sup>193</sup> 530 U.S. 703, 727 (2000). *But see id.* at 763 (Scalia, J., dissenting) (arguing that by upholding the buffer zone, the Court is allowing Colorado to make the task of persuading women not to have abortions “an impossible one”).

<sup>194</sup> Citizens for a Pro-Life Society, for instance, claims that “[s]idewalk counseling is one of the most effective ways to save unborn babies from abortion.” Monica Migliorino Miller, *Effective Sidewalk Counseling*, CITIZENS FOR A PRO-LIFE SOCIETY, <http://www.prolifesociety.com/prolifesociety/pages/AboutUs/sidewalkcounseling/sidewalk.aspx> (last visited Mar. 24, 2015).

<sup>195</sup> *McCullen v. Coakley*, 134 S. Ct. 2518, 2535 (2014).

walk counseling” also allegedly extends far beyond the three named litigants, with groups claiming to have dissuaded thousands of women.<sup>196</sup> While McCullen and Zarella alone could not possibly account for the magnitude of the drop, collectively, persuasive speech may account for at least part of the drop. That said, the right panel of Figure 11 does not exhibit an increase when Massachusetts changed from a floating to a fixed buffer zone in 2007.

*Substitution.* Might Massachusetts residents have traveled to other states, so that the state decline is really a substitution effect? AGI and CDC both have subsets of the data that separate frequencies by state of occurrence versus state of residence. These data are limited; they do not contain systematic information about the full substitution matrix and appear to be a model-based inference. Nonetheless, these data do not suggest that women traveling to neighboring states account for the decrease in Massachusetts. The drop is just as prevalent among residents of Massachusetts, and there is no substantial increase in non-resident abortions in neighboring states.

*Alternative Policy Changes.* Inferences may be confounded if we have not controlled for other policy changes occurring sharply around the time of enactment specific to the treatment or control groups. While we have controlled for many policy changes (e.g., expanded eligibility for family planning services under Medicaid and mandatory contraceptive insurance coverage), other (unobserved) policy shifts might still confound our inferences. Whether or not to “control” for policy variables should be further informed by substantive research and is not always entirely straightforward. For instance, consider the only result in Table 4 under which the buffer zone effect is statistically insignificant, which occurs with CDC data when policy variables are introduced. This result appears largely driven by controlling for contraceptive care coverage, which begins in Massachusetts in 2003. Yet is coverage truly a covariate? If the ineffectiveness of the buffer zone increased the political will to enact mandatory contraceptive care coverage in Massachusetts, it is not an appropriate control. If the 2003 implementation in Massachusetts is truly independent, the right panel of Figure 11 may falsely attribute the decrease in the abortion rate to

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<sup>196</sup> See, e.g., *Sidewalk Counseling*, PRO-LIFE ACTION LEAGUE, <http://prolifeaction.org/sidewalk/> (last visited Mar. 24, 2015) (claiming that “[t]housands of children are alive today because the Pro-Life Action League was there at the moment of crisis”); *January 2009 to Present — Partial List of Babies Saved Due to Sidewalk Counseling in Richmond Virginia*, LIFE & LIBERTY, <http://www.lifeandlibertyministries.com/archives/000373.php> (last visited Mar. 24, 2015) (claiming to have dissuaded over sixty women from abortions in a little over two years).

the buffer zone.<sup>197</sup> The synthetic control approach highlights the complementarity with qualitative research, but also underscores why a research design with more granular data (i.e., at the municipality level) is the most promising path forward.

\* \* \* \*

Our results provide suggestive evidence both about the state interest (as providers may be closing in response to protest activity), and the distributive effects across types of speech. Contra Holmes, speech restrictions may allow different types of, and perhaps more persuasive, voices to be heard.

## VI IMPLICATIONS

Should constitutional law become more empirical? In one basic way, the answer is indisputably yes. Judges should base their decisions on accurate facts,<sup>198</sup> and at the very least should make factually accurate statements about the state of the law. *McCullen* distinguishes *Burson* on the basis of the prohibition of police officers around the polls and the presence of police around abortion clinics: “under state law, ‘law enforcement officers generally are barred from the vicinity of the polls.’”<sup>199</sup> Yet it would not take much of a clerk’s time to consult state election laws to determine that this is false. *Burson* talked only about Tennessee. And Tennessee is one of only four states that affirmatively bar police officers from the vicinity of the polling booth.<sup>200</sup> There may well be grounds to distinguish abortion clinics and voting places, but police presence is not one of them.<sup>201</sup>

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<sup>197</sup> Indeed, the principal reason why the CDC results in Table 4 are sensitive to policy variables is because of contraceptive coverage. The saturated regression model in Table 4 controls for contraceptive coverage as one of the twenty policy variables.

<sup>198</sup> We note, but cannot possibly document, our belief that many appellate judicial misstatements of fact are not the ones that misuse cited studies or published data, but rather are the ones that lurk behind seemingly simple factual assertions. To give just one example, the Supreme Court in *New York Times v. Sullivan*, 376 U.S. 254, 277 (1964), made a number of factual and causal claims about newspaper behavior in the face of possible libel judgments, but it is far from clear that these claims were accurate. See Frederick Schauer, *Uncoupling Free Speech*, 92 COLUM. L. REV. 1321, 1328–43 (1992) (suggesting that newspaper behavior was less influenced by changes in libel law than the *Sullivan* Court posited). By advocating that judges should base their decisions on “accurate facts,” we do not mean to suggest that courts should prize accuracy over all other concerns (e.g., the cost or suitability of determining such facts).

<sup>199</sup> *McCullen*, 134 S. Ct. at 2540 (quoting *Burson v. Freeman*, 504 U.S. 191, 207 (1992)).

<sup>200</sup> See *supra* note 149 (listing these states).

<sup>201</sup> This is not the occasion to explain why it is important that appellate courts get their facts right. One possibility is that doing so will produce different or better outcomes, but we do not claim that getting the facts right will necessarily or even usually produce

A more systematic examination of voting buffer zones also suggests an internal contradiction in *Burson*: by impeding exit polling, the case may itself undercut the compelling state interest of curbing voting fraud. “Requiring pollsters to stand more than a few feet from the exit to a building in which voting occurs is death to an exit poll.”<sup>202</sup> Sharp disparities between exit polls and official voting results can be used to reveal, investigate, or deter voting fraud.<sup>203</sup> Indeed, exit polls are a quintessential tool for determining the truth along many dimensions, with few good substitutes. Numerous questions (e.g., What were the major issues causing voters to make decisions? Does racially cohesive voting persist? Are voter ID requirements neutrally administered?) could not be answered easily absent exit polls.<sup>204</sup> *Burson* de facto validated numerous state laws that made exit polling much more difficult in its immediate aftermath.<sup>205</sup> Buffer zones in numerous states prohibit the *presence* of individuals, regardless of whether they are engaged in campaign activity. Several courts, most notably the New Jersey Supreme Court, have used *Burson*’s reasoning to uphold state laws interpreted to prohibit exit polling.<sup>206</sup> Post-*Burson*, the

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different outcomes. However, even if factual accuracy of this variety is only sometimes outcome-determinative, judicial accuracy about facts is likely to have other beneficial consequences, including increased confidence in the courts and increased accuracy by others who rely on judicial factual statements.

<sup>202</sup> D. James Greiner & Kevin M. Quinn, *Long Live the Exit Poll*, 141 DAEDALUS, Fall 2012, at 9, 11.

<sup>203</sup> SAMUEL J. BEST & BRIAN S. KRUEGER, EXIT POLLS: SURVEYING THE AMERICAN ELECTORATE, 1972–2010, at 9 (2012). *But see* Greiner & Quinn, *supra* note 202, at 14–15 (questioning the likelihood that an exit poll could detect election fraud, because “the presence of an exit poll might deter the behavior it is attempting to detect” and because of high variability in estimates).

<sup>204</sup> *See* Greiner & Quinn, *supra* note 202, at 15 (discussing how exit polls are essential for understanding critical questions of election administration, including questions of registration, purging of voters, ballot design, waiting times, language translation, voting machines, and voter ID).

<sup>205</sup> *Burson* only discussed exit polling in passing, as Tennessee’s statute did not appear to apply to exit polling. *See* *Burson v. Freeman*, 504 U.S. 191, 223–24 (1992) (Stevens, J., dissenting) (noting that “exit polling . . . presents at least as great a potential interference with orderly access to the polls”).

<sup>206</sup> *In re* Attorney General’s “Directive on Exit Polling: Media and Non-Partisan Public Interest Groups,” 981 A.2d 64, 82 (N.J. 2009) (upholding, relying on *Burson*, a 100-foot buffer zone as applied to exit polling, when state law prohibited obstruction, electioneering, and soliciting); *see also* N.J. Press Ass’n v. Guadagno, No. 12-06353 (JAP), 2012 WL 5498019, at \*5 (D.N.J. Nov. 13, 2012) (relying on *Burson* in denying preliminary injunction from enforcement of a 100-foot buffer zone against exit polling); *but see* Am. Broad. Cos. v. Wells, 669 F. Supp. 2d 483, 490 (D.N.J. 2009) (issuing preliminary injunction to allow exit polling). *But see* Daily Herald Co. v. Munro, 838 F.2d 380, 389 (9th Cir. 1988) (striking down, pre-*Burson*, a 300-foot buffer zone as applied to exit polling); Nat’l Broad. Co. v. Cleland, 697 F. Supp. 1204, 1217 (N.D. Ga. 1988) (limiting, pre-*Burson*, buffer zone from 250 to 25 feet as applied to exit polling); News-Press Publ’g Co. v. Firestone, 527 So. 2d 223, 226 (Fla. Dist. Ct. App. 1988) (striking down, pre-*Burson*, a fifty-foot buffer zone).

ability to conduct an exit poll within the buffer zone was highly uncertain and subject to a great degree of litigation.<sup>207</sup> In the aftermath of the 2000 election, one legislator even attempted to expand the buffer zone specifically with regard to exit polling to 1000 feet.<sup>208</sup> While most courts have carved out exemptions for the media over time, *Burson's* net effect may have been to exacerbate, rather than alleviate, the potential for voter fraud.

*Burson* may similarly impede the search for democratic truth and the integrity of the franchise in other unanticipated ways. Voting buffer zones impede the signature-gathering process for initiatives.<sup>209</sup> Public interest groups cannot provide nonpartisan voter information cards,<sup>210</sup> which some of the political science literature suggests may be precisely the kind of informational cues that enable voters to act as if they are informed.<sup>211</sup>

In a more general way, our results provide some of the first evidence regarding the validity of the marketplace of ideas meme, even if in a novel and not exclusively epistemic way, in one of its most common contexts of time, place, and manner restrictions. We draw several conclusions from our analysis of these results.

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<sup>207</sup> See, e.g., *Beacon Journal Publ'g v. Blackwell Co.*, 389 F.3d 683, 685 (6th Cir. 2004) (striking down Ohio's buffer zone as applied against press); *Am. Broad. Cos. v. Ritchie*, No. 08-5285 (MJD/AJB), 2008 WL 4635377, at \*8 (D. Minn. Oct. 15, 2008) (issuing a preliminary injunction against applying a 100-foot buffer zone to exit polling); *Am. Broad. Co. v. Blackwell*, 479 F. Supp. 2d 719, 744 (S.D. Ohio 2006) (striking down interpretation by the secretary of state applying a 100-foot buffer zone to exit polling); *CBS Broad., Inc. v. Cobb*, 470 F. Supp. 2d 1365, 1372 (S.D. Fla. 2006) (striking down 100-foot buffer zone as applied to exit polling); *Am. Broad. Cos. v. Heller*, No. 2:06 CV-01268-PMP-RJJ, 2006 WL 3149365, at \*13 (D. Nev. Nov. 1, 2006) (issuing an injunction against the application of a 100-foot buffer zone to exit polling).

<sup>208</sup> Kevin O'Hanlon, *Proposal Aims to Keep Exit Pollsters Away from Voters*, *McCook DAILY GAZETTE*, Jan. 19, 2001, at 3.

<sup>209</sup> See, e.g., *United Food & Commercial Workers Local 1099 v. City of Sidney*, 364 F.3d 738, 747 (6th Cir. 2003) ("Appellants' complaint in this case makes clear that the Helmlingers were deterred from soliciting signatures on the public sidewalk . . . because the sidewalk was within the 100-foot campaign-free zone established by [state statute]."); *Schirmer v. Edwards*, 2 F.3d 117, 124 (5th Cir. 1993) (noting that a 600-foot buffer zone is not an excessive infringement of First Amendment rights of signature-seeking people but acknowledging buffer zones compromise those rights).

<sup>210</sup> *In re Attorney General's "Directive on Exit Polling: Media and Non-Partisan Public Interest Groups"*, 981 A.2d at 80 n.13.

<sup>211</sup> On the kinds of informational cues that will help voters decide and that will help voters feel informed, see generally SAMUEL L. POPKIN, *THE REASONING VOTER: COMMUNICATION AND PERSUASION IN PRESIDENTIAL CAMPAIGNS* (1994) (developing a model of "low information rationality" to describe and analyze voter behavior); Arthur Lupia, *Shortcuts Versus Encyclopedias: Information and Voting Behavior in California Insurance Reform Elections*, 88 AM. POL. SCI. REV. 63, 72 (1994) (finding that relatively uninformed California voters used the "shortcut" of determining the insurance industry position on a ballot measure in determining how to vote in accordance with their interests).

First, our results show that generalities about the marketplace of ideas are unlikely to help us understand its operation in actual settings. Thus, marketplace enthusiasts might be right in a non-obvious way about voting buffer zones. By reducing cues to voters, turnout may be reduced. And here Holmes comes back into the picture, because if Holmes's version of the marketplace was essentially a claim about democracy, then a reduction in voter turnout as a result of closing a marketplace of ideas may harm democracy.<sup>212</sup> On the other hand, the abortion buffer zones were either ineffective and/or "channeled" speech to a more persuasive kind. Empirically examining actual speech restrictions is critical to understanding the marketplace of ideas in operation.

Second, whether the effects of speech restrictions promote truth is, obviously, contested. In the voting context, a long literature in political science questions the *Burson* Court's assumption about higher turnout as an end in itself. For example, increased turnout may have unequal distributional effects.<sup>213</sup> In addition, the marginal voter is likely to be less informed on policy issues, and her participation may impede revealing truthful preferences and decrease social welfare.<sup>214</sup> In the abortion context, it is at a minimum contestable whether what the *McCullen* Court calls "persuasion" inclines toward a position of "truth" about abortion. One woman's persuasion is another's harassment. It is of course this very difficulty of measuring truth in many contexts that makes the epistemic dimensions of the marketplace claim difficult to test in the most commonly contested contexts, and that prompted our search for broader consequences.

Third, research design is critical.<sup>215</sup> Recent advances in the design of natural experiments are particularly promising to the extent that scholars are interested in understanding the impact of constitutional

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<sup>212</sup> But it is possible that there is a turnout-turnout tradeoff. If the lack of buffer zones causes congestion, general turnout may decline (for fear of long voting lines), while turnout within the buffer zone increases. The optimal buffer zone size would trade off (amongst other considerations) these two turnout effects.

<sup>213</sup> See, e.g., Ian McAllister, *Compulsory Voting, Turnout and Party Advantage in Australia*, 21 POL. 89, 89, 92 (1986) (finding that even with a system of mandatory voting, not everyone votes, but higher turnout benefits parties of the left); Arend Lijphart, *Unequal Participation: Democracy's Unresolved Dilemma*, 91 AM. POL. SCI. REV. 1, 10-11 (1997) (describing how voter turnout falls with socioeconomic status and proposing the adoption of compulsory voting to solve this problem of democratic inequality).

<sup>214</sup> There is, however, no consensus in this literature as to the desirability of voter turnout. For a recent discussion of how intuitions are incomplete because they fail to account for the effect on equilibrium behavior of politicians, see Scott Ashworth & Ethan Bueno De Mesquita, *Is Voter Competence Good for Voters?: Information, Rationality, and Democratic Performance*, 108 AM. POL. SCI. REV. 565 (2014).

<sup>215</sup> This point is argued with great vigor in Lee Epstein & Gary King, *The Rules of Inference*, 69 U. CHI. L. REV. 1 (2002).

law. At the same time, such designs hinge critically on substantive legal and institutional knowledge, where legal scholars may possess a distinct comparative advantage.<sup>216</sup> Do cognate statutory provisions (e.g., about police presence or the scope of prohibited speech) affect the credibility of conventional assumptions, such as homogeneity of treatment effects and the control pool? How does enforcement practice affect our understanding of critical assumptions, such as a discontinuity at the threshold? Our analysis above, and indeed the design of the symposium itself (pairing constitutional law experts with empirical methodologists), illustrates what we view as an essential synergy between substance and methodology in empirical constitutional law.

### CONCLUSION

Our results provide a path toward empirically studying the consequences—epistemic or not—of the marketplace of ideas, and thus of empirically studying the First Amendment more generally. Wild factual disagreements between the litigants and Supreme Court opinion blocs existed in both *Burson* and *McCullen*. Our article hopes to show how some of the conjectures about the marketplace of ideas and buffer zones may be empirically tested in the future. Of course, we cannot inform all considerations in more general First Amendment analysis. But, surely, some empirical grounding is better than anecdotal conjecture.

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<sup>216</sup> See, e.g., John J. Donohue, Daniel E. Ho & Patrick Leahy, *Do Police Reduce Crime? A Reexamination of a Natural Experiment*, in *EMPIRICAL LEGAL ANALYSIS: ASSESSING THE PERFORMANCE OF LEGAL INSTITUTIONS* 125 (Yun-chien Chang ed., 2014) (demonstrating how substantively understanding the mechanism of police relocation is critical to interpreting difference-in-differences analysis); Ho & Rubin, *supra* note 8 (illustrating the critical role of qualitative assumptions in ascertaining whether matching or regression-discontinuity is more appropriate with prison misconduct data); Daniel E. Ho & Larry Kramer, *Introduction: The Empirical Revolution in Law*, 65 *STAN. L. REV.* 1195, 1201 (2013) (“The design of credible empirical work must be informed by substantive, institutional knowledge.”) (emphasis omitted).



## APPENDIX A. SOURCES FOR VOTING BUFFER ZONE JURISDICTIONS

<b>Columns</b>	<b>Source</b>
Voting (2008 Early voting)	MICHAEL McDONALD, GEORGE MASON UNIVERSITY DEPARTMENT OF PUBLIC AND INTERNATIONAL AFFAIRS, UNITED STATES ELECTIONS PROJECT: 2012 EARLY VOTING STATISTICS, <a href="http://www.electproject.org/2012_early_vote">http://www.electproject.org/2012_early_vote</a> (last updated Nov. 6, 2012).
Voting (Voter file cost; Voter file available)	Most voter files were obtained from individual state board of elections or registrar websites. If voter file cost was quoted per record, we multiplied the cost by the number of voters in the state. If cost and availability were not available on board of elections or registrar websites, we called the registrar's office to ask about availability and obtain a cost estimate.
Poll. place (Preference for public; No private building)	ALA. CODE § 17-6-4 (2014); ALASKA STAT. § 15.10.020 (2013); ARIZ. REV. STAT. ANN. § 16-411 (2014); ARK. CODE ANN. § 7-5-101 (2014); CAL. ELEC. CODE § 12282 (West 2014); COLO. REV. STAT. § 1-5-102 (2014); CONN. GEN. STAT. § 9-168 (2013); DEL. CODE ANN. tit. 15, § 4512 (2014); FLA. STAT. § 101.71 (2013); GA. CODE ANN. § 21-2-266 (2012); HAW. REV. STAT. § 11-92.2 (2014); IDAHO CODE ANN. § 34-302 (2014); 10 ILL. COMP. STAT. 5/11-4.1 (2014); IND. CODE § 3-11-8-3.1 (2014); IOWA CODE § 49.21 (2013); IOWA CODE § 49.24 (2013); KAN. STAT. ANN. § 25-2701 (2013); KY. REV. STAT. ANN. § 117.065 (West 2014); LA. REV. STAT. ANN. § 18:533 (2013); ME. REV. STAT. ANN. tit. 21-A, § 627 (2014); MD. CODE ANN., ELEC. LAW § 10-101 (LexisNexis 2014); MASS. GEN. LAWS ANN. ch. 54 § 24 (2014); MICH. COMP. LAWS § 168.662 (2014); MINN. STAT. § 204B.16 (2013); MISS. CODE ANN. § 23-15-259 (2014); MO. REV. STAT. § 115.117 (2013); MONT. CODE ANN. § 13-3-105 (2013); NEB. REV. STAT. § 32-904 (2013); NEV. REV. STAT. § 293.437 (2013); N.J. STAT. ANN. § 19:8-3 (West 2014); N.M. STAT. § 1-3-7 (2013); N.Y. ELEC. LAW § 4-104 (Consol. 2014); N.C. GEN. STAT. § 163-128 (2006); N.D. CENT. CODE § 16.1-04-02 (2013); OHIO REV. CODE ANN. § 3501.18 (LexisNexis 2014); OKLA. STAT. tit. 26, § 3-123 (2013); OR. REV. STAT. § 254.472 (2013); 25 PA. CONS. STAT. ANN. § 2727 (2014); R.I. GEN. LAWS § 17-11-1 (2013); S.C. CODE ANN. § 7-7-910 (2012); S.D. CODIFIED LAWS § 12-14-1 (2014); TENN. CODE ANN. § 2-3-107 (2014); TEX. ELEC. CODE ANN. § 43.031 (Vernon 2013); UTAH CODE ANN. § 20A-5-403 (2013); VT. STAT. ANN. tit. 17, § 2502 (2013); VA. CODE ANN. § 24.2-310 (2014); WASH. REV. CODE § 29A.40.160 (2013); W. VA. CODE § 3-1-23 (2013); WIS. STAT § 5.25 (2013); WYO. STAT. ANN. § 22-12-101 (2014).

<p>Buffer zone (Radius; Campaign Activity; Person)</p>	<p>ALA. CODE § 17-9-50 (2014); ALASKA STAT. § 15.15.170 (2013); ARIZ. REV. STAT. ANN. § 16-515 (2014); ARK. CODE ANN. § 7-1-103(9)(A) (2014); CAL. ELEC. CODE § 18370 (West 2014); CAL. ELECTIONS CODE § 319.5 (West 2014); COLO. REV. STAT. § 1-13-714 (2014); CONN. GEN. STAT. § 9-236 (2013); DEL. CODE ANN. tit. 15, § 4942 (2014); FLA. STAT. § 102.031 (2013); GA. CODE ANN. § 21-2-414 (2014); HAW. REV. STAT. § 11-132 (2014); IDAHO CODE ANN. § 18-2318 (2014); 10 ILL. COMP. STAT. 5/17-29 (2014); IND. CODE § 3-5-2-10 (2014); IOWA CODE § 39A.4 (2013); KAN. STAT. ANN. § 25-2430 (2013); KY. REV. STAT. ANN. § 117.235 (West 2014); LA. REV. STAT. ANN. § 18:1462 (2013); ME. REV. STAT. ANN. tit. 21-A, § 682 (2014); MD. CODE ANN., ELEC. LAW § 16-206 (LexisNexis 2014); MASS. GEN. LAWS ANN. ch. 54 § 65 (2014); MICH. COMP. LAWS § 168.744 (2014); MICH. COMP. LAWS § 168.931 (2014); MINN. STAT. § 211B.11 (2013); MINN. STAT. § 204C.06 (2013); MISS. CODE ANN. § 23-15-245 (2014); MISS. CODE ANN. § 23-15-895 (2014); MO. REV. STAT. § 115.637 (2013); MONT. CODE ANN. § 13-35-211 (2013); NEB. REV. STAT. § 32-1524 (2013); NEV. REV. STAT. § 293.740 (2013); N.H. REV. STAT. ANN. § 659:43 (2014); N.J. STAT. ANN. § 19:34-15 (West 2014); N.M. STAT. § 1-20-16 (2013); N.Y. ELEC. LAW § 8-104 (Consol. 2014); N.C. GEN. STAT. § 163-166.4 (2013); N.D. CENT. CODE § 16.1-10-06 (2013); OHIO REV. CODE ANN. § 3501.30 (LexisNexis 2014); OHIO REV. CODE ANN. § 3501.35 (LexisNexis 2014); OKLA. STAT. tit. 26, § 16-111 (2013); OKLA. STAT. tit. 26, § 7-108 (2013); OR. REV. STAT. § 260.695 (2013); 25 PA. CONS. STAT. ANN. § 3060 (2014); R.I. GEN. LAWS § 17-19-49 (2013); S.C. CODE ANN. § 7-25-180 (2012); S.D. CODIFIED LAWS § 12-18-3 (2014); TENN. CODE ANN. § 2-7-111 (2014); TEX. ELEC. CODE ANN. § 61.003 (Vernon 2013); UTAH CODE ANN. § 20A-3-501 (2013); VT. STAT. ANN. tit. 17, § 2508 (2013); VA. CODE ANN. § 24.2-604 (2014); WASH. REV. CODE § 29A.84.510 (2013); W. VA. CODE § 3-1-37 (2013); W. VA. CODE § 3-9-9; WIS. STAT § 12.03 (2013); WYO. STAT. ANN. § 22-26-113 (2014).</p>
<p>Police presence (Def. barred; Def. allowed; Unrestricted)</p>	<p>ALA. CODE § 17-9-50 (2014); ALASKA STAT. § 15.15.170 (2013); ARIZ. REV. STAT. ANN. § 16-515 (2014); ARK. CODE ANN. § 7-4-107 (2014); CAL. ELEC. CODE § 18544 (West 2014); COLO. REV. STAT. § 1-13-714 (2014); CONN. GEN. STAT. § 9-236 (2013); DEL. CODE ANN. tit. 15, § 4933 (2014); FLA. STAT. § 102.101 (2013); GA. CODE ANN. § 21-2-414 (2012); GA. CODE ANN. § 21-2-593 (2012); HAW. REV. STAT. § 11-132 (2014); IDAHO CODE ANN. § 18-2318 (2014); 10 ILL. COMP. STAT. 5/17-23 (2014); IND. CODE § 3-11-8-15 (2014); IOWA CODE § 49.104 (2013); KAN. STAT. ANN. § 25-2413 (2013); KY.</p>

	<p>REV. STAT. ANN. § 117.235 (West 2014); LA. REV. STAT. ANN. § 18:428 (2013); ME. REV. STAT. ANN. tit. 21-A, § 681 (2014); MD. CODE ANN., ELEC. LAW § 10-304 (LexisNexis 2014); MASS. GEN. LAWS ANN. ch. 54 § 65 (2014); MICH. COMP. LAWS § 168.744 (2014); MINN. STAT. § 204C.06 (2013); MISS. CODE ANN. § 23-15-245 (2014); MISS. CODE ANN. § 23-15-257 (2014); MISS. CODE ANN. § 23-15-895 (2014); MO. REV. STAT. § 115.059 (2013); MO. REV. STAT. § 115.409 (2013); MONT. CODE ANN. § 13-13-122 (2013); NEB. REV. STAT. § 32-910 (2013); NEV. REV. STAT. § 293.740 (2013); N.H. REV. STAT. ANN. § 659:21 (2014); N.J. STAT. ANN. § 19:15-8 (West 2014); N.J. STAT. ANN. § 19:32-22 (West 2014); N.M. STAT. § 1-12-5 (2013); N.Y. ELEC. LAW § 8-104 (Consol. 2014); N.C. GEN. STAT. § 163-166.3 (2013); N.C. GEN. STAT. § 163-166.4 (2013); N.D. CENT. CODE § 16.1-10-06 (2013); OHIO REV. CODE ANN. § 3501.33 (LexisNexis 2014); OKLA. STAT. tit. 26, § 7-108 (2013); OKLA. STAT. tit. 26, § 7-112 (2013); OR. REV. STAT. § 260.695 (2013); 25 PA. CONS. STAT. ANN. § 1207 (2014); R.I. GEN. LAWS § 17-19-21 (2013); S.C. CODE ANN. § 7-13-160 (2012); S.D. CODIFIED LAWS § 12-18-9.2 (2014); TENN. CODE ANN. § 2-7-103 (2014); TEX. ELEC. CODE ANN. § 61.010 (Vernon 2013); UTAH CODE ANN. § 20A-3-501 (2013); VT. STAT. ANN. tit. 17, § 2508 (2013); VA. CODE ANN. § 24.2-606 (2014); WASH. REV. CODE § 29A.84.510 (2013); W. VA. CODE § 3-1-37 (2013); WIS. STAT. § 12.03 (2013); WYO. STAT. ANN. § 22-13-103 (2014); WYO. STAT. ANN. § 22-26-113 (2014).</p>
Densest jurisdiction	<p>U. S. CENSUS BUREAU, 2010 CENSUS SUMMARY FILE 1: POPULATION, HOUSING UNITS, AREA, AND DENSITY: STATE — COUNTY/COUNTY EQUIVALENT (2010), <a href="http://factfinder2.census.gov/faces/nav/jsf/pages/community_facts.xhtml">http://factfinder2.census.gov/faces/nav/jsf/pages/community_facts.xhtml</a> (last visited Apr. 3, 2015) (data for each jurisdiction can be obtained by manually searching for the city or county in question).</p>
Pop. Density	<p>U. S. CENSUS BUREAU, 2010 CENSUS SUMMARY FILE 1: POPULATION, HOUSING UNITS, AREA, AND DENSITY: COUNTY — COUNTY SUBDIVISION AND PLACE (2010), <a href="http://factfinder2.census.gov/faces/nav/jsf/pages/community_facts.xhtml">http://factfinder2.census.gov/faces/nav/jsf/pages/community_facts.xhtml</a> (last visited Apr. 3, 2015) (same).</p>
Demographics (Poverty rate; Median income; % College degree)	<p>U. S. CENSUS BUREAU, 2008–2012 AMERICAN COMMUNITY SURVEY 5-YEAR ESTIMATES tbl.DP03- Selected Economic Characteristics (2012), <a href="http://factfinder2.census.gov/faces/nav/jsf/pages/community_facts.xhtml">http://factfinder2.census.gov/faces/nav/jsf/pages/community_facts.xhtml</a> (last visited Apr. 3, 2015) (same).</p>
Demographics (% Renter; % Non-white)	<p>U. S. CENSUS BUREAU, 2010 CENSUS SUMMARY FILE 1 (2010), <a href="http://factfinder2.census.gov/faces/nav/jsf/pages/community_facts.xhtml">http://factfinder2.census.gov/faces/nav/jsf/pages/community_facts.xhtml</a> (last visited Apr. 3, 2015) (same).</p>

APPENDIX B. SOURCES FOR ABORTION BUFFER ZONE DATA

TABLE 7

Source	Period	Description
Alan Guttmacher Institute (AGI)	1973–2011	Number of abortions by the state in which the abortion occurred
	1978–2008	Rate of abortions by the state in which the abortion occurred per 1,000 women of child-bearing age (15-44 years) Number of abortion providers in the state Number of abortions by the state of residence of the mother Rate of abortions by the state of residence of the mother per 1,000 women of child-bearing age (15-44 years)
Centers for Disease Control (CDC)	1990–2010	Number of abortions by the state in which the abortion occurred
		Rate of abortions by the state in which the abortion occurred per 1,000 women of child-bearing age (15-44 years) Number of abortions by the state of residence of the mother Rate of abortions by the state of residence of the mother per 1,000 women of child-bearing age (15-44 years)
U.S. Department of Commerce Bureau of Economic Analysis	1973–2011	Population, based on U.S. Census Bureau’s midyear population estimates, which is adjusted for births, migration, and deaths, from Population Estimates Program Per Capita Personal Income
U.S. Bureau of Labor Statistics	1976–2013	Unemployment rate based on CPS, Current Employment Statistics program, and regular state unemployment insurance systems from Local Area Unemployment Statistics program
Current Population Survey (CPS)	1977–2013	Proportion of population that is female (CPS: SEX) Proportion of population that are women of child-bearing age (15-44) (CPS: SEX, AGE) Proportion of population that is white (CPS: RACE) Proportion of women that are white (CPS: RACE, SEX) Proportion of population that is African-American (CPS: RACE) Proportion of women that are African-American (CPS: RACE, SEX) Proportion of population that is of Hispanic, Spanish, or Latino origin (CPS: HISPAN) Proportion of women that are of Hispanic, Spanish, or Latina origin (CPS: HISPAN, SEX) Proportion of households in rural area, omitting unidentifiable and missing / unknown respondents (CPS: METRO) Proportion of population living in poverty (IPUMS: OFFPOV, OFFPOVUNIV) Proportion of women living in poverty (IPUMS: OFFPOV, CPS: SEX) Unemployment rate (CPS: EMPSTAT, LABFORCE) Female Unemployment rate (CPS: EMPSTAT, LABFORCE, SEX) Proportion of population attending at least 12 years of school (CPS: EDUC, recoded out of HIGRADE and EDUC99) Proportion of population attending at least 16 years of school (CPS: EDUC, recoded out of HIGRADE and EDUC99) Proportion of population that is married (CPS: MARST) Proportion of population that receives public assistance at least one month out of the year (CPS: GOTWELFR, MTHWELFR) Proportion of population that is covered by Medicaid (CPS: HIMCAID, edited by the Census Bureau)
Handcoded	1973–2013	Binary indicator coded as 1 if Medicaid coverage for family planning is extended and 0 otherwise

National Abortion and Reproductive Rights Action League (NARAL, Kreitzer)	1973–2013	<p>Binary indicator coded as 1 if insurance coverage is mandated for contraception and 0 otherwise</p> <p>Binary indicator coded as 1 if public funding is restricted to abortions necessary to save the life of the mother and/or restricted to only medically necessary abortions and 0 otherwise</p> <p>Binary indicator coded as 1 if informed consent is required when obtaining an abortion, meaning the doctor must provide information on alternatives to abortion and 0 otherwise</p> <p>Binary indicator coded as 1 if waiting period is required before the abortion procedure and 0 otherwise</p> <p>Binary indicator coded as 1 if parental consent or notification is required for abortion if under 18 years old and 0 otherwise</p> <p>Binary indicator coded as 1 if abortion is banned sometime prior to 21 weeks of pregnancy (varies by state) and 0 otherwise</p> <p>Binary indicator coded as 1 if abortion is banned sometime between 21 weeks and prior to viability (varies by state) and 0 otherwise</p> <p>Binary indicator coded as 1 if abortion is banned post viability and 0 otherwise</p> <p>Binary indicator coded as 1 if a mandatory ultrasound is required before abortion and 0 otherwise</p> <p>Binary indicator coded as 1 if ban of IDE abortion procedure (used for partial-birth abortions) and 0 otherwise</p> <p>Binary indicator coded as 1 if ban of state-funded personnel to provide counseling and/or giving referrals to women for abortion services and 0 otherwise</p> <p>Binary indicator coded as 1 if ban of abortions performed at public facilities and 0 otherwise</p> <p>Binary indicator coded as 1 if private insurance companies are restricted from covering abortion services and 0 otherwise</p> <p>Binary indicator coded as 1 if public insurance companies are restricted from covering abortions and 0 otherwise</p> <p>Binary indicator coded as 1 if restriction on medication abortions (mifepristone) exists and 0 otherwise</p> <p>Binary indicator coded as 1 if an insurance waiver is required for abortion and 0 otherwise</p> <p>Binary indicator coded as 1 if health-care providers are allowed to refuse to provide medically necessary services and 0 otherwise</p> <p>Binary indicator coded as 1 if a mandatory viability test is required before abortion and 0 otherwise</p> <p>Binary indicator coded as 1 if requirement that doctors get additional licenses to perform abortions and 0 otherwise</p> <p>Binary indicator coded as 1 if abortions must be performed at hospitals and 0 otherwise</p> <p>Binary indicator coded as 1 if a fetal disposal method exists and 0 otherwise</p> <p>Binary indicator coded as 1 if ban on sex-selective abortion and 0 otherwise</p> <p>Binary indicator coded as 1 if pro-life license plates are legal and 0 otherwise</p>
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*Notes:* Description of variables in state-year dataset on abortion clinic buffer zones. AGI abortion data was linearly interpolated for missing years (1983, 1986, 1989–90, 1993–94, 1997–98, 2001–03, 2006, and 2009). CPS microdata were collected via the Integrated Public Use Microdata Series (IPUMS) from the Minnesota Population Center at the University of Minnesota. While CPS coverage starts in 1973, unique state identifiers do not exist until 1977. There were thirteen states and ten state groups. CPS weights were used to calculate population-level state/year data. We used NARAL and AGI reports to identify states with expanded Medicaid coverage for family planning via 1115 Waivers and consulted Lexis and state Medicaid websites to identify enactment dates. We used NARAL reports to determine the enactment dates of mandated contraceptive insurance coverage and verified effective dates using Westlaw. Rebecca Kreitzer provided data on state enactments of laws regulating abortion and abortion clinics, based on NARAL’s annual “Who Decides” reports.