**No. 124100**

IN THE

# Supreme Court of Illinois

THE PEOPLE OF THE STATE OF ILLINOIS,
*Plaintiff-Appellant*,

*v.*

VIVIAN CLAUDINE BROWN,
*Defendant-Appellee.*

On Appeal from the
Circuit Court of the Second Judicial Circuit,
White County, Illinois, No. 2017CM60.
The Honorable Mark R. Stanley, Judge Presiding.

**BRIEF OF AMICI CURIAE STATE'S ATTORNEYS STEWART J. UMHOLTZ AND BRANDON J. ZANOTTI, PROFESSORS OF SECOND AMENDMENT LAW, FIREARMS POLICY COALITION, FIREARMS POLICY FOUNDATION, CITIZENS COMMITTEE FOR THE RIGHT TO KEEP AND BEAR ARMS, MILLENNIAL POLICY CENTER, INDEPENDENCE INSTITUTE, AND CARLISLE MOODY IN SUPPORT OF VIVIAN CLAUDINE BROWN AND AFFIRMANCE**

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## POINTS AND AUTHORITIES

## INTEREST OF THE AMICI CURIAE

**Amici State's Attorneys** Stewart J. Umholtz (State's Attorney, Tazewell County) and Brandon J. Zanotti (State's Attorney, Williamson County) are concerned about the overcriminalization of peaceable citizens both by the current FOID law, for citizens similarly situated to Ms. Brown, and by the State's proposal to expand the traditional understanding of the FOID law to include a broad category of constructive possession.

**Amici professors** are law professors who teach and write on the Second Amendment: Robert Cottrol (George Washington), Nicholas Johnson (Fordham), Nelson Lund (George Mason), Joseph Olson (Mitchell Hamline), Glenn Reynolds (Tennessee), and Gregory Wallace (Campbell). As described in Appendix A, the above professors were cited extensively by the Supreme Court in *District of Columbia v. Heller* and *McDonald v. City of Chicago*. Oft-cited by lower courts as well, these professors include authors of the first law school textbook on the Second Amendment, as well as many other books and law review articles on the subject.

**Firearms Policy Coalition** is a non-profit membership organization that serves its members and the public through programs including direct and grassroots advocacy, legal action, outreach, and education. Its purposes include defending the United States Constitution and the People's rights, privileges, and immunities deeply rooted in the Nation's history and tradition, including the fundamental right to keep and bear arms.

**Firearms Policy Foundation** is a nonprofit membership organization that serves its members and the public through charitable programs including research, education, and legal efforts, with a focus on the United States Constitution and the People's rights, privileges, and immunities deeply rooted in the Nation's history and tradition, with a focus on the fundamental right to keep and bear arms.

**Citizens Committee for the Right to Keep and Bear Arms** is a non-profit organization dedicated to protecting firearms rights by educating grassroot activists, the public, legislatures, and the media.

**Millennial Policy Center** is a research and educational center that develops and promotes policy solutions to advance freedom and opportunity for the Millennial Generation.

**Independence Institute** is a non-partisan public policy research organization. The Institute's amicus briefs in *Heller* and *McDonald* (under the name of lead amicus Int'l Law Enforcement Educators & Trainers Association ("ILEETA")) were cited in the opinions of Justices Breyer (*Heller*), Alito (*McDonald*), and Stevens (*McDonald*).

**Carlisle Moody** is Professor of Economics at the College of William and Mary, in Williamsburg, Virginia. Professor Moody's research was cited by the Supreme Court's opinion in *McDonald v. Chicago*, 561 U.S. 742, 751 n.2 (2010) ("providing comparisons of Chicago's rates of assault, murder, and robbery to average crime rates in 24 other large cities").

## SUMMARY OF ARGUMENT

Vivian Brown has been criminalized for exercising her core Second Amendment right. She is a lifelong peaceable citizen with no criminal history. She possessed a most modest firearm, a single-shot .22 bolt-action rifle, for the undisputed purpose of self-defense in her home. The law making it criminal for Ms. Brown to engage in activity at the Second Amendment's core is unconstitutional as applied to her.

This Court applies a Two-Part Test to Second Amendment challenges. Part One requires a historical analysis to determine whether the burdened activity falls within the Second Amendment's protection at the time of ratification. Part Two requires the application of heightened scrutiny, in which the State bears the burden of justifying the law.

From the earliest colonial days through the Second Amendment's ratification, in-home firearm possession was mandated by colonies and states in hundreds of acts. While militia participation was typically for able-bodied males between 16 and 60, several colonies had broader possession mandates. These mandates often applied regardless of sex; they covered heads of households, recipients of land grants, persons living self-sufficiently, or taxable persons. No colony or state restricted gun possession in the home by free citizens. The restriction on Ms. Brown's right to possess a simple rifle in her home thus falls within the Second Amendment's historical protection.

Since the restriction severely burdens the ability of a law-abiding citizen to exercise her core right of self-defense in the home—where the right is most acute—and because no other state imposes a more severe burden on that right, strict scrutiny is appropriate.

For persons like Ms. Brown, the law fails both strict and intermediate scrutiny. The State bears the burden of proving the law constitutional under any level of heightened scrutiny, and to carry that burden it must, at a minimum, provide actual evidence. Here, the State did not offer data, statistics, or empirical evidence to justify the constitutionality of the law. If the State cannot proffer sufficient evidence justifying the law, it must be stricken.

Social science data shows that laws like the FOID system have no statistically significant effect on homicide or suicide. The finding is confirmed by comparisons of pairs of states, and by national data covering all states that enacted or repealed laws similar to Illinois's FOID.

## ARGUMENT

### I. This Court applies a Two-Part Test to Second Amendment challenges: first, determine whether the restriction burdens the founding-era scope of the right; if so, then apply heightened scrutiny.

This Court employs a Two-Part Test for Second Amendment challenges. *Wilson v. Cty. of Cook*, 2012 IL 112026, ¶ 41.[1] "First, we conduct a textual and historical analysis of the second amendment 'to determine whether the challenged law imposes a burden on conduct that was understood to be within the scope of the second amendment's protection at the time of ratification.'" *People v. Chairez*, 2018 IL 121417, ¶ 21 (quoting *Wilson*, 2012 IL at ¶ 41). "[I]f the historical evidence is inconclusive or suggests that the regulated activity is not categorically unprotected, then we apply the appropriate level of heightened means-ends scrutiny . . ." *Id.* (citations omitted).

### II. Part One of the Two-Part Test: The State's restriction on firearm possession in the home burdens the founding-era scope of the right.

The Supreme Court in *District of Columbia v. Heller* announced its "adoption of the original understanding of the Second Amendment." 554 U.S.

[1] Some amici believe that the proper test for a Second Amendment analysis is based on the text, informed by history and tradition. *See*, e.g., *Heller v. District of Columbia*, 670 F.3d 1244, 1271–1285 (D.C. Cir. 2011) ("*Heller II*") (Kavanaugh, J., dissenting). This brief does not address the question of whether such a test should replace the Two-Part Test. The result is the same regardless, because the FOID system fails either test for persons such as Ms. Brown. *See* Part II.

570, 625 (2008). "*Heller* focused almost exclusively on the original public meaning of the Second Amendment, consulting the text and relevant historical materials to determine how the Amendment was understood at the time of ratification." *Ezell v. City of Chicago*, 651 F.3d 684, 700–01 (7th Cir. 2011) ("*Ezell I*"). Thus, in this Court's analysis, "The threshold question . . . involves a textual and historical inquiry to determine whether the conduct was understood to be within the scope of the right at the time of ratification." *Wilson*, 2012 IL at ¶ 41. *See also Ezell I*, 651 F.3d at 701 ("Is the restricted activity protected by the Second Amendment in the first place? The answer requires a textual and historical inquiry into original meaning.") (citation omitted).

A historical inquiry shows that long gun possession in the home is at the utmost core of the Second Amendment. Indeed, keeping guns in the home epitomized the exercise of the right throughout early American history.

### A. Historically, firearm possession in the home was required, not prohibited.

Every state that ratified the Second Amendment *mandated* long gun possession in the home from colonial days through ratification—the sole exception being Pennsylvania, whose pacifist Quaker population resisted such a mandate until the French and Indian War. No colony or state restricted long gun possession in the home. *See generally*, David B. Kopel & Joseph G.S.

Greenlee, *The Second Amendment Rights of Young Adults*, 43 S. ILL. U. L.J. (2019, forthcoming)[2] (describing the ages and arms requirements of all militiamen, and other arms provisions, throughout the seventeenth and eighteenth centuries). Additionally, the federal Uniform Militia Act of 1792 required that every militiaman "provide himself with a good musket or firelock . . . or with a good rifle." 1 Stat. 271 (1792).

Beyond the hundreds of colonial and founding-era laws requiring militia-aged men to keep guns at home, some laws applied more generally.

**1. Maryland**

In 1638, Maryland enacted a law requiring every head of a house, regardless of sex or age, to keep a long gun in "his her or their house." 1 PROCEEDINGS AND ACTS OF THE GENERAL ASSEMBLY OF MARYLAND JANUARY 1637/8—SEPTEMBER 1664, at 77 (William Hand Browne ed, 1883).

**2. North Carolina**

To encourage settlement in 1664, North Carolina issued land grants to every freeman and every freewoman with a servant, but only on the condition that "each of them [were] armed with a good firelock or matchlock bore, twelve

[2] https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3205664.

bullets to the pound, ten pounds of powder, and twenty pounds of bullets."[3] 1 AMERICA'S FOUNDING CHARTERS: PRIMARY DOCUMENTS OF COLONIAL AND REVOLUTIONARY ERA GOVERNANCE 210 (Jon L. Wakelyn ed. 2006). Additional land was provided for each person over 14 who kept the same arms. *Id.* at 210–11.

### 3. Delaware

In 1741 Delaware, every man or woman who was living self-sufficiently (i.e., "every Freeholder and taxable Person") had to own "[o]ne well fixed Musket or Firelock, one Cartouch-Box, with twelve Charges of Gun-Powder and Ball therein, and Three good Flints," and was "obliged to keep such Arms and Ammunition by him, during the Continuance of this Act." GEORGE H. RYDEN, DELAWARE—THE FIRST STATE IN THE UNION 117 (1938).

### 4. Vermont

A 1779 Vermont statute required that every householder "always be provided with, and have in constant readiness, a well fixed firelock, the barrel not less than three feet and a half long, or other good fire-arms." VERMONT STATE PAPERS, BEING A COLLECTION OF RECORDS AND DOCUMENTS, CONNECTED

[3] A "firelock" is the same as a "flintlock." The gunpowder is ignited by the spark of flint striking steel. A "matchlock" is ignited when the trigger lowers a slow-burning cord of hemp. Matchlocks were common in the early colonial days and were replaced over time by flintlocks ("firelocks"). A matchlock or firelock could be either a long gun or a handgun.

WITH THE ASSUMPTION AND ESTABLISHMENT OF GOVERNMENT BY THE PEOPLE OF VERMONT 307 (1823).

### 5. Virginia

Starting in 1639, all Virginians were "to be provided with arms and ammunition or be fined." 1 William Waller Hening, THE STATUTES AT LARGE: BEING A COLLECTION OF ALL THE LAWS OF VIRGINIA, FROM THE FIRST SESSION OF THE LEGISLATURE, IN THE YEAR 1619, at 226 (1809).

As of 1659, everyone able to bear arms had to "have in his house a fixt gunn two pounds of powder and eight pound of shott at least." *Id.* at 525. In the colonial and founding eras, "gun" meant long gun. Handguns were called "pistols." *See* Gun, AMERICAN DICTIONARY OF THE ENGLISH LANGUAGE (Noah Webster, 1828) ("one species of fire-arms, the pistol, is never called a *gun*").[4]

Like North Carolina decades prior, Virginia issued land grants in 1701, but only to grantees that could keep on the property a man between 16 and 60 who was "continually provided with a well fixt musquett or fuzee." 3 William Waller Hening, THE STATUTES AT LARGE: BEING A COLLECTION OF ALL THE LAWS OF

[4] http://webstersdictionary1828.com/Dictionary/gun.

VIRGINIA, FROM THE FIRST SESSION OF THE LEGISLATURE, IN THE YEAR 1619, at 206–07 (1823).[5]

In 1720, Virginia appropriated one thousand pounds to distribute "to each christian titheable, one firelock, musket." 4 William Waller Hening, THE STATUTES AT LARGE: BEING A COLLECTION OF ALL THE LAWS OF VIRGINIA, FROM THE FIRST SESSION OF THE LEGISLATURE, IN THE YEAR 1619, at 77–78 (1823). In Virginia, everyone over 16 except for free white women was titheable (that is, taxable under a head or capitation tax). *See* Terri L. Snyder, *Marriage on the Margins: Free Wives, Enslaved Husbands, and the Law in Early Virginia*, 30 L. & HIST. REV. 141, 166 (2012). Thus, to ensure widespread firearm ownership in the home, Virginia provided guns to colonists.

**B. There is no historical tradition of restricting guns in the home.**

In contrast to the hundreds of militia laws requiring in-home firearm ownership, and the laws requiring or incenting gun ownership among non-militiamen, few historical laws required a license to keep a firearm in the home. Those that did were grossly discriminatory.

[5] A fuzee, like a musket, was a long gun: "a light, smoothbore shoulder arm of smaller size and caliber than the regular infantry weapon." George C. Neumann, BATTLE WEAPONS OF THE AMERICAN REVOLUTION 19 (2011).

A 1723 Virginia law required “all negros, mullattos, or indians” living on plantations to acquire a license “to keep and use guns.” 4 Hening, THE STATUTES AT LARGE at 131.

To keep a firearm in the Mississippi territory in 1799, free African American householders had to apply for a 12-month license from “the commanding officers of legions.” Slaves were also eligible for licenses, “on application of their owners, shewing sufficient cause . . . why such indulgence should be granted.” 1799 Laws of the Miss. Terr. 118. Starting in 1822, justices of the peace became the licensing authority for slaves, and county courts became the licensing authority for free African Americans. 1822 Miss. Laws 179, 181–83, §§ 10, 12.

An 1806 Virginia law provided that “no free negro or mulatto shall be suffered to keep or carry any fire-lock of any kind, any military weapon, or any powder or lead, without first obtaining a license.” 1806 Va. Acts 51, ch. 94.

In 1832, Delaware made it unlawful for “free negros and free mulattoes to have, own, keep or possess any gun, pistol, sword or any warlike instrument,” except that they could own a “gun or fowling piece” “upon application . . . to one of the justices of the peace,” if the application was certified by “five or more respectable and judicious citizens” and showed “that the circumstances of his case justify his keeping and using a gun.” 8 LAWS OF THE STATE OF DELAWARE

208 (1841), ch. 176, § 1. The police power was said to justify restrictions like "the prohibition of free negroes to own or have in possession fire arms or warlike instruments." *State v. Allmond*, 7 Del. 612, 641 (Gen. Sess. 1856).

North Carolina, in 1841, started requiring free persons of color to obtain a license from the Court of Pleas and Quarter Sessions to own or carry a gun. 1840–41 N.C. Laws 61–62, ch. 30. The North Carolina Supreme Court upheld the carry license in *State v. Newsom*, explaining that "free people of color have been among us, as a separate and distinct class, requiring, from necessity, in many cases, separate and distinct legislation." 27 N.C. 250, 252 (1844). Thus, it was left to "the control of the County Court, giving them the power to say, in the exercise of a sound discretion, who, of this class of persons, shall have a right to the licence, or whether any shall." *Id.* at 253.

Maryland passed a law in 1860 providing that, "No free negro shall be suffered to keep or carry a firelock of any kind, any military weapon, or any powder or lead, without first obtaining a license from the court of the county or corporation in which he resides . . . " Art. 66, § 73, 1 Maryland Code 464 (1860).

After the Civil War, former Confederate states enacted Black Codes to keep African Americans in a condition of de facto servitude. Florida's first legislative session after the Confederate surrender prohibited "any negro, mulatto, or other person of color" from owning a gun without first obtaining a license from a probate judge based on "the recommendation of two respectable citizens." 1865 Laws of Fla. 25, 27, ch. 1,466, no. 3, § 12.

Mississippi in 1865 prohibited any "freedman, free negro or mulatto" from keeping "fire-arms of any kind" unless "licensed so to do by the board of police." 1865 Miss. Laws 165, ch. 23, § 1.

In response to such laws, the federal government passed the Second Freedmen's Bureau Act, which ensured to all persons the "full and equal benefit of all laws and proceedings for the security of person and estate including the constitutional right of bearing arms." 14 Stat. 173, 176–77 (1866). "The Civil Rights Act of 1866, 14 Stat. 27, which was considered at the same time as the Freedmen's Bureau Act, similarly sought to protect the right of all citizens to keep and bear arms." *McDonald v. City of Chicago, Ill.*, 561 U.S. 742, 774 (2010). The Civil Rights Act of 1871, 17 Stat. 13, and most importantly, the Fourteenth Amendment, served the same purpose. *McDonald*, 561 U.S. at 776–78. *See*, *e.g.*, Clayton E. Cramer, Nicholas J. Johnson & George A. Mocsary, *"This Right Is Not Allowed by Governments That Are Afraid of the People": The Public Meaning of the Second Amendment When the Fourteenth Amendment Was Ratified*, 17 GEO. MASON L. REV. 823 (2010) (cited in *McDonald*, 561 U.S. at 773 n.21, 776 n.25, 780).

Undeterred, in 1893, Florida passed a new discriminatory licensing law. The law made it "unlawful to carry or own a Winchester or other repeating rifle or without first taking out a license from the County Commissioners . . ." 1893 Fla. Laws 71, ch. 4147, § 1. Amended in 1901, the law required a license for someone "to have a pistol, Winchester rifle or other repeating rifle in his manual possession." 1901 Fla. Laws 1901, ch. 4928, § 1. Although neutrally worded—as it had to be after the enactment of the Fourteenth Amendment—the statute served a discriminatory purpose. As Florida Supreme Court Justice Rivers H. Buford pointed out, "the Act was passed for the purpose of disarming the negro laborers" and "was never intended to be applied to the white population and in practice has never been so applied." *Watson v. Stone*, 148 Fla. 516, 524 (1941) (Buford, J., concurring specially). Justice Burford added that "there had never been, within my knowledge, any effort to enforce the provisions of this statute as to white people, because it has been generally conceded to be in contravention of the Constitution and non-enforceable if contested." *Id.*

The discriminatory historical basis for licensing laws cannot justify present-day licensing laws like Illinois's.

Chicago enacted a licensing law in 1911, but it did not apply to the simple possession of arms, and it did not apply to long guns. Rather, a permit was required "to purchase any pistol, revolver, derringer, bowie knife, dirk or other weapon of like character which can be concealed on the person." 1911 Chi. Code ch. 53.[6]

Throughout American history, as was the case in Chicago, long guns have been subjected to fewer restrictions than handguns, and virtually no restrictions in the home.[7] The D.C. Circuit so recognized in *Heller II*, 670 F.3d 1244. The court considered several regulations related to firearms registration that applied to both handguns and long guns. Specifically, for a registration certificate, an applicant had to appear in person, re-register each firearm every three years, demonstrate knowledge of firearms, be fingerprinted and photographed, complete a firearms training course, and submit to a background check every six years. *Id.* at 1255. While the court held "the basic requirement to register a handgun is longstanding in American law," *id.* at

[6] This Court upheld the licensing law in *Biffer v. City of Chicago*, 278 Ill. 562, 570 (1917), based on the reasoning "that the sale of deadly weapons may be absolutely prohibited under the police power of the state" without violating the Second Amendment. Such reasoning is in conflict with *McDonald*, 561 U.S. 742, but in any event, the sale of firearms is not at issue here.

[7] The exception is machine guns, which are strictly regulated. But the United States Supreme Court recognizes the difference between ordinary arms—like Ms. Brown's .22 bolt-action rifle—that fire "only one shot with each pull of the trigger," and "traditionally have been widely accepted as lawful possessions," versus machine guns, which have the "quasi-suspect character we attributed to owning hand grenades." *Staples v. United States*, 511 U.S. 600, 603 n.1, 611–12 (1994).

1254, the other laws were "novel, not historic," including "*all* the requirements as applied to long guns." *Id.* at 1255 (emphasis in original).

## III. Part Two of the Two-Part Test: Strict scrutiny should apply, but because the State did not provide any evidence, the law fails any form of heightened scrutiny.

### A. Strict scrutiny is appropriate because the law severely burdens the core right of a law-abiding citizen.

In determining the appropriate standard of scrutiny for Second Amendment challenges, this Court has "elect[ed] to continue to follow the Seventh Circuit Court of Appeals." *Chairez*, 2018 IL at ¶ 34 n.3. *See also People v. Mosley*, 2015 IL 115872, ¶ 34; *People v. Aguilar*, 2013 IL 112116, ¶ 20. "The rigor of this means-end analysis 'depends on how close the law comes to the core of the Second Amendment right and the severity of the law's burden on the right.'" *Chairez*, 2018 IL at ¶ 45 (quoting *Ezell v. City of Chicago*, 846 F.3d 888, 892 (7th Cir. 2017) ("*Ezell II*")). When a facet of the Second Amendment right outside its core is implicated, Illinois courts must determine "how rigorously to apply intermediate scrutiny to second amendment cases." *Chairez*, 2018 IL at ¶ 35 (determining the validity of a ban on public firearm carriage within 1,000 feet of a park). When the core of the Second Amendment right is threatened, strict scrutiny is appropriate, as shown next.

### 1. ***Ezell I***

The Seventh Circuit articulated its Second Amendment framework in *Ezell I*, which involved a challenge to Chicago's ban on firing ranges within city limits:

> Both *Heller* and *McDonald* suggest that broadly prohibitory laws restricting the core Second Amendment right—like the handgun bans at issue in those cases, which prohibited handgun possession even in the home—are categorically unconstitutional. *Heller,* 554 U.S. at 628–35, 128 S.Ct. 2783 ("We know of no other enumerated constitutional right whose core protection has been subjected to a freestanding 'interest-balancing' approach."); *McDonald,* 130 S.Ct. at 3047–48. For all other cases, however, we are left to choose an appropriate standard of review from among the heightened standards of scrutiny the Court applies to governmental actions alleged to infringe enumerated constitutional rights; the answer to the Second Amendment "infringement" question depends on the government's ability to satisfy whatever standard of means-end scrutiny is held to apply.

*Ezell I*, 651 F.3d at 703. The court added, "we know that *Heller*'s reference to 'any standard of scrutiny' means any *heightened* standard of scrutiny." *Id.* at 701 (emphasis in original). The court identified these heightened standards and explained how they apply to enumerated constitutional rights.

Laws at the core of the First Amendment right, "[f]or example, content-based regulations" on speech "get strict scrutiny." *Id.* at 707 (quoting *R.A.V. v.*

*City of St. Paul*, 505 U.S. 377, 382 (1992)) (brackets and quotation marks omitted). So do "regulations in a traditional public or designated public forum," "[l]aws that burden political speech," and "election-law cases" involving "laws imposing severe burdens." *Id.* (quoting *Citizens United v. Fed. Election Comm'n*, 558 U.S. 310, 340 (2010)).

In contrast, "in commercial-speech cases, the Court applies an intermediate standard of review that accounts for the 'subordinate position' that commercial speech occupies 'in the scale of First Amendment values.'" *Id.* at 708 (quoting *Bd. of Trs. of State Univ. of N.Y. v. Fox*, 492 U.S. 469, 477 (1989)).

The Seventh Circuit "distill[ed] this First Amendment doctrine and extrapolate[d] a few general principles to the Second Amendment context." *Id.* Specifically, the court adopted the First Amendment approach to determining the appropriate level of heightened scrutiny: "Borrowing from the Court's First Amendment doctrine, the rigor of [the Seventh Circuit's] judicial review [] depend[s] on how close the law comes to the core of the Second Amendment right and the severity of the law's burden on the right." *Id.* at 703.

"[A] severe burden on the core Second Amendment right of armed self-defense will require an extremely strong public-interest justification and a close fit between the government's means and its end." *Id.* at 708. And just as "more modest regulatory measures need only be reasonable" and may be "justified by an important governmental interest" under the First Amendment, "laws restricting activity lying closer to the margins of the Second Amendment right, laws that merely regulate rather than restrict, and modest burdens on the right may be more easily justified." *Id.* at 708. For these, a more lenient "exacting" or "intermediate standard of scrutiny" is appropriate. *Id.* at 707–08.

Thus, under both the First and Second Amendments, proximity to the core of the right determines the strength of scrutiny to be applied. Courts in the Seventh Circuit are "left to choose an appropriate standard of review from among the heightened standards of scrutiny." *Id.* at 703. "Severe burdens" get strict scrutiny; "modest" burdens get intermediate scrutiny. *Id.* at 707–08. The burden is on the government to "satisfy whatever standard of means-end scrutiny is held to apply." *Id.* at 703.

Acknowledging that the Second Amendment's core includes "the right to possess operable firearms . . . for self-defense . . . in the home," *Ezell I* applied this framework to a Chicago law banning firing ranges within city limits. *Id.* at 689, 690. The Seventh Circuit observed that "the core right wouldn't mean much without the training and practice that make it effective." *Id.* at 704. Maintaining proficiency in firearm use was therefore "an important corollary to the meaningful exercise of the core right to possess firearms for self-defense." *Id.* at 708. Because Chicago's range ban was tangent to, but not within, the core of the Second Amendment, and because the *Ezell I* plaintiffs were "law-abiding, responsible citizens," the Seventh Circuit applied "not quite 'strict scrutiny'" and enjoined the law. *Id.*

Although the Seventh Circuit has not applied heightened scrutiny to a law that it clearly said was within the Second Amendment's core, it would presumably subject such a law to more robust review—strict scrutiny—than *Ezell I* applied to a restriction on a "corollary" to the core right.[8] For support of the Two-Part Test, the *Ezell I* court cited sister Circuits that had already adopted the test. *Id.* at 703–04. These courts left strict scrutiny available for Second Amendment challenges. *See United States v. Chester*, 628 F.3d 673, 682 (4th Cir. 2010) ("Our task, therefore, is to select between strict scrutiny and intermediate scrutiny."); *United States v. Marzzarella*, 614 F.3d 85, 101 (3d Cir. 2010) ("because [18 U.S.C] § 922(k) would pass muster under either intermediate scrutiny or strict scrutiny, Marzzarella's conviction must stand"); *United States v. Reese*, 627 F.3d 792, 804 n.4 (10th Cir. 2010) ("Even if we were to apply a strict scrutiny test . . . the government could satisfy these requirements.") (citation omitted).

[8] The total ban on public firearm carriage at issue in *Moore v. Madigan* was so extreme that the Seventh Circuit held, without need for resort to heightened scrutiny, that it was categorically unconstitutional. *Moore v. Madigan*, 702 F.3d 933 (7th Cir. 2012). In its analysis, the court noted that some self-defense needs in public can be more pressing than others in the home. *Id.* at 937.

### 2. *Ezell II*

The Seventh Circuit applied *Ezell I*'s heightened scrutiny approach in *Ezell II*. It "note[d] for good measure that most other circuits have adopted the framework." *Id.* at 893.

The court cited additional cases from the Fifth, Sixth, Ninth, and D.C. Circuits applying *Ezell*'s framework. Like the Third, Fourth, and Tenth Circuits cited in *Ezell I*, these courts allow for strict scrutiny in Second Amendment cases. *See Nat'l Rifle Ass'n of Am., Inc. v. Bureau of Alcohol, Tobacco, Firearms, & Explosives*, 700 F.3d 185, 195 (5th Cir. 2012) ("A regulation that threatens a right at the core of the Second Amendment . . . triggers strict scrutiny."); *Jackson v. City & Cty. of San Francisco*, 746 F.3d 953, 965 (9th Cir. 2014) ("[B]ecause it does not impose a substantial burden on conduct protected by the Second Amendment, we apply intermediate scrutiny."); *Heller II*, 670 F.3d at 1257 ("As between strict and intermediate scrutiny, we conclude the latter is the more appropriate standard for review of gun registration laws."); *Tyler v. Hillsdale Cty. Sheriff's Dep't*, 775 F.3d 308, 328 (6th Cir. 2014), *reh'g en banc granted, opinion vacated* (Apr. 21, 2015), *on reh'g en banc*, 837 F.3d 678 (6th Cir. 2016) ("we prefer strict scrutiny over intermediate scrutiny"). Although the *Tyler* panel opinion was later vacated, on rehearing en banc the Sixth Circuit again recognized strict scrutiny as an option. *Tyler v. Hillsdale Cty. Sheriff's Dep't*, 837 F.3d 678, 690 (6th Cir. 2016) (en banc) ("[T]he choice is between intermediate and strict scrutiny.").

Some of these cases recognized that the Seventh Circuit allowed for the application of strict scrutiny. The Fifth Circuit cited *Ezell I* as supporting the contention that "the second step [of the Second Amendment inquiry] is to determine whether to apply intermediate or strict scrutiny to the law." *Nat'l Rifle Ass'n of Am., Inc.*, 700 F.3d at 194. And the Ninth Circuit explained that in another Seventh Circuit case, "the government was obliged to meet a higher level of scrutiny than intermediate scrutiny to justify a 'blanket prohibition' on carrying an operable gun in public." *Jackson*, 746 F.3d at 964–65 (quoting *Moore v. Madigan*, 702 F.3d 933, 940 (7th Cir. 2012)).

When reviewing regulations that directly impact the core of the Second Amendment right, the Seventh Circuit provides for the application of strict scrutiny. Although it has not had occasion to do so thus far, the instant case would appropriately trigger such searching review.

### B. Restrictions on keeping arms for self-defense in the home directly impacts the core of the Second Amendment right.

Ms. Brown was exercising her core Second Amendment right to keep a firearm for self-defense in her home.

### 1. The burden is severe because it applies in the home where the core right of self-defense is most acute.

The Supreme Court held that self-defense is the Second Amendment's "core lawful purpose." *Heller*, 554 U.S. at 630. And "the home [is] where the need for defense of self, family, and property is most acute." *Id.* at 628. Thus, the Second Amendment "elevates above all other interests the right of law-abiding, responsible citizens to use arms in defense of hearth and home." *Id.* at 635. *See also McDonald*, 561 U.S. at 780 ("the Second Amendment protects a personal right to keep and bear arms for lawful purposes, most notably for self-defense within the home."); *Ezell I*, 651 F.3d at 689 ("*Heller* held that . . . the core component of [the Second Amendment] is the right to possess operable firearms . . . for self-defense, most notably in the home."). By requiring a FOID to exercise "the core lawful purpose of self-defense" in her home, where the right is "most acute," the regulation burdens Ms. Brown's core Second Amendment right. *Heller*, 554 U.S. at 628, 630.

### 2. No other state imposes a more severe burden on long gun possession in the home.

In *Moore*, the Seventh Circuit carefully examined the challenged law's severity compared to corresponding laws in other jurisdictions. *Moore* repeatedly focused on the Illinois carry restrictions being the most severe in the nation. *See* 702 F.3d at 940 ("Illinois is the *only* state that maintains a flat ban on carrying ready-to-use guns outside the home") (emphasis in original); *id.* ("There is no suggestion that some unique characteristic of criminal activity in Illinois justifies the state's taking a different approach from the other 49 states."); *id.* at 941 ("our analysis is not based on degrees of scrutiny, but on Illinois's failure to justify the most restrictive gun law of any of the 50 states."); *id.* at 942 ("Illinois had to provide us with more than merely a rational basis for believing that its uniquely sweeping ban is justified by an increase in public safety.").

Illinois's requirement of a FOID for the home possession of a protected arm is similarly among the most restrictive in the nation. Massachusetts is the only other state that requires a license to own a long gun in the home. Mass. Gen. Laws ch. 140, § 129B. The District of Columbia requires that long guns be registered, D.C. Code § 7-2502.01; Connecticut requires a permit to *acquire* a long gun, Conn. Gen. Stat. Ann. § 29-37a(c); and Hawaii requires both a permit to acquire a long gun and that the gun be registered. Haw. Rev. Stat. §§ 134-2(a), 134-3(b).[9]

[9] While New York City requires a license for home long gun possession, New York City, N.Y., Code §§ 10-131, 10-303 et seq., the State of New York requires a license only for handgun possession. N.Y. Penal Law § 400.00. Some other states similarly require a permit for handguns, but not long guns. For instance, Maryland, North Carolina, and Rhode Island require permits to purchase handguns, but not long guns. Md. Code Ann., Pub. Safety § 5-117.1; N.C. Gen. Stat. §§ 14-402–14-404; R.I. Gen. Laws § 11-47-35.

New Jersey has a general requirement for licenses for long guns and handguns, but the requirement does not apply to arms in the home. *See* N.J. Stat. § 2C:39-5b(1) ("Any person who knowingly has in his possession any handgun, including any antique handgun, without first having obtained a permit to carry the same as provided in N.J.S.2C:58-4, is guilty of a crime of the second degree."); § 2C:39-5c(1) ("Any person who knowingly has in his possession any rifle or shotgun without having first obtained a firearms purchaser identification card in accordance with the provisions of N.J.S.2C:58-3, is guilty of a crime of the third degree."). The home is exempt from these licensing rules: "Nothing in subsections b., c., and d. of N.J.S.2C:39-5 shall be construed to prevent a person keeping or carrying about his place of business, residence, premises or other land owned or possessed by him, any firearm . . ." N.J. Stat. § 2C:39-6e.

New Jersey's home exemption reflects a longstanding American legal tradition respecting the sanctity of the home. *See*, *e.g.*, *Payton v. New York*, 445 U.S. 573, 601 (1980) ("the overriding respect for the sanctity of the home that has been embedded in our traditions since the origins of the Republic").

A license to own a long gun is more restrictive than a license to acquire one. Whether Illinois imposes a more severe burden than Massachusetts, Hawaii, and the District of Columbia is debatable, but at a minimum, Illinois imposes a more severe requirement than 47 other states.

### 3. The burden is severe because it applies to a law-abiding citizen.

This Court and the Seventh Circuit have found that the most burdensome restrictions are those that apply to law-abiding adults. Restrictions that apply only to criminals, by comparison, are less severe and warrant only intermediate scrutiny.

For example, "[i]ntermediate scrutiny was appropriate in *Skoien* because the claim was not made by a 'law-abiding, responsible citizen.'" *Ezell I*, 651 F.3d at 708 (citing *United States v. Skoien*, 614 F.3d 638 (7th Cir. 2010) (en banc)). *See also United States v. Williams,* 616 F.3d 685, 692 (7th Cir. 2010) (applying intermediate scrutiny to a ban on firearms possession by convicted felons); *United States v. Meza-Rodriguez*, 798 F.3d 664, 672–73 (7th Cir. 2015) (applying intermediate scrutiny to a ban on unauthorized aliens because they were not law-abiding); *United States v. Yancey,* 621 F.3d 681, 683 (7th Cir. 2010) (applying intermediate scrutiny to a ban on firearms possession by unlawful users of controlled substances because they were not law-abiding).

"Here, in contrast, [Ms. Brown is among] the 'law-abiding, responsible citizens' whose Second Amendment rights are entitled to full solicitude under *Heller*." *Ezell I*, 651 F.3d at 708. Burdens on law-abiding citizens are substantially more severe. For example, this Court deemed a restriction around public parks "a severe burden on the recognized second amendment right of self-defense," because it "affects the gun rights of the entire law-abiding population of Illinois." *Chairez*, 2018 IL at ¶ 49.

Similarly, the Seventh Circuit "held that banishing firing ranges from the city was a severe encroachment on the right of law-abiding, responsible Chicagoans" in the *Ezell* cases. *Ezell II*, 846 F.3d at 893 (citing *Ezell I*, 651 F.3d at 708). And in considering a ban on carrying arms in public that applied to "the entire law-abiding adult population of Illinois," the Seventh Circuit explained that the State "would have to make a stronger showing in this case than the government did in *Skoien*," where it satisfied intermediate scrutiny. *Moore*, 702 F.3d at 940. It is appropriate to treat law-abiding citizens differently than criminals because the Second Amendment rights of "law-abiding, responsible citizens" are "elevate[d] above all" others. *Heller*, 554 U.S. at 635.

Ms. Brown has been prosecuted for the core right of possessing a firearm for self-defense in the home. "[T]he Amendment's 'core lawful purpose' is self-defense" and "the need for self-defense is most pressing in the home." *Wrenn v. D.C.*, 864 F.3d 650, 657 (D.C. Cir. 2017).

In *Heller III*, the D.C. Circuit held that a registration requirement on long guns imposed a de minimis burden and therefore did not violate the Second Amendment. *Heller v. District of Columbia*, 801 F.3d 264, 274–75 (D.C. Cir. 2015) ("*Heller III*"). *But see Murphy v. Guerrero*, No. 1:14-CV-00026, 2016 WL 5508998, at *9 (D. N. Mar. I. Sept. 28, 2016) ("Surely, if rational basis scrutiny does not suffice for Second Amendment consideration, then *no* scrutiny, as the D.C. Circuit applied in *Heller III*, cannot be sustained. Because even de minimis burdens on enumerated rights must at the very least be supported by a substantial government interest reasonably related to its law.").

Notably, *Heller III*'s holding was not based on the consideration of competing evidence on the issue. Instead, the holding was based on the appellants' failure to address the issue. Although the court "allowed Heller, during the discovery proceedings on remand, the opportunity to introduce evidence" distinguishing long guns from handguns, for which registration was rooted in history, Mr. "Heller offered no evidence distinguishing the basic registration requirement as applied to long guns. Indeed, he did not even argue the point." *Heller III*, 801 F.3d at 273 (citation omitted).

Additional D.C. regulations requiring that registrants pay a fee and appear in person to be fingerprinted and photographed were upheld under intermediate scrutiny. But each of these holdings was based on the regulation's facilitation of the registration requirement, and therefore predicated on the registration requirement being constitutional to begin with—which, again, was based merely on the appellants' lack of argument. *Id.* at 275–77.

Other regulations requiring that the registrant bring the firearm to the police department, that firearms be periodically re-registered, that registrants be limited to acquiring one handgun per month, and that registrants pass a test on local gun laws failed intermediate scrutiny because the District provided insufficient evidence. *Id.* at 277 (The District "has offered no evidence—let alone substantial evidence—from which it can be inferred that verification [by bringing the firearm] will promote public safety."); *id.* at 277 ("The District has offered three justifications for the requirement that a gun owner re-register his firearm every three years. None is supported by substantial evidence"); *id.* at 278–79 ("The District . . . has presented no evidence from which it could conclude that passing a test of knowledge about local gun laws" reduces accidents involving firearms); *id.* at 279–80 ("The District has not presented substantial evidence to support the conclusion that its prohibition on the registration of 'more than one pistol per registrant during any 30–day period,' promotes a substantial government interest.") (citation omitted).

*Heller III*, therefore, says little about the constitutionality of restrictions on long guns in the home. The principle from *Heller III* is the important role of evidence in justifying a statute.

**C. The State failed to carry its burden under any form of heightened scrutiny by failing to provide evidence.**

"In all cases the government bears the burden of justifying its law under a heightened standard of scrutiny." *Ezell II*, 846 F.3d at 892. To carry its burden, the State "cannot defend its regulatory scheme 'with shoddy data or reasoning. The [State's] evidence must fairly support the [State's] rationale for its ordinance.'" *Ezell II*, 846 F.3d at 896 (quoting *Ezell I*, 651 F.3d at 709). At a minimum, "'there must be *evidence*' to support the [State's] rationale for the 'challenged regulations; 'lawyers' talk is insufficient.'" *Id.* (quoting *Annex Books, Inc. v. City of Indianapolis*, 581 F.3d 460, 463 (7th Cir. 2009) (emphasis in original)).

Here, the State offered no evidence. Courts have consistently struck down laws in Second Amendment challenges where the government failed to provide any evidence.

Under the "elevated intermediate scrutiny" applied in *Chairez*, "the government bears the burden of showing a very strong public-interest justification and a close fit between the government's means and its end, as well as proving that the 'public's interests are strong enough to justify so substantial an encumbrance on individual Second Amendment rights.'" 2018 IL at ¶ 50 (quoting *Ezell I*, 651 F.3d at 708–09). Consequently, this Court struck down restrictions on public carriage in *Chairez* because "the State provide[d] no evidentiary support for its claims." 2018 IL at ¶ 54. The State could not carry its burden "[w]ithout specific data or other meaningful evidence," *id.* at ¶ 54, and its "propositions [we]re devoid of any useful statistics or empirically supported conclusions." *Id.* at ¶ 53.

In *Ezell I*, the range ban was held unconstitutional because "the City produced no empirical evidence whatsoever and rested its entire defense of the range ban on speculation about accidents and theft." 651 F.3d at 709.

In *Ezell II*, the Seventh Circuit struck down zoning restrictions on firearm ranges, repeatedly emphasizing the City's lack of evidence. 846 F.3d at 895 ("The City has provided no evidentiary support for these claims . . . the City continues to assume, as it did in *Ezell I*, that it can invoke these interests as a general matter and call it a day. It simply asserts, without evidence, that shooting ranges generate increased crime, cause airborne lead contamination in the adjacent neighborhood, and carry a greater risk of fire than other uses."); *id.* ("The City's own witnesses . . . repeatedly admitted that they knew of no data or empirical evidence to support any of these claims.") (emphasis omitted); *id.* ("the City submitted a list of 16 thefts . . . no evidence suggests that these thefts caused a spike in crime in the surrounding neighborhood."); *id.* ("The City's assertions about environmental and fire risks are likewise unsupported by actual evidence"); *id.* ("As for the concern about fire, the City provided no evidence").

Also in *Ezell II*, the Seventh Circuit struck down a law banning minors from firing ranges because "the City lacked any data or empirical evidence to justify its blanket no-one-under-18 rule." 846 F.3d at 897–98.

Under Seventh Circuit precedent, the State is required to "establish a close fit between the challenged [] regulations and the actual public benefits they serve—and to do so with actual evidence, not just assertion." *Id.* at 894.

Here, the State did not provide evidence. Therefore, the law should be struck down for persons like Ms. Brown. "If the State cannot proffer evidence establishing both the law's strong public-interest justification and its close fit to this end, the law must be held unconstitutional." *Id.* at ¶ 45.

## IV. The amicus brief in support of the State fails to carry its burden of justifying the FOID statute.

### A. The amicus brief's argument shows that this Court should invalidate the law as applied to Ms. Brown: the FOID requirement cannot apply to guns on one's property, but can apply for gun purchases.

Perhaps cognizant of the State's failure to attempt to meet its burden of proof, the State's amicus offers a discussion of social science. Much of the discussion is an attack on the 2007 repeal of Missouri's handgun permit-to-purchase statute, and a celebration of Connecticut's enactment of such a statute in 1995. Amicus analogizes these laws to Illinois's FOID system.

The analogies are inapt. The Connecticut and Missouri laws applied only to handgun purchases. Handguns constitute 40.6% of the firearms supply, and yet "are the overwhelmingly favorite weapon of armed criminals." *Heller*, 554 U.S. at 682 (Breyer, J., dissenting).[10] Long guns are disproportionately *under*-represented in gun crime. This case involves a long gun.

Assuming arguendo that the amicus brief's argument is sufficient to carry the State's burden of proof, the argument shows that this Court should rule in favor of Ms. Brown. The ruling could be characterized as "as-applied" or it could be characterized as a narrow facial ruling involving the home. The scope would cover situations, like Ms. Brown's, in which: (1) the gun does not leave the owner's property; (2) the gun is lawful under Illinois law; and (3) the gun owner is not prohibited by any other law from owning firearms. A home-based ruling would leave intact the requirement to have a FOID to *purchase* a firearm in a

[10] The figure is calculated by starting with data on the U.S. civilian firearms stock as of 1994. *See* Gary Kleck, TARGETING GUNS: FIREARMS AND THEIR CONTROL 96-97, tbl. 3.1 (1997). To this was added annual ATF data on U.S. manufacture, plus imports, minus exports. *See* ATF, *Firearms Commerce in the United States: Annual Statistical Update 2018*, at 1 (exhibit 1, U.S. manufacture), 3 (exports), 5 (imports), https://www.atf.gov/file/130436/download.

store or from an individual. Any benefits that inure from permit-to-purchase laws would be preserved by a ruling confined to the home or other property of the owner.

Notably, the State's brief advocates for drastic expansion of the traditional scope of the FOID law. According to the State, if five people live in a home, and one of them owns a firearm, all five people must obtain a FOID Card, because they are supposedly in "constructive possession." Appellant's Br. at 15–16. Yet the State provides no citation showing that Illinois courts have interpreted the FOID law to be so sweeping. A home-based as-applied ruling from this Court would clarify that the law does not criminalize such a broad category of persons—including persons who never use guns, but who could be deemed to be in "constructive possession."

### B. The ATF and Congress have specifically warned against the misuse of ATF data, such as the misuse in the amicus brief.

The amicus brief asserts that FOID-style laws reduce illegal trafficking of guns. For support, the brief cites newspaper interviews with and articles by Daniel Webster, who is the Bloomberg Professor of American Health at the Johns Hopkins Bloomberg School of Public Health. Amicus Br. at 13–14.

Unfortunately, Prof. Webster's research misuses firearms trace reports, ignoring express warnings against such misuse from the very organization that provides the data: the federal Bureau of Alcohol, Tobacco, Firearms and

Explosives ("ATF"). While Prof. Webster bases his research on firearms trace data reported by ATF, the ATF itself has announced that such data are inappropriate for drawing conclusions about the illicit flow of guns: "The firearms selected [for ATF tracing] do not constitute a random sample and should not be considered representative of the larger universe of all firearms used by criminals, or any subset of that universe." ATF, Firearms Trace Data – 2017.[11] The same caution has been required by Congress since 2004 and appears on every ATF state tracing report. *See* Consolidated and Further Continuing Appropriations Act, 2013, div. B, § 514 (18 U.S.C. 923 note; P.L. 113–6; 127 Stat. 271–72) (currently-applicable provision) ("For fiscal year 2013 and thereafter, the Bureau of Alcohol, Tobacco, Firearms and Explosives shall include in all such data releases . . ."). Like the federal Employee Polygraph Protection Act, 29 U.S.C. § 2001, the congressionally mandated disclosure is a strong warning against reliance on dubious evidence.

### C. Permit-to-purchase laws have no statistically significant effect on homicides.

#### 1. The definition and importance of statistical significance.

Because social statistics are always fluctuating, social scientists take care to avoid assuming that if A and B each changed at the same time, the change

[11] https://www.atf.gov/resource-center/firearms-trace-data-2017.

in A must have caused the change in B. The method for discerning an actual association is expressed as a "p value." The value can be between zero and one. A p-value of 0.4 indicates that there is a 40% possibility that the relationship between A (e.g., changes in gun laws) and B (changes in homicides) is due to chance variation, rather than true association. The presence of an association does not necessarily mean that a causal relationship exists between A and B; for example, B may be affecting A, or a third variable, C, may be affecting both A and B.

"In most scientific work, the level of statistical significance required to reject the null hypothesis (i.e., to obtain a statistically significant result) is set conventionally at 0.05, or 5%." Federal Judicial Center, REFERENCE MANUAL ON SCIENTIFIC EVIDENCE 320 (3d ed. 2011). "A study that is statistically significant has results that are unlikely to be the result of random error . . ." *Id.* at 573.

Thus, if the p-value is 0.04, there is a 4% possibility that the relationship is due to random variation, and a 96% possibility that A really is associated with B. *Id.* at 240–41. As will be described below, changes in permit-to-purchase laws are not associated with statistically significant changes in homicide or suicide.

Not every decision by a court must be based on statistical significance. For example, a drug company's failure to disclose certain reports of adverse effects from a drug was a "material omission" for SEC purposes, because reasonable investors would want to know about the adverse effects, even if there was not proof of statistical significance. *See Matrixx Initiatives, Inc. v. Siracusano*, 563 U.S. 27 (2011). "This is not to say that statistical significance (or the lack thereof) is irrelevant—only that it is not dispositive of every case." *Id.* at 43.

In the instant case, the high standard of statistical significance is appropriate. Unlike the materiality of nonpublic information about stocks on a public exchange, the default position of a fundamental right, like the right to arms, *McDonald*, 561 U.S. at 767–80, is that government will not interfere with it. All the more so for the peaceable exercise of a fundamental right in one's home. When the government seeks to impose criminal sanctions on the simplest exercise of a fundamental right in the home, the government should meet a rigorous burden of proof.

The State's amicus brief concentrates on claims about homicide and suicide in Missouri and Connecticut. The instant brief addresses homicide first, then suicide. For Missouri and Connecticut, this brief provides a comparison with a neighboring state—Kansas and Rhode Island. This brief also provides a national analysis for homicide and suicide, covering all six states that have changed their handgun permit-to-purchase laws, and including data from all the other states that had no change.

The data analyses in this brief were prepared by Prof. Carlisle Moody, of William & Mary. Data sources and a glossary of terms are in Appendix B. Prof. Moody's curriculum vitae is in Appendix C. Appendices D, E, F, and G show the Stata program tables that were used for the analyses.[12] All numerical results mentioned in this brief's text are bolded (along with their section headings) in the appendices and are pin-cited in the brief.

### 2. Missouri

The amicus brief claims that the 2007 repeal of Missouri's handgun permit-to-purchase law raised firearms homicide rates. Amicus Br. at 10 (citing "Daniel W. Webster, et al., *Effects of the Repeal of Missouri's Handgun Purchaser Licensing Law on Homicides*, 91 J. Urban Health 598, 296-97 (2014)").[13]

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[12] Stata is the premier statistical data analysis software package. STATA SOFTWARE, https://www.stata.com/; *Stata Alternatives & Reviews*, ALTERNATIVE.ME, https://alternative.me/stata#read_more.

[13] The pin-cite is incorrect. An article that begins on page 598 cannot have a pin-cite to pages 296–97. The correct beginning page for the article is 293. As Prof. Webster has forthrightly acknowledged, the reported results in his article are incorrect because of errors in his data tables. A follow-up article reported and accounted for the data corrections. That article said that the 2007 Missouri repeal made Missouri's subsequent homicide rate 25% higher than it would have been otherwise. *See* Daniel Webster, et al., *Erratum to: Effects of the*

Prof. Webster and his coauthors chose to study the period 1999–2010 for homicides and firearm homicides and 1999–2012 for murder rates. If one looks at a broader time period, the claim of increased murder or homicide fails.

Figure 1, below, shows Missouri trends from 1960 through 2016. 1960 was chosen as the starting point because it is the first year for which full data are available.[14] 2016 is the end year because full data for more recent years are not yet available. As Figure 1 shows, murder, firearms homicide, and firearms suicide were all declining from the late 1990s until 2003, when they began to increase. The turning point is 2003, not 2007. Firearm homicide began increasing four years before Missouri repealed its PTP law, making it unlikely that the repeal caused the increase in firearm homicide (or murder, or suicide).

The crime surge in Missouri was well-known before the permit-to-purchase law was repealed. *See Police chief cites 'thugs and drugs' in rising crime rate*, (Springfield) NEWS-LEADER, May 23, 2007; *St. Louis plans to overhaul police force as crime grows*, ASSOC. PR., Dec. 22, 2006 ("FBI statistics that show violent crime in St. Louis grew 10.3 percent during the first half of 2006 when compared with the same period last year"); Tim O'Neil, *Chief plans overhaul to fight crime rise*, ST. L. POST-DISPATCH, Dec. 21, 2006 (St. Louis police chief

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*Repeal of Missouri's Handgun Purchaser Licensing Law on Homicides*, 91 J. URBAN HEALTH 598, 598 (2014).

[14] Full suicide data begin in 1986.

"said the number of violent crimes also began rising in 2004 after a steady decrease since 1996"); Christopher Leonard, *Report says crime makes St. Louis most dangerous U.S. city*, ASSOC. PR., Oct. 31, 2006 ("Violent crime surged nearly 20 percent there from 2004 to last year"); Jason Szep, *Violent crime rising in much of United States*, SEATTLE TIMES, Aug. 21, 2006 ("From Kansas City, Mo., to Indianapolis, Ind., places that rarely attract notice on annual FBI crime surveys are seeing significant increases in homicides."); Christopher Leonard, *Hannibal suffering through New York-like crime wave*, ST. L. POST-DISPATCH, Dec. 11, 2005; *City looks to stem rising crime rate As part of '06 budget process, council could add more officers*, K.C. STAR (Mo.), June 22, 2005. Perhaps the on-going crime surge contributed to the Missouri legislature's decision to remove a burdensome process that interfered with law-abiding citizens' ability to buy a defensive handgun promptly, rather than waiting for bureaucratic processing.[15]

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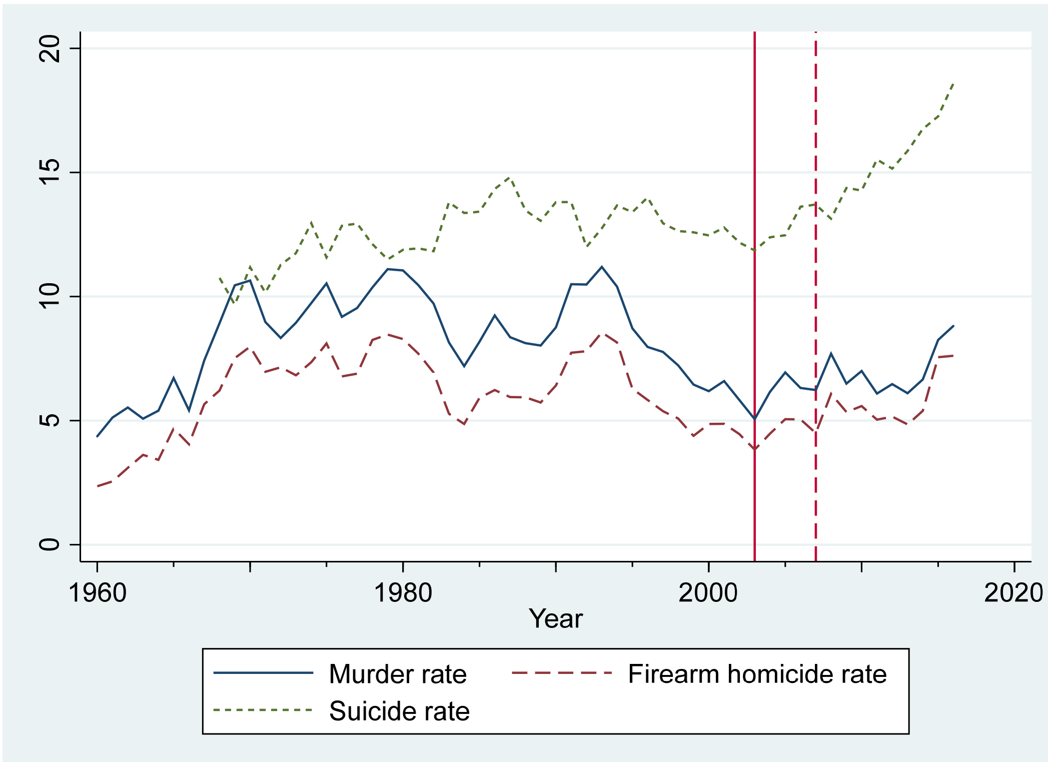
[15] *See* Tim Barker, *Gun sales surge on change in Missouri law*, ST. LOUIS POST-DISPATCH, Sept. 6, 2007 ("[C]ritics argued that the old law was often overly restrictive as interpreted by some sheriffs. In 1989, for example, the St. Louis city sheriff required applicants to get letters of recommendation from two reputable people, such as ministers or businesspeople. The requirement was later dropped."); Daniel C. Smith, *Your Views* (letter to the editor), ST. LOUIS POST-DISPATCH, Sept. 16, 2007 ("[O]ne of the primary reasons the old law was repealed - the misuse and abuse of the PTA provisions by those whose responsibility was to enforce it. Nowhere did the old law allow Missouri sheriffs to create their own criteria to deny a person a permit to acquire a handgun -

**Figure 1:** Murder, suicide, and firearm homicide Missouri 1960-2016




Note: "Murder" is from FBI Uniform Crime Reports. "Firearm homicide" and "suicide" are from the Centers for Disease Control.

Looking at the full range of data shows that Missouri's overall murder rate after 2007 is lower than in nearly all the previous years in which the permit-

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yet that is precisely what many sheriffs did."). The problem was not new. "Permits are automatically denied in St. Louis to wives who don't have their husbands' permission, homosexuals, and non-voters .... As one of my students recently learned, a 'personal interview' is now required for every St. Louis application. After many delays, he finally got to see the Sheriff—who looked at him only long enough to see that he wasn't black, yelled 'he's all right' to the permit secretary, and left." Donald B. Kates, *On Reducing Violence or Liberty*, CIVIL LIBERTIES REV. 16, 18–19 (Aug./Sept. 1976) (ACLU periodical; author was a law professor at St. Louis U.) (original available at U. of Ill. Brookens Library in Springfield, Ill.).

to-purchase law was in effect. Indeed, Missouri outperformed neighboring Kansas. As Table 1 shows, Missouri's average post-2007 murder rate, compared to its average pre-2007 rate, showed more improvement than did Kansas over the same period. (Data comparing Missouri with all other states, including those that changed their permit-to-purchase laws, is presented *infra*.)

**Table 1:** Murder rate in Missouri and Kansas before and after repeal of Missouri's PTP law

| | **Average murder rate** | | **Difference** | **Difference in differences** |
|---|---|---|---|---|
| | **Year<=2007** | **Year>2007** | | |
| **Missouri** | 8.107 | 7.065 | -1.041 | -0.022 |
| **Kansas** | 4.804 | 3.785 | -1.019 | |

Calculations for Table 1 are presented in Appendix D and the above results are on page App. 18. In the data appendices, the cited data, along with their subsection headings, are bolded.

Even within the limited period studied by Prof. Webster, his methodology was inherently flawed, leading to an incorrect finding of statistical significance. If only one state (e.g., Missouri) makes a policy change, while all other states make no policy change (true for the period Prof. Webster studied), and the researcher uses a fixed-effect regression model with clustered standard errors (as Prof. Webster did), the standard errors associated with the significance test on the policy variable will be underestimated by about a factor of 17 (16.86). *See* Timothy G. Conley & Christopher R. Taber, *Inferences with "Difference in Differences" with a small number of policy changes*, 93 REV. OF ECON. & STATS. 113, 122 table 3 (2011).[16] Accounting for this 17-fold

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[16] Professors Conley and Taber examine, *inter alia*, the precision of "cluster-by-group" method of testing the effects of small numbers of policy changes. Conley & Taber, at 121. This is the approach used by Prof. Webster's study. Webster, et al., *Effects of the Repeal of Missouri's Handgun Purchaser Licensing Law on Homicides*, 91 J. URBAN HEALTH at 296. (Because the Conley-Taber article is not on the public Internet, a copy is attached as Appendix H. An earlier version is available at https://www.ssc.wisc.edu/~ctaber/Papers/taber-conley.pdf.) Professors Conley and Taber found that the cluster-by-group approach applied to a policy change in a single state incorrectly rejected a true null hypothesis in 84.28% of trials, rather than the 5% rejection rate that one would ordinarily expect due to sampling error when using a p-value of 5%; the result is a 16.86-fold increase in the probability that a mistaken conclusion will be reached by the study. Conley & Taber, *supra*, at 121–22 & tbl. 3 (Size of Test/Cluster column, Number of treatments = 1 row); *see* Federal Judicial Center, REFERENCE MANUAL, *supra* 240–41 & n.84 (explaining that the p-value threshold used for statistical significance is equal to the probability of improperly rejecting a true null hypothesis due to sampling error); *supra* Section B.1.

This brief's state-to-state comparisons are not at risk of the error described by Profs. Conley and Taber because they are not the cluster-by-group or t-test methods that Profs. Conley and Taber found to be vulnerable to misestimation when used with few policy changes. This brief's national comparisons are vulnerable to the errors described by Profs. Conley and Taber at a 2- to 7-fold factor. Conley & Taber, *supra*, at 121–22 & tbl. 3 (showing 9.52% and 35.74% rejection rates for true null hypotheses). This error, however, only strengthens the national studies' conclusions: As Tables 2 and 4 show, *without* accounting for the estimation error described by Profs. Conley and Taber, permit-to-purchase laws have no statistically significant association with homicide or underestimate, the corrected p-value of the Webster article is 0.298. This is well above the threshold for statistical significance, which requires a p-value of no more than 0.05.

### 3. National data

Instead of looking at a single state, the better approach is to look at a larger sample of all states that have changed the relevant policy. After controlling for social variables—unemployment, police per capita, and so on—the effects of changes in state policy can be compared to trends in all other states (plus the District of Columbia), including states where there was no change in policy. Accordingly, Prof. Moody examined not only the 2007 Missouri repeal, but also the enactment of handgun permit-to-purchase laws in Connecticut (1995), Iowa (1978), Maryland (2013), Minnesota (1977), and Nebraska (1991). The data analysis controlled for twenty variables; the methodology is shown in Appendix E.

Prof. Moody examined whether changes in permit-to-purchase laws affected murder, firearm homicide, or homicide. The results are in Table 2, below, with p-values in the right-hand column. All the p-values are, again, well above the 0.05 required for statistical significance. Accordingly, the burden of

suicide. Accounting for the error discovered by Profs. Conley and Taber would make these associations even more insignificant.

proof for justifying permit-to-purchase under heightened scrutiny has not been met. Indeed, the results indicate that, should the Illinois FOID law be declared void in its entirety, there would be no statistically significant effect on murder, gun homicide, or homicide in general.

**Table 2:** Do PTP laws reduce murder, gun homicide, or homicide?

| **Dependent variable** | **PTP Coef.** | **St. Err.** | **T-ratio** | **P-value** |
|---|---|---|---|---|
| **Murder rate** | 0.111 | 0.180 | 0.62 | 0.540 |
| **Firearm homicide rate** | 0.075 | 0.127 | 0.59 | 0.557 |
| **Homicide rate** | 0.186 | 0.177 | 1.05 | 0.300 |

Note: "Murder" is from FBI Uniform Crime Reports. "Firearm homicide" and "homicide" are from the Centers for Disease Control. "Murder" is not the same as "homicide," since some homicides are justifiable and lawful, and since some unlawful homicides are not "murder." For completeness, this brief analyzed the data for both FBI murder and CDC homicide.

Control variables are a time trend, population density, crack cocaine prevalence, beer per capita, police per capita, incarceration, income, welfare payments, poverty rate, percent black, unemployment rate, employment, military employment, construction employment, stand your ground law, right-to-carry law, "Saturday night special" law, percent population in 5-year age groups 15-65+, and two lags of the dependent variable. Complete results are presented in Appendix E.

### 4. Connecticut

According to the State's amicus brief, Connecticut realized, "a staggering 40% reduction in gun homicides" after enacting a handgun permit-to-purchase law in 1995. Amicus Br. at 11 (citing Kara E. Rudolph, et al., *Association*

*Between Connecticut's Permit-to-Purchase Handgun Law and Homicides*, 105 AM. J. PUB. HEALTH e49 (2015)). But gun homicides did not decline 40% in Connecticut after the law was enacted. Nor does the cited study so claim. Rather, the study contends that gun homicides would have been 40% greater without the law.

The contention is dubious. First, the study does not account for changes in control variables (e.g., unemployment, incarceration rate, police per capita) after the law's 1995 enactment. The problem arises because the article does not compare Connecticut to any actual state or states. Instead, the study compares Connecticut to a "synthetic state," which is a blend of varying amounts of California, Maryland, Nevada, New Hampshire, and Rhode Island, plus small proportions of dozens of other states. *Id.* at e50. The study invents one synthetic state for firearms homicides and a different synthetic state for nonfirearm homicides. *Id.*

A second problem of the "synthetic state" methodology is that is fails to account for long-standing cultural and geographical differences between states. For example, seasonal affective disorder is large in northern states and drives up suicide rates. The synthetic state methodology cannot account for this (and other) ways that California or Nevada are fundamentally different from Connecticut.

The creation of synthetic "states" also allows for relatively easy outcome manipulation—by choosing a synthetic group that compares to the tested group as the author desires. In contrast, state-to-state comparisons are harder for an author to manipulate, because the author must take the states as they are and cannot invent new "states" for comparison.

As noted above, the implementation of the Connecticut permit-to-purchase law is included in the national analyses reported in Table 2, showing that such laws have no statistically significant effect.

Still, it is possible that the Connecticut law did have a significant effect in that state in a way that was somehow washed out by the absence of effects in the other five states. It is easy to check this possibility by using a simple difference in differences analysis like the Missouri-Kansas comparison *supra*. In this case, Rhode Island is the control state for Connecticut. As Table 3 indicates, Connecticut's post-1995 firearm homicide rate declined by 8.3%, whereas Rhode Island's rate was unchanged. The p-value for this small fluctuation is 0.787. *See* Appendix D at 40. In other words, there is about a 79% probability that the change was due to random fluctuation, rather than the effect of the 1995 law.

**Table 3:** Difference in differences analysis of Connecticut's 1995 PTP law

| | Average gun homicide rate | | Difference | Difference in differences |
|---|---|---|---|---|
| | Year<=1995 | Year>1995 | | |
| **Connecticut** | 2.319 | 2.235 | -.084 | -.083 |
| **Rhode Island** | 1.596 | 1.595 | -.001 | |

In short, the implementation of a handgun permit-to-purchase law in Connecticut has apparently had no significant effect on firearm homicide rates—unsurprising, given the national results in Table 2. (Nationally, the p-value was .557, indicating the permit-to-purchase laws were far from a statistically significant effect.) The 8.3% Connecticut decline is considerably smaller than the "staggering" 40% decline claimed by the amicus brief and is very likely the result of random fluctuation.

### D. Permit-to-purchase laws may reduce suicide by firearm but do not reduce total suicide.

#### 1. Connecticut

According to the State's amicus brief, a "measurable reduction in gun suicides after Connecticut's adoption of a licensing law and the spike in gun suicides after Missouri repealed such a law further show that Illinois' FOID Card Act substantially advances public safety. . . " Amicus Br. at 12 (citing

Cassandra Kercher Crifasi, et al., *Effects of Changes in Permit-to-Purchase Handgun Laws in Connecticut and Missouri on Suicide Rates*, 79 PREV. MED. 43 (2015)). The cited article reads: "Connecticut's PTP law was associated with a 12% reduction in firearm suicide rates (p=0.004), a 14% increase in rates of non-firearm suicide (p=0.002), and no association with overall suicide rates. . . . The repeal of Missouri's PTP law was not associated with changes in any of the suicide measures." *Id.* at 47.[17]

In other words, the article cited by the amicus brief shows that in Connecticut, the law reduced firearm suicide but other methods were substituted, so there was no change in the overall number of suicides. In Missouri, the law had no effect at all.

Prof. Crifasi and her coauthors chose to limit their data to 1981–2012. This brief looks at a broader range of data: from 1968 (the first year data are available) through 2016 (the most recent year of available data). Again, this brief begins with two-state comparisons and then presents national data.

Comparing Connecticut and Rhode Island before and after the 1995 Connecticut law, the dependent variable is the number of gun suicides per

[17] Among 20-to-29-year-olds, the article did find that the decrease in firearm suicide was not fully offset by an increase in other methods. *Id.* at 47. Apparently the net effect for this group, for whom suicide rates are generally lower than for older age groups, was fairly small in comparison to the statewide rate.

capita. The p-value for firearm suicide reduction is 0.074. *See* Appendix F at 58. This is not quite low enough for a finding of statistical significance, but it is close—far closer than permit-to-purchase laws' relation to firearm homicide (as discussed *supra* Table 2).

The above result is consistent with scholarly research indicating that lower firearms availability is associated with a lower percentage of suicide committed with firearms—but not with an overall reduction in suicide.[18]

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[18] Recent scholarship by Prof. Gary Kleck examines previous studies on firearms availability and suicide, and also presents new research. The studies discussed were not about the effects of particular gun control laws. Rather, they investigated the relationship between levels of gun ownership and suicide. Professor Kleck's 1993 book POINT BLANK: GUNS AND VIOLENCE IN AMERICA won the American Society of Criminology's Michael J. Hindelang Award for "the most outstanding contribution to criminology" in a three-year period.

Reviewing all studies of firearms and suicide based on large populations (nations, regions, states, or cities), Prof. Kleck reported that "most analyses find a significant positive association between firearms prevalence and the rate of *firearms* suicide, consistent with the view that where guns are more widely available, more people will commit suicide *with guns*." Gary Kleck, *Macro-level research on the effect of firearms prevalence on suicide rates: A systemic review and new evidence*, 100 SOC. SCI. Q. 935, 945 (2019) (emphasis in original).

Further, "15 of the 29 analyses did not find a significant association of firearms prevalence with the suicide rate." *Id.* In Prof. Kleck's view, the studies that did report a significant association were flawed for two reasons. The first problem was their "distorted" "estimates of the effects of gun prevalence." *Id.* at 937. Indeed, this particular problem with some of those studies was pointed out by the National Research Council. NATIONAL RESEARCH COUNCIL, FIREARMS AND VIOLENCE: A CRITICAL REVIEW 169–70 (2004) (Committee to Improve Research Information and Data on Firearms).

A second problem was inadequate controls for "confounding variables." Kleck, *Macro-level research*, at 942–45. A confounding variable is something

Unfortunately, there are several substitute methods of suicide that are nearly as likely to be fatal.[19]

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that independently affects both gun ownership and suicide. For example, married people are more likely to own guns and less likely to commit suicide. Gary Kleck, *The effect of firearms on suicide*, GUN STUDIES: INTERDISCIPLINARY APPROACHES TO POLITICS, POLICY, AND PRACTICE 309, 310 (Jennifer Carlson et al. eds. 2019). Poor people are less likely to own guns and more likely to commit suicide. *Id.* Thus, comparisons of various states or cities should account for different levels of marriage or poverty in the different jurisdictions. Prior research has identified 15 confounding variables for firearms and suicide. *Id.* at 310–11. (Plus four more potential confounders that are known to affect gun ownership, and that could be related to suicide, such as residence in a high-crime neighborhood. *Id.* at 311.)

All of the large-scale studies that (1) avoided the error identified by the National Research Council and (2) controlled for more than two confounding variables found no significant association between firearms prevalence and suicide levels. Kleck, *Macro-level research*, at 948. This was consistent with Prof. Kleck's new research, which studied all fifty states and controlled for eight confounders. It found no significant association between gun prevalence and suicide but did find that gun prevalence affects the percentage of suicides that are committed by gun. *Id.* at 946–47. Kleck's earlier study of 170 cities, which also controlled for eight confounders, had found similar results. *Id.* at 946.

[19] Fatality rates of suicide attempts are as follows: shooting 83.7%, hanging 76.7%, and 67.2% for drowning. Kleck, *The effect of firearms on suicide* at 319, table 17.3 (using "the largest set of suicides and suicide attempts ever employed in the computation of method-specific suicide fatality rates"). The low rates for some methods (e.g., cutting, drugs) indicate that these forms of self-inflicted injury are often a cry for help, and not an earnest attempt at fatality. *Id.* at 321–23.

"The array of feasible alternative methods of killing available to prospective suicides is…quite different from the methods available to murderers, at least partly because there are no resisting victims in suicide attempts. Murderers almost never kill their victims by hanging, but it is quite common for people to kill themselves by hanging. Likewise, it is quite unusual, outside the pages of murder mysteries, for killers to push their victims from high places or to drown

There was a clear substitution effect in Connecticut. The p-value is 0.998 for the relationship between the 1995 Connecticut law and the overall suicide rate. *See* Appendix F at 58. In other words, the possibility that Connecticut's law is associated with an overall lower suicide rate is 2 in 1,000.

### 2. Missouri

The results are similar for Missouri. Using Kansas as the control state, the p-value is 0.144 for an increase in Missouri firearm suicides after the 2007 repeal of the permit-to-purchase law. *See* Appendix F at App. 37.

More fundamentally, the p-value for effect on total suicide in Missouri is 0.624, very far from the statistically significant level of 0.05. *Id.* In the unlikely event that there was an effect, the 95% confidence interval[20] includes the possibility that the 2007 repeal is associated with *decreased* suicide. *Id.*

### 3. National data

An examination of all fifty states, plus D.C., looked for suicide changes among the six states that changed their handgun permit-to-purchase laws. As presented in Table 4, the p-value for gun suicide reduction is 0.062 (almost but

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them, but it is fairly common that people kill themselves by jumping from high places or drowning." *Id.* at 317 (citations omitted).

[20] "The confidence level indicates the percentage of the time that intervals from repeated samples would cover the true value." FEDERAL JUDICIAL CENTER, REFERENCE MANUAL ON SCIENTIFIC EVIDENCE, at 247. A confidence interval of 95% covers two degrees of the standard errors that are caused by the inevitable random errors in statistical sampling. *Id.* at 244.

not quite as low as the statistically significant 0.05). The p-value for overall suicide reduction is 0.247, indicating no statistically significant effect.

**Table 4:** Do permit-to-purchase laws reduce suicide or firearm suicide?

| Dependent variable | PTP Coef. | St. Err. | T-ratio | P-value |
|---|---|---|---|---|
| **Suicide rate** | -0.142 | 0.121 | -1.17 | 0.247 |
| **Gun suicide rate** | -0.150 | 0.079 | -1.91 | 0.062 |

Note: Control variables are density, beer consumption per capita, income per capita, unemployment rate, total employment, and the percent of the population aged 50–59. Calculations are in Appendix F, results at App. 40 (gun suicide) & 43 (total suicide).

Thus, there is some weak evidence that permit-to-purchase laws reduce *firearm* suicide. Even so, the laws have no significant effect on total suicides, indicating that reduction of firearm suicides is offset by corresponding substitution-driven increases in non-firearm suicides.

* * *

The data from Missouri, Connecticut, and other states that changed their handgun permit-to-purchase laws show no net beneficial effects on firearm homicide or overall homicide or murder. They do show a possible reduction in firearm suicide, but no net beneficial effect on overall suicide. The burden of proof to uphold the FOID law under heightened scrutiny has not been met.

## CONCLUSION

The Circuit Court's opinion should be affirmed.

Respectfully submitted,

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May 31, 2019

## CERTIFICATE OF COMPLIANCE

I certify that this brief conforms to the requirements of Rules 341(a) and (b). The length of this brief, excluding the words contained in the Rule 341(d) cover, the Rule 341(h)(1) statement of points and authorities, the Rule 341(c) certificate of compliance, the certificate of service, and those matters to be appended to the brief under Rule 342(a), is 12,561 words.

/s/ *Gregory A. Bedell*
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## CERTIFICATE OF FILING AND SERVICE

The foregoing Brief of Amici Curiae State's Attorneys Stewart J. Umholtz and Brandon J. Zanotti, Professors of Second Amendment Law, Firearms Policy Coalition, Firearms Policy Foundation, Citizens Committee for the Right to Keep and Bear Arms, Millennial Policy Center, Independence Institute, and Carlisle Moody in Support of Vivian Claudine Brown and Affirmance was electronically filed with the Supreme Court of Illinois on May 31, 2019, and served upon the following on May 31, 2019, by email:

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Within five days of acceptance by the Court, the undersigned will cause thirteen copies of the Brief to be mailed with postage prepaid addressed to:

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Under penalties by law pursuant to Section 1-109 of the Code of Civil Procedure, the undersigned certifies that the statements set forth in this Certificate of Filing and Service are true and correct.

/s/ *Gregory A. Bedell*
GREGORY A. BEDELL

Dated May 31, 2019

## Appendix A: Amici Professors

Robert J. Cottrol is Harold Paul Green Research Professor of Law at George Washington. His scholarship was cited in Justice Thomas's concurring opinions in *McDonald v. Chicago* and *Printz v. United States*, and by the Fourth Circuit in *Kolbe v. Hogan*, 849 F.3d 114 (2017) (Traxler, J., dissenting). Prof. Cottrol is author of four legal history books on race and law, and editor of a three-volume anthology of the right to arms. He wrote the entries for "The Right to Bear Arms" in *The Oxford International Encyclopedia of Legal History* and "The Second Amendment" in *The Oxford Companion to the Supreme Court of the United States*. His Second Amendment scholarship has been published in the *Yale Law Journal*, *Georgetown Law Journal*, and *Journal of American Legal History*.

Nicholas J. Johnson is Professor of Law at Fordham University, School of Law. He is co-author of the first law school textbook on the Second Amendment, *Firearms Law and the Second Amendment: Regulation, Rights, and Policy* (Aspen Pub. 2d ed. 2017) (with David B. Kopel, George A. Mocsary, and Michael P. O'Shea). The casebook has been cited by majorities in *People v. Chairez* (Supreme Court of Illinois) and *Grace v. District of Columbia* (D.C. Cir.), and by dissents in *Drake v. Filko* (3d Cir.) and *Heller II* (D.C. Cir.). Professor Johnson is also author of *Negroes and the Gun: The Black Tradition of Arms* (2014). His articles on the right to arms have been published by the *Hastings Law Review*, *Ohio State Law Journal*, and *Wake Forest Law Review*. Other courts citing his right to arms scholarship include the Eastern District of New York, and Washington Court of Appeals, and the Seventh Circuit in *Ezell v. City of Chicago* (2011).

Nelson Lund is University Professor at George Mason University, Antonin Scalia Law School. He is author of the entry on "District of Columbia v. Heller," in The Oxford Guide to United States Supreme Court Decisions (2d ed. 2009). His Second Amendment scholarship has appeared in the *UCLA Law Review*, *Hastings Law Journal*, *Georgetown Journal of Law and Policy*, and *Constitutional Commentary*. That scholarship has been cited by the D.C., Third, Fifth, Eighth, and Ninth Circuits and by the Seventh Circuit in *Moore v. Madigan* (2012), *Ezell v. City of Chicago* (2011), *Gonzalez v. Village of West Milwaukee* (2012); federal district courts in Virginia and Illinois (*Shepherd v. Madigan* (S.D. Ill. 2012); and the Virginia Court of Appeals, the Washington Supreme Court, the Wyoming Supreme Court, and the Illinois Appellate Court (*People v. Foster* (1st Dist. 2012)).

Joseph E. Olson is emeritus Professor of Law at Mitchell Hamline School of Law, where he taught Second Amendment, business law, and tax law. His scholarship on the right to arms was cited by *District of Columbia v. Heller*, and also by the Ninth Circuit, Eastern District of New York, and Washington Supreme Court. His articles on the right to arms have appeared in the *Stanford Law and Policy Review*, *Georgetown Journal of Law & Public Policy*, and *Michigan Journal of Law Reform*.

Glenn H. Reynolds is Beauchamp Brogan Distinguished Professor of Law at the University of Tennessee College of Law, where he teaches constitutional law and technology law. His constitutional scholarship has been published in the *Columbia Law Review*, *Virginia Law Review*, *University of Pennsylvania Law Review*, *Wisconsin Law Review*, and *Northwestern University Law Review*. The Seventh Circuit cited his scholarship as a model of "originalist interpretive method as applied to the Second Amendment." *Ezell v. City of Chicago*, 651 F.3d 684, 699 n.11 (7th Cir. 2011). He was also cited by this Circuit in *United States v. Yancey* (2010), and *Kanter v. Barr* (2019). In addition, his right to arms scholarship has been cited by the First, Third, Fourth, Fifth, Eighth, and Ninth Circuits; by federal district courts in Texas and, in this Circuit, by *United States v. Luedtke* (E.D. Wis. 2008) and *Kanter v. Sessions* (E.D. Wis. 2017); and by the Supreme Courts of Kentucky and Oregon.

E. Gregory Wallace is Professor of Law at Campbell University School of Law, where his constitutional law courses include the Second Amendment. He recently supervised a Campbell Symposium on the anniversary of the *Heller* decision, and is author of an article on "assault weapons" in a recent symposium of the *Southern Illinois Law Journal*. He is co-author of forthcoming online supplemental chapters in the Johnson et al. *Firearms Law* textbook.

## Appendix B: Glossary and data sources

### Glossary

The following terms were used in the text or tables:

*Confounding variable.* In a study of the association between A and B, a confounding variable is an independent variable that may affect both A and B. For example, in a study about how varying levels of sunlight affect the growth of a particular species of plant, the amount of fertilizer each plant receives is a confounding variable.

*Control variable.* Something that is kept constant during the experiment. In the sunlight-crop yield study, the effect of fertilizer is controlled for by its inclusion in the regression as an independent variable.

*Dependent variable.* Something that may be changed by the effect of an independent variable.

*Differences in differences* (abbreviated "DD" in the appendices). The methodology examines differences between a treatment group (e.g., a state that changed permit-to-purchase law) and a control group (a state that did not change). The average change over time is compared. The "difference in difference" is the gap between the change in the two groups. For example, if the treatment group homicide rate fell by 1.35, and the control group homicide rate fell by 1.54, the "difference in differences" would be .19.

*Null hypothesis.* Social science studies begin with the null hypothesis: that there is no association between A and B. The null hypothesis is refuted with the p-value of the A-B relationship is shown to be .05 or less.

*P-value.* A statistic used for testing statistical hypotheses. For every test a significance level is chosen, traditionally 5% (.05). If the P value is equal to or smaller than the significance level, it suggests that the observed changes or differences are too large to be explained by chance alone.

*PTP Coef.* (The coefficient for permit-to-purchase laws). A regression coefficient is a number associated with an explanatory variable in a regression that indicates the change in the dependent variable for a one-unit change in the explanatory variable. (In this case, the enactment or repeal of a PTP law.) A coefficient that is close to zero implies that the dependent variable does not change when a PTP law is enacted.

*St. Err.* (Standard Error). A representation of the amount of variation or dispersion associated with a data set, equal to the square root of the average squared difference between each observation in the set and the set's mean.

*T-ratio*. The ratio of the coefficient to its standard error. This is the test statistic, which is distributed according to the t-distribution, derived from the normal distribution. The larger the T-ratio, the less likely the coefficient is truly equal to zero and the observed value is due to random error. A good rule of thumb is that, if the T-ratio is greater than two, the p-value is less than .05.

**Data Sources**

The statistical analyses are based on a data set that Prof. Moody and his frequent co-author Thomas B. Marvell have maintained since 2001 and is updated annually. With the exception of cocaine data, the sources are the standard government sources.

The data used in these analyses are in an Excel data table, available at http://cemood.people.wm.edu/Illinois.xls.

Data sources are as follows:

Income: real total personal income. Total personal income, divided by the consumer price index, both taken from the US Dept. of Commerce, Bureau of Economic Analysis.

Unemployment rate: Bureau of Labor Statistics (BLS).

Employment, military employment, construction employment: BLS.

Population, population density: US Census Bureau.

Incarceration (prison population per capita): US Dept. of Justice, Bureau of Justice Statistics (BJS).

Police: number of sworn officers (per capita): BJS.

Cocaine: Fryer RG, Heaton PS, Levitt SD, Murphy KM. *Measuring Crack Cocaine and Its Impact*. Economic Inquiry. 2013;51 (3):1651-1681, https://scholar.harvard.edu/fryer/publications/measuring-crack-cocaine-and-its-impact.

Beer: Haughwout SP & Slater ME, *Apparent Per Capita Alcohol Consumption: National, State, and Regional Trends, 1977-2015.* National Institutes of Health (Apr. 2017).

Poverty rate: US Census Bureau.

Welfare payments: US Dept. of Health and Human Services.

Consumer price index: BLS.

Murder: FBI Uniform Crime Reports.

Homicide, firearm homicide, suicide: Centers of Disease Control, CDC WONDER online data base.

## Appendix C: Dr. Carlisle Moody

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I am Dr. Carlisle E. Moody, Professor of Economics at the College of William & Mary, in Virginia. I graduated from Colby College in 1965 with a major in Economics. I received my graduate training from the University of Connecticut, earning a Master of Economics degree in 1966 and a Ph.D. in Economics in 1970, with fields in mathematical economics and econometrics.

I began my academic career in 1968 as Lecturer in Econometrics at the University of Leeds, Leeds, England. In 1970 I joined the Economics Department at William and Mary as an Assistant Professor, I was promoted to Associate Professor in 1975 and to full Professor in 1989. I was Chair of the Economics Department from 1997-2003. I am still teaching full time at William and Mary. I teach undergraduate and graduate courses in Econometrics, Mathematical Economics, and Time Series Analysis.

I have published over 40 refereed journal articles and several articles in law journals and elsewhere. Nearly all these articles analyze government policies of various sorts. I have been doing research in guns, crime, and gun policy since 2000. I have published 14 articles directly related to guns and gun policy.

I have also consulted for a variety of private and public entities, including the United States Department of Energy, U.S. Government Accountability Office, Washington Consulting Group, Decision Analysis Corporation of Virginia, SAIC Corporation, and the Independence Institute.

In the past five years, I have written export reports, been deposed, or testified at trial in the following matters:

2018-2019
*State ex rel. Hawley v. Choi*, Case No. 16BA CV0258 (Boone Cty. Cir. Ct.), *Barondes v. Choi*, Case No. 16BA CV03144 (Boone Cty. Cir. Ct.) (combined actions, submitted expert report, deposed, have not yet testified).
2018
*Maryland Shall Issue, Inc v. Laurence Hogan et al.* Civil Action No. 16-cv-3311-MJG, US District Court for the District of Maryland (submitted expert report, deposed, did not testify).
*Duncan, et al. v. Becerra, et al.* United States District Court (S.D. Cal.) Case No. 3:17-cv-01017-BEN-JLB, March 26, 2018 (submitted expert report, deposed, did not testify)
2017

*Rocky Mountain Gun Owners v. Hickenlooper*, Dist. Ct., City and County of Denver, Case No. 2013-CV-33897, May 1, 2017 (submitted expert report, deposed, testified).
*William Wiese, et al v. Becerra,* U.S. Dist. Ct., E. Dist. of Cal., Case No. 2:17-cv-00903-WBS-KJN, April 28, 2017 (submitted expert report, not deposed, did not testify).

*Education*

B.A., Colby College, Waterville, Maine, 1965 (Economics)
M.A., University of Connecticut, Storrs, Connecticut, 1966 (Economics)
Ph.D., University of Connecticut, Storrs, Connecticut, 1970 (Economics)

*Experience*

Professor of Economics, College of William and Mary, 1989-
Chair of the Department of Economics, College of William and Mary 1997-2003
Associate Professor of Economics, College of William and Mary, 1975-1989.
Assistant Professor of Economics, College of William and Mary, 1970-1975.
Lecturer in Econometrics, University of Leeds, Leeds, England, 1968-1970.

*Consultant*

Stanford Research Institute
Virginia Marine Resources Commission
U.S. General Accounting Office
U.S. Department of Transportation
U.S. Department of Energy
National Center for State Courts
Oak Ridge National Laboratory
Justec Research.
The Orkand Corporation
Washington Consulting Group
Decision Analysis Corporation of Virginia
SAIC Corporation
West Publishing Group
Independence Institute

*Research and Teaching Fields*

Law and Economics
Econometrics
Time Series Analysis

*Honors*

National Defense Education Act Fellow, University of Connecticut, 1965-1968.

Bredin Fellow, College of William and Mary, 1982.

Member, Methodology Review Panel, Prison Population Forecast, Virginia Department of Planning and Budget, 1987-1993.

Notable Individuals, Micro Computer Industry, 1983.

Speaker, Institute of Medicine and National Research Council Committee of Priorities for a Public Health Research Agenda to Reduce the Threat of Firearm-related Violence, National Academies of Science, Washington, DC, April 23, 2013.

Member, Methodology Review Panel, Prison Population Forecast, Virginia Department of Corrections, 2012-.

*Refereed Publications*

"Do Right to Carry Laws Increase Violent Crime? A Comment on Donohue, Aneja, and Weber," (with T.B. Marvell) *Econ Journal Watch*, 16(1), 2019.

"Is the United States an Outlier in Mass Public Shootings? A Comment on Adam Lankford." (with John Lott) *Econ Journal Watch*, 16(1), 2019.

"Clustering and Standard Error Bias in Fixed Effects Panel Data Regressions," (with T.B. Marvell) *Journal of Quantitative Criminology*, 2018, https://doi.org/10.1007/s10940-018-9383-z.

"The Impact of Right-to-Carry Laws: Critique of the 2014 version of Aneja, Donohue, and Zhang," (with T.B. Marvell) *Econ Journal Watch*, February 2018.

"Firearms and the Decline in Violence in Europe 1201-2010," *Review of European Studies*, 9(2) 2017.

"The Impact of Right-to-Carry Laws on Crime: An Exercise in Replication," (with T.B. Marvell, P.R. Zimmerman and Fisal Alemante) *Review of Economics and Finance*, 4(1) 2014, 33-43.

"Did John Lott Provide Bad Data to the NRC? A Note on Aneja, Donohue, and Zhang," (with J.R. Lott and T.B. Marvell) *Econ Journal Watch*, January 2013.

"On the Choice of Control Variables in the Crime Equation," (with T.B. Marvell) *Oxford Bulletin of Economics and Statistics*, 72(5) 2010, 696-715.

"The Debate on Shall-Issue Laws, Continued," (with T.B. Marvell) *Econ Journal Watch*, 6(2) March 2009, 203-217.

"The Debate on Shall-Issue Laws," (with T.B. Marvell) *Econ Journal Watch*, 5(3) September 2008, 269-293.

"Can and Should Criminology Research Influence Policy? Suggestions for Time-Series Cross-Section Studies" (with T.B. Marvell) *Criminology and Public Policy* 7(1) August, 2008, 359-364.

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## Appendix D: Difference in difference analyses for Tables 1 and 3

This is the Stata log file reporting both the Stata commands and the resulting output. This log file was used to generate Table 1 (Missouri/Kansas homicide) and Table 3 (Conn./R.I. homicide).

```
-----------------------------------------------------------------------------------------
-----------------------------------------------------------------------------------------
--------------------------------------------------------------------------
      name:  <unnamed>
       log:  C:\Users\cemood\Box Sync\Illinois\report\permit.log
  log type:  text
 opened on:  11 May 2019, 18:00:24

.
. keep if year>=1960
(0 observations deleted)

. keep if year<=2017
(0 observations deleted)

. preserve

.
. /* MO v KS PTP law */
. keep if stnm=="MO" | stnm=="KS"
(2,793 observations deleted)

. gen DP=(stnm=="MO")

. gen D2=(year>2007)

. gen DD=DP*D2

. * The following produces Figure 2
. twoway (scatter crmurpc year), xtick(1960(10)2020) xmtick(1965(5)2015) xline(2007)

.
. regress crmurpc DP D2 DD

      Source |       SS           df       MS      Number of obs   =       114
-------------+----------------------------------   F(3, 110)       =     43.96
       Model |  326.255237         3  108.751746   Prob > F        =    0.0000
    Residual |  272.151105       110  2.47410096   R-squared       =    0.5452
-------------+----------------------------------   Adj R-squared   =    0.5328
       Total |  598.406342       113  5.29563135   Root MSE        =    1.5729

------------------------------------------------------------------------------
     crmurpc |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
          DP |   3.302456   .3210725    10.29   0.000     2.666166    3.938747
          D2 |  -1.019124   .5713527    -1.78   0.077    -2.151411    .1131632
          DD |  -.0223711   .8080147    -0.03   0.978    -1.623667    1.578924
```

```
       _cons |   4.804491   .2270325    21.16   0.000     4.354566    5.254416
------------------------------------------------------------------------------

. predict yhat
(option xb assumed; fitted values)

. label var year “Year”

* This command produces Figure 3
. twoway (scatter crmurpc year)(line yhat year), xtick(1960(10)2020)
xmtick(1965(5)2015) xline(2007)

. sort state year

. list state stnm year crmurpc

     +---------------------------------+
     | state   stnm   year     crmurpc |
     |---------------------------------|
  1. |    17     KS   1960    2.931745 |
  2. |    17     KS   1961    1.851016 |
  3. |    17     KS   1962    2.823846 |
  4. |    17     KS   1963    2.571042 |
  5. |    17     KS   1964    3.395201 |
     |---------------------------------|
  6. |    17     KS   1965    2.719855 |
  7. |    17     KS   1966    3.545455 |
  8. |    17     KS   1967    4.096495 |
  9. |    17     KS   1968    3.880867 |
 10. |    17     KS   1969     3.62254 |
     |---------------------------------|
 11. |    17     KS   1970    4.759027 |
 12. |    17     KS   1971    5.119163 |
 13. |    17     KS   1972    4.034061 |
 14. |    17     KS   1973    6.049674 |
 15. |    17     KS   1974     6.92217 |
     |---------------------------------|
 16. |    17     KS   1975     5.35335 |
 17. |    17     KS   1976    4.523919 |
 18. |    17     KS   1977    6.600099 |
 19. |    17     KS   1978     5.70047 |
 20. |    17     KS   1979    5.537323 |
     |---------------------------------|
 21. |    17     KS   1980    6.880427 |
 22. |    17     KS   1981    6.331611 |
 23. |    17     KS   1982      5.7471 |
 24. |    17     KS   1983    5.671617 |
 25. |    17     KS   1984    3.671502 |
     |---------------------------------|
 26. |    17     KS   1985    4.984733 |
 27. |    17     KS   1986    4.439665 |
 28. |    17     KS   1987    4.498266 |
 29. |    17     KS   1988    3.452496 |
 30. |    17     KS   1989    5.580612 |
     |---------------------------------|
```

| | | | | |
|---|---|---|---|---|
| 31. | 17 | KS | 1990 | 3.949465 |
| 32. | 17 | KS | 1991 | 6.12313 |
| 33. | 17 | KS | 1992 | 5.962737 |
| 34. | 17 | KS | 1993 | 6.297557 |
| 35. | 17 | KS | 1994 | 6.587837 |
| 36. | 17 | KS | 1995 | 6.113017 |
| 37. | 17 | KS | 1996 | 6.502065 |
| 38. | 17 | KS | 1997 | 5.691969 |
| 39. | 17 | KS | 1998 | 6.502297 |
| 40. | 17 | KS | 1999 | 5.973854 |
| 41. | 17 | KS | 2000 | 6.273942 |
| 42. | 17 | KS | 2001 | 3.404681 |
| 43. | 17 | KS | 2002 | 2.874479 |
| 44. | 17 | KS | 2003 | 4.590518 |
| 45. | 17 | KS | 2004 | 4.461718 |
| 46. | 17 | KS | 2005 | 3.679016 |
| 47. | 17 | KS | 2006 | 4.524181 |
| 48. | 17 | KS | 2007 | 3.807765 |
| 49. | 17 | KS | 2008 | 4.024107 |
| 50. | 17 | KS | 2009 | 4.412745 |
| 51. | 17 | KS | 2010 | 3.393003 |
| 52. | 17 | KS | 2011 | 3.867708 |
| 53. | 17 | KS | 2012 | 2.944966 |
| 54. | 17 | KS | 2013 | 4.041967 |
| 55. | 17 | KS | 2014 | 3.169674 |
| 56. | 17 | KS | 2015 | 4.396146 |
| 57. | 17 | KS | 2016 | 3.81799 |
| 58. | 26 | MO | 1960 | 4.368932 |
| 59. | 26 | MO | 1961 | 5.127615 |
| 60. | 26 | MO | 1962 | 5.531329 |
| 61. | 26 | MO | 1963 | 5.077414 |
| 62. | 26 | MO | 1964 | 5.402972 |
| 63. | 26 | MO | 1965 | 6.715917 |
| 64. | 26 | MO | 1966 | 5.416759 |
| 65. | 26 | MO | 1967 | 7.424543 |
| 66. | 26 | MO | 1968 | 8.931699 |
| 67. | 26 | MO | 1969 | 10.45259 |
| 68. | 26 | MO | 1970 | 10.6474 |
| 69. | 26 | MO | 1971 | 8.977375 |
| 70. | 26 | MO | 1972 | 8.331005 |
| 71. | 26 | MO | 1973 | 8.943032 |
| 72. | 26 | MO | 1974 | 9.73779 |
| 73. | 26 | MO | 1975 | 10.53057 |
| 74. | 26 | MO | 1976 | 9.183793 |
| 75. | 26 | MO | 1977 | 9.535047 |
| 76. | 26 | MO | 1978 | 10.36673 |

```
 77. |    26     MO   1979   11.10562 |
 78. |    26     MO   1980   11.05249 |
 79. |    26     MO   1981   10.46212 |
 80. |    26     MO   1982   9.717124 |
     |--------------------------------|
 81. |    26     MO   1983   8.151745 |
 82. |    26     MO   1984    7.19557 |
 83. |    26     MO   1985   8.179585 |
 84. |    26     MO   1986   9.237383 |
 85. |    26     MO   1987   8.365148 |
     |--------------------------------|
 86. |    26     MO   1988   8.127153 |
 87. |    26     MO   1989   8.026188 |
 88. |    26     MO   1990   8.754348 |
 89. |    26     MO   1991   10.50128 |
 90. |    26     MO   1992   10.48475 |
     |--------------------------------|
 91. |    26     MO   1993   11.19295 |
 92. |    26     MO   1994   10.40474 |
 93. |    26     MO   1995   8.720313 |
 94. |    26     MO   1996   7.971937 |
 95. |    26     MO   1997   7.772031 |
     |--------------------------------|
 96. |    26     MO   1998    7.22595 |
 97. |    26     MO   1999   6.454573 |
 98. |    26     MO   2000   6.188378 |
 99. |    26     MO   2001   6.594409 |
100. |    26     MO   2002   5.832779 |
     |--------------------------------|
101. |    26     MO   2003   5.061825 |
102. |    26     MO   2004   6.158941 |
103. |    26     MO   2005   6.942646 |
104. |    26     MO   2006   6.315569 |
105. |    26     MO   2007   6.233427 |
     |--------------------------------|
106. |    26     MO   2008   7.697611 |
107. |    26     MO   2009   6.492104 |
108. |    26     MO   2010   7.004609 |
109. |    26     MO   2011   6.089255 |
110. |    26     MO   2012   6.472526 |
     |--------------------------------|
111. |    26     MO   2013   6.105523 |
112. |    26     MO   2014   6.662459 |
113. |    26     MO   2015   8.251595 |
114. |    26     MO   2016   8.813393 |
     +--------------------------------+

. scalar drop _all

. summarize crmurpc if stnm==“MO” & year<=2007

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
     crmurpc |         48    8.106948    1.943318   4.368932   11.19295
```

```
. scalar p1=r(mean)

. summarize crmurpc if stnm==“MO” & year>2007

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
     crmurpc |          9    7.065453    .9735679   6.089255   8.813393

. scalar p2=r(mean)

. summarize crmurpc if stnm==“KS” & year<=2007

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
     crmurpc |         48    4.804491     1.34431   1.851016    6.92217

. scalar s1=r(mean)

. summarize crmurpc if stnm==“KS” & year>2007

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
     crmurpc |          9    3.785367    .5167979   2.944966   4.412745

. scalar s2=r(mean)

. scalar dd=(p2-p1)-(s2-s1)

. scalar diffp=p2-p1

. scalar diffs=s2-s1

. scalar dd2=diffp-diffs
```

```
. scalar list
       dd2 = -.02237109
     diffs = -1.0191237
     diffp = -1.0414948
        dd = -.02237109
        s2 =  3.7853675
        s1 =  4.8044911
        p2 =  7.0654528
        p1 =  8.1069476
```

```
.
. regress homrate DP D2 DD

      Source |       SS           df       MS      Number of obs   =       114
-------------+----------------------------------   F(3, 110)       =     51.70
       Model |  402.704976         3  134.234992   Prob > F        =    0.0000
    Residual |  285.593253       110   2.5963023   R-squared       =    0.5851
-------------+----------------------------------   Adj R-squared   =    0.5738
       Total |  688.298229       113  6.09113477   Root MSE        =    1.6113

------------------------------------------------------------------------------
```

```
     homrate |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
          DP |   3.743466   .3289062    11.38   0.000     3.091651    4.395281
          D2 |  -.7400548   .5852928    -1.26   0.209    -1.899968    .4198581
          DD |  -.2435934    .827729    -0.29   0.769    -1.883958    1.396771
       _cons |   4.880199   .2325718    20.98   0.000     4.419297    5.341102
------------------------------------------------------------------------------

. regress gunhomrate DP D2 DD

      Source |       SS           df       MS      Number of obs   =       114
-------------+----------------------------------   F(3, 110)       =     44.59
       Model |  223.026014         3  74.3420046   Prob > F        =    0.0000
    Residual |  183.383696       110  1.66712451   R-squared       =    0.5488
-------------+----------------------------------   Adj R-squared   =    0.5365
       Total |   406.40971       113  3.59654611   Root MSE        =    1.2912

------------------------------------------------------------------------------
  gunhomrate |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
          DP |   2.761091   .2635593    10.48   0.000     2.238779    3.283404
          D2 |  -.2672691   .4690073    -0.57   0.570    -1.196731    .6621932
          DD |   .2073885   .6632764     0.31   0.755     -1.10707    1.521847
       _cons |   3.146629   .1863646    16.88   0.000     2.777298     3.51596
------------------------------------------------------------------------------

.
.
. restore

. preserve

.
. /* CT v RI */
. keep if stnm==“RI” | stnm==“CT”
(2,793 observations deleted)

. gen DP=(stnm==“CT”)

. gen D2=(year>1995)

. gen DD=DP*D2

. regress crmurpc DP D2 DD

      Source |       SS           df       MS      Number of obs   =       114
-------------+----------------------------------   F(3, 110)       =      4.03
       Model |   14.770001         3  4.92333366   Prob > F        =    0.0093
    Residual |  134.504403       110   1.2227673   R-squared       =    0.0989
-------------+----------------------------------   Adj R-squared   =    0.0744
       Total |  149.274404       113  1.32101242   Root MSE        =    1.1058

------------------------------------------------------------------------------
     crmurpc |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
```

```
          DP |   .7464454   .2606368     2.86   0.005     .2299245    1.262966
          D2 |  -.1658661   .3036326    -0.55   0.586    -.7675947    .4358625
          DD |  -.3595353   .4294014    -0.84   0.404    -1.210508    .4914375
       _cons |   3.009627    .184298    16.33   0.000     2.644391    3.374862
------------------------------------------------------------------------------

. regress homrate DP D2 DD

      Source |       SS           df       MS      Number of obs   =       114
-------------+----------------------------------   F(3, 110)       =      4.67
       Model |  17.7185327         3  5.90617756   Prob > F        =    0.0041
    Residual |  138.999512       110  1.26363193   R-squared       =    0.1131
-------------+----------------------------------   Adj R-squared   =    0.0889
       Total |  156.718045       113  1.38688535   Root MSE        =    1.1241

------------------------------------------------------------------------------
     homrate |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
          DP |   .5024285   .2649562     1.90   0.061    -.0226524    1.027509
          D2 |  -.6476528   .3086646    -2.10   0.038    -1.259354    -.035952
          DD |   .1458689   .4365177     0.33   0.739    -.7192066    1.010944
       _cons |   3.513616   .1873523    18.75   0.000     3.142327    3.884904
------------------------------------------------------------------------------

. regress gunhomrate DP D2 DD

      Source |       SS           df       MS      Number of obs   =       114
-------------+----------------------------------   F(3, 110)       =      7.34
       Model |  13.7455514         3  4.58185046   Prob > F        =    0.0002
    Residual |  68.6200132       110  .623818302   R-squared       =    0.1669
-------------+----------------------------------   Adj R-squared   =    0.1442
       Total |  82.3655646       113  .728898802   Root MSE        =    .78982

------------------------------------------------------------------------------
  gunhomrate |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
          DP |   .7227051   .1861628     3.88   0.000     .3537742    1.091636
          D2 |  -.0010239    .216873    -0.00   0.996    -.4308154    .4287675
          DD |   -.083057   .3067048    -0.27   0.787    -.6908739      .52476
       _cons |   1.595893   .1316369    12.12   0.000      1.33502    1.856767
------------------------------------------------------------------------------

.
. summarize gunhomrate if stnm==“CT” & year<=1995

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
  gunhomrate |         36    2.318598    1.092741   .5027069   4.502633

. scalar p1=r(mean)

. summarize gunhomrate if stnm==“CT” & year>1995

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
```

```
  gunhomrate |         21    2.234517    .5457846    1.53784   3.566414

. scalar p2=r(mean)

. summarize gunhomrate if stnm==“RI” & year<=1995

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
  gunhomrate |         36    1.595893    .7017961   .1165501   2.659805

. scalar s1=r(mean)

. summarize gunhomrate if stnm==“RI” & year>1995

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
  gunhomrate |         21    1.594869    .4261089     .94732   2.570772

. scalar s2=r(mean)

. scalar dd=(p2-p1)-(s2-s1)

. scalar diffp=p2-p1

. scalar diffs=s2-s1

. scalar dd2=diffp-diffs

. scalar pctdd=dd/p1

. scalar list
     pctdd = -.03582205
       dd2 = -.08305696
     diffs = -.00102394
     diffp = -.08408089
        dd = -.08305696
        s2 =  1.5948694
        s1 =  1.5958933
        p2 =  2.2345175
        p1 =  2.3185984

.
.
. log close
      name:  <unnamed>
       log:  C:\Users\cemood\Box Sync\Illinois\report\permit.log
  log type:  text
 closed on:  11 May 2019, 18:00:25
-------------------------------------------------------------------------------------
-------------------------------------------------------------------------------------
-----------------------------------------------------------------------
```

## Appendix E: Fixed effects models used to generate Tables 2 and 4

This is the Stata log file reporting both the Stata commands and the resulting output. This log file was used to generate Table 2 (effects of all six state changes in PTP laws on homicide).

```
These are the calculations
--------------------------------------------------------------------------------------
--------------------------------------------------------------------------------------
---------------------------------------------------------------------------
      name:  <unnamed>
       log:  C:\Users\cemood\Box Sync\Illinois\report\permit.fe.log
  log type:  text
 opened on:  11 May 2019, 18:02:23

. scalar drop _all;

. set matsize 1000;

. tsset state year;
       panel variable:  state (strongly balanced)
        time variable:  year, 1960 to 2016
                delta:  1 unit

. replace permithg=1 if stnm==“MD” & year>2013;
(0 real changes made)

. replace permithg=1 if stnm==“MO” & year<=2007;
(22 real changes made)

. gen PTP=permithg;

. /* state and year dummies */
> quietly: tab year, gen(yrdum);

. quietly: tab state, gen(stdum);

. replace employ=employ/poptot;
(2,448 real changes made)

. replace empconpc=100*empcon/poptot;
(2,448 real changes made)

. replace empmilpc=100*empmil/poptot;
(2,448 real changes made)

. label var rwelfare “Real welfare per capita”;

. label var pp1519 “Percent Population 15-19”;

. label var pp2024 “Percent Population 20-24”;
```

```
. label var pp2529 “Percent Population 25-29”;

. label var pp3034 “Percent Population 30-34”;

. label var pp3539 “Percent Population 35-39”;

. label var pp4044 “Percent Population 40-44”;

. label var pp4549 “Percent Population 45-49”;

. label var pp5054 “Percent Population 50-54”;

. label var pp5559 “Percent Population 55-59”;

.  label var pp6064 “Percent Population 60-64”;

. label var pp6500 “Percent Population 65+”;

. /* murder per capita */
>
> regress crmurpc
> PTP
> trend
> density
> crack beerpc
> L.prisonpc L.policepc
> incomepc rwelfarepc povrate pctblack
> unrate employ empmilpc empconpc
> syg shall snsban
> pp*
> L.crmurpc L2.crmurpc
> yrdum* stdum*
> , robust cluster(state);
note: yrdum1 omitted because of collinearity
note: yrdum2 omitted because of collinearity
note: yrdum3 omitted because of collinearity
note: yrdum4 omitted because of collinearity
note: yrdum5 omitted because of collinearity
note: yrdum6 omitted because of collinearity
note: yrdum7 omitted because of collinearity
note: yrdum8 omitted because of collinearity
note: yrdum9 omitted because of collinearity
note: yrdum10 omitted because of collinearity
note: yrdum11 omitted because of collinearity
note: yrdum56 omitted because of collinearity
note: yrdum57 omitted because of collinearity
note: stdum9 omitted because of collinearity

Linear regression                               Number of obs     =      2,332
                                                F(49, 50)         =          .
                                                Prob > F          =          .
                                                R-squared         =     0.9597
                                                Root MSE          =     1.2526

                                  (Std. Err. adjusted for 51 clusters in state)
```

| crmurpc | Coef. | Robust Std. Err. | t | P>\|t\| | [95% Conf. | Interval] |
|---|---|---|---|---|---|---|
| **PTP** | **.1107306** | **.1796722** | **0.62** | **0.540** | -.2501516 | .4716128 |
| trend | -.0065349 | .0230758 | -0.28 | 0.778 | -.0528841 | .0398142 |
| density | -.000289 | .0009783 | -0.30 | 0.769 | -.0022541 | .0016761 |
| crack | -.2104416 | .3193952 | -0.66 | 0.513 | -.8519657 | .4310825 |
| beerpc | -.000435 | .0090109 | -0.05 | 0.962 | -.0185339 | .017664 |
| prisonpc | | | | | | |
| L1. | .0007448 | .0016323 | 0.46 | 0.650 | -.0025337 | .0040234 |
| policepc | | | | | | |
| L1. | .0035114 | .0026835 | 1.31 | 0.197 | -.0018786 | .0089014 |
| incomepc | -.0882737 | .0703839 | -1.25 | 0.216 | -.2296441 | .0530966 |
| rwelfarepc | -4.897233 | 2.900179 | -1.69 | 0.098 | -10.72241 | .9279477 |
| povrate | .019689 | .0154221 | 1.28 | 0.208 | -.0112873 | .0506652 |
| pctblack | .0441891 | .0256297 | 1.72 | 0.091 | -.0072897 | .0956678 |
| unrate | -.0602787 | .0309128 | -1.95 | 0.057 | -.1223689 | .0018114 |
| employ | 11.02152 | 6.558695 | 1.68 | 0.099 | -2.152012 | 24.19504 |
| empmilpc | 21.10345 | 301.6255 | 0.07 | 0.945 | -584.7293 | 626.9362 |
| empconpc | -75.20837 | 474.0648 | -0.16 | 0.875 | -1027.396 | 876.9789 |
| syg | .3360771 | .1781683 | 1.89 | 0.065 | -.0217844 | .6939387 |
| shall | .0024793 | .1217041 | 0.02 | 0.984 | -.2419706 | .2469292 |
| snsban | -.0693576 | .1475308 | -0.47 | 0.640 | -.365682 | .2269667 |
| pp1519 | -.0930286 | .110737 | -0.84 | 0.405 | -.3154504 | .1293931 |
| pp2024 | .1087729 | .0957626 | 1.14 | 0.261 | -.083572 | .3011177 |
| pp2529 | -.2135979 | .1511154 | -1.41 | 0.164 | -.5171221 | .0899264 |
| pp3034 | -.2766986 | .2347771 | -1.18 | 0.244 | -.7482622 | .1948651 |
| pp3539 | .1237191 | .2962628 | 0.42 | 0.678 | -.4713422 | .7187805 |
| pp4044 | -.210354 | .3287872 | -0.64 | 0.525 | -.8707426 | .4500345 |
| pp4549 | .1131298 | .28318 | 0.40 | 0.691 | -.4556539 | .6819136 |
| pp5054 | -.1297812 | .2423689 | -0.54 | 0.595 | -.6165935 | .3570312 |
| pp5559 | .033805 | .256096 | 0.13 | 0.896 | -.480579 | .5481889 |
| pp6064 | -.3273787 | .1907053 | -1.72 | 0.092 | -.7104216 | .0556643 |
| pp6500 | .0888445 | .0910688 | 0.98 | 0.334 | -.0940725 | .2717615 |
| crmurpc | | | | | | |
| L1. | .7400668 | .1145166 | 6.46 | 0.000 | .5100535 | .9700801 |
| L2. | .0837225 | .1042355 | 0.80 | 0.426 | -.1256406 | .2930857 |
| yrdum1 | 0 | (omitted) | | | | |
| yrdum2 | 0 | (omitted) | | | | |
| yrdum3 | 0 | (omitted) | | | | |
| yrdum4 | 0 | (omitted) | | | | |
| yrdum5 | 0 | (omitted) | | | | |
| yrdum6 | 0 | (omitted) | | | | |
| yrdum7 | 0 | (omitted) | | | | |
| yrdum8 | 0 | (omitted) | | | | |
| yrdum9 | 0 | (omitted) | | | | |
| yrdum10 | 0 | (omitted) | | | | |
| yrdum11 | 0 | (omitted) | | | | |
| yrdum12 | .4316705 | .4182015 | 1.03 | 0.307 | -.408312 | 1.271653 |

| | | | | | | |
|---|---|---|---|---|---|---|
| yrdum13 | .2650826 | .3330941 | 0.80 | 0.430 | -.4039567 | .9341218 |
| yrdum14 | .3552591 | .4467982 | 0.80 | 0.430 | -.5421616 | 1.25268 |
| yrdum15 | .8070703 | .4600301 | 1.75 | 0.085 | -.1169273 | 1.731068 |
| yrdum16 | .4243925 | .509118 | 0.83 | 0.408 | -.5982011 | 1.446986 |
| yrdum17 | -.4482468 | .5078935 | -0.88 | 0.382 | -1.468381 | .5718873 |
| yrdum18 | .3011217 | .6240287 | 0.48 | 0.632 | -.9522767 | 1.55452 |
| yrdum19 | .1831121 | .4218493 | 0.43 | 0.666 | -.6641973 | 1.030421 |
| yrdum20 | .4619671 | .4509048 | 1.02 | 0.311 | -.4437018 | 1.367636 |
| yrdum21 | .6599118 | .5505456 | 1.20 | 0.236 | -.4458916 | 1.765715 |
| yrdum22 | .3398106 | .623775 | 0.54 | 0.588 | -.9130783 | 1.5927 |
| yrdum23 | -.0372525 | .5362101 | -0.07 | 0.945 | -1.114262 | 1.039757 |
| yrdum24 | -.3790462 | .6220223 | -0.61 | 0.545 | -1.628415 | .8703224 |
| yrdum25 | -.4633126 | .4678951 | -0.99 | 0.327 | -1.403108 | .4764824 |
| yrdum26 | .0574612 | .4534433 | 0.13 | 0.900 | -.8533065 | .968229 |
| yrdum27 | .661317 | .5989478 | 1.10 | 0.275 | -.5417049 | 1.864339 |
| yrdum28 | -.0304216 | .5783853 | -0.05 | 0.958 | -1.192143 | 1.1313 |
| yrdum29 | .487414 | 1.051331 | 0.46 | 0.645 | -1.624246 | 2.599074 |
| yrdum30 | .4421359 | .8134445 | 0.54 | 0.589 | -1.191716 | 2.075987 |
| yrdum31 | .5094704 | .6132968 | 0.83 | 0.410 | -.7223725 | 1.741313 |
| yrdum32 | .5045509 | .581241 | 0.87 | 0.390 | -.6629061 | 1.672008 |
| yrdum33 | -.4623548 | .3313256 | -1.40 | 0.169 | -1.127842 | .2031322 |
| yrdum34 | -.0020258 | .3134583 | -0.01 | 0.995 | -.6316254 | .6275738 |
| yrdum35 | -.8107222 | .4818944 | -1.68 | 0.099 | -1.778636 | .1571913 |
| yrdum36 | -1.075314 | .509632 | -2.11 | 0.040 | -2.09894 | -.0516881 |
| yrdum37 | -1.167703 | .4084272 | -2.86 | 0.006 | -1.988053 | -.347353 |
| yrdum38 | -1.686625 | .7907771 | -2.13 | 0.038 | -3.274947 | -.0983019 |
| yrdum39 | -1.707834 | .6777549 | -2.52 | 0.015 | -3.069145 | -.3465238 |
| yrdum40 | -1.911168 | .7055462 | -2.71 | 0.009 | -3.328299 | -.4940366 |
| yrdum41 | -1.884441 | .6979362 | -2.70 | 0.009 | -3.286287 | -.4825948 |
| yrdum42 | -1.525502 | .7015988 | -2.17 | 0.034 | -2.934705 | -.1162992 |
| yrdum43 | -1.388099 | .5711726 | -2.43 | 0.019 | -2.535333 | -.2408647 |
| yrdum44 | -1.094386 | .4774674 | -2.29 | 0.026 | -2.053407 | -.135364 |
| yrdum45 | -1.267906 | .4569444 | -2.77 | 0.008 | -2.185705 | -.3501057 |
| yrdum46 | -1.090705 | .4287214 | -2.54 | 0.014 | -1.951817 | -.2295926 |
| yrdum47 | -1.189334 | .5398107 | -2.20 | 0.032 | -2.273576 | -.1050923 |
| yrdum48 | -1.159755 | .5598686 | -2.07 | 0.043 | -2.284284 | -.0352257 |
| yrdum49 | -1.214768 | .5039153 | -2.41 | 0.020 | -2.226912 | -.2026244 |
| yrdum50 | -.6726128 | .2488975 | -2.70 | 0.009 | -1.172538 | -.1726875 |
| yrdum51 | -.3286735 | .1795984 | -1.83 | 0.073 | -.6894076 | .0320605 |
| yrdum52 | .0434461 | .2166039 | 0.20 | 0.842 | -.3916155 | .4785078 |
| yrdum53 | -.2497565 | .1585859 | -1.57 | 0.122 | -.5682856 | .0687726 |
| yrdum54 | -.4018574 | .183406 | -2.19 | 0.033 | -.7702391 | -.0334757 |
| yrdum55 | -.4290554 | .1500631 | -2.86 | 0.006 | -.730466 | -.1276448 |
| yrdum56 | 0 | (omitted) | | | | |
| yrdum57 | 0 | (omitted) | | | | |
| stdum1 | .4375947 | 15.41543 | 0.03 | 0.977 | -30.5252 | 31.40039 |
| stdum2 | .7643054 | 16.07301 | 0.05 | 0.962 | -31.51928 | 33.04789 |
| stdum3 | .5604264 | 15.60988 | 0.04 | 0.972 | -30.79293 | 31.91379 |
| stdum4 | .0623129 | 15.34592 | 0.00 | 0.997 | -30.76087 | 30.8855 |
| stdum5 | 1.934664 | 16.00804 | 0.12 | 0.904 | -30.21842 | 34.08775 |
| stdum6 | -.2843766 | 15.48579 | -0.02 | 0.985 | -31.3885 | 30.81974 |
| stdum7 | .0139775 | 14.96643 | 0.00 | 0.999 | -30.04698 | 30.07493 |
| stdum8 | -1.293916 | 14.99341 | -0.09 | 0.932 | -31.40906 | 28.82123 |
| stdum9 | 0 | (omitted) | | | | |
| stdum10 | .1504445 | 15.01099 | 0.01 | 0.992 | -30.00002 | 30.30091 |

```
     stdum11 |   .3414011   15.35793     0.02   0.982     -30.5059    31.18871
     stdum12 |  -.2581782   15.60785    -0.02   0.987    -31.60748    31.09112
     stdum13 |  -.8953226    15.4173    -0.06   0.954    -31.86188    30.07123
     stdum14 |   .6750224   15.50948     0.04   0.965    -30.47669    31.82674
     stdum15 |   .1745729   15.31603     0.01   0.991    -30.58859    30.93773
     stdum16 |  -1.436564   15.19192    -0.09   0.925    -31.95044    29.07731
     stdum17 |  -1.109973   15.03115    -0.07   0.941    -31.30093    29.08098
     stdum18 |   .7499463   15.72683     0.05   0.962    -30.83833    32.33822
     stdum19 |    1.11672   15.38154     0.07   0.942      -29.778    32.01144
     stdum20 |  -.4937893   15.71283    -0.03   0.975    -32.05393    31.06635
     stdum21 |   .7206283   15.21061     0.05   0.962    -29.83078    31.27204
     stdum22 |   .0947439   15.10166     0.01   0.995    -30.23783    30.42732
     stdum23 |   1.462145   15.79588     0.09   0.927    -30.26481     33.1891
     stdum24 |  -.6808072   15.51981    -0.04   0.965    -31.85326    30.49165
     stdum25 |   .4232835   15.49691     0.03   0.978    -30.70318    31.54975
     stdum26 |  -.3057119    15.0378    -0.02   0.984    -30.51003     29.8986
     stdum27 |  -1.150757    15.5066    -0.07   0.941    -32.29668    29.99516
     stdum28 |  -1.739285   15.09536    -0.12   0.909    -32.05921    28.58064
     stdum29 |    .041669   15.39733     0.00   0.998    -30.88477    30.96811
     stdum30 |  -1.033056   15.75942    -0.07   0.948    -32.68679    30.62067
     stdum31 |   .4205328   14.67031     0.03   0.977    -29.04565    29.88672
     stdum32 |   1.211335   16.11951     0.08   0.940    -31.16566    33.58833
     stdum33 |   1.303844   15.60515     0.08   0.934    -30.04002     32.6477
     stdum34 |  -.1103259   15.22813    -0.01   0.994    -30.69692    30.47627
     stdum35 |  -2.456517   15.23258    -0.16   0.873    -33.05205    28.13902
     stdum36 |   .3303549   15.43362     0.02   0.983    -30.66898    31.32969
     stdum37 |  -.0852764   15.11884    -0.01   0.996    -30.45237    30.28181
     stdum38 |   .0201567   15.69293     0.00   0.999    -31.50002    31.54033
     stdum39 |   .5913524   15.55885     0.04   0.970    -30.65952    31.84223
     stdum40 |  -.0092342   14.97105    -0.00   1.000    -30.07946      30.061
     stdum41 |   .0972719   15.28874     0.01   0.995    -30.61106    30.80561
     stdum42 |  -2.265866   15.03429    -0.15   0.881    -32.46312    27.93139
     stdum43 |   .1387373   15.28842     0.01   0.993    -30.56897    30.84644
     stdum44 |   .6568452    15.5076     0.04   0.966    -30.49108    31.80477
     stdum45 |  -.8233415   15.49754    -0.05   0.958    -31.95106    30.30438
     stdum46 |  -1.327974   15.62992    -0.08   0.933    -32.72158    30.06564
     stdum47 |  -.0886737   15.33551    -0.01   0.995    -30.89096    30.71361
     stdum48 |   .7102131   16.04433     0.04   0.965    -31.51577     32.9362
     stdum49 |   1.224527   16.26061     0.08   0.940    -31.43586    33.88492
     stdum50 |  -.5977885   15.49482    -0.04   0.969    -31.72005    30.52447
     stdum51 |  -2.207395   15.20172    -0.15   0.885    -32.74096    28.32617
       _cons |   .7473899   17.87626     0.04   0.967    -35.15813    36.65291
------------------------------------------------------------------------------

. /* gun homicide rate */
>
> regress gunhomrate
> PTP
> trend
> density
> crack beerpc
> L.prisonpc L.policepc
> incomepc rwelfarepc povrate pctblack
> unrate employ empmilpc empconpc
> pp*
```

```
> L.gunhomrate L2.gunhomrate
> yrdum* stdum*
> , robust cluster(state);
note: yrdum1 omitted because of collinearity
note: yrdum2 omitted because of collinearity
note: yrdum3 omitted because of collinearity
note: yrdum4 omitted because of collinearity
note: yrdum5 omitted because of collinearity
note: yrdum6 omitted because of collinearity
note: yrdum7 omitted because of collinearity
note: yrdum8 omitted because of collinearity
note: yrdum9 omitted because of collinearity
note: yrdum10 omitted because of collinearity
note: yrdum11 omitted because of collinearity
note: yrdum56 omitted because of collinearity
note: yrdum57 omitted because of collinearity
note: stdum33 omitted because of collinearity

Linear regression                               Number of obs     =      2,313
                                                F(49, 50)         =          .
                                                Prob > F          =          .
                                                R-squared         =     0.9572
                                                Root MSE          =     .89724

                               (Std. Err. adjusted for 51 clusters in state)
```

| gunhomrate | Coef. | Robust Std. Err. | t | P>\|t\| | [95% Conf. | Interval] |
|---|---|---|---|---|---|---|
| PTP | **.0752779** | **.1273874** | **0.59** | **0.557** | -.1805872 | .3311431 |
| trend | .0141212 | .0120485 | 1.17 | 0.247 | -.010079 | .0383213 |
| density | -.000228 | .0005882 | -0.39 | 0.700 | -.0014095 | .0009534 |
| crack | -.1287401 | .2182084 | -0.59 | 0.558 | -.5670246 | .3095444 |
| beerpc | -.0031681 | .0057075 | -0.56 | 0.581 | -.0146319 | .0082957 |
| prisonpc | | | | | | |
| L1. | .0004658 | .0011228 | 0.41 | 0.680 | -.0017894 | .002721 |
| policepc | | | | | | |
| L1. | .0026651 | .0021179 | 1.26 | 0.214 | -.001589 | .0069191 |
| incomepc | -.0758751 | .0519236 | -1.46 | 0.150 | -.1801667 | .0284164 |
| rwelfarepc | -3.984223 | 2.123619 | -1.88 | 0.066 | -8.249637 | .2811903 |
| povrate | .0190885 | .0117007 | 1.63 | 0.109 | -.0044131 | .04259 |
| pctblack | .02234 | .0149734 | 1.49 | 0.142 | -.0077349 | .0524149 |
| unrate | -.0485981 | .0220062 | -2.21 | 0.032 | -.0927989 | -.0043972 |
| employ | 6.137754 | 5.007113 | 1.23 | 0.226 | -3.919328 | 16.19484 |
| empmilpc | -191.6265 | 343.0634 | -0.56 | 0.579 | -880.6895 | 497.4365 |
| empconpc | 469.638 | 688.5074 | 0.68 | 0.498 | -913.2699 | 1852.546 |
| pp1519 | -.0938747 | .0659299 | -1.42 | 0.161 | -.2262987 | .0385493 |
| pp2024 | .127589 | .0884199 | 1.44 | 0.155 | -.0500076 | .3051857 |
| pp2529 | -.1821892 | .1369003 | -1.33 | 0.189 | -.4571616 | .0927832 |
| pp3034 | -.1332713 | .145469 | -0.92 | 0.364 | -.4254544 | .1589118 |
| pp3539 | .0287709 | .1716502 | 0.17 | 0.868 | -.3159986 | .3735405 |
| pp4044 | -.0512054 | .2084604 | -0.25 | 0.807 | -.4699103 | .3674996 |

| | | | | | | |
|---|---|---|---|---|---|---|
| pp4549 | -.0450881 | .1645901 | -0.27 | 0.785 | -.375677 | .2855008 |
| pp5054 | -.0270562 | .1696047 | -0.16 | 0.874 | -.3677172 | .3136048 |
| pp5559 | -.0411975 | .1879792 | -0.22 | 0.827 | -.4187647 | .3363697 |
| pp6064 | -.1762127 | .1476864 | -1.19 | 0.238 | -.4728494 | .1204241 |
| pp6500 | .0617747 | .0587038 | 1.05 | 0.298 | -.0561353 | .1796847 |
| | | | | | | |
| gunhomrate | | | | | | |
| L1. | .8287941 | .1419823 | 5.84 | 0.000 | .5436143 | 1.113974 |
| L2. | .0168314 | .1427107 | 0.12 | 0.907 | -.2698116 | .3034743 |
| | | | | | | |
| yrdum1 | 0 | (omitted) | | | | |
| yrdum2 | 0 | (omitted) | | | | |
| yrdum3 | 0 | (omitted) | | | | |
| yrdum4 | 0 | (omitted) | | | | |
| yrdum5 | 0 | (omitted) | | | | |
| yrdum6 | 0 | (omitted) | | | | |
| yrdum7 | 0 | (omitted) | | | | |
| yrdum8 | 0 | (omitted) | | | | |
| yrdum9 | 0 | (omitted) | | | | |
| yrdum10 | 0 | (omitted) | | | | |
| yrdum11 | 0 | (omitted) | | | | |
| yrdum12 | .0934054 | .2285373 | 0.41 | 0.684 | -.3656253 | .5524362 |
| yrdum13 | .3809947 | .2508566 | 1.52 | 0.135 | -.1228657 | .8848551 |
| yrdum14 | -.0264915 | .2501018 | -0.11 | 0.916 | -.5288357 | .4758528 |
| yrdum15 | .5467903 | .3606601 | 1.52 | 0.136 | -.1776169 | 1.271197 |
| yrdum16 | .0038194 | .3509593 | 0.01 | 0.991 | -.7011031 | .708742 |
| yrdum17 | -.48454 | .4447649 | -1.09 | 0.281 | -1.377877 | .4087966 |
| yrdum18 | -.1116641 | .4488524 | -0.25 | 0.805 | -1.013211 | .7898825 |
| yrdum19 | .0711582 | .3346246 | 0.21 | 0.832 | -.6009552 | .7432715 |
| yrdum20 | -.0598727 | .268896 | -0.22 | 0.825 | -.5999662 | .4802208 |
| yrdum21 | .1024646 | .3574763 | 0.29 | 0.776 | -.6155477 | .8204769 |
| yrdum22 | -.1024732 | .4578473 | -0.22 | 0.824 | -1.022087 | .8171401 |
| yrdum23 | -.5124539 | .4099552 | -1.25 | 0.217 | -1.335873 | .3109652 |
| yrdum24 | -.8869838 | .4424313 | -2.00 | 0.050 | -1.775633 | .0016657 |
| yrdum25 | -.4863586 | .4080887 | -1.19 | 0.239 | -1.306029 | .3333117 |
| yrdum26 | -.3536159 | .3678114 | -0.96 | 0.341 | -1.092387 | .3851551 |
| yrdum27 | .1066659 | .3973482 | 0.27 | 0.789 | -.6914315 | .9047632 |
| yrdum28 | -.5311961 | .4291952 | -1.24 | 0.222 | -1.39326 | .3308679 |
| yrdum29 | .0812321 | .783004 | 0.10 | 0.918 | -1.491478 | 1.653942 |
| yrdum30 | .018069 | .5423276 | 0.03 | 0.974 | -1.071228 | 1.107366 |
| yrdum31 | .0495656 | .4843952 | 0.10 | 0.919 | -.9233708 | 1.022502 |
| yrdum32 | .0866727 | .3946474 | 0.22 | 0.827 | -.706 | .8793454 |
| yrdum33 | -.5039997 | .2420684 | -2.08 | 0.042 | -.9902084 | -.017791 |
| yrdum34 | -.1509365 | .2611699 | -0.58 | 0.566 | -.6755116 | .3736386 |
| yrdum35 | -.8067834 | .3776963 | -2.14 | 0.038 | -1.565409 | -.048158 |
| yrdum36 | -1.152908 | .2985119 | -3.86 | 0.000 | -1.752487 | -.5533294 |
| yrdum37 | -1.033028 | .2291523 | -4.51 | 0.000 | -1.493294 | -.5727618 |
| yrdum38 | -1.267325 | .4056995 | -3.12 | 0.003 | -2.082196 | -.4524534 |
| yrdum39 | -1.637592 | .4550716 | -3.60 | 0.001 | -2.55163 | -.7235537 |
| yrdum40 | -1.405884 | .3459328 | -4.06 | 0.000 | -2.10071 | -.7110575 |
| yrdum41 | -1.494662 | .4853267 | -3.08 | 0.003 | -2.469469 | -.5198544 |
| yrdum42 | -1.20512 | .4561423 | -2.64 | 0.011 | -2.121308 | -.288931 |
| yrdum43 | -1.131759 | .4309197 | -2.63 | 0.011 | -1.997287 | -.2662317 |
| yrdum44 | -1.042385 | .349294 | -2.98 | 0.004 | -1.743963 | -.3408073 |
| yrdum45 | -1.215403 | .3291954 | -3.69 | 0.001 | -1.876612 | -.5541946 |

| | | | | | | |
|---|---|---|---|---|---|---|
| yrdum46 | -.8513165 | .2815684 | -3.02 | 0.004 | -1.416863 | -.2857696 |
| yrdum47 | -.9999458 | .3567254 | -2.80 | 0.007 | -1.71645 | -.2834418 |
| yrdum48 | -1.018832 | .3344528 | -3.05 | 0.004 | -1.6906 | -.3470635 |
| yrdum49 | -.9935382 | .2768631 | -3.59 | 0.001 | -1.549634 | -.4374423 |
| yrdum50 | -.6452652 | .1713711 | -3.77 | 0.000 | -.9894742 | -.3010563 |
| yrdum51 | -.369908 | .1733981 | -2.13 | 0.038 | -.7181883 | -.0216277 |
| yrdum52 | -.333626 | .1957676 | -1.70 | 0.095 | -.7268367 | .0595847 |
| yrdum53 | -.3994883 | .1434212 | -2.79 | 0.008 | -.6875582 | -.1114184 |
| yrdum54 | -.6367909 | .1382574 | -4.61 | 0.000 | -.9144891 | -.3590928 |
| yrdum55 | -.7093388 | .1469978 | -4.83 | 0.000 | -1.004593 | -.414085 |
| yrdum56 | 0 | (omitted) | | | | |
| yrdum57 | 0 | (omitted) | | | | |
| stdum1 | -.3575617 | .3704333 | -0.97 | 0.339 | -1.101599 | .3864755 |
| stdum2 | -.284081 | .485353 | -0.59 | 0.561 | -1.258941 | .6907793 |
| stdum3 | -.6534606 | .3644311 | -1.79 | 0.079 | -1.385442 | .0785208 |
| stdum4 | -.806791 | .3764668 | -2.14 | 0.037 | -1.562947 | -.0506352 |
| stdum5 | .4407937 | .2966536 | 1.49 | 0.144 | -.1550526 | 1.03664 |
| stdum6 | -1.10548 | .3700136 | -2.99 | 0.004 | -1.848674 | -.3622861 |
| stdum7 | -.7160503 | .4863737 | -1.47 | 0.147 | -1.692961 | .2608599 |
| stdum8 | -1.658962 | .5872656 | -2.82 | 0.007 | -2.838519 | -.4794041 |
| stdum9 | -.2328857 | 9.871983 | -0.02 | 0.981 | -20.06135 | 19.59558 |
| stdum10 | -.7892342 | .5287575 | -1.49 | 0.142 | -1.851275 | .2728064 |
| stdum11 | -.5194922 | .3909539 | -1.33 | 0.190 | -1.304746 | .2657618 |
| stdum12 | -1.09579 | .2763693 | -3.96 | 0.000 | -1.650894 | -.540686 |
| stdum13 | -1.669302 | .4586491 | -3.64 | 0.001 | -2.590525 | -.7480777 |
| stdum14 | -.3027394 | .223012 | -1.36 | 0.181 | -.7506723 | .1451934 |
| stdum15 | -.841217 | .415713 | -2.02 | 0.048 | -1.676201 | -.006233 |
| stdum16 | -1.854877 | .5936641 | -3.12 | 0.003 | -3.047286 | -.6624671 |
| stdum17 | -1.688252 | .65434 | -2.58 | 0.013 | -3.002533 | -.3739715 |
| stdum18 | -.4700484 | .2592725 | -1.81 | 0.076 | -.9908125 | .0507158 |
| stdum19 | .0949473 | .407726 | 0.23 | 0.817 | -.7239944 | .913889 |
| stdum20 | -1.273634 | .2651024 | -4.80 | 0.000 | -1.806108 | -.7411606 |
| stdum21 | -.256348 | .372047 | -0.69 | 0.494 | -1.003626 | .4909303 |
| stdum22 | -.715724 | .4366315 | -1.64 | 0.107 | -1.592724 | .1612762 |
| stdum23 | .187925 | .2532562 | 0.74 | 0.462 | -.320755 | .696605 |
| stdum24 | -1.34227 | .3393634 | -3.96 | 0.000 | -2.023901 | -.6606384 |
| stdum25 | -.3750893 | .4523181 | -0.83 | 0.411 | -1.283597 | .5334183 |
| stdum26 | -.9973579 | .5787561 | -1.72 | 0.091 | -2.159824 | .1651079 |
| stdum27 | -1.732837 | .4294997 | -4.03 | 0.000 | -2.595513 | -.8701614 |
| stdum28 | -2.037928 | .6428912 | -3.17 | 0.003 | -3.329213 | -.7466433 |
| stdum29 | -.8796521 | .3892277 | -2.26 | 0.028 | -1.661439 | -.0978653 |
| stdum30 | -1.536149 | .3488548 | -4.40 | 0.000 | -2.236844 | -.8354531 |
| stdum31 | -.5827817 | .6550149 | -0.89 | 0.378 | -1.898418 | .7328545 |
| stdum32 | -.3791706 | .3992802 | -0.95 | 0.347 | -1.181148 | .4228073 |
| stdum33 | 0 | (omitted) | | | | |
| stdum34 | -.863216 | .4309194 | -2.00 | 0.051 | -1.728743 | .002311 |
| stdum35 | -2.567318 | .6371667 | -4.03 | 0.000 | -3.847105 | -1.287531 |
| stdum36 | -.6039865 | .3134241 | -1.93 | 0.060 | -1.233517 | .0255442 |
| stdum37 | -1.030376 | .5482886 | -1.88 | 0.066 | -2.131646 | .0708937 |
| stdum38 | -.8745441 | .2560392 | -3.42 | 0.001 | -1.388814 | -.3602742 |
| stdum39 | -.4439156 | .2346164 | -1.89 | 0.064 | -.9151564 | .0273253 |
| stdum40 | -.928362 | .4877782 | -1.90 | 0.063 | -1.908093 | .0513694 |
| stdum41 | -.7537775 | .4630021 | -1.63 | 0.110 | -1.683745 | .1761894 |
| stdum42 | -2.466878 | .6824734 | -3.61 | 0.001 | -3.837666 | -1.09609 |
| stdum43 | -.5527224 | .3221175 | -1.72 | 0.092 | -1.199714 | .0942697 |

```
     stdum44 |  -.4336017   .3875962    -1.12   0.269    -1.212112    .3449081
     stdum45 |  -1.615021   .4765507    -3.39   0.001    -2.572201   -.6578403
     stdum46 |  -1.870732   .4363904    -4.29   0.000    -2.747248   -.9942164
     stdum47 |  -.8474233   .3963057    -2.14   0.037    -1.643427   -.0514199
     stdum48 |  -.4220863   .3224428    -1.31   0.197    -1.069732    .2255592
     stdum49 |  -.1672587   .5069763    -0.33   0.743    -1.185551    .8510332
     stdum50 |  -1.170296   .4123112    -2.84   0.007    -1.998447   -.3421444
     stdum51 |   -2.58305   .6555332    -3.94   0.000    -3.899727   -1.266372
       _cons |   2.408934   1.856182     1.30   0.200    -1.319317    6.137185
------------------------------------------------------------------------------

. /* homicide rate */
>
> regress homrate
> PTP
> trend
> density
> crack beerpc
> L.prisonpc L.policepc
> incomepc rwelfarepc povrate pctblack
> unrate employ empmilpc empconpc
> syg shall snsban
> pp*
> L.homrate L2.homrate
> yrdum* stdum*
> , robust cluster(state);
note: yrdum1 omitted because of collinearity
note: yrdum2 omitted because of collinearity
note: yrdum3 omitted because of collinearity
note: yrdum4 omitted because of collinearity
note: yrdum5 omitted because of collinearity
note: yrdum6 omitted because of collinearity
note: yrdum7 omitted because of collinearity
note: yrdum8 omitted because of collinearity
note: yrdum9 omitted because of collinearity
note: yrdum10 omitted because of collinearity
note: yrdum11 omitted because of collinearity
note: yrdum56 omitted because of collinearity
note: yrdum57 omitted because of collinearity
note: stdum24 omitted because of collinearity

Linear regression                               Number of obs     =      2,320
                                                F(49, 50)         =          .
                                                Prob > F          =          .
                                                R-squared         =     0.9603
                                                Root MSE          =     1.1576

                                 (Std. Err. adjusted for 51 clusters in state)
------------------------------------------------------------------------------
             |               Robust
     homrate |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
         PTP |   .1855054   .1771955     1.05   0.300    -.1704023     .541413
       trend |  -.0045036   .0179101    -0.25   0.802    -.0404771    .0314699
     density |    .000314   .0008023     0.39   0.697    -.0012976    .0019255
```

| | | | | | | |
|---|---|---|---|---|---|---|
| crack | -.1431384 | .2755966 | -0.52 | 0.606 | -.6966904 | .4104137 |
| beerpc | .0015243 | .0071311 | 0.21 | 0.832 | -.0127988 | .0158475 |
| | | | | | | |
| prisonpc | | | | | | |
| L1. | .000539 | .0013305 | 0.41 | 0.687 | -.0021335 | .0032114 |
| | | | | | | |
| policepc | | | | | | |
| L1. | .0034623 | .0025793 | 1.34 | 0.186 | -.0017184 | .008643 |
| | | | | | | |
| incomepc | -.0941383 | .067875 | -1.39 | 0.172 | -.2304692 | .0421925 |
| rwelfarepc | -4.759132 | 2.755593 | -1.73 | 0.090 | -10.2939 | .7756398 |
| povrate | .0327149 | .0135756 | 2.41 | 0.020 | .0054476 | .0599822 |
| pctblack | .0301541 | .0212569 | 1.42 | 0.162 | -.0125415 | .0728498 |
| unrate | -.0536763 | .0307744 | -1.74 | 0.087 | -.1154884 | .0081359 |
| employ | 8.934725 | 5.914364 | 1.51 | 0.137 | -2.944624 | 20.81407 |
| empmilpc | -352.7113 | 365.7717 | -0.96 | 0.340 | -1087.385 | 381.9629 |
| empconpc | 692.1556 | 672.2564 | 1.03 | 0.308 | -658.1112 | 2042.422 |
| syg | .2668367 | .1521757 | 1.75 | 0.086 | -.0388173 | .5724906 |
| shall | .0147993 | .1086545 | 0.14 | 0.892 | -.2034396 | .2330383 |
| snsban | -.0464115 | .2044885 | -0.23 | 0.821 | -.4571386 | .3643157 |
| pp1519 | -.1721019 | .0906991 | -1.90 | 0.064 | -.3542765 | .0100726 |
| pp2024 | .1757749 | .1161283 | 1.51 | 0.136 | -.0574757 | .4090255 |
| pp2529 | -.2611672 | .1702143 | -1.53 | 0.131 | -.6030527 | .0807184 |
| pp3034 | -.265775 | .2101953 | -1.26 | 0.212 | -.6879647 | .1564146 |
| pp3539 | .1315885 | .2478117 | 0.53 | 0.598 | -.366156 | .629333 |
| pp4044 | -.1005893 | .2943341 | -0.34 | 0.734 | -.6917768 | .4905981 |
| pp4549 | -.0963103 | .2514373 | -0.38 | 0.703 | -.601337 | .4087164 |
| pp5054 | .1287862 | .2180198 | 0.59 | 0.557 | -.3091193 | .5666918 |
| pp5559 | -.0616582 | .2120028 | -0.29 | 0.772 | -.4874784 | .3641619 |
| pp6064 | -.2701185 | .2016594 | -1.34 | 0.186 | -.6751634 | .1349264 |
| pp6500 | .0686425 | .074766 | 0.92 | 0.363 | -.0815293 | .2188144 |
| | | | | | | |
| homrate | | | | | | |
| L1. | .7120692 | .1102446 | 6.46 | 0.000 | .4906364 | .933502 |
| L2. | .1181232 | .0986066 | 1.20 | 0.237 | -.079934 | .3161804 |
| | | | | | | |
| yrdum1 | 0 | (omitted) | | | | |
| yrdum2 | 0 | (omitted) | | | | |
| yrdum3 | 0 | (omitted) | | | | |
| yrdum4 | 0 | (omitted) | | | | |
| yrdum5 | 0 | (omitted) | | | | |
| yrdum6 | 0 | (omitted) | | | | |
| yrdum7 | 0 | (omitted) | | | | |
| yrdum8 | 0 | (omitted) | | | | |
| yrdum9 | 0 | (omitted) | | | | |
| yrdum10 | 0 | (omitted) | | | | |
| yrdum11 | 0 | (omitted) | | | | |
| yrdum12 | .2914514 | .3156401 | 0.92 | 0.360 | -.3425305 | .9254333 |
| yrdum13 | .3258287 | .33009 | 0.99 | 0.328 | -.3371765 | .988834 |
| yrdum14 | .1387676 | .3361331 | 0.41 | 0.681 | -.5363756 | .8139108 |
| yrdum15 | .6421209 | .4448441 | 1.44 | 0.155 | -.2513747 | 1.535617 |
| yrdum16 | .2631963 | .4805929 | 0.55 | 0.586 | -.7021029 | 1.228496 |
| yrdum17 | -.4877022 | .4992779 | -0.98 | 0.333 | -1.490531 | .5151269 |
| yrdum18 | .1426497 | .5429746 | 0.26 | 0.794 | -.9479468 | 1.233246 |
| yrdum19 | .1414024 | .3913013 | 0.36 | 0.719 | -.6445493 | .9273542 |

| | | | | | | |
|---|---|---|---|---|---|---|
| yrdum20 | .4781133 | .4308235 | 1.11 | 0.272 | -.3872212 | 1.343448 |
| yrdum21 | .60709 | .4327739 | 1.40 | 0.167 | -.2621621 | 1.476342 |
| yrdum22 | .1768952 | .6206164 | 0.29 | 0.777 | -1.06965 | 1.42344 |
| yrdum23 | -.3485041 | .5198302 | -0.67 | 0.506 | -1.392614 | .6956056 |
| yrdum24 | -.8418266 | .5009984 | -1.68 | 0.099 | -1.848111 | .1644582 |
| yrdum25 | -.4808839 | .4656383 | -1.03 | 0.307 | -1.416146 | .4543781 |
| yrdum26 | -.17095 | .4000636 | -0.43 | 0.671 | -.9745013 | .6326014 |
| yrdum27 | .6575323 | .5296081 | 1.24 | 0.220 | -.4062167 | 1.721281 |
| yrdum28 | -.2139135 | .5529038 | -0.39 | 0.700 | -1.324453 | .8966265 |
| yrdum29 | .3253423 | .8986154 | 0.36 | 0.719 | -1.47958 | 2.130264 |
| yrdum30 | .422627 | .7282769 | 0.58 | 0.564 | -1.04016 | 1.885414 |
| yrdum31 | .2799521 | .5757482 | 0.49 | 0.629 | -.8764722 | 1.436376 |
| yrdum32 | .5656743 | .5294068 | 1.07 | 0.290 | -.4976705 | 1.629019 |
| yrdum33 | -.4882437 | .3006149 | -1.62 | 0.111 | -1.092046 | .1155591 |
| yrdum34 | -.1827835 | .3032255 | -0.60 | 0.549 | -.7918299 | .4262629 |
| yrdum35 | -.8471528 | .4489374 | -1.89 | 0.065 | -1.74887 | .0545646 |
| yrdum36 | -1.107905 | .4472231 | -2.48 | 0.017 | -2.006179 | -.209631 |
| yrdum37 | -1.227813 | .3589615 | -3.42 | 0.001 | -1.948809 | -.5068179 |
| yrdum38 | -1.499973 | .5960644 | -2.52 | 0.015 | -2.697204 | -.3027424 |
| yrdum39 | -1.862434 | .5578779 | -3.34 | 0.002 | -2.982965 | -.7419036 |
| yrdum40 | -1.921647 | .6102669 | -3.15 | 0.003 | -3.147405 | -.6958902 |
| yrdum41 | -1.878741 | .5987992 | -3.14 | 0.003 | -3.081465 | -.6760178 |
| yrdum42 | -.9927515 | .5752938 | -1.73 | 0.091 | -2.148263 | .1627601 |
| yrdum43 | -1.784847 | .6259629 | -2.85 | 0.006 | -3.04213 | -.5275634 |
| yrdum44 | -1.362061 | .4061513 | -3.35 | 0.002 | -2.17784 | -.546282 |
| yrdum45 | -1.378085 | .4073066 | -3.38 | 0.001 | -2.196184 | -.5599853 |
| yrdum46 | -1.068664 | .383637 | -2.79 | 0.008 | -1.839222 | -.2981065 |
| yrdum47 | -1.176681 | .4771359 | -2.47 | 0.017 | -2.135036 | -.218325 |
| yrdum48 | -1.23853 | .4714225 | -2.63 | 0.011 | -2.18541 | -.2916504 |
| yrdum49 | -1.183946 | .3878523 | -3.05 | 0.004 | -1.962971 | -.4049219 |
| yrdum50 | -.8018666 | .1942892 | -4.13 | 0.000 | -1.192108 | -.4116252 |
| yrdum51 | -.3603941 | .2069386 | -1.74 | 0.088 | -.7760425 | .0552543 |
| yrdum52 | -.2488955 | .2318183 | -1.07 | 0.288 | -.7145163 | .2167253 |
| yrdum53 | -.394798 | .1673681 | -2.36 | 0.022 | -.7309668 | -.0586293 |
| yrdum54 | -.6224248 | .1656691 | -3.76 | 0.000 | -.955181 | -.2896687 |
| yrdum55 | -.6411169 | .1741148 | -3.68 | 0.001 | -.9908369 | -.291397 |
| yrdum56 | 0 | (omitted) | | | | |
| yrdum57 | 0 | (omitted) | | | | |
| stdum1 | 1.398932 | .5075776 | 2.76 | 0.008 | .3794329 | 2.418432 |
| stdum2 | 1.528258 | .654725 | 2.33 | 0.024 | .2132045 | 2.843312 |
| stdum3 | 1.195666 | .2791379 | 4.28 | 0.000 | .6350012 | 1.756331 |
| stdum4 | .8820813 | .4549493 | 1.94 | 0.058 | -.0317112 | 1.795874 |
| stdum5 | 2.4216 | .5665217 | 4.27 | 0.000 | 1.283708 | 3.559493 |
| stdum6 | .4184319 | .2142878 | 1.95 | 0.056 | -.0119779 | .8488417 |
| stdum7 | .4110802 | .5090695 | 0.81 | 0.423 | -.6114161 | 1.433576 |
| stdum8 | -.584618 | .7893974 | -0.74 | 0.462 | -2.170169 | 1.000933 |
| stdum9 | -4.53893 | 13.33598 | -0.34 | 0.735 | -31.32504 | 22.24718 |
| stdum10 | .7672035 | .6339976 | 1.21 | 0.232 | -.5062182 | 2.040625 |
| stdum11 | 1.210121 | .5158955 | 2.35 | 0.023 | .1739142 | 2.246327 |
| stdum12 | .343366 | .1866802 | 1.84 | 0.072 | -.0315923 | .7183243 |
| stdum13 | -.320594 | .3885742 | -0.83 | 0.413 | -1.101068 | .4598802 |
| stdum14 | 1.374337 | .3055988 | 4.50 | 0.000 | .7605239 | 1.988151 |
| stdum15 | .7021597 | .3345452 | 2.10 | 0.041 | .030206 | 1.374113 |
| stdum16 | -.7463618 | .4133229 | -1.81 | 0.077 | -1.576545 | .0838216 |
| stdum17 | -.3245087 | .5831793 | -0.56 | 0.580 | -1.495859 | .8468413 |

```
     stdum18 |   1.193534   .2930518     4.07   0.000     .6049217    1.782146
     stdum19 |   1.881053    .581462     3.24   0.002      .713152    3.048953
     stdum20 |  -.0143251    .349268    -0.04   0.967    -.7158505    .6872004
     stdum21 |   1.340689   .5165866     2.60   0.012     .3030943    2.378284
     stdum22 |   .3828127   .4384883     0.87   0.387    -.4979169    1.263542
     stdum23 |   2.045859    .379112     5.40   0.000      1.28439    2.807328
     stdum24 |          0  (omitted)
     stdum25 |   1.395888   .6588383     2.12   0.039     .0725723    2.719204
     stdum26 |   .4217748   .5455102     0.77   0.443    -.6739147    1.517464
     stdum27 |  -.5402299   .4035963    -1.34   0.187    -1.350877    .2704171
     stdum28 |  -.9483512   .5461273    -1.74   0.089     -2.04528    .1485777
     stdum29 |   .5690561   .4230684     1.35   0.185    -.2807017    1.418814
     stdum30 |  -.5353886   .3563616    -1.50   0.139    -1.251162    .1803846
     stdum31 |   .5547107   .7588165     0.73   0.468    -.9694171    2.078838
     stdum32 |   1.616871   .5210092     3.10   0.003     .5703928    2.663348
     stdum33 |   1.760484   .4439079     3.97   0.000     .8688685    2.652099
     stdum34 |    .701076   .5067549     1.38   0.173    -.3167713    1.718923
     stdum35 |  -1.643687   .6165156    -2.67   0.010    -2.881995   -.4053794
     stdum36 |   .8622121   .2458993     3.51   0.001     .3683087    1.356115
     stdum37 |   .7189691   .5059774     1.42   0.162    -.2973166    1.735255
     stdum38 |   .6462021   .2679033     2.41   0.020     .1081025    1.184302
     stdum39 |   1.054089   .2767335     3.81   0.000     .4982529    1.609924
     stdum40 |    .056052   .5719844     0.10   0.922    -1.092812    1.204916
     stdum41 |   .9000378   .5941943     1.51   0.136    -.2934366    2.093512
     stdum42 |  -1.302831   .7056731    -1.85   0.071    -2.720217    .1145557
     stdum43 |    .881317   .4154334     2.12   0.039     .0468944     1.71574
     stdum44 |   1.317488   .3038362     4.34   0.000     .7072148    1.927761
     stdum45 |  -.0479637   .4072204    -0.12   0.907      -.86589    .7699626
     stdum46 |  -.8844631   .5307319    -1.67   0.102    -1.950469    .1815434
     stdum47 |   .6487192   .4293903     1.51   0.137    -.2137366    1.511175
     stdum48 |   1.301106   .5374104     2.42   0.019     .2216852    2.380527
     stdum49 |   1.566369   .6946226     2.25   0.029     .1711785     2.96156
     stdum50 |   .0801679   .2659793     0.30   0.764    -.4540672    .6144031
     stdum51 |  -1.686291   .7955656    -2.12   0.039    -3.284232   -.0883507
       _cons |   .9995648   2.355841     0.42   0.673    -3.732281     5.73141
------------------------------------------------------------------------------

. log close;
      name:  <unnamed>
       log:  C:\Users\cemood\Box Sync\Illinois\report\permit.fe.log
  log type:  text
 closed on:  11 May 2019, 18:02:25
----------------------------------------------------------------------------------------
----------------------------------------------------------------------------------------
--------------------------------------------------------------------------
```

## Appendix F: Stata log file used to produce the suicide analysis

This is for the Missouri/Kansas and Connecticut/Rhode Island suicide studies, and for Table 4 (covering all six states that changed their permit-to-purchase laws).

```
-----------------------------------------------------------------------------------------
-----------------------------------------------------------------------------------------
-------------------------------------------------------------------------
      name:  <unnamed>
       log:  C:\Users\cemood\Box Sync\Illinois\report\suicide.log
  log type:  text
 opened on:  11 May 2019, 18:03:09

.
. keep if year>=1960
(0 observations deleted)

. keep if year<=2017
(0 observations deleted)

.
. replace permithg=1 if stnm==“MD” & year>2013
(0 real changes made)

. replace permithg=1 if stnm==“MO” & year<=2007
(22 real changes made)

. gen PTP=permithg

. preserve

.
. /* CT v RI */
. keep if stnm==“RI” | stnm==“CT”
(2,793 observations deleted)

. gen DP=(stnm==“CT”)

. gen D2=(year>1995)

. gen DD=DP*D2

.
. regress suicidepc DP D2 DD

      Source |       SS           df       MS      Number of obs   =        98
-------------+----------------------------------   F(3, 94)        =      1.36
       Model |  9.59479195         3  3.19826398   Prob > F        =    0.2604
    Residual |  221.337438        94   2.3546536   R-squared       =    0.0415
-------------+----------------------------------   Adj R-squared   =    0.0110
       Total |   230.93223        97  2.38074464   Root MSE        =    1.5345

------------------------------------------------------------------------------
```

```
   suicidepc |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
          DP |  -.4323148   .4101092    -1.05   0.295    -1.246596    .3819666
          D2 |  -.4557485   .4429685    -1.03   0.306    -1.335273     .423776
          DD |  -.0015407   .6264521    -0.00   0.998    -1.245376    1.242295
       _cons |   9.789038    .289991    33.76   0.000     9.213254    10.36482
------------------------------------------------------------------------------

. regress gunsuicidepc DP D2 DD

      Source |       SS           df       MS      Number of obs   =        98
-------------+----------------------------------   F(3, 94)        =     12.95
       Model |  16.5369843         3  5.5123281    Prob > F        =    0.0000
    Residual |  39.9975706        94  .425506071   R-squared       =    0.2925
-------------+----------------------------------   Adj R-squared   =    0.2699
       Total |  56.5345549        97  .582830463   Root MSE        =    .65231

------------------------------------------------------------------------------
gunsuicidepc |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
          DP |   .8236578   .1743367     4.72   0.000     .4775082    1.169807
          D2 |   -.251491   .1883052    -1.34   0.185    -.6253753    .1223934
          DD |  -.4803567   .2663037    -1.80   0.074    -1.009109    .0483957
       _cons |    2.85997   .1232747    23.20   0.000     2.615205    3.104734
------------------------------------------------------------------------------

.
. summarize gunsuicidepc if stnm=="CT" & year<=1995

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
gunsuicidepc |         28    3.683628    .5378929   2.828542     4.6323

. scalar p1=r(mean)

. summarize gunsuicidepc if stnm=="CT" & year>1995

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
gunsuicidepc |         21     2.95178    .3922187   2.211342   3.596384

. scalar p2=r(mean)

. summarize gunsuicidepc if stnm=="RI" & year<=1995

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
gunsuicidepc |         28     2.85997    .8447968   1.180258   4.345177

. scalar s1=r(mean)

. summarize gunsuicidepc if stnm=="RI" & year>1995

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
```

```
gunsuicidepc |         21    2.608479    .7014121    1.12009   3.796341

. scalar s2=r(mean)

. scalar dd=(p2-p1)-(s2-s1)

. scalar diffp=p2-p1

. scalar diffs=s2-s1

. scalar dd2=diffp-diffs

. scalar pctdd=dd/p1

. scalar list
     pctdd = -.13040316
       dd2 = -.48035665
     diffs = -.25149099
     diffp = -.73184764
        dd = -.48035665
        s2 =  2.6084787
        s1 =  2.8599697
        p2 =  2.9517799
        p1 =  3.6836275

.
.
.
. restore

. preserve

.
. /* MO v KS PTP law */
. keep if stnm==“MO” | stnm==“KS”
(2,793 observations deleted)

. gen DP=(stnm==“MO”)

. gen D2=(year>2007)

. gen DD=DP*D2

.
.
. regress suicidepc DP D2 DD

      Source |       SS           df       MS      Number of obs   =        98
-------------+----------------------------------   F(3, 94)        =     30.51
       Model |  129.307174         3  43.1023915   Prob > F        =    0.0000
    Residual |  132.805923        94  1.41282897   R-squared       =    0.4933
-------------+----------------------------------   Adj R-squared   =    0.4772
       Total |  262.113098        97  2.70219688   Root MSE        =    1.1886

------------------------------------------------------------------------------
```

```
    suicidepc |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
          DP |   .3562109   .2657846     1.34   0.183    -.1715106    .8839325
          D2 |   2.761737   .4385222     6.30   0.000     1.891041    3.632433
          DD |   .3052098    .620164     0.49   0.624    -.9261405     1.53656
       _cons |   12.23663   .1879381    65.11   0.000     11.86347    12.60978
------------------------------------------------------------------------------

. regress gunsuicidepc DP D2 DD

      Source |       SS           df       MS      Number of obs   =        98
-------------+----------------------------------   F(3, 94)        =      6.08
       Model |   13.743907         3  4.58130235   Prob > F        =    0.0008
    Residual |  70.7997005        94  .753188303   R-squared       =    0.1626
-------------+----------------------------------   Adj R-squared   =    0.1358
       Total |  84.5436076        97  .871583583   Root MSE        =    .86786

------------------------------------------------------------------------------
gunsuicidepc |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
          DP |   .2002661   .1940603     1.03   0.305    -.1850452    .5855775
          D2 |   .4728021   .3201832     1.48   0.143    -.1629292    1.108533
          DD |   .6672173   .4528075     1.47   0.144    -.2318426    1.566277
       _cons |   7.578492   .1372214    55.23   0.000     7.306036    7.850948
------------------------------------------------------------------------------

.
. predict yhat
(option xb assumed; fitted values)

. label var year “Year”

. *twoway (scatter gunsuicidepc year)(line yhat year), xtick(1960(10)2020)
xmtick(1965(5)2015) xline(2007)
.
. scalar drop _all

. summarize gunsuicidepc if stnm==“MO” & year<=2007

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
gunsuicidepc |         40    7.778758      .89101    5.75431   9.452814

. scalar p1=r(mean)

. summarize gunsuicidepc if stnm==“MO” & year>2007

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
gunsuicidepc |          9    8.918778    1.110496   7.106785   10.66798

. scalar p2=r(mean)

. summarize gunsuicidepc if stnm==“KS” & year<=2007
```

```
    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
gunsuicidepc |         40    7.578492    .7247887   5.921185   8.800132

. scalar s1=r(mean)

. summarize gunsuicidepc if stnm==“KS” & year>2007

    Variable |        Obs        Mean    Std. Dev.       Min        Max
-------------+---------------------------------------------------------
gunsuicidepc |          9    8.051294    1.088842   6.588141   10.29006

. scalar s2=r(mean)

. scalar dd=(p2-p1)-(s2-s1)

. scalar diffp=p2-p1

. scalar diffs=s2-s1

. scalar dd2=diffp-diffs

. scalar list
       dd2 =  .66721726
     diffs =  .47280209
     diffp =  1.1400194
        dd =  .66721726
        s2 =  8.0512941
        s1 =   7.578492
        p2 =  8.9187775
        p1 =  7.7787582

.
.
. restore

. preserve

.
. sort state year

. tsset state year
       panel variable:  state (strongly balanced)
        time variable:  year, 1960 to 2016
                delta:  1 unit

.
. /* state and year dummies */
. quietly: tab year, gen(yrdum)

. quietly: tab state, gen(stdum)

.
. replace employ=employ/poptot
(2,448 real changes made)
```

```
. replace empconpc=100*empcon/poptot
(2,448 real changes made)

. replace empmilpc=100*empmil/poptot
(2,448 real changes made)

.
.
.
. label var rwelfare “Real welfare per capita”

.
.
.
. /* gun suicide rate */
.
.
. gen z1529=pp1519+pp2024+pp2529
(510 missing values generated)

. gen z3039=pp3034 +pp3539
(510 missing values generated)

. gen z4049=pp4044 +pp4549
(510 missing values generated)

. gen z5059=pp5054 +pp5559
(510 missing values generated)

. gen z6000=pp6064 +pp6500
(510 missing values generated)

. drop pp*

.
. gen pp5059=z5059
(510 missing values generated)

.
. label var pp5059 “Percent population 50-59”

.
.
. /* gun suicide rate */
.
. regress gunsuicidepc PTP density beerpc incomepc unrate employ z5059 ///
> L.gunsuicidepc L2.gunsuicidepc yrdum* stdum* , robust cluster(state)
note: yrdum1 omitted because of collinearity
note: yrdum2 omitted because of collinearity
note: yrdum3 omitted because of collinearity
note: yrdum4 omitted because of collinearity
note: yrdum5 omitted because of collinearity
note: yrdum6 omitted because of collinearity
note: yrdum7 omitted because of collinearity
```

```
note: yrdum8 omitted because of collinearity
note: yrdum9 omitted because of collinearity
note: yrdum10 omitted because of collinearity
note: yrdum24 omitted because of collinearity
note: yrdum57 omitted because of collinearity
note: stdum9 omitted because of collinearity

Linear regression                               Number of obs     =      2,331
                                                F(49, 50)         =          .
                                                Prob > F          =          .
                                                R-squared         =     0.9279
                                                Root MSE          =     .83561

                                  (Std. Err. adjusted for 51 clusters in state)
```

| gunsuicidepc | Coef. | Robust Std. Err. | t | P>\|t\| | [95% Conf. | Interval] |
|---|---|---|---|---|---|---|
| **PTP** | **-.1500572** | **.0787211** | **-1.91** | **0.062** | **-.3081733** | **.0080588** |
| density | .0006783 | .0002365 | 2.87 | 0.006 | .0002033 | .0011533 |
| beerpc | .0122237 | .0055251 | 2.21 | 0.032 | .0011261 | .0233213 |
| incomepc | -.0150964 | .0281299 | -0.54 | 0.594 | -.0715969 | .0414042 |
| unrate | .0071223 | .0224104 | 0.32 | 0.752 | -.0378904 | .052135 |
| employ | 4.310417 | 2.015011 | 2.14 | 0.037 | .2631477 | 8.357685 |
| z5059 | .1151417 | .0707895 | 1.63 | 0.110 | -.0270432 | .2573265 |
| | | | | | | |
| gunsuicidepc | | | | | | |
| L1. | .3142008 | .0278863 | 11.27 | 0.000 | .2581895 | .370212 |
| L2. | .2819929 | .0569036 | 4.96 | 0.000 | .1676986 | .3962872 |
| | | | | | | |
| yrdum1 | 0 | (omitted) | | | | |
| yrdum2 | 0 | (omitted) | | | | |
| yrdum3 | 0 | (omitted) | | | | |
| yrdum4 | 0 | (omitted) | | | | |
| yrdum5 | 0 | (omitted) | | | | |
| yrdum6 | 0 | (omitted) | | | | |
| yrdum7 | 0 | (omitted) | | | | |
| yrdum8 | 0 | (omitted) | | | | |
| yrdum9 | 0 | (omitted) | | | | |
| yrdum10 | 0 | (omitted) | | | | |
| yrdum11 | .2156133 | .3291037 | 0.66 | 0.515 | -.4454109 | .8766375 |
| yrdum12 | .3676989 | .2375174 | 1.55 | 0.128 | -.1093687 | .8447666 |
| yrdum13 | .4623287 | .2969024 | 1.56 | 0.126 | -.1340173 | 1.058675 |
| yrdum14 | .0119887 | .2243781 | 0.05 | 0.958 | -.4386881 | .4626654 |
| yrdum15 | .38985 | .2700299 | 1.44 | 0.155 | -.152521 | .932221 |
| yrdum16 | .4392839 | .1834897 | 2.39 | 0.020 | .0707341 | .8078337 |
| yrdum17 | .1449077 | .16159 | 0.90 | 0.374 | -.1796554 | .4694709 |
| yrdum18 | .6229938 | .1985273 | 3.14 | 0.003 | .22424 | 1.021748 |
| yrdum19 | -.1208567 | .1708609 | -0.71 | 0.483 | -.464041 | .2223277 |
| yrdum20 | -.1891485 | .2308069 | -0.82 | 0.416 | -.6527379 | .2744409 |
| yrdum21 | -.225414 | .1424799 | -1.58 | 0.120 | -.5115934 | .0607653 |
| yrdum22 | .0766742 | .1463891 | 0.52 | 0.603 | -.217357 | .3707053 |
| yrdum23 | .3355485 | .2148973 | 1.56 | 0.125 | -.0960854 | .7671825 |
| yrdum24 | 0 | (omitted) | | | | |
| yrdum25 | .1549686 | .1364188 | 1.14 | 0.261 | -.1190365 | .4289738 |

| | | | | | | |
|---|---|---|---|---|---|---|
| yrdum26 | .1190345 | .1128083 | 1.06 | 0.296 | -.1075476 | .3456166 |
| yrdum27 | .5641753 | .1471114 | 3.84 | 0.000 | .2686933 | .8596572 |
| yrdum28 | .4610025 | .1959397 | 2.35 | 0.023 | .0674461 | .854559 |
| yrdum29 | -.0623613 | .1570129 | -0.40 | 0.693 | -.3777311 | .2530084 |
| yrdum30 | .1018995 | .1945469 | 0.52 | 0.603 | -.2888595 | .4926585 |
| yrdum31 | .2798289 | .121553 | 2.30 | 0.026 | .0356825 | .5239753 |
| yrdum32 | .0584577 | .1245564 | 0.47 | 0.641 | -.1917212 | .3086367 |
| yrdum33 | -.1466259 | .1533551 | -0.96 | 0.344 | -.4546487 | .1613968 |
| yrdum34 | .330385 | .1569172 | 2.11 | 0.040 | .0152075 | .6455626 |
| yrdum35 | .0117139 | .1629536 | 0.07 | 0.943 | -.315588 | .3390159 |
| yrdum36 | -.1351285 | .169305 | -0.80 | 0.429 | -.4751876 | .2049307 |
| yrdum37 | -.2326846 | .173045 | -1.34 | 0.185 | -.5802557 | .1148865 |
| yrdum38 | -.5622607 | .13762 | -4.09 | 0.000 | -.8386787 | -.2858427 |
| yrdum39 | -.7145961 | .2413889 | -2.96 | 0.005 | -1.19944 | -.2297522 |
| yrdum40 | -1.053418 | .2480448 | -4.25 | 0.000 | -1.551631 | -.5552058 |
| yrdum41 | -.932931 | .2625124 | -3.55 | 0.001 | -1.460203 | -.4056593 |
| yrdum42 | -.7979246 | .2910151 | -2.74 | 0.008 | -1.382446 | -.2134035 |
| yrdum43 | -.6483435 | .2329955 | -2.78 | 0.008 | -1.116329 | -.1803583 |
| yrdum44 | -.970748 | .2755766 | -3.52 | 0.001 | -1.52426 | -.4172362 |
| yrdum45 | -1.081898 | .3849844 | -2.81 | 0.007 | -1.855161 | -.3086337 |
| yrdum46 | -.9705816 | .3527773 | -2.75 | 0.008 | -1.679156 | -.2620075 |
| yrdum47 | -1.237648 | .3756897 | -3.29 | 0.002 | -1.992243 | -.4830531 |
| yrdum48 | -1.157932 | .3829943 | -3.02 | 0.004 | -1.927199 | -.3886652 |
| yrdum49 | -.7353429 | .3360952 | -2.19 | 0.033 | -1.41041 | -.0602758 |
| yrdum50 | -.7756608 | .4364332 | -1.78 | 0.082 | -1.652263 | .1009411 |
| yrdum51 | -.5557638 | .4142767 | -1.34 | 0.186 | -1.387863 | .2763355 |
| yrdum52 | -.5915577 | .4332355 | -1.37 | 0.178 | -1.461737 | .2786214 |
| yrdum53 | -.4266665 | .4062021 | -1.05 | 0.299 | -1.242547 | .3892144 |
| yrdum54 | -.3594706 | .4100857 | -0.88 | 0.385 | -1.183152 | .4642107 |
| yrdum55 | -.5157194 | .4380939 | -1.18 | 0.245 | -1.395657 | .3642181 |
| yrdum56 | -.296669 | .4205606 | -0.71 | 0.484 | -1.14139 | .5480519 |
| yrdum57 | 0 | (omitted) | | | | |
| stdum1 | 12.58269 | 3.721465 | 3.38 | 0.001 | 5.107912 | 20.05748 |
| stdum2 | 13.70122 | 3.571895 | 3.84 | 0.000 | 6.526856 | 20.87558 |
| stdum3 | 13.15427 | 3.729642 | 3.53 | 0.001 | 5.663061 | 20.64547 |
| stdum4 | 12.79026 | 3.720117 | 3.44 | 0.001 | 5.318189 | 20.26234 |
| stdum5 | 11.05152 | 3.579963 | 3.09 | 0.003 | 3.860953 | 18.24209 |
| stdum6 | 12.22572 | 3.552445 | 3.44 | 0.001 | 5.090428 | 19.36102 |
| stdum7 | 9.633591 | 3.441959 | 2.80 | 0.007 | 2.720213 | 16.54697 |
| stdum8 | 10.4067 | 3.466686 | 3.00 | 0.004 | 3.443655 | 17.36974 |
| stdum9 | 0 | (omitted) | | | | |
| stdum10 | 12.00214 | 3.646196 | 3.29 | 0.002 | 4.678545 | 19.32574 |
| stdum11 | 12.25275 | 3.636593 | 3.37 | 0.001 | 4.948439 | 19.55706 |
| stdum12 | 9.373822 | 3.438233 | 2.73 | 0.009 | 2.467927 | 16.27972 |
| stdum13 | 13.19015 | 3.677422 | 3.59 | 0.001 | 5.803833 | 20.57647 |
| stdum14 | 10.13444 | 3.543751 | 2.86 | 0.006 | 3.016608 | 17.25227 |
| stdum15 | 11.53661 | 3.615943 | 3.19 | 0.002 | 4.27378 | 18.79945 |
| stdum16 | 10.9371 | 3.57146 | 3.06 | 0.004 | 3.76361 | 18.11059 |
| stdum17 | 11.65866 | 3.575965 | 3.26 | 0.002 | 4.476125 | 18.8412 |
| stdum18 | 12.77678 | 3.706702 | 3.45 | 0.001 | 5.33165 | 20.22191 |
| stdum19 | 12.35551 | 3.67953 | 3.36 | 0.002 | 4.964956 | 19.74606 |
| stdum20 | 11.61997 | 3.612657 | 3.22 | 0.002 | 4.363738 | 18.87621 |
| stdum21 | 10.50067 | 3.543564 | 2.96 | 0.005 | 3.383211 | 17.61813 |
| stdum22 | 8.882394 | 3.340857 | 2.66 | 0.011 | 2.172085 | 15.5927 |
| stdum23 | 11.24183 | 3.638823 | 3.09 | 0.003 | 3.933034 | 18.55062 |

```
     stdum24 |   10.74122   3.539219     3.03   0.004     3.632491    17.84995
     stdum25 |   12.43793   3.718796     3.34   0.002     4.968505    19.90735
     stdum26 |   11.89154   3.611002     3.29   0.002     4.638632    19.14446
     stdum27 |   13.45451   3.649103     3.69   0.001     6.125073    20.78395
     stdum28 |   10.88311   3.527042     3.09   0.003     3.798835    17.96738
     stdum29 |   13.76718   3.616596     3.81   0.000     6.503033    21.03133
     stdum30 |   10.70435   3.534905     3.03   0.004     3.604288    17.80442
     stdum31 |   9.014521   3.392851     2.66   0.011      2.19978    15.82926
     stdum32 |   13.33972   3.737866     3.57   0.001     5.831993    20.84744
     stdum33 |   9.626596   3.549539     2.71   0.009     2.497138    16.75605
     stdum34 |   11.94313   3.605201     3.31   0.002     4.701868    19.18439
     stdum35 |   11.08189   3.483515     3.18   0.003     4.085047    18.07874
     stdum36 |    10.9764    3.56883     3.08   0.003     3.808199    18.14461
     stdum37 |   12.70946   3.695503     3.44   0.001     5.286821     20.1321
     stdum38 |   12.33317   3.619477     3.41   0.001     5.063235     19.6031
     stdum39 |    10.9404   3.601757     3.04   0.004     3.706056    18.17474
     stdum40 |   9.162929   3.375677     2.71   0.009     2.382682    15.94318
     stdum41 |   12.03838   3.641889     3.31   0.002     4.723434    19.35333
     stdum42 |   11.54418   3.562022     3.24   0.002     4.389649    18.69871
     stdum43 |   12.42101   3.628022     3.42   0.001      5.13391     19.7081
     stdum44 |   11.69295   3.605614     3.24   0.002      4.45086    18.93504
     stdum45 |   12.70608   3.677114     3.46   0.001     5.320384    20.09178
     stdum46 |   11.84561    3.55415     3.33   0.002     4.706893    18.98433
     stdum47 |   11.78492   3.604122     3.27   0.002      4.54583    19.02402
     stdum48 |   11.66295   3.610692     3.23   0.002     4.410659    18.91524
     stdum49 |   12.99105   3.806341     3.41   0.001     5.345791    20.63631
     stdum50 |   10.70939    3.53169     3.03   0.004     3.615786      17.803
     stdum51 |   13.83384   3.613639     3.83   0.000     6.575634    21.09205
       _cons |  -12.71002   4.680349    -2.72   0.009    -22.11077   -3.309259
------------------------------------------------------------------------------

.
. /* suicide rate */
.
. regress suicidepc PTP density beerpc incomepc unrate employ z5059 ///
> L.suicidepc L2.suicidepc yrdum* stdum* , robust cluster(state)
note: yrdum1 omitted because of collinearity
note: yrdum2 omitted because of collinearity
note: yrdum3 omitted because of collinearity
note: yrdum4 omitted because of collinearity
note: yrdum5 omitted because of collinearity
note: yrdum6 omitted because of collinearity
note: yrdum7 omitted because of collinearity
note: yrdum8 omitted because of collinearity
note: yrdum9 omitted because of collinearity
note: yrdum10 omitted because of collinearity
note: yrdum24 omitted because of collinearity
note: yrdum57 omitted because of collinearity
note: stdum9 omitted because of collinearity

Linear regression                               Number of obs     =      2,346
                                                F(49, 50)         =          .
                                                Prob > F          =          .
                                                R-squared         =     0.9022
                                                Root MSE          =     1.1225
```

(Std. Err. adjusted for 51 clusters in state)

| suicidepc | Coef. | Robust Std. Err. | t | P>\|t\| | [95% Conf. | Interval] |
|---|---|---|---|---|---|---|
| **PTP** | **-.1418705** | **.1211172** | **-1.17** | **0.247** | -.3851416 | .1014007 |
| density | .0007479 | .0002246 | 3.33 | 0.002 | .0002968 | .001199 |
| beerpc | .0208412 | .0071937 | 2.90 | 0.006 | .0063921 | .0352902 |
| incomepc | -.0822314 | .0414462 | -1.98 | 0.053 | -.1654785 | .0010158 |
| unrate | .022881 | .028482 | 0.80 | 0.426 | -.0343268 | .0800889 |
| employ | 3.47576 | 2.556984 | 1.36 | 0.180 | -1.660093 | 8.611613 |
| z5059 | .2622278 | .0941282 | 2.79 | 0.008 | .0731657 | .4512899 |
| | | | | | | |
| suicidepc | | | | | | |
| L1. | .3625092 | .0294696 | 12.30 | 0.000 | .3033178 | .4217007 |
| L2. | .3002099 | .0400099 | 7.50 | 0.000 | .2198477 | .380572 |
| | | | | | | |
| yrdum1 | 0 | (omitted) | | | | |
| yrdum2 | 0 | (omitted) | | | | |
| yrdum3 | 0 | (omitted) | | | | |
| yrdum4 | 0 | (omitted) | | | | |
| yrdum5 | 0 | (omitted) | | | | |
| yrdum6 | 0 | (omitted) | | | | |
| yrdum7 | 0 | (omitted) | | | | |
| yrdum8 | 0 | (omitted) | | | | |
| yrdum9 | 0 | (omitted) | | | | |
| yrdum10 | 0 | (omitted) | | | | |
| yrdum11 | .4356943 | .3854324 | 1.13 | 0.264 | -.3384695 | 1.209858 |
| yrdum12 | .5556203 | .3402577 | 1.63 | 0.109 | -.1278075 | 1.239048 |
| yrdum13 | .3370504 | .3620303 | 0.93 | 0.356 | -.3901089 | 1.06421 |
| yrdum14 | .1011661 | .2865608 | 0.35 | 0.726 | -.4744083 | .6767406 |
| yrdum15 | .3697405 | .3629165 | 1.02 | 0.313 | -.3591987 | 1.09868 |
| yrdum16 | .6615427 | .282016 | 2.35 | 0.023 | .0950969 | 1.227988 |
| yrdum17 | .1350245 | .2064422 | 0.65 | 0.516 | -.2796269 | .5496759 |
| yrdum18 | .8062656 | .2909352 | 2.77 | 0.008 | .221905 | 1.390626 |
| yrdum19 | -.285485 | .2586631 | -1.10 | 0.275 | -.8050251 | .2340551 |
| yrdum20 | -.3762046 | .2967443 | -1.27 | 0.211 | -.9722331 | .2198239 |
| yrdum21 | -.3948035 | .1958797 | -2.02 | 0.049 | -.7882396 | -.0013675 |
| yrdum22 | -.0024573 | .2188568 | -0.01 | 0.991 | -.4420441 | .4371294 |
| yrdum23 | .2868649 | .2311598 | 1.24 | 0.220 | -.1774332 | .751163 |
| yrdum24 | 0 | (omitted) | | | | |
| yrdum25 | .4683969 | .1751413 | 2.67 | 0.010 | .1166152 | .8201786 |
| yrdum26 | .3038055 | .1762099 | 1.72 | 0.091 | -.0501225 | .6577334 |
| yrdum27 | 1.101573 | .229724 | 4.80 | 0.000 | .6401583 | 1.562987 |
| yrdum28 | .9268991 | .2991654 | 3.10 | 0.003 | .3260078 | 1.52779 |
| yrdum29 | .0099184 | .2331913 | 0.04 | 0.966 | -.45846 | .4782969 |
| yrdum30 | .2183786 | .2361884 | 0.92 | 0.360 | -.2560198 | .6927771 |
| yrdum31 | .5858106 | .210523 | 2.78 | 0.008 | .1629626 | 1.008659 |
| yrdum32 | .2712886 | .1636334 | 1.66 | 0.104 | -.0573787 | .599956 |
| yrdum33 | .0469388 | .2128729 | 0.22 | 0.826 | -.3806289 | .4745066 |
| yrdum34 | .6124047 | .2061021 | 2.97 | 0.005 | .1984364 | 1.026373 |
| yrdum35 | .3017597 | .1766735 | 1.71 | 0.094 | -.0530994 | .6566188 |
| yrdum36 | .2204143 | .2515708 | 0.88 | 0.385 | -.2848805 | .7257091 |
| yrdum37 | .0398854 | .2580766 | 0.15 | 0.878 | -.4784768 | .5582476 |

| | | | | | | |
|---|---|---|---|---|---|---|
| yrdum38 | -.109179 | .2268607 | -0.48 | 0.632 | -.5648422 | .3464841 |
| yrdum39 | -.2230519 | .3085249 | -0.72 | 0.473 | -.8427424 | .3966386 |
| yrdum40 | -.8830286 | .278834 | -3.17 | 0.003 | -1.443083 | -.322974 |
| yrdum41 | -.727179 | .3482243 | -2.09 | 0.042 | -1.426608 | -.0277499 |
| yrdum42 | -.1776571 | .4126329 | -0.43 | 0.669 | -1.006455 | .6511405 |
| yrdum43 | -.2118653 | .3422585 | -0.62 | 0.539 | -.8993117 | .475581 |
| yrdum44 | -.5832658 | .3529017 | -1.65 | 0.105 | -1.29209 | .1255582 |
| yrdum45 | -.4769007 | .477087 | -1.00 | 0.322 | -1.435158 | .4813568 |
| yrdum46 | -.6499211 | .4852059 | -1.34 | 0.186 | -1.624486 | .3246437 |
| yrdum47 | -.639264 | .4593253 | -1.39 | 0.170 | -1.561846 | .283318 |
| yrdum48 | -.4155356 | .5353992 | -0.78 | 0.441 | -1.490916 | .6598453 |
| yrdum49 | -.1018213 | .4876625 | -0.21 | 0.835 | -1.08132 | .8776776 |
| yrdum50 | -.3810404 | .5779268 | -0.66 | 0.513 | -1.541841 | .7797597 |
| yrdum51 | .1952034 | .5467013 | 0.36 | 0.723 | -.9028786 | 1.293285 |
| yrdum52 | .0956001 | .6224616 | 0.15 | 0.879 | -1.154651 | 1.345851 |
| yrdum53 | .3315218 | .5338184 | 0.62 | 0.537 | -.740684 | 1.403728 |
| yrdum54 | .1620503 | .6283892 | 0.26 | 0.798 | -1.100107 | 1.424207 |
| yrdum55 | .5639499 | .6603891 | 0.85 | 0.397 | -.7624807 | 1.89038 |
| yrdum56 | .9620236 | .5682977 | 1.69 | 0.097 | -.179436 | 2.103483 |
| yrdum57 | 0 | (omitted) | | | | |
| stdum1 | 11.13636 | 3.784784 | 2.94 | 0.005 | 3.534403 | 18.73833 |
| stdum2 | 13.53406 | 3.652412 | 3.71 | 0.001 | 6.197977 | 20.87015 |
| stdum3 | 12.72915 | 3.844363 | 3.31 | 0.002 | 5.007523 | 20.45078 |
| stdum4 | 11.66224 | 3.780605 | 3.08 | 0.003 | 4.068669 | 19.25581 |
| stdum5 | 11.34119 | 3.70546 | 3.06 | 0.004 | 3.898549 | 18.78382 |
| stdum6 | 12.70345 | 3.665679 | 3.47 | 0.001 | 5.340721 | 20.06619 |
| stdum7 | 9.967569 | 3.592826 | 2.77 | 0.008 | 2.751166 | 17.18397 |
| stdum8 | 10.48443 | 3.582072 | 2.93 | 0.005 | 3.289623 | 17.67923 |
| stdum9 | 0 | (omitted) | | | | |
| stdum10 | 11.79508 | 3.780491 | 3.12 | 0.003 | 4.201741 | 19.38842 |
| stdum11 | 11.17789 | 3.693722 | 3.03 | 0.004 | 3.758831 | 18.59695 |
| stdum12 | 10.37448 | 3.556042 | 2.92 | 0.005 | 3.231958 | 17.517 |
| stdum13 | 12.58678 | 3.753134 | 3.35 | 0.002 | 5.04839 | 20.12517 |
| stdum14 | 10.03331 | 3.65533 | 2.74 | 0.008 | 2.691364 | 17.37526 |
| stdum15 | 11.02375 | 3.704326 | 2.98 | 0.004 | 3.583387 | 18.4641 |
| stdum16 | 10.71721 | 3.677677 | 2.91 | 0.005 | 3.330376 | 18.10404 |
| stdum17 | 11.33606 | 3.64603 | 3.11 | 0.003 | 4.012792 | 18.65933 |
| stdum18 | 11.64949 | 3.77549 | 3.09 | 0.003 | 4.066195 | 19.23278 |
| stdum19 | 10.95825 | 3.75236 | 2.92 | 0.005 | 3.421409 | 18.49508 |
| stdum20 | 11.23114 | 3.709432 | 3.03 | 0.004 | 3.780522 | 18.68175 |
| stdum21 | 10.21925 | 3.670624 | 2.78 | 0.008 | 2.846588 | 17.59192 |
| stdum22 | 9.254421 | 3.465579 | 2.67 | 0.010 | 2.2936 | 16.21524 |
| stdum23 | 10.87566 | 3.753597 | 2.90 | 0.006 | 3.336342 | 18.41498 |
| stdum24 | 10.77035 | 3.642169 | 2.96 | 0.005 | 3.454838 | 18.08586 |
| stdum25 | 10.82379 | 3.777032 | 2.87 | 0.006 | 3.237399 | 18.41018 |
| stdum26 | 11.33327 | 3.713079 | 3.05 | 0.004 | 3.875331 | 18.79121 |
| stdum27 | 12.78292 | 3.748874 | 3.41 | 0.001 | 5.253081 | 20.31275 |
| stdum28 | 10.42867 | 3.61015 | 2.89 | 0.006 | 3.177474 | 17.67987 |
| stdum29 | 13.60034 | 3.754266 | 3.62 | 0.001 | 6.059673 | 21.141 |
| stdum30 | 10.48572 | 3.668393 | 2.86 | 0.006 | 3.117539 | 17.85391 |
| stdum31 | 8.983408 | 3.541887 | 2.54 | 0.014 | 1.869318 | 16.0975 |
| stdum32 | 13.13991 | 3.858584 | 3.41 | 0.001 | 5.389717 | 20.8901 |
| stdum33 | 9.654414 | 3.67742 | 2.63 | 0.011 | 2.268099 | 17.04073 |
| stdum34 | 11.04445 | 3.66691 | 3.01 | 0.004 | 3.679247 | 18.40966 |
| stdum35 | 10.71417 | 3.554954 | 3.01 | 0.004 | 3.573839 | 17.85451 |

```
     stdum36 |   10.48448    3.67303     2.85   0.006     3.106987    17.86198
     stdum37 |   12.11124   3.775963     3.21   0.002     4.526995    19.69549
     stdum38 |   12.16748    3.71904     3.27   0.002     4.697567    19.63739
     stdum39 |   10.59062    3.72816     2.84   0.006     3.102392    18.07885
     stdum40 |   9.508879   3.514931     2.71   0.009     2.448931    16.56883
     stdum41 |   10.79447   3.707857     2.91   0.005     3.347025    18.24192
     stdum42 |   11.45761    3.64295     3.15   0.003     4.140533     18.7747
     stdum43 |    11.3488   3.689924     3.08   0.003     3.937366    18.76023
     stdum44 |   10.81993   3.685488     2.94   0.005     3.417406    18.22244
     stdum45 |   13.04792   3.745425     3.48   0.001     5.525011    20.57083
     stdum46 |   11.43352   3.641139     3.14   0.003     4.120082    18.74697
     stdum47 |   11.23903   3.695953     3.04   0.004     3.815489    18.66257
     stdum48 |   11.77498      3.725     3.16   0.003     4.293101    19.25687
     stdum49 |   11.52255   3.900466     2.95   0.005     3.688234    19.35687
     stdum50 |   10.51281   3.644911     2.88   0.006     3.191795    17.83383
     stdum51 |   13.22569    3.69275     3.58   0.001     5.808587     20.6428
       _cons |  -12.15374   4.835672    -2.51   0.015    -21.86648   -2.441009
------------------------------------------------------------------------------

.
.
.
. log close
      name:  <unnamed>
       log:  C:\Users\cemood\Box Sync\Illinois\report\suicide.log
  log type:  text
 closed on:  11 May 2019, 18:03:10
---------------------------------------------------------------------------------------
---------------------------------------------------------------------------------------
-------------------------------------------------------------------------
```

## Appendix G: Stata program file used to produce Figure 1

This is the file used to produce Figure 1, the historical chart of homicide and suicide rates in Missouri.

```
cd "C:\Users\cemood\Box Sync\Illinois\report"
use Illinois.dta, clear

keep if year>=1960
label var suicidepc "Suicide rate"
label var year "Year"

twoway (line crmurpc year) (line gunhomrate year,lpattern(dash)) (line
suicidepc year,lpattern(shortdash)) if stnm=="MO", xline(2003)
xline(2007,lpattern(dash)) xtick(1960(10)2020) xmtick(1965(5)2015)

/* Note that all three series had a turning point in 2003 and started
increasing then, four years before ptp repeal */
```

## Appendix H: Conley-Taber article

This is the article discussed in Part IV.C.2 of the brief, Timothy G. Conley & Christopher R. Taber, *Inferences with "Difference in Differences" with a small number of policy changes*, 93 REV. OF ECON. & STATS. 113 (2011). Because the article is paywalled, it is included here for the Court's convenience.

# INFERENCE WITH "DIFFERENCE IN DIFFERENCES" WITH A SMALL NUMBER OF POLICY CHANGES

Timothy G. Conley and Christopher R. Taber*

*Abstract*—In difference-in-differences applications, identification of the key parameter often arises from changes in policy by a small number of groups. In contrast, typical inference assumes that the number of groups changing policy is large. We present an alternative inference approach for a small (finite) number of policy changers, using information from a large sample of nonchanging groups. Treatment effect point estimators are not consistent, but we can consistently estimate their asymptotic distribution under any point null hypothesis about the treatment. Thus, treatment point estimators can be used as test statistics, and confidence intervals can be constructed using test statistic inversion.

## I. Introduction

THIS paper presents a new method of inference for difference-in-differences type fixed-effect regression methods for circumstances in which only a small number of groups provide information about treatment parameters of interest. In the difference-in-differences methodology, identification of the treatment parameter typically arises when a group changes some particular policy. We use $N_1$ to denote the number of treatment groups that change their policy in the data and $N_0$ to denote the number of control groups that do not change their policy. The usual asymptotic approximations assume that both $N_1$ and $N_0$ are large. However, even when the total number of observations is large, the number of actual policy changes observed in the data is often very small. For example, often only a few states change a law within the time span ($T$) of the data. In such cases, we argue that the standard large-sample approximations used for inference are not appropriate.[1] We develop an alternative approach to inference under the assumption that $N_1$ is finite, using asymptotic approximations that let $N_0$ grow large (with $T$ fixed). Point estimators of the treatment effect parameter(s) are not consistent since $N_1$ and $T$ are fixed. However, we can use information from the $N_0$ control groups to consistently estimate the distribution of these point estimators up to the true values of the parameter. This allows us to use treatment parameter point estimators as test statistics for any hypothesized true treatment parameter values and to construct confidence intervals by inverting these test statistics.

The following simple model illustrates our basic problem and approach to its solution:

$$Y_{jt} = \alpha d_{jt} + \theta_j + \gamma_t + \eta_{jt}, \tag{1}$$

where $d_{jt}$ is a policy variable whose coefficient $\alpha$ is the object of interest, $\theta_j$ is a time-invariant fixed effect for group $j$, $\gamma_t$ is a time fixed effect that is common across all groups but varies across time $t = 1, \ldots, T$, and $\eta_{jt}$ is a group $\times$ time random effect.

Suppose that only the $j = 1$ group experiences a treatment change and that it happens to be a permanent one-unit change at period $t^*$. All other groups have a time-invariant policy: $d_{j1} = \ldots = d_{jT}$. Consider estimating model (1) by using ordinary least squares (OLS), controlling for group and time effects using dummy variables. Let $\widehat{\alpha}_{FE}$ be this regression estimate of $\alpha$. It is straightforward to show that $\widehat{\alpha}_{FE}$ can be written as a difference of differences:

$$\begin{aligned}\widehat{\alpha}_{FE} = \alpha &+ \left[\frac{1}{T-t^*}\sum_{t=t^*+1}^{T}\eta_{1t} - \frac{1}{t^*}\sum_{t=1}^{t^*}\eta_{1t}\right]\\ &- \left(\frac{1}{(N_0)}\sum_{j=2}^{N_0+1}\frac{1}{(T-t^*)}\sum_{t=t^*+1}^{T}\eta_{jt}\right.\\ &\left.- \frac{1}{(N_0)}\sum_{j=2}^{N_0+1}\frac{1}{t^*}\sum_{t=1}^{t^*}\eta_{jt}\right).\end{aligned}$$

Under the usual assumption that $\eta_{jt}$ has mean zero conditional on the regressors, $\widehat{\alpha}_{FE}$ is unbiased. However, it is not consistent. As the number of groups grows, only the term in parentheses vanishes; the term in brackets remains unchanged as $N_0$ gets large (with $T$ fixed), that is

$$(\widehat{\alpha}_{FE} - \alpha) \xrightarrow{p} W \equiv \left[\frac{1}{T-t^*}\sum_{t=t^*+1}^{T}\eta_{1t} - \frac{1}{t^*}\sum_{t=1}^{t^*}\eta_{1t}\right].$$

In other words, the $\widehat{\alpha}_{FE}$ estimate is equal to the true parameter of interest $\alpha$ plus noise $W$. The key issue is that because $T$ is fixed and the number of treatment groups is fixed at $N_1$, the noise $W$ does not vanish as the total number of groups grows larger.

This problem is rarely acknowledged in empirical work, and researchers often ignore it when calculating standard errors. If the classical linear model were applicable, standard methods would yield the correct small sample inference (see Donald & Lang, 2007). However, for many applications, the classical model does not apply (e.g., due to nonnormal $\eta_{jt}$ or serial correlation in $\eta_{jt}$). In such cases, classical inference can be misleading.

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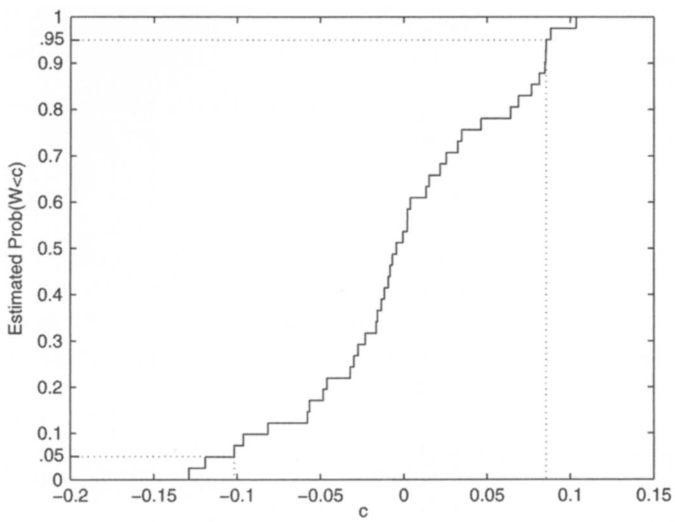
[1] Of course in some special cases, the classical linear model assumptions will be satisfied, enabling small sample inference (see, e.g., Donald & Lang, 2007). Here our methods remain useful as specification checks, but they will be most valuable when the classical model may not be applicable.

*The Review of Economics and Statistics*, February 2011, 93(1): 113–125

FIGURE 1.—EXAMPLE ESTIMATE OF CDF FOR $W$




In this paper, we show that although $\widehat{\alpha}_{FE}$ is not consistent, we can still conduct inference and construct confidence intervals for $\alpha$ with a general $\eta_{jt}$ distribution. The key idea behind our approach is that although the control groups are uninformative regarding $\alpha$, they can still contain information about the distribution of the noise $W$, a linear combination of $\eta$'s. Thus, the large number of observations for the controls may allow consistent estimation of the $W$ distribution. To be precise, a necessary condition for our approach is that the distribution of $W$ can be identified from the population of controls. A sufficient condition is random assignment of treatment change conditional on group and time dummy variables, which implies common $\eta$ distributions for treatments and controls. Under such an assumption, we can use the residuals from the control groups to learn about the limiting distribution of $W$. Let $\hat{\eta}_{jt}$ denote residuals and $\hat{W}_j$ denote the function of residuals that is analogous to $W$:

$$\hat{W}_j \equiv \frac{1}{T-t^*} \sum_{t=t^*+1}^{T} \widehat{\eta}_{jt} - \frac{1}{t^*} \sum_{t=1}^{t^*} \widehat{\eta}_{jt}.$$

As $N_0$ gets large, the $\hat{W}_j$ will have the same distribution as $W$. A test of the hypothesis that $\alpha = \alpha_0$ is easily conducted by comparing $(\widehat{\alpha}_{FE} - \alpha_0)$ with the empirical distribution of $\{\hat{W}_j\}_{j=2}^{N_0}$. The null hypothesis is rejected when $(\widehat{\alpha}_{FE} - \alpha_0)$ is a sufficiently unlikely (tail) event according to this distribution.

We illustrate this approach in figure 1 which is based on data from our empirical example in section IV. We present the empirical distribution of $\hat{W}_j$, a consistent estimate of the distribution of $(\widehat{\alpha}_{FE} - \alpha_0)$ under the null hypothesis that $\alpha = \alpha_0$. An acceptance region can be constructed by finding appropriate quantiles of this empirical distribution. For example, the interval $-.11$ to .09 in figure 1 corresponds to an approximately 90% acceptance region. If $(\widehat{\alpha}_{FE} - \alpha_0)$ does not fall within that range, the null hypothesis $\alpha = \alpha_0$ is rejected. The set of $\alpha_0$ thats fails to be rejected provides an approximate 90% confidence interval for $\alpha$. In this example, $\hat{\alpha}$ is approximately .08, which yields a 90% confidence interval for $\alpha$ of $-.01$ to .19.

Our approach is related to a large body of existing work on difference-in-differences models and inference in more general group effect models.[2] It is complementary to typical approaches focusing on situations where the numbers of treatment and control groups, $N_1$ and $N_0$, are both large (Moulton, 1990) or both small (Donald & Lang, 2007). It is also in the spirit of comparisons of changes in treatment groups to changes in control groups often done by careful applied researchers. For example, Anderson and Meyer (2000) examine the effect of changes in the unemployment insurance payroll in Washington State on a number of outcomes using a difference-in-differences approach with all other states representing the control groups. In addition to standard analysis, they compare the change in the policy in Washington State to the distribution of changes across other states during the same period of time in order to determine whether it is an outlier consistent with a policy effect.[3]

This approach is relevant for a wide range of applications. Examples include Gruber, Levine, and Staiger (1999) who use comparisons between the five treatment states that legalized abortion prior to *Roe* v. *Wade* versus the remaining states. Our results apply directly, with $N_1$ corresponding to the five initial movers. For expositional and motivational purposes, we focus on the difference-in-differences case, but our approach is appropriate more generally in treatment effect models with a large number of controls and a small number of treatments.[4] Hotz, Mullin, and Sanders

[2] There are so many examples of difference-in-differences style empirical work that we do not attempt to survey them. See Meyer (1995), Angrist and Krueger (1999), and Bertrand, Duflo, and Mullainathan (2004) for overviews of difference-in-differences methods. Wooldrige (2003) provides an excellent and concise survey of closely related group effect models.

[3] Though it does not appear in the published version, section 4.6 of Bertrand, Duflo, and Mullainathan (2002) describes a placebo laws experiment that is related to some aspects of our approach. They use simulation experiments under specific joint hypotheses about the policy and distribution of covariates to assess the size and power of typical tests (based on large-$N_0$ and large-$N_1$). Such experiments could also be used to recover the finite sample distribution of a treatment effect parameter under a particular null hypothesis.

Abadie, Diamond, and Hainmuller (2010) (ADH) is another related paper that uses placebo laws to do inference. However, their main focus is on how to choose the best comparisons for the treated units using combinations of untreated units, which they call synthetic controls. They provide theoretical justification for the use of synthetic controls and compare estimates obtained for the treated units to estimated placebo effects for untreated units to test the null of no treatment effect. In contrast, our paper focuses on inference for treatment parameters after the important choice of controls has been made by the researcher.

[4] One can also find many studies that use a small number of treatments and controls. However, if there exist group × time effects, the usual approach for inference is inappropriate. An alternative sample design is to collect many control groups (with the inherent cost of a reduction of match quality). One could then use our methods for appropriate inference. For example, Card and Krueger (1994) examine the impact of the New Jersey minimum wage law change on employment in the fast food industry. Their sample design has only one control group (eastern Pennsylvania), but they could have collected data from many control states to contrast with the available treatment state. We view this not as a substitute for the analysis that they perform, but rather a complement to check the robustness of the results.

(1997) provide a good example outside the difference-in-differences literature: they estimate the effect of teenage pregnancy on labor market outcomes of mothers. The key to their analysis is using miscarriage as an instrument for teenage motherhood. Of their sample of 980 women who had a teenage pregnancy, only 68 experienced miscarriages. Our basic approach could be extended to this type of application, with the 68 miscarriages taken as fixed like $N_1$ and the approximate distributions of estimators calculated treating only the nonmiscarried pregnancies as a large sample.

Our final example is the study of merit aid policies, which we use in section IV to illustrate our methods. Merit aid programs provide college tuition assistance to students who attend college in state and maintain a sufficiently high grade point average during high school. Some of the studies in the literature estimate the effect using only a single state that changed its law (Georgia), while newer studies make use of ten states.[5] We demonstrate our methodology and show that accounting for the small number of treatment states is important as the confidence intervals become substantially larger than those formed by the standard approach.

The closest analog to our inference method in econometrics is work on testing for end-of-sample structural breaks—in particular, work such as that by Dufour, Ghysels, and Hall (1994) and Andrews (2003) on the problem of testing for a structural break over a fixed and perhaps very short interval at the end of a sample. They develop tests that are asymptotically valid as the number of observations before the potential break point grows, holding fixed the number of time periods after the break. Their exact models, hypotheses of interest, and structure of proofs differ considerably from ours, but we both use the same basic idea for inference. This idea is to use the small number of observations after the break or $N_1$ changers as the basis for constructing a test statistic whose reference distribution can be well estimated using the large number of observations before the potential break or $N_0$ controls.

The remainder of this paper presents our approach in the simplest case of group $\times$ time data (e.g., collected at the state $\times$ year level) and a common treatment parameter in section II. Extensions to allow heterogeneity in treatment parameters across groups, individual-level data, and cross-sectional dependence and heteroskedasticity are described in section III. In section IV, we present an illustrative example of our approach by studying the effect of merit scholarships. Section V presents the results of a small simulation study of our estimator's performance, followed by a brief conclusion in section VI. Proofs of propositions 1 and 2 are contained in an appendix; all other material is contained in a Web appendix available at the *Review*'s Web site, http://www.mitpressjournals.org/doi/suppl/10.1162/REST_a_00049.

## II. Base Model

Our base model is for situations where data are available at a group $\times$ time level:

$$Y_{jt} = \alpha d_{jt} + X_{jt}'\beta + \theta_j + \gamma_t + \eta_{jt}, \tag{2}$$

where $d_{jt}$ is the policy variable that need not be binary, $X_{jt}$ is a vector of regressors with parameter vector $\beta$, $\theta_j$ is a time-invariant fixed effect for group $j$, $\gamma_t$ is a time effect that is common across all groups but varies across time $t = 1, \ldots, T$, and $\eta_{jt}$ is a group $\times$ time random effect. We take $\alpha$ to be the parameter of interest. We use the label "group" because in typical applications, $j$ would index states, counties, or countries, though nothing precludes a group from being a single individual. This data could be either intrinsically group level or aggregates of individuals within a group. In section IIIB, we extend this framework to data with multiple individuals per group, retaining the feature that $d_{jt}$ varies only across group-time cells not within them.

The key problem motivating our approach is that for many groups, there is no temporal variation in $d_{jt}$. We adopt the convention of indexing the $N_1$ groups whose value of $d_{jt}$ changes during the observed time span with the integers 1 to $N_1$. The integers from $N_1 + 1$ to $N_1 + N_0$ then refer to the remaining groups for which $d_{jt}$ is constant from $t = 1$ to $T$. We treat $N_1$ and $T$ as fixed, taking limits as $N_0$ grows large. We assume throughout that at least one group changes its policy so that $N_1 \geq 1$.

It is convenient to partial out variation explained by indicators for groups and times and to have notation for averages across groups and time. Therefore, for generic variable $Z_{jt}$, we define $\overline{Z}_j = \frac{1}{T}\sum_{t=1}^{T} Z_{jt}$, $\overline{Z}_t = \frac{1}{N_0+N_1}\sum_{j=1}^{N_0+N_1} Z_{jt}$, and use the notation $\overline{Z}$ for the average of $Z_{jt}$ across both groups and time periods. We define a variable $\widetilde{Z}_{jt}$ that equals the residual from a projection of $Z_{jt}$ on group and time indicators: $\widetilde{Z}_{jt} = Z_{jt} - \overline{Z}_j - \overline{Z}_t + \overline{Z}$. The essence of difference in differences is that we can rewrite regression model (2) as

$$\widetilde{Y}_{jt} = \alpha\widetilde{d}_{jt} + \widetilde{X}_{jt}'\beta + \widetilde{\eta}_{jt}, \tag{3}$$

and we can then estimate $\alpha$ by regressing $\widetilde{Y}_{jt}$ on $\widetilde{d}_{jt}$ and $\widetilde{X}_{jt}$. Let $\widehat{\alpha}$ and $\widehat{\beta}$ denote the OLS estimates of $\alpha$ and $\beta$ in equation (3).

We assume a set of regularity conditions stated as assumption 1, most of them routine. The conditions need to imply that changes in $\eta_{jt}$ are uncorrelated with changes in regressors, and the usual moment and rank conditions hold. The only (slightly) unusual condition we use describes the cross-sectional dependence of our data. We generalize the standard independence assumption to allow the data to be cross-sectionally strong mixing (see Conley, 1999). This presumes the existence of a coordinate space in which our observations can be indexed. Mixing refers to observations approaching independence as their distance grows, a direct analog of the time series property with the same name. We omit an explicit notation for these coordinates for ease of exposition.

**Assumption 1.** *$((X_{j1}, \eta_{j1}), \ldots, (X_{jT}, \eta_{jT}))$ is strong mixing across groups; $(\eta_{j1}, \ldots, \eta_{jT})$ is expectation zero conditional on $(d_{j1}, \ldots, d_{jT})$ and $(X_{j1}, \ldots, X_{jT})$; all random variables have finite second moments. The regressors in equation (3),*

[5] Our specifications are motivated by Dynarski (2004).

*$\widetilde{d}_{jt}, \widetilde{X}_{jt}$, are linearly independent. Finally, we assume that after the projection of X on time and group fixed effects, the residual regressors $\widetilde{X}_{jt}$ still have variation in the limit, which we state as*

$$\frac{1}{N_0 + N_1} \sum_{j=1}^{N_0+N_1} \sum_{t=1}^{T} \widetilde{X}_{jt}\widetilde{X}_{jt}' \xrightarrow{p} \Sigma_x,$$

*where $\Sigma_x$ is finite and of full rank.*

Assumption 1 is similar but weaker than the standard set of assumptions made in difference-in-differences applications. It is weaker in that we allow the data to be weakly dependent across groups rather than the usual assumption of independence across groups. The key difference between our setup and the usual setting is that we are assuming $N_1$ is small and fixed versus the usual assumption that it is large, and our corresponding assumption that there is temporal variation in $d_{jt}$ only for $N_1$ observations. In proposition 1, we state that OLS yields a consistent estimator of $\beta$ (as $N_0 \to \infty, N_1, T$ fixed), and we derive the limiting distribution of $\widehat{\alpha}$:

**Proposition 1.** *Under assumption 1, $N_0 \to \infty : \widehat{\beta} \xrightarrow{p} \beta$ and $\widehat{\alpha}$ is unbiased and converges in probability to $\alpha + W$, with:*

$$W = \frac{\sum_{j=1}^{N_1} \sum_{t=1}^{T} (d_{jt} - \overline{d}_j)(\eta_{jt} - \overline{\eta}_j)}{\sum_{j=1}^{N_1} \sum_{t=1}^{T} (d_{jt} - \overline{d}_j)^2}. \tag{4}$$

**Proof.** See the appendix.

The proposition states that while $\widehat{\alpha}$ is unbiased, it is not consistent (as $N_0 \to \infty, N_1, T$ fixed). Its limiting distribution is centered at $\alpha$, with deviation from $\alpha$ given by $W$, a linear combination of $(\eta_{jt} - \overline{\eta}_j)$ for $j = 1$ to $N_1$ and $t = 1$ to $T$. The nice aspect of this result is that inference for $\alpha$ remains feasible if we can estimate relevant aspects of the distribution of $W$.

Our approach is to estimate the conditional distribution of $W$ given the observable $d_{jt}$ for the treatment groups. Thus, we need to identify the conditional distribution of $\{(\eta_{jt} - \overline{\eta}_j)\}$ for $j = 1$ to $N_1$ and $t = 1$ to $T$ given the corresponding set of $d_{jt}$ values. In order to do so, we assume that the distribution of $(\eta_{jt} - \overline{\eta}_j)$ given $d_{jt}$ for the treatments is the same as that for the controls. The time-invariant $d_{jt}$ for our controls cannot be informative about all forms of conditional $\eta_{jt}$ distributions given the treatments' time-varying $d_{jt}$ series. Thus for feasibility, we must restrict ourselves to a model that is estimable with time-invariant $d_{jt}$. Random assignment of $d_{jt}$ conditional on $X_{jt}$, time dummies, and group dummies would be sufficient here, implying common $(\eta_{jt} - \overline{\eta}_j)$ distributions for treatments and controls. Assumptions implying common $\eta$ distributions for treatments and controls are beyond what is necessary for difference-in-differences applications with large $N_1$. Large $N_1$ allows more heterogeneity in the distribution of $\eta$ conditional on $d_{jt}$ to be tolerated. Terms like $W$ will vanish, and distribution approximations can exploit the large treatment sample size. However, in many cases, researchers justify their difference-in-differences approach by arguing that it is reasonable to think of $d_{jt}$ as randomly assigned (conditional on group and time dummy variables). When this is the case, our approach imposes no further restrictions.

For ease of exposition, we first discuss estimation under a simple model in which the $(\eta_{j1}, \ldots, \eta_{jT})$ are independent of regressors and independent and identically distributed (i.i.d.) across groups, stated as assumption 2. This still allows arbitrary serial correlation in $\eta_{jt}$. It is important to note that assumption 2 is not necessary for our approach; it can be replaced by any model of cross-sectionally stationary data, with, for example, spatially correlated or conditionally heteroskedastic $\eta_{jt}$, that is, estimable given data from the controls.[6] In the Web appendix, we present an example model that allows temporal and spatial dependence in $\eta_{jt}$ and heteroskedasticity depending on group population.[7]

**Assumption 2.** *$(\eta_{j1}, \ldots, \eta_{jT})$ is i.i.d. across j and independent of $(d_{j1}, \ldots, d_{jT})$ and $(X_{j1}, \ldots, X_{jT})$, with a bounded density.*

To see how the distribution of $(\eta_{jt} - \overline{\eta}_j)$ can be estimated under assumption 2, consider the residual for a member of the control group ($j > N_1$),

$$\begin{aligned} \widetilde{Y}_{jt} - \widetilde{X}_{jt}'\hat{\beta} &= \widetilde{X}_{jt}'(\hat{\beta} - \beta) + (\eta_{jt} - \overline{\eta}_j - \overline{\eta}_t + \overline{\eta}) \\ &\xrightarrow{p} (\eta_{jt} - \overline{\eta}_j). \end{aligned} \tag{5}$$

The term involving $\widetilde{X}_{jt}$ vanishes since $\hat{\beta}$ is consistent, and the $\eta$ term simplifies because $\overline{\eta}_t$ and $\overline{\eta}$ vanish. Thus, if $\{(\eta_{jt} - \overline{\eta}_j)\}_{t=1}^{T}$ is i.i.d. across groups, its distribution for the treatment groups, $j \le N_1$, is trivially identified using residuals for control groups $j > N_1$.

We first consider estimators implied by the sample analog estimator of the distribution of $\{(\eta_{jt} - \overline{\eta}_j)\}_{t=1}^{T}$, that is, the empirical distribution of residuals from control groups.[8] This implies an estimator of the conditional distribution of $W$ given the $d_{jt}$ for the treatment groups. Defining this distribution as $\Gamma(w) \equiv \Pr(W < w \mid \{d_{jt}, j = 1, \ldots, N_1, t = 1, \ldots, T\})$, its sample analog estimator is

$$\widehat{\Gamma}(w) \equiv \left(\frac{1}{N_0}\right)^{N_1} \sum_{\ell_1 = N_1+1}^{N_1+N_0} \cdots \sum_{\ell_{N_1} = N_1+1}^{N_1+N_0} 1\left( \frac{\sum_{j=1}^{N_1} \sum_{t=1}^{T} (d_{jt} - \overline{d}_j)(\widetilde{Y}_{\ell_j t} - \widetilde{X}_{\ell_j t}'\hat{\beta})}{\sum_{j=1}^{N_1} \sum_{t=1}^{T} (d_{jt} - \overline{d}_j)^2} < w \right).$$

[6] Stationarity refers to the joint distribution of observations indexed in a Euclidean space being invariant to translation in their indexes. Observations have identical marginal distributions, and sets of observations with indexes that differ only by a translation have identical distributions.

[7] See Conley and Taber (2005) for an alternative model in this framework that allows for heteroskedasticity arising from variation in group populations along with arbitrary serial dependence but with spatial independence.

[8] Of course, the residuals could also be used to estimate any parametric model of their distribution. This may be a preferable practical strategy in applications with moderately large $N_0$.

We state a consistency result for $\widehat{\Gamma}(w)$ as proposition 2.

**Proposition 2.** *Under assumptions 1 and 2 and assuming* $\beta$ *is interior to a compact parameter space, as* $N_0 \to \infty$, $\widehat{\Gamma}(w)$ *converges in probability to* $\Gamma(w)$ *uniformly on any compact subset of the support of* $W$.

**Proof.** See the appendix.

Given the consistent estimator $\widehat{\Gamma}(w)$, it is straightforward to conduct hypothesis tests regarding $\alpha$ using $\widehat{\alpha}$ as a test statistic. Under the null hypothesis that the true value of $\alpha = \alpha_0$, the large sample ($N_0$ large) approximation following from proposition 1 is that $\hat{\alpha}$ is distributed as $\alpha_0 + W$ conditional on $\{d_{jt}, j = 1, \ldots, N_1, t = 1, \ldots, T\}$. Therefore, we consistently estimate the distribution function $\Pr(\hat{\alpha} < c)$ via $\hat{\Gamma}(c - \alpha_0)$ and use its appropriate quantiles to define an asymptotically valid acceptance region for this null hypothesis.[9] For example, a 90% acceptance region could be estimates as $[\widehat{\alpha}_{lower}, \widehat{\alpha}_{upper}]$ with these end points being the 5th and 95th percentiles of this distribution: $\hat{\Gamma}(\widehat{\alpha}_{lower} - \alpha_0) \approx .05$ and $\hat{\Gamma}(\widehat{\alpha}_{upper} - \alpha_0) \approx .95$.[10] A 90% confidence interval for the true value of $\alpha$ can then be constructed as the set of all values of $\alpha_0$ where one fails to reject the null hypothesis that $\alpha_0$ is the true value of $\alpha$.

This might look complicated, but it is actually easy to implement. To see this, consider the example in which we have only one treatment ($N_1 = 1$) and want to test the null hypothesis that $\alpha = 0$. We use the following procedure:

1. Run the regression of $\tilde{Y}$ on $\tilde{X}$.
2. Take the residuals of the regression for the controls from group $j$ and call them $\tilde{\eta}_{jt}$.
3. Use these to form the empirical distribution of
$$\frac{\sum_{t=1}^{T}(d_{1t} - \overline{d}_1)\tilde{\eta}_{jt}}{\sum_{t=1}^{T}(d_{1t} - \overline{d}_1)^2}.$$
4. If $\hat{\alpha}$ is in the tails of this empirical distribution, reject the null hypothesis.

With more than one treatment group or a different null hypothesis, it is only marginally more difficult; step 3 is conducted with a different linear combination of residuals.

An alternative, asymptotically equivalent estimator is heuristically motivated by the literature on permutation or randomization inference (see Rosenbaum, 2002). In randomization inference, random assignment of the treatment is the basis for inference, and the exact, small sample distributions statistics are computable. The applications we have in mind are not situations with random assignment of treatment; at best, they could be described as having treatment randomly assigned conditional on $X$. In this scenario, even if recentering by subtracting $X'\beta$ were sufficient to accomplish conditioning on $X$, this would still not be enough to implement exact inference because $\beta$ must be estimated. However, we anticipate that if $\hat{\beta}$ is a good estimate of $\beta$, then plugging $\hat{\beta}$ into a permutation estimator in place of $\beta$ should provide good approximations of the small sample distribution of $W$. Such an estimator requires forming residuals under the null hypothesis for the treatment groups $(\widetilde{Y}_{\ell_j t} - \alpha_0 \widetilde{d}_{\ell_j t} - \widetilde{X}'_{\ell_j t}\hat{\beta})$, using them along with residuals from controls and using the distribution of $N_1$ draws without replacement from $N_1 + N_0$ residuals as the underlying reference distribution in place of the empirical distribution of control residuals. This gives us an estimator:

$$\widehat{\Gamma}^*(w) \equiv \frac{1}{(N_0 + N_1)(N_0 + N_1 - 1)\ldots(N_0)} \times \left[ \sum_{\ell_1 \in [1:N_1+N_0]} \sum_{\substack{\ell_2 \in [1:N_1+N_0] \\ \ell_2 \neq \ell_1}} \cdots \sum_{\substack{\ell_{N_1} \in [1:N_1+N_0] \\ \ell_{N_1} \notin \{\ell_1, \ldots, \ell_{N_1 - 1}\}}} 1\left( \frac{\sum_{j=1}^{N_1}\sum_{t=1}^{T}(d_{jt} - \overline{d}_j)(\widetilde{Y}_{\ell_j t} - \alpha_0 \widetilde{d}_{\ell_j t} - \widetilde{X}'_{\ell_j t}\hat{\beta})}{\sum_{j=1}^{N_1}\sum_{t=1}^{T}(d_{jt} - \overline{d}_j)^2} < w \right) \right].$$

The summations are over all possible assignments of treatment status to $N_1$ of the $N_1 + N_0$ total groups. While $\widehat{\Gamma}^*(w)$ is motivated by (infeasible) estimators with known exact distributions, we note that it is not an exact estimate of the distribution of $W$. The rigorous justification of $\widehat{\Gamma}^*(w)$ is that it is asymptotically equivalent (as $N_0 \to \infty, N_1, T$ fixed) to $\widehat{\Gamma}(w)$.[11]

## III. Extensions

This section presents extensions of our base model to accommodate treatment parameter heterogeneity and individual-level data. Extensions of our model to accommodate spatial dependence are presented in the Web appendix.

### A. *Treatment Parameter Heterogeneity*

It is straightforward to modify equation (2) to allow heterogeneity in treatment parameters across groups.

[9] We note that no test in this framework can be consistent as $N_1 \to \infty$ since a finite number of observations are informative regarding $\alpha$. We also make no claim that this test is optimal.

[10] We cannot obtain exact equality in these expressions because $\widehat{\Gamma}$ is a step function, but we can choose the closest point, and asymptotically the coverage probability will converge to 90%.

[11] We expect $\widehat{\Gamma}^*(w)$ to outperform $\widehat{\Gamma}(w)$ in situations for which $\hat{\beta}$ is well estimated but $N_1$ is still small enough for the empirical distribution in $\widehat{\Gamma}(w)$ to perform poorly. There are certainly applications where this is likely to be the case. For example, suppose that data are collected at the state level and that demographic regressors like income or population have substantial variation. With such large-variance regresssors, $\hat{\beta}$ may be well estimated with, say, $N_1 = 20$ states, while with only twenty observations, the empirical distribution will do a mediocre job at best of estimating conventional critical values. This situation will also arise when the model is extended to individual-level data in section III. With only individual-level regressors, coefficients analogous to $\beta$ will be estimated extremely well regardless of $N_1$ if there are many individuals within each group. This situation is routine with repeated cross-section data and arises in our empirical example to merit aid programs discussed in section IV.

Consider the extension to allow group-specific treatment parameters:

$$Y_{jt} = \alpha_j d_{jt} + X_{jt}'\beta + \theta_j + \gamma_t + \eta_{jt}. \tag{6}$$

Using the notation defined above, we can rewrite this as

$$\widetilde{Y}_{jt} = \alpha_j \widetilde{d}_{jt} + \widetilde{X}_{jt}'\beta + \widetilde{\eta}_{jt}.$$

Note that $\widetilde{d}_{jt}$ is 0 for all of the control groups; thus, we estimate treatment parameters only for $j = 1$ to $N_1$ and stack these estimable parameters in the vector $A = [\alpha_1, \ldots, \alpha_{N_1}]'$. We define $D_{jt}$ to be the $N_1 \times 1$ vector of interactions between $d_{jt}$ and group indicators. That is, the $\ell$th element of the vector $D_{jt} = d_{jt}$ if $j = \ell$ and is zero otherwise. We can then write

$$\widetilde{Y}_{jt} = \widetilde{D}_{jt}'A + \widetilde{X}_{jt}'\beta + \widetilde{\eta}_{jt}.$$

We refer to OLS estimates of $(A, \beta)$ in this regression as $(\widehat{A}, \widehat{\beta})$.

**Proposition 3.** *If assumption 1 holds, then as $N_0 \to \infty$, $\widehat{\beta} \xrightarrow{p} \beta$ and $\widehat{A}$ converges in probability to $A + W$, where $W$ is an $N_1 \times 1$ random vector with generic element*

$$W(j) = \frac{\sum_{t=1}^{T}(d_{jt} - \overline{d}_j)(\eta_{jt} - \overline{\eta}_j)}{\sum_{t=1}^{T}(d_{jt} - \overline{d}_j)^2}.$$

**Proof.** See the Web appendix, section A.1.

Testing and inference can proceed exactly as in section II. A consistent sample analog estimator of the distribution of $\widehat{A}$ under the null hypothesis that $A_0$ is the true value of $A$ can be constructed with residuals from controls. This allows testing any point null hypothesis about the heterogeneous treatment effects, and inversion of this test provides a joint confidence set for the elements of $A$. Alternatively, the distribution of any function of the elements of $A$ (e.g., their mean across groups) can also be consistently estimated, allowing analogous hypothesis testing and confidence set construction.

We have restricted the form of the treatment effect heterogeneity to vary only with $j$ for ease in exposition. Our method can be extended to allow $\alpha_{jt}$ to vary across $j$ and $t$ by inverting a corresponding set of point hypotheses tests on the $\alpha_{jt}$ for a set of groups and time periods. Extensions to situations where treatment effects depend on an observable covariates, such as the time since the policy was adopted, are also straightforward.[12]

## B. *Individual-Level Data*

Our approach can easily be applied with repeated cross-sections or panels of individual data, the relevant data type for many situations. We restrict ourselves to repeated cross-sections for ease of exposition. Let $i$ index an individual who is observed in group $j(i)$ at a single time period $t(i)$. Allowing individual-specific regressors $Z_i$ (for example, demographic characteristics) and noise $\varepsilon_i$, we arrive at a model:

$$Y_i = \lambda_{j(i)t(i)} + Z_i'\delta + \varepsilon_i \tag{7}$$

$$\lambda_{jt} = \alpha d_{jt} + X_{jt}'\beta + \theta_j + \gamma_t + \eta_{jt}. \tag{8}$$

In equation (8), $i$ subscripts have been dropped because its components vary only at the group $\times$ time level: $\lambda_{j(i)t(i)} = \lambda_{jt}$ for all individuals $i$ in group $j$ at time $t$. The difference between $Z_i$ and $X_{jt}$ is that we assume that $Z_i$ varies within a group $\times$ time cell, while $X_{jt}$ does not.

There are at least three ways to approach estimation of the above model. A one-step approach would plug equation (8) into equation (7), and the resulting model could be estimated by least squares under the assumption that the error terms $\varepsilon$, $\eta$ were orthogonal to the regressors. The Web appendix, section A.2.4, contains a rigorous demonstration that our methods extend to this approach, and we use this in our empirical example below. Another option would be to first aggregate the data within the group-time cell and proceed to estimate our base model as in section II.

Here, we focus on the third approach: the well-known two-step approach to estimation.[13] We obtain estimates for $\alpha$ by first estimating $\lambda_{jt}$ in equation (7) for all groups and time periods using a regression of $Y_i$ on a full set of indicators for group $\times$ time and $Z_i$. In the second step, the estimated $\lambda_{jt}$ are then used as the outcome variable in equation (8), and the inference procedures described in section II can be applied directly to this second-step regression. The main difference between the three approaches is in the estimation of $\delta$. Estimating $\delta$ in the one-step approach uses all variation, averaging first uses only between variation, and the two-step estimator we suggest uses only within variation. Our preference for this two-step approach is driven by its ease of exposition and that it is more flexible than the one-step estimator because it does not require orthogonality between $Z$ and $\eta$.

A variety of assumptions could be made about the behavior of the number of individuals per group. Let $M(j, t)$ be the set of individuals observed in group $j$ at time $t$ and $|M(j, t)|$ denote the number of individuals in this set. We focus on the case in which $|M(j, t)|$ is growing with $N_0$ and continue to assume $T$ is fixed. However, in the Web appendix (section A.2.3) we provide a rigorous demonstration that our test procedures remain asymptotically valid when the number of individuals per group $\times$ time is fixed but common across group $\times$ time cells.[14]

Let $I_i$ be a set of fully interacted indicators for all group $\times$ time cells. Now consider a regression of $Y_i$ on $Z_i$ and $I_i$. Let

[12] A common example would be an event study analysis such as in Jacobson, LaLonde, and Sullivan (1993). In this approach, one would let the effect of the treatment be time varying relative to when it was introduced—that is, the effect of the policy one year after it was passed may be different from the effect five years later.

[13] See, e.g., Hanushek (1974) or Amemiya (1978), who discuss aspects of this approach.

[14] In Conley and Taber (2005) we present a complementary strategy with fixed sample sizes that vary across group $\times$ time cells. This is considerably more difficult because of the need to solve a deconvolution problem.

$\widehat{\lambda}_{jt}$ be the regression coefficient on the dummy variable for group $j$ at time $t$. It is straightforward to show that

$$\widehat{\lambda}_{jt} = \lambda_{jt} + \left[ \frac{1}{|M(j,t)|} \sum_{i \in M(j,t)} Z_i'(\delta - \widehat{\delta}) + \frac{1}{|M(j,t)|} \sum_{i \in M(j,t)} \varepsilon_i \right], \tag{9}$$

where $\widehat{\delta}$ is the regression coefficient obtained in the first step. As $|M(j,t)|$ grows large, the term in brackets vanishes. The second step is then simply to plug in $\widehat{\lambda}_{jt}$ for $\lambda_{jt}$ in equation (8) and run a fixed-effect OLS. We recycle notation and use $\widehat{\beta}$ and $\widehat{\alpha}$ in this section to refer to the second-step OLS estimators of equation (8). The results of section II apply to these estimators under a straightforward set of conditions. Aside from the usual orthogonality and rank conditions, we need to specify the rate at which $|M(j,t)|$ grows; these are stated as assumption 3:

**Assumption 3.** *$\varepsilon_i$ is i.i.d., independent of $[Z_i \quad I_i]$ and has a finite second moment. $[Z_i \quad I_i]$ is full rank. For all $j$, $|M(j,t)|$ grows uniformly at the same rate as $N_0$.*

**Proposition 4.** *Under assumptions 1, 2, and 3 and assuming $\beta$ is interior to a compact parameter space, as $N_0 \to \infty$, the conclusions of propositions 1 and 2 apply to the Amemiya (1978) second-step OLS estimators $\widehat{\beta}$ and $\widehat{\alpha}$ of equation (8): $\widehat{\beta} \xrightarrow{p} \beta$ and $\widehat{\alpha} \xrightarrow{p} \alpha + W$, where $W$ has exactly the same form given by equation (4). Using the notation $\widetilde{Z}$ to refer to the residual from a linear projection of a variable $Z$ on a full set of time and group indicators, define $\widehat{\Gamma}$ as*

$$\widehat{\Gamma}(w) \equiv \left(\frac{1}{N_0}\right)^{N_1} \sum_{\ell_1=N_1+1}^{N_1+N_0} \cdots \sum_{\ell_{N_1}=N_1+1}^{N_1+N_0} 1\left( \frac{\sum_{j=1}^{N_1} \sum_{t=1}^{T} (d_{jt} - \overline{d}_j)\left(\widetilde{\widehat{\lambda}}_{\ell_j t} - \widetilde{X}'_{\ell_j t}\hat{\beta}\right)}{\sum_{j=1}^{N_1} \sum_{t=1}^{T} \left(d_{jt} - \overline{d}_j\right)^2} < w \right).$$

*$\widehat{\Gamma}(w)$ converges in probability to $\Gamma(w)$ uniformly on any compact subset of the support of $W$.*

**Proof.** The proof is in the Web appendix, section A.2.2.

With access to data containing a large number of individuals within group $\times$ time cells, it is straightforward to extend our approach to models with a nonlinear first step. For example, consider the following latent variable model for a binary outcome $Y_i$,

$$Y_i = 1(\lambda_{j(i)t(i)} + Z_i'\delta + \varepsilon_i \geq 0) \tag{10}$$

$$\lambda_{jt} = \alpha d_{jt} + X_{jt}'\beta + \theta_j + \gamma_t + \eta_{jt} \tag{11}$$

Equation (11) is, of course, the same as equation (8), with $i$ subscripts dropped because its components vary only at the group $\times$ time level. The parameters in equation (10) can easily be consistently estimated in a standard way such as, probit, logit, or even semiparametrically, depending on the assumption one is willing to make on $\varepsilon_i$. The resulting $\lambda_{jt}$ estimates, $\widehat{\lambda}_{jt}$, are simply the estimated group $\times$ time cell intercepts from the first step. Inference regarding $\alpha$ can then be conducted exactly as above with a linear first step. The $\widehat{\lambda}_{jt}$ can used as outcome variables in equation (11), which can again be estimated using OLS and our test procedure applied to the resulting $\alpha$ estimates. We use a logistic first-step procedure in our empirical application in the following section.

## IV. Empirical Example: The Effect of Merit Aid Programs on Schooling Decisions

In the past fifteen years a number of states have adopted merit-based college aid programs that provide subsidies for tuition and fees to students who meet certain merit-based criteria. The largest and probably the best-known program is the Georgia HOPE (Helping Outstanding Pupils Educationally) scholarship, which started in 1993. This program provides full tuition as well as some fees to eligible students who attend in-state public colleges.[15] Eligibility for the program requires maintaining a 3.0 grade point average during high school. A number of previous papers have examined the effect of HOPE and other merit-based aid programs.[16] Given the large amount of previous work on this subject, we leave full discussion of the details of these programs to these other papers and focus on our methodological contribution.

Our work most closely relates to Dynarski (2004) by focusing on the effects of HOPE and other merit aid programs on college enrollment of 18 and 19 year olds using the October CPS from 1989 to 2000. Our specifications are motivated by some of hers, but we do not replicate her entire analysis. Our goal is to illustrate the use of our method, and our analysis falls well short of a complete empirical analysis of merit scholarship effects.

During the 1989–2000 time period, 10 states initiated merit aid programs. We use two specifications, with the first focusing on the HOPE program alone. In this case, we ignore data from the other 9 treatment states and use 41 controls (40 states plus the district of Columbia). In the second case, we study the effect of merit-based programs together and use all 51 units.[17] The outcome variable in all cases is an indicator

[15] A subsidy for private colleges is also part of the program.

[16] Examples include Dynarski (2000, 2004); Cornwell, Mustard, and Sridhar (2006); Bugler, Henry, and Rubenstein (1999); and Henry and Rubenstein (2002).

[17] Note that these merit programs are quite heterogeneous. This exercise does not necessarily mean that we are assuming that the impact of all of these programs is the same. One could interpret this as estimation of a weighted average of the treatment effects. Alternatively, we can think of this as a test of the joint null hypothesis that all of the effects are 0. We could estimate more general confidence intervals allowing for heterogeneous treatment effects, but we focus on the simplest case here.

TABLE 1.—ESTIMATES FOR THE EFFECT OF GEORGIA HOPE PROGRAM ON COLLEGE ATTENDANCE

| | A | B | C |
|---|---|---|---|
| | Linear Probability | Logit | Population-Weighted Linear Probability |
| Hope Scholarship | 0.078 | 0.359 | 0.072 |
| Male | −0.076 | −0.323 | −0.077 |
| Black | −0.155 | −0.673 | −0.155 |
| Asian | 0.172 | 0.726 | 0.173 |
| State dummies | Yes | Yes | Yes |
| Year dummies | Yes | Yes | Yes |
| 95% confidence intervals for hope effect | | | |
| Standard cluster by State × Year | (0.025,0.130) | (0.119,0.600) [0.030,0.149] | (0.025, 0.119) |
| Standard cluster by state | (0.058,0.097) | (0.274,0.444) [0.068,0.111] | (0.050,0.094) |
| Conley-Taber | (−0.010,0.207) | (−0.039,0.909) [−0.010,0.225] | (−0.015,0.212) |
| Sample size | | | |
| Number of states | 42 | 42 | 42 |
| Number of individuals | 34,902 | 34,902 | 34,902 |

Confidence intervals for parameters are presented in parentheses. We use the $\widehat{\Gamma}^*$ formula to construct the Conley-Taber standard errors. Brackets contain a confidence interval for the program impact on a person whose college attendance probability in the absence of the program would be 45%.

variable representing whether the individual is currently enrolled in college.

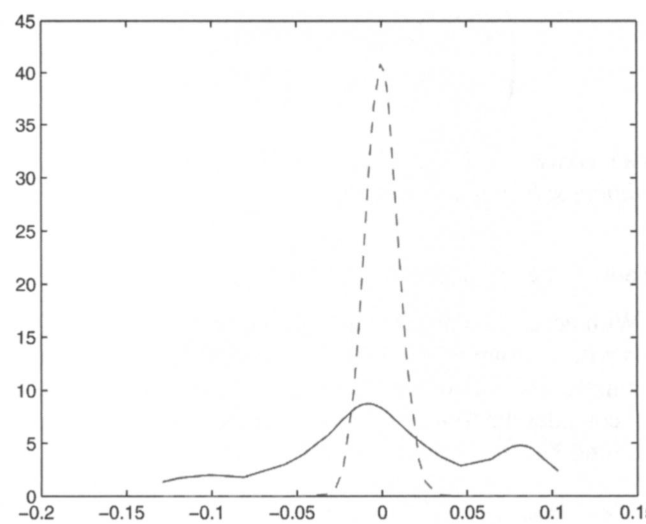
In constructing the confidence intervals, two issues arise due to the fact that we have only 41 control states. The first issue is whether 41 is large enough for the asymptotics to be valid. With that in mind, we use the $\widehat{\Gamma}^*$ estimator described in section II, motivated by its anticipated good finite sample properties. The second issue can be seen in figure 1. The estimated CDF is of course a step function, and with a single treatment state and 41 controls, its probability increments are limited to 1/41. To approximate intervals with conventional, say 95%, coverage probabilities, we use a conservative interval so that the limiting coverage probability is at least 95%. As a practical matter, this is usually relevant only for the case of a single treatment group. With two or more treatment groups, the empirical CDF will have a number of steps on the order of the number of ways to choose the $N_1$ treatment groups out of the total number of groups ($\frac{(N_1+N_0)!}{N_1!N_0!}$). Thus, the number of steps in the CDF is typically large for two or more treatments with corresponding small probability increments.

In table 1 we present results for the HOPE program with Georgia as the only treatment state. We compare three estimators: column A corresponds to the approach described in section IIIB, equations (7) and (8), and columns B and C present two natural alternatives. The estimates in both columns A and B are obtained from Amemiya's (1978) two-step approach. The estimates reported in column A use a first-step linear probability model (OLS), and in column B, the first step is a logit; regressors in both case include demographics and state × year indicators. The second step in both A and B estimates equation (8) with OLS using the estimated state × year coefficients as the dependent variable. Column C presents results from a one-step estimator, which is simply a linear probability model estimated using OLS using the entire sample. Thus, the column C treatment effect estimates will be population weighted across states, while in column A, states are equally weighted. The top panel of table 1 presents point estimates for all three estimators, and the bottom panel presents interval estimates for the treatment parameter, both using our methods with $\widehat{\Gamma}^*$ and the typical approaches clustering by state and state-by-time.

Although results differ depending on the clustering used, interval estimates in column A using typical methods indicate significant treatment effects. An interval of 2.5% to 13.0% obtains with clustering by state and year, which allows the error terms of individuals within the same state and year to be arbitrarily correlated with each other. This interval shrinks to 5.8% to 9.7% when clustering is done by state, which allows serial correlation in $\eta_{jt}$. Clearly one should be worried about the assumption that the number of states changing treatment status is large, which underlies these routine confidence interval estimates since only one state, Georgia, contributes to the estimate of the treatment effect.

The estimated confidence interval using our method reported in the last row of column A is −1% to 21%. This confidence interval is formed by inverting the test statistic $(\hat{\alpha} - \alpha_0)$ using our $\widehat{\Gamma}^*$ estimator. It is centered at a larger value and much wider than the intervals obtained with conventional inference—wide enough to include 0 despite its shift in centering. To better understand these discrepancies, Figure 2 displays a kernel smooth estimate (solid line) of the distribution of $(\widehat{\alpha} - \alpha)$ under the null hypothesis that the true value of $\alpha$ is 0. This distribution is estimated from the control states. For comparison, the dashed line plots an estimate implied by the usual asymptotic approximation with clustering by state. This curve is a gaussian density centered at 0 with a standard deviation equal to 0.0098: the standard error on $\widehat{\alpha}$ from a fixed-effect regression that clusters by state. The pronounced differences between the spread and symmetry (lack thereof) of these distributions are what drive our interval

FIGURE 2.—ESTIMATED DENSITY OF $\hat{\alpha}$ UNDER $H_0 : \alpha_0 = 0$




Solid line: Kernel-smoothed density estimate for Conley-Taber approach. Dashed line: Density estimate using standard asymptotics.

estimates of $\alpha$ to differ from those resulting from conventional methods.

In column B, we present a logit version of the model as in equations (10) and (11) with $\varepsilon_i$ logistic. The estimates in this column were obtained in exactly the same manner as for column A, except that in the first step, we use a logit model of the college attendance indicator so the predicted parameter has the interpretation of a logit index coefficient. The pattern is very similar to column A. Intervals from our method are again centered higher than conventional ones, but enough wider that the HOPE treatment effect becomes marginally insignificant. This contrasts with effects that are highly significant using standard inference methods. To display the magnitude of the program impact, we calculate a 95% confidence interval for changes in college attendance probability for a particular individual. We consider an individual (without the treatment) whose logit index puts his probability of college attendance at the sample unconditional average attendance probability of 45% (i.e., an individual with a logit index of −.20). The bracketed intervals reported in column 2 are 95% confidence intervals for the change in attendance probability for our reference individual (intervals in parentheses are 95% confidence intervals for $\alpha$).[18]

In column C we present results from a linear probability that estimates equations (7) to (8) using OLS with all 34,902 observations. The details for constructing the confidence intervals are formally presented in the Web appendix (section A4.4). These results are close to those presented in column A. The difference between these two estimates is that in column A, the states are equally weighted, while in column C, they are population weighted.

In table 2 we present results estimating the effect of merit aid using all ten states that added programs during this time period. The format of the table is identical to table 1. There are a few notable features of this table. First, the weighting matters substantially, as the effect is much smaller when we weight all the states equally as opposed to the population-weighted estimates in column C. Second, in contrast to table 1, the confidence intervals are quite similar when we cluster by state compared to clustering by state × year. Most important, our approach changes the confidence intervals substantially, but less dramatically than in table 1. In particular, the treatment effect with equal weighting across states is still statistically significant at conventional levels.

TABLE 2.—ESTIMATES FOR MERIT AID PROGRAMS ON COLLEGE ATTENDANCE

| | A | B | C |
|---|---|---|---|
| | Linear Probability | Logit | Population-Weighted Linear Probability |
| Merit scholarship | 0.051 | 0.229 | 0.034 |
| Male | −0.078 | −0.331 | −0.079 |
| Black | −0.150 | −0.655 | −0.150 |
| Asian | 0.168 | 0.707 | 0.169 |
| State dummies | Yes | Yes | Yes |
| Year dummies | Yes | Yes | Yes |
| 95% confidence intervals for merit aid program effect | | | |
| Standard cluster by State × Year | (0.024,0.078) | (0.111,0.346) [0.028,0.086] | (0.006,0.062) |
| Standard cluster by state | (0.028,0.074) | (0.127,0.330) [0.032,0.082] | (0.008,0.059) |
| Conley-Taber | (0.012,0.093) | (0.056,0.407) [0.014,0.101] | (−0.003,0.093) |
| Sample size | | | |
| Number of states | 51 | 51 | 51 |
| Number of individuals | 42,161 | 42,161 | 42,161 |

Confidence intervals for parameters are presented in parentheses. We use the $\widehat{\Gamma}^*$ formula to construct the Conley-Taber standard errors. Brackets contain a confidence interval for the program impact on a person whose college attendance probability in the absence of the program would be 45%.

## V. Monte Carlo

In this section we discuss the results of a small Monte Carlo study evaluating the performance of our method and comparing it to typical approaches. The specification that we examine is

$$Y_{jt} = \alpha d_{jt} + \beta X_{jt} + \theta_j + \gamma_t + \eta_{jt},$$

in which we focus on the model of section II with group-level data since that is our base case. Note that we focus here on a single regressor. We assign a binary treatment, $d_{jt}$, that is 0 for controls and at some point in the data turns permanently from 0 to 1 for each treatment group. We assume that the error term within group has a first-order autoregressive structure:

$$\eta_{jt} = \rho\eta_{jt-1} + u_{jt},$$
$$u_{jt} \sim N(0, 1).$$

Finally, we want controlling for $X_{jt}$ to be important (as it often is in real data); therefore, we build in a correlation between $X$ and the treatment:

$$X_{jt} = a_x d_{jt} + \nu_{jt},$$
$$\nu_{jt} \sim N(0, 1).$$

In our base case model, we let the total number of groups $(N_1 + N_0)$ be 100, $T = 10$ and let five groups change treatment status during the time period. The turn-on time periods for the base case are periods 2, 4, 6, 8, and 10. We set the remaining parameters to have the values $\alpha = 1, \rho = 0.5, a_x = 0.5, \beta = 1$.

In table 3, we present the results of testing the true null hypothesis ($\alpha = 1$) and a false one ($\alpha = 0$) at the 5% level using 10,000 trials and present the percentage of times the hypothesis is rejected. Thus, if the test works well, we should reject the hypothesis $\alpha = 1$ around 5% of the trials and reject $\alpha = 0$ much more frequently. We present four different approaches: a standard $t$-test adjusted for degrees of freedom (as suggested by Donald & Lang, 2007), a cluster-by-group approach (as suggested by Bertrand, Duflo, & Mullainathan, 2004), and then our approach using both the $\widehat{\Gamma}$ and $\widehat{\Gamma}^*$

[18] These confidence intervals for changes in attendance probabilities are calculated directly from the 95% CI for $\alpha$. Specifically, when the CI for $\alpha$ is $[c_1, c_2]$, letting $\Lambda$ denote the logistic CDF, we report an interval for the change in predicted probability for our reference individual of $(\Lambda(-.2 + c_1) - 45\%)$ to $(\Lambda(-.2 + c_2) - 45\%)$.

TABLE 3.—MONTE CARLO RESULTS: SIZE AND POWER OF TEST OF AT MOST 5% LEVEL
BASIC MODEL
$Y_{jt} = \alpha d_{jt} + \beta X_{jt} + \theta_j + \gamma_t + \eta_{jt}$
$\eta_{jt} = \rho\eta_{jt-1} + \varepsilon_{jt}, \alpha = 1, X_{jt} = a_x d_{jt} + \nu_{jt}$

| Percentage of Times Hypothesis Is Rejected out of 10,000 Simulations | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Size of Test ($H_0 : \alpha = 1$) | | | | Power of Test ($H_0 : \alpha = 0$) | | | |
| | Classic Model | Cluster | Conley Taber ($\widehat{\Gamma}^*$) | Conley Taber ($\widehat{\Gamma}$) | Classic Model | Cluster | Conley Taber ($\widehat{\Gamma}^*$) | Conley Taber ($\widehat{\Gamma}$) |
| Base model[a] | 14.23 | 16.27 | 4.88 | 5.52 | 73.23 | 66.10 | 54.08 | 55.90 |
| Total groups = 1000 | 14.89 | 17.79 | 4.80 | 4.95 | 73.97 | 67.19 | 55.29 | 55.38 |
| Total groups = 50 | 14.41 | 15.55 | 5.28 | 6.65 | 71.99 | 64.48 | 52.21 | 56.00 |
| Time periods = 2 | 5.32 | 14.12 | 5.37 | 6.46 | 49.17 | 58.54 | 49.13 | 52.37 |
| Number treatments = 1[b] | 18.79 | 84.28 | 4.13 | 5.17 | 40.86 | 91.15 | 13.91 | 15.68 |
| Number treatments = 2[b] | 16.74 | 35.74 | 4.99 | 5.57 | 52.67 | 62.15 | 29.98 | 31.64 |
| Number treatments = 10[b] | 14.12 | 9.52 | 4.88 | 5.90 | 93.00 | 84.60 | 82.99 | 84.21 |
| Uniform error[c] | 14.91 | 17.14 | 5.30 | 5.86 | 73.22 | 65.87 | 53.99 | 55.32 |
| Mixture error[d] | 14.20 | 15.99 | 4.50 | 5.25 | 55.72 | 51.88 | 36.01 | 37.49 |
| $\rho = 0$ | 4.86 | 15.30 | 5.03 | 5.57 | 82.50 | 86.42 | 82.45 | 83.79 |
| $\rho = 1$ | 30.18 | 16.94 | 4.80 | 5.87 | 54.72 | 34.89 | 19.36 | 20.71 |
| $a_x = 0$ | 14.30 | 16.26 | 4.88 | 5.55 | 73.38 | 66.37 | 54.08 | 55.93 |
| $a_x = 2$ | 14.18 | 16.11 | 4.82 | 5.49 | 73.00 | 65.91 | 54.33 | 55.76 |
| $a_x = 10$ | 10.36 | 9.86 | 11.00 | 11.90 | 51.37 | 47.78 | 53.29 | 54.59 |

In the results for the Conley-Taber ($\widehat{\Gamma}^*$) with smaller sample sizes, we cannot get a size of exactly 5% due to the discreteness of the empirical distribution. When this happens, we choose the size to be the largest value possible that is under 5%.
[a]For the base model, the total number of groups is 100, with five treatments, and ten periods. Parameter values: $\rho = 0.5, a_x = 0.5, \beta = 1, \varepsilon_{jt} \sim N(0, 1), \nu_{jt} \sim N(0, 1)$.
[b]With T treatments and five periods, the changes occur during periods 2, 4, 6, 8, and 10. For one treatment, it is in period 6; for two treatments, it is in periods 3 and 7; and for ten treatments, it is periods 2, 2, 3, 4, 5, 6, 7, 8, 9, and 10.
[c]The range of the uniform is $[-\sqrt{3}, \sqrt{3}]$ so that it has unit variance.
[d]The mixture model we consider is a mixtures of a $N(0, 1)$ and a $N(2, 1)$ in which the standard normal is drawn 80% of the time.

formulas. The results for the base case are presented in the first row. One can see that our approach performs much better than either of the alternatives, both of which miss the size by a factor of about three.[19]

We then consider other cases by altering some of the parameters in the data-generating process (DGP), one at a time. The labels in the left column indicate the parameters that differ from the base case setting. For example, the fifth row decreases the number of treatment groups from five to two, holding all other parameters at the base setting. This decrease in information results in a large drop in power for both the $\widehat{\Gamma}$ and $\widehat{\Gamma}^*$ estimators with little size distortion. With treatments reduced to two, the classic estimator suffers a large drop in power and a small increase in size distortion, whereas the cluster estimator suffers a large increase in size distortion along with a small drop in power. In both the $T = 2$ and $\rho = 0$ lines, we see alternate specifications where our Monte Carlo DGP collapses to the classical linear model. However, $\widehat{\Gamma}^*$ appears to perform on par with the classical model here, and $\widehat{\Gamma}$ does reasonably well too. Thus, our methods have comparable size and power characteristics to the classical test in some scenarios where it is ideal.

As anticipated, $\widehat{\Gamma}^*$ does seem to work a little better than $\widehat{\Gamma}$ with smaller samples, as seen in size in the Groups = 50 row. However, across all scenarios, the similarities between the performance of $\widehat{\Gamma}$ and $\widehat{\Gamma}^*$ are more salient than the slight size advantage of $\widehat{\Gamma}^*$.

[19]Their power is higher here, but this is likely in large part because the size is too large; that is, the confidence intervals are tighter than they should be.

We do not expect our approaches to work well when there is a great deal of estimation error in $\widehat{\beta}$. This can be seen in our simulations as the parameter $a_x$ increases. We get a substantial size distortion for both $\widehat{\Gamma}$ and $\widehat{\Gamma}^*$ with $a_x = 10$. This means that the distribution of $X_{jt}$ is $N(0, 1)$ without the treatment, but then jumps to $N(10, 1)$ after the treatment is implemented. The classical and cluster methods also struggle here, so our method is not dominated by these alternatives even in this case.

Perhaps the starkest result is how poorly the cluster approach works with a small number of treatment changers. The size in the base case is triple what it should be. Performance here is very sensitive to the number of treatment groups. When this is decreased to one or two, the performance is terrible. However, it does better when one gets up to ten treatments and, in results not shown, it works well at forty treatments. However, even with ten treatments, although the size of the test is down to 9.52%, the power is not much better than for our approach. These results show that cluster standard errors can be very misleading when the number of groups changing status is small.

## VI. Conclusion

This paper presents an inference method for difference-in-differences fixed-effect models when the number of policy changes observed in the data is small. This method is an alternative to typical asymptotic inference based on a large number of policy changes and classical small sample inference. Our approach will be most valuable in applications where the classical model does not apply—for example, due to nongaussian or serially correlated errors. We provide an estimator $\widehat{\Gamma}^*$ that is large-$N_0$ asymptotically valid and appears to have good finite sample properties with serially dependent, cross-sectionally i.i.d. data. Our approach can also be applied with much weaker conditions on the data. Many forms of cross-sectional dependence and heteroskedasticity, for example, can be readily accommodated. We provide an example application studying the effect of merit scholarship programs on college attendance for which our approach seems appropriate. It results in very different inference from conventional methods. We also perform a Monte Carlo analysis, which indicates that our approach fares far better than the standard alternatives when the number of treatment groups is small and performs well even in cases that are tailored to ensure good performance of these alternatives.

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## APPENDIX

*A1 Proof of Proposition 1.* First, a standard application of the partitioned inverse theorem makes it straightforward to show that

$$\begin{aligned}\widehat{\beta} = \beta + \Bigg( & \frac{\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{X}_{jt}\widetilde{X}'_{jt}}{N_0+N_1} \\ & - \frac{\left[\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}\widetilde{X}_{jt}\right]\left[\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}\widetilde{X}'_{jt}\right]}{(N_0+N_1)\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}^{\,2}}\Bigg)^{-1} \\ \times \Bigg( & \frac{\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{X}_{jt}\widetilde{\eta}_{jt}}{N_0+N_1} \\ & - \frac{\left[\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}\widetilde{X}_{jt}\right]\left[\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}\widetilde{\eta}_{jt}\right]}{(N_0+N_1)\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}^{\,2}}\Bigg).\end{aligned} \tag{A1}$$

Now consider each piece in turn.

First, assumption 1 states that

$$\frac{1}{N_0+N_1}\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{X}_{jt}\widetilde{X}'_{jt} \xrightarrow{p} \Sigma_x < \infty.$$

The mixing components of assumption 1 imply that a strong law of large numbers (LLN) applies here (see, e.g., Jenish & Prucha, 2009). This LLN and the zero-conditional expectation component of assumption 1 imply that

$$\frac{1}{N_0+N_1}\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{X}_{jt}\widetilde{\eta}_{jt} \xrightarrow{p} E\left[\sum_{t=1}^{T}\widetilde{X}_{jt}\widetilde{\eta}_{jt}\right] = 0.$$

For control groups $j > N_1$, the treatment does not vary over time, so $d_{jt} = \overline{d}_j$. Therefore,

$$\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}^{2} = \sum_{j=1}^{N_1}\sum_{t=1}^{T}(d_{jt} - \overline{d}_j - \overline{d}_t + \overline{d})^2 + \sum_{j=N_1+1}^{N_1+N_0}\sum_{t=1}^{T}(\overline{d} - \overline{d}_t)^2$$

where

$$\sum_{j=N_1+1}^{N_1+N_0}\sum_{t=1}^{T}(\overline{d} - \overline{d}_t)^2 = N_0\sum_{t=1}^{T}\left(\frac{1}{N_1+N_0}\sum_{\ell=1}^{N_1+N_0}\left[\left(\frac{1}{T}\sum_{\tau=1}^{T}d_{\ell\tau}\right) - d_{\ell t}\right]\right)^2 \xrightarrow{p} 0.$$

Now consider the other term,

$$\sum_{j=1}^{N_1}\sum_{t=1}^{T}(d_{jt}-\overline{d}_j-\overline{d}_t+\overline{d})^2 \xrightarrow{p} \sum_{j=1}^{N_1}\sum_{t=1}^{T}(d_{jt}-\overline{d}_j)^2,$$

since $(\overline{d}_t-\overline{d})$ converges in probability to 0 due to the finite number of groups with intertemporal variation in treatments. Thus,

$$\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}^2 \xrightarrow{p} \sum_{j=1}^{N_1}\sum_{t=1}^{T}(d_{jt}-\overline{d}_j)^2 > 0,$$

since $N_1 \geq 1$.

Now consider

$$\begin{aligned}\frac{1}{\sqrt{N_0+N_1}}\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}\widetilde{X}_{jt} &= \frac{1}{\sqrt{N_0+N_1}}\sum_{j=1}^{N_1}\sum_{t=1}^{T}(d_{jt}-\overline{d}_j)\widetilde{X}_{jt}\\ &\quad+\sum_{t=1}^{T}(\overline{d}-\overline{d}_t)\frac{1}{\sqrt{N_0+N_1}}\sum_{j=1}^{N_1+N_0}\widetilde{X}_{jt}\\ &\xrightarrow{p} 0 \text{ as } N_0\to\infty.\end{aligned}$$

This result follows because the first term involves a sum of a finite number of $O_p(1)$ random variables normalized by an $O(N_0)$ term and the second term is identically 0 due to differencing.

Likewise,

$$\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}\widetilde{\eta}_{jt} = \sum_{j=1}^{N_1}\sum_{t=1}^{T}(d_{jt}-\overline{d}_j)(\eta_{jt}-\overline{\eta}_j-\overline{\eta}_t+\overline{\eta}),$$

which is $O_p(1)$; thus,

$$\frac{1}{\sqrt{N_0+N_1}}\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}\widetilde{\eta}_{jt} \xrightarrow{p} 0.$$

Consistency for $\widehat{\beta}$ follows on plugging the pieces back into equation (A1) and applying Slutsky's theorem.

From the normal equation for $\widehat{\alpha}$, it is straightforward to show that

$$\widehat{\alpha} = \alpha + \frac{\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}\widetilde{\eta}_{jt}}{\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}^2} + \left[\frac{\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}\widetilde{X}'_{jt}}{\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}^2}\right](\beta-\widehat{\beta}). \quad \text{(A2)}$$

From above, we know that

$$\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}^2 \xrightarrow{p} \sum_{j=1}^{N_1}\sum_{t=1}^{T}(d_{jt}-\overline{d}_j)^2$$

$$\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}\widetilde{X}_{jt} = \sum_{j=1}^{N_1}\sum_{t=1}^{T}(d_{jt}-\overline{d}_j-\overline{d}_t+\overline{d})\widetilde{X}_{jt}$$

$$(\beta-\widehat{\beta}) \xrightarrow{p} 0.$$

Thus,

$$\left[\frac{\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}\widetilde{X}'_{jt}}{\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}^2}\right](\beta-\widehat{\beta}) \xrightarrow{p} 0.$$

We showed above that

$$\sum_{j=1}^{N_0+N_1}\sum_{t=1}^{T}\widetilde{d}_{jt}\widetilde{\eta}_{jt} = \sum_{j=1}^{N_1}\sum_{t=1}^{T}(d_{jt}-\overline{d}_j)(\eta_{jt}-\overline{\eta}_j-\overline{\eta}_t+\overline{\eta}).$$

The variables $\overline{\eta}_t$ and $\overline{\eta}$ both converge to 0 in probability as $N_0\to\infty$; therefore,

$$\sum_{j=N_1+1}^{N_1+N_0}\sum_{t=1}^{T}(d_{jt}-\overline{d}_j)\widetilde{\eta}_{jt} \xrightarrow{p} \sum_{j=1}^{N_1}\sum_{t=1}^{T}(d_{jt}-\overline{d}_j)(\eta_{jt}-\overline{\eta}_j).$$

Plugging these pieces into equation (A2) gives the result.

*A2 Proof of Proposition 2.* Since $\Gamma$ is defined conditional on $d_{jt}$ for $j=1,\ldots,N_1$, $t=1,\ldots,T$, every probability in this proof conditions on this set. To simplify the notation, we omit this explicit conditioning. Thus, every probability statement and distribution function in this proof should be interpreted as conditioning on $d_{jt}$ for $j=1,\ldots,N_1$, $t=1,\ldots,T$.

It is convenient to define

$$\rho_{jt} = \frac{(d_{jt}-\overline{d}_j)}{\sum_{\ell=1}^{N_1}\sum_{t=1}^{T}(d_{\ell t}-\overline{d}_\ell)^2}.$$

For each $j=1,\ldots,N_1$, define the random variable

$$W_j \equiv \sum_{t=1}^{T}\rho_{jt}\eta_{jt}.$$

Let $F_j$ be the distribution of $W_j$ for $j=1,\ldots,N_1$.

Then note that

$$\begin{aligned}\Gamma(w) &= \Pr\left(\sum_{j=1}^{N_1}\sum_{t=1}^{T}\rho_{jt}\eta_{jt} < w\right)\\ &= \int\cdots\int 1\left(\sum_{j=1}^{N_1}W_j < w\right)dF_1(W_1)\ldots dF_{N_1}(W_{N_1}).\end{aligned}$$

We can also write

$$\widehat{\Gamma}(w) = \int\cdots\int 1\left(\sum_{j=1}^{N_1}W_j < w\right)d\widehat{F}_1(W_1;\widehat{\beta})\ldots d\widehat{F}_{N_1}(W_{N_1};\widehat{\beta}),$$

where $\widehat{F}_j(\cdot;\widehat{\beta})$ is the empirical CDF one gets from the residuals using the control groups only. That is, more generally,

$$\widehat{F}_j(w_j;b) \equiv \frac{1}{N_0}\sum_{m=1}^{N_0}1\left(\sum_{t=1}^{T}\rho_{jt}(\widetilde{Y}_{mt}-\widetilde{X}'_{mt}b) < w_j\right).$$

To avoid repeating the expression, we define

$$\phi_j(w_j,b) \equiv \Pr\left(\sum_{t=1}^{T}\rho_{jt}(\eta_{mt}-X'_{mt}(\beta-b)) < w_j\right).$$

Note that $\phi_j(w_j,\beta)=F_j(w_j)$. The proof strategy is first to demonstrate that $\widehat{F}_j(w_j;\widehat{\beta})$ converges to $\phi_j(w_j,\beta)$ uniformly over $w_j$. We will then show that $\widehat{\Gamma}(a)$ is a consistent estimate of $\Gamma(a)$.

Define

$$\widehat{\omega}_j = \sum_{t=1}^{T}\rho_{jt}(\overline{\eta}_t-\overline{X}_t(\beta-\hat{\beta})).$$

Let $\Omega$ be a compact parameter space for $w$ and $\Theta$ a compact subset of the parameter space for $(\widehat{\beta},\widehat{\omega}_j)$ in which $(\beta,0)$ is an interior point.

For each $j = 1, \ldots, N_1$, consider the difference between $\widehat{F}_j(w_j; \widehat{\beta})$ and $\phi_j(w_j, \beta)$:

$$\begin{aligned}
&\sup_{w_j \in \Omega} |\widehat{F}_j(w_j; \widehat{\beta}) - \phi_j(w_j, \beta)| \\
&= \sup_{w_j \in \Omega} \left| \frac{1}{N_0} \sum_{m=N_1+1}^{N_0} 1 \right. \\
&\quad \left. \times \left( \sum_{t=1}^{T} \rho_{jt}(\eta_{mt} - \overline{\eta}_t - (X_{mt} - \overline{X}_t)'(\beta - \hat{\beta})) < w_j \right) - \phi_j(w_j, \beta) \right| \\
&\leq \sup_{\substack{w_j \in \Omega \\ (b, \omega_j) \in \Theta}} \left| \frac{1}{N_0} \sum_{m=N_1+1}^{N_0} 1 \right. \\
&\quad \left. \times \left( \sum_{t=1}^{T} \rho_{jt}(\eta_{mt} - X'_{mt}(\beta - b)) < w_j + \omega_j \right) - \phi_j(w_j + \omega_j, b) \right| \\
&\quad + \Pr((\hat{\beta}, \widehat{\omega}_j) \notin \Theta) + \sup_{w_j \in \Omega} |\phi_j(w_j + \widehat{\omega}_j, \widehat{\beta}) - \phi_j(w_j, \beta)|. \qquad \text{(A3)}
\end{aligned}$$

First, consider $\sup_w |\phi_j(w_j, \widehat{\beta}) - \phi_j(w_j, \beta)|$. Using a standard mean-value expansion of $\phi$, for some $(\widetilde{\omega}_j, \widetilde{\beta})$,

$$\begin{aligned}
&\sup_{w_j \in \Omega} |\phi_j(w_j + \widehat{\omega}_j, \widehat{\beta}) - \phi_j(w, \beta)| \\
&= \sup_{w} \left| \frac{\partial \phi_j(w_j + \widetilde{\omega}_j, \widetilde{\beta})}{\partial \beta} (\widehat{\beta} - \beta) + \frac{\partial \phi_j(w_j, \widetilde{\beta})}{\partial w_j} (\widehat{\omega}_j) \right|.
\end{aligned}$$

To see that the derivative $\frac{\partial \phi_j(w_j, b)}{\partial b}$ is bounded, first note that

$$\frac{\partial \phi_j(w_j, b)}{\partial b} = E\left( f_j \sum_{t=1}^{T} \rho_{jt} \widetilde{X}'_{jt} \right),$$

where $f_j$ is the density associated with $F_j$. Since $f_j$ is bounded and $X_{jt}$ has first moments, this term is bounded. Clearly $\frac{\partial \phi_j(w_j, b)}{\partial w_j}$ is also bounded for the same reason. Thus, $\sup_{w_j \in \Omega} |\phi_j(w + \widehat{\omega}_j, \widehat{\beta}) - \phi_j(w_j, \beta)|$ converges to 0 since $\widehat{\beta}$ is consistent.

Since $(\widehat{\beta}, \widehat{\omega}_j)$ converges in probability to $(\beta, 0)$, an interior point of $\Theta$, $\Pr((\hat{\beta}, \widehat{\omega}_j) \notin \Theta)$ converges to 0.

Next consider the first term on the right side of equation (A3). Note that the function

$$1\left( \sum_{t=1}^{T} \rho_{jt}(\widetilde{Y}_{mt} - \widetilde{X}'_{mt} b) < w_j + \omega_j \right)$$

is continuous at each $(b, w, \omega)$ with probability 1, and its absolute value is bounded by 1, so applying lemma 2.4 of Newey and McFadden (1994), $\widehat{F}_j(w_j; b)$ converges uniformly to $\phi(w_j, b)$. Thus, putting the three pieces of equation (A3) together gives

$$\sup_{w_j \in \Omega} |\widehat{F}(w_j; \widehat{\beta}) - \phi(w_j, \beta)| \xrightarrow{p} 0.$$

Now to see that $\widehat{\Gamma}(w)$ converges to $\Gamma(w)$, we can write

$$\begin{aligned}
&|\widehat{\Gamma}(w) - \Gamma(w)| \\
&= \left| \left\{ \int \left[ \widehat{F}_1 \left( \left[ w - \sum_{j=2}^{N_1} W_j \right]; \widehat{\beta} \right) - F_1 \left( w - \sum_{j=2}^{N_1} W_j \right) \right] \right. \right. \\
&\quad \left. \times d\widehat{F}_2(W_2; \widehat{\beta}) \ldots d\widehat{F}_{N_1}(W_{N_1}; \widehat{\beta}) \right\} \\
&\quad + \left\{ \int \left[ \widehat{F}_2 \left( \left[ w - \sum_{\substack{j=1 \\ j \neq 2}}^{N_1} W_j \right]; \widehat{\beta} \right) - F_2 \left( w - \sum_{\substack{j=1 \\ j \neq 2}}^{N_1} W_j \right) \right] \right. \\
&\quad \left. \times dF_1(W_1) d\widehat{F}_3(W_3; \widehat{\beta}) \ldots d\widehat{F}_{N_1}(W_{N_1}; \widehat{\beta}) \right\} \\
&\quad + \ldots \\
&\quad + \left\{ \int \left[ \widehat{F}_{N_1} \left( \left[ w - \sum_{j=1}^{N_1 - 1} W_j \right]; \widehat{\beta} \right) - F_{N_1} \left( w - \sum_{j=1}^{N_1 - 1} W_j \right) \right] \right. \\
&\quad \left. \left. \times dF_1(W_1) \ldots dF_{N_1 - 1}(W_{N_1}) \right\} \right|.
\end{aligned}$$

Since each $\widehat{F}_j(w; \widehat{\beta})$ converges uniformly to $F_j(w)$, the right-hand side of this expression must converge to 0, so $\widehat{\Gamma}(a)$ converges to $\Gamma(a)$.