No. 22-915

In the
Supreme Court of the United States

UNITED STATES,
Petitioner,

v.

ZACKEY RAHIMI,
Respondent.

On Writ of Certiorari to the United States Court of Appeals for the Fifth Circuit

BRIEF OF *AMICUS CURIAE* CRIME PREVENTION RESEARCH CENTER IN SUPPORT OF RESPONDENT

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INTEREST OF *AMICUS CURIAE*

The Crime Prevention Research Center (CPRC) is a research and education organization dedicated to conducting academic quality research on the relationship between laws regulating the ownership or use of guns, crime, and public safety; educating the public on the results of such research; and supporting other organizations, projects, and initiatives that are organized and operated for similar purposes. It has 501(C)(3) status, and does not accept donations from gun or ammunition makers or organizations such as NRA or any other organization involved in the gun control debate on either side of the issue.[1]

SUMMARY OF ARGUMENT

Precedent makes several things clear: First, the Second Amendment protects an individual right to keep and bear arms and is a fundamental right, co-equal with other rights ensconced in the Bill of Rights. It is therefore appropriate to reason by analogy from precedent developed surrounding those other amendments when analyzing the Second Amendment. Second, fundamental constitutional rights should be, and are, enforced against the government regardless of stated claims regarding public safety, or other policy concerns. Third, despite this, social science research (often highly flawed) is frequently cited to create the impression that it is necessary to balance citizens'

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[1] Pursuant to Supreme Court Rule 37.6, the undersigned affirms that no counsel for a party authored this brief in whole or in part, and no person or entity other than *amicus curiae* or its counsel, made a monetary contribution specifically for the preparation or submission of this brief.

rights on one side of the scale vs. public safety on the other. This brief shows that the data, when properly analyzed, shows otherwise: the law challenged by Mr. Rahimi (18 U.S.C. § 922(g)(8)) has no beneficial impact on public safety.

ARGUMENT

In *Heller*, this Court concluded that just as the First and Fourth Amendments codified pre-existing rights, the Second Amendment codified a pre-existing right. *District of Columbia v. Heller*, 554 U.S. 570, 592 (2008). The Court analogized to the First and Fourth Amendments in analyzing the Second Amendment. *Id.* at 582. The Court stated:

> Just as the First Amendment protects modern forms of communications, [ ] and the Fourth Amendment applies to modern forms of search, [ ], the Second Amendment extends, prima facie, to all instruments that constitute bearable arms, even those that were not in existence at the time of the founding.

Id. (Internal cites omitted).

In *McDonald*, this Court rejected the petitioner's attempt to get the Court to treat the Second Amendment with a different set of rules than it would when considering other guarantees of the Bill of Rights. *McDonald v. City of Chicago*, 561 U.S. 742, 780 (2010).

Most recently, this Court, in *Bruen*, continued the Second Amendment's equality with the other constitutional rights via two comparisons. First, this

Court compared the manner in which it works to how the First Amendment's protection in relation to unpopular speech or the free exercise of religion works. *N.Y. State Rifle & Pistol Ass'n v. Bruen*, 142 S. Ct. 2111, 2156 (2022). Second, the Court directly compared the Second Amendment to the Sixth Amendment's Confrontation Clause. *Id.* Both of those comparisons were central to this Court's decision. *Id.* Therefore, by consistently discussing the Second Amendment in relation to other amendments in the Bill of Rights, this Court has recognized that the Second Amendment is equal to other portions of the Bill of Rights.

"The constitutional right to bear arms in public for self-defense is not 'a second-class right, subject to an entirely different body of rules than the other Bill of Rights guarantees.'" *Bruen* at 2156 (quoting *McDonald v. City of Chicago*, 561 U.S. 742, 780 (2010)). The Second Amendment is equal to other constitutionally protected, fundamental rights.

When analyzing and enforcing fundamental individual rights, this court has consistently rejected utopian arguments based on fear regarding hypothetical harms.

> The right to keep and bear arms, however, is not the only constitutional right that has controversial public safety implications. All of the constitutional provisions that impose restrictions on law enforcement and on the prosecution of crimes fall into the same category. See, *e.g.*, *Hudson v. Michigan,* 547 U.S. 586,

> 591, 126 S.Ct. 2159, 165 L.Ed.2d 56 (2006) ("The exclusionary rule generates 'substantial social costs,' *United States v. Leon,* 468 U.S. 897, 907, 104 S.Ct. 3405, 82 L.Ed.2d 677 (1984), which sometimes include setting the guilty free and the dangerous at large"); *Barker v. Wingo,* 407 U.S. 514, 522, 92 S.Ct. 2182, 33 L.Ed.2d 101 (1972) (reflecting on the serious consequences of dismissal for a speedy trial violation, which means "a defendant who may be guilty of a serious crime will go free"); *Miranda v. Arizona,* 384 US 436, 517, 86 S.Ct. 1602, 16 L.Ed.2d 694 (1966) (Harlan, J., dissenting); *id.,* at 542, 86 S.Ct. 1602 (White, J., dissenting) (objecting that the Court's rule "[i]n some unknown number of cases ... will return a killer, a rapist or other criminal to the streets ... to repeat his crime"); *Mapp v. Ohio,* 367 U.S. 643, 659, 81 S.Ct. 1684, 6 L.Ed.2d 1081 (1961). Municipal respondents cite no case in which we have refrained from holding that a provision of the Bill of Rights is binding on the States on the ground that the right at issue has disputed public safety implications.

McDonald v. City of Chicago, Ill, 561 U.S. 742, 783; 130 S. Ct. 3020; 177 L. Ed. 2d 894 (2010)

In fact, Miranda himself was convicted of kidnapping and rape both before his successful appeal to this Court, and after. *See State v. Miranda*, 98 Ariz.

18, 401 P.2d 721 (1965), *rev'd,* 384 U.S. 436, 86 S. Ct. 1602, 16 L. Ed. 2d 694 (1966). *See also, State v. Miranda*, 104 Ariz. 174; 450 P.2d 364 (1969). So, in this case, the Court should similarly not be swayed by alarms about what harm may result from the full application of a fundamental, individual Constitutional right. "Moreover, *Heller* and *McDonald* expressly rejected the application of any 'judge-empowering interest-balancing inquiry that asks whether the statute burdens a protected interest in a way or to an extent that is out of proportion to the statute's salutary effects upon other important governmental interests.'" *Bruen*, 142 S. Ct. at 2129 (quoting *Heller*, 554 U.S. at 634).

However, to the extent that any public policy concerns must be addressed, it must be noted that the simplistic false dichotomy of "guns versus safety" does not hold up to thorough scrutiny.

The balance of this brief contains an analysis of publicly available data by Drs. Lott and Moody from a paper that is certainly relevant, and has been accepted for publication; but, is not yet published as of the date of filing of this brief.[2] It shows that 18 U.S.C. § 922(g)(8) has no beneficial impact on domestic murder, domestic femicide, domestic gun murder, or domestic gun femicide. No one wants to endanger

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[2] We based the research portion of this brief on the forthcoming paper: John R. Lott, Jr. and Carl Moody, "Does the Federal law forbidding people under domestic violence restraining orders from possessing firearms save lives?" Economics, Law, and Policy, Vol. 6, No. 3, December 2023.

lives, and it is important that the impact of such laws be carefully evaluated.

A. The Empirical Theory

Regardless of the legal arguments with respect to § 922(g)(8), it may be relevant to determine the law's merits. What would be lost if the law is overturned? We assess the benefits of banning individuals who are subject to domestic violence protection orders from possessing firearms. Alternatively, we are assessing the costs, if any, of declaring § 922(g)(8) unconstitutional. Previous studies haven't specifically looked at the impact of the federal law. Nor have they taken into account the large differences in state laws.[3] For example, states

[3] A review of the literature cited by amicus briefs filed as of September 24, 2023 reveals three major shortcomings. The first is that the great majority of the cited studies show associations between firearms and domestic violence, but such associations do not imply that a particular law, such as 18 U.S.C. § 922(g)(8) effectively reduces such violence. The second is that none of the cited studies address the effect of 18 U.S.C. § 922(g)(8) on domestic murders directly. Instead, the most often cited study that addresses the effect of prohibiting persons subject to domestic violence protective orders from possessing firearms is concerned with the effectiveness of *state* firearm surrender laws on domestic murders. (Carolina Diez et al., State Intimate Partner Violence-Related Firearm Laws and Intimate Partner Homicide Rates in the United States, 1991 to 2005, Annals of Internal Med. (2017), https://www.acpjournals.org/doi/full/10.7326/M16-2849.)
Moreover, that study has several problems. (1) It uses a single dummy variable to measure the effect of the state surrender laws on domestic murder even though the laws vary widely in their application and severity. It has been shown (Clément de Chaisemartin and Xavier D'Haultfoeuille, Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects Am.

vary in terms of how long guns can be taken away without due process.

Zackey Rahimi probably isn't someone you'd want as a neighbor. In 2019, Rahimi, a drug dealer who sold marijuana and cocaine, fired a gun at a passerby who witnessed him dragging his girlfriend through a parking lot.[4] Months later, he shot at a driver he had gotten into an accident with. And in 2020, he assaulted another girlfriend and threatened her with a gun. Finally, in 2021, Rahimi shot a gun in

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Econ. Rev. 2020, 110, 2964-2996) that such studies can be seriously biased. (2) It uses imputed data to replace missing data values and does not subject the replacement to a robustness test to show that it did not affect the results. (3) The authors did not use clustered standard errors which causes the significance of the results to be overestimated. (4) The study used only three control variables, after listing 24 potentially relevant variables, without testing whether the discarded variables were significant as a group (a test for possible omitted variable bias).

The Amicus Brief of Public Health Researchers and Lawyers cites two papers on the 1996 Lautenberg Amendment, not 18 U.S.C. § 922(g)(8) (Wallin MA, Holliday CN, Zeoli AM. The Association of Federal and State-level Firearm Restriction Policies With Intimate Partner Homicide: A Re-analysis by Race of the Victim. J Interpers Violence. 2022 Sep;37(17-18):NP16509-NP16533. doi: 10.1177/08862605211021988. Epub 2021 Jun 18. PMID: 34144667 and Raissian, K.M. (2016), Hold Your Fire: Did the 1996 Federal Gun Control Act Expansion Reduce Domestic Homicides?. J. Pol. Anal. Manage., 35: 67-93). But more importantly, the two studies also do not take into account the differences in existing state laws and just use a simple dummy variable for the Lautenberg Amendment.

[4] https://www.supremecourt.gov/DocketPDF/22/22-915/259334/20230317174308399_Rahimi%20Pet%20-%20final.pdf

the air when his friend's credit card was declined.[5] That 2020 assault on his girlfriend led to a domestic violence restraining order, which forbade Rahimi from owning guns. But, the use of the domestic violence restraining order is a second-best approach. Rahimi had engaged in multiple felonies before a court granted the order. But if Rahimi had been convicted of these felonies, or had been detained awaiting trial, or subject to stringent, enforceable bond conditions because the court viewed him as a danger to others, he would also have been banned from obtaining guns.

The theory is straightforward. If people who are subject to a civil restraining order are dangerous, prohibiting them from possessing firearms could save lives. On the other hand, if people under restraining orders are truly dangerous, then they are unlikely to obey such a law. Someone who is willing to commit a serious assault or murder is already facing a significant prison sentence, a life sentence, or the death penalty. The additional penalties for illegally obtaining a gun or violating a protective order are unlikely to provide marginal deterrence.

Despite a protective order, Mr. Rahimi still illegally obtained and used a firearm. In any application of a restraining order, there are false positives and false negatives. Restraining orders mistakenly applied to law-abiding individuals will

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[5] John Fritze, "How a Second Amendment case at the Supreme Court is putting gun rights groups in a jam," USA Today, July 12, 2023 (https://www.usatoday.com/story/news/politics/2023/07/12/guns-supreme-court-second-amendment-rahimi/70383454007/).

likely be effective in disarming them, so that they will not be able to defend themselves and others. As just noted, at the same time, restraining orders might not impose a real marginal penalty on those who are truly dangerous. Thus, the law is most likely to restrain only those who are most law-abiding and fail to restrain the most dangerous. Indeed, Federal law didn't impose a lifetime ban on felons having guns until 1968 and California was the first state to impose a restriction on pistols in 1923.[6]

The justification for disarming felons is commonly accepted as the necessity to protect the public's safety. The courts have traded off the protection of rights versus preventing crime.[7] This leads to important questions regarding the distinction between violent felons and non-violent felons and their relative threats to the community's physical safety. There should be even more stringent questioning of the distinction between felons, who generally receive a full measure of due process, and individuals subjected to a civil process where most defendants obtain only the bare minimum due process which includes a lower probability of determining guilt. Presumably, on average, individuals going through a civil process are less likely to engage in crime with a firearm than those convicted of a felony. The greater

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[6] C. Kevin Marshall, *Why Can't Martha Stewart Have a Gun?*, 32 Harv. J.L. & Pub. Pol'y (Spring 2009) (https://papers.ssrn.com/sol3/papers.cfm?abstract_id=1367231).

[7] *Kanter v. Barr*, 919 F.3d 437 (7th Cir. 2019), abrogated by *New York State Rifle & Pistol Ass'n, Inc. v. Bruen*, 142 S. Ct. 2111, 213 L. Ed. 2d 387 (2022).

these mistakes the more likely there will be no statistically significant beneficial effect. To the extent that innocent people are disarmed, such laws can increase crime.

As witnessed by the rise in murder rates after handgun bans in Chicago and Washington, D.C., even complete gun bans didn't prevent criminals from obtaining guns.[8] While gun control advocates argue that gun bans won't work unless you ban guns everywhere in a country, every single time around the world that countries banned all guns or all handguns, murder/homicide rates have risen.[9] That has been true even in island nations with no neighbors supplying guns. One would think that out of randomness, one time, they would go down or at least stay the same.

A final consideration is what is known as the Peltzman effect. Enacting safety measures can cause people to engage in offsetting behavior. Researchers have found this in areas from traffic safety regulations to COVID-19 policies. When people were mandated to wear seatbelts or had airbags in their cars, people drove more recklessly and got into accidents more

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[8] John R. Lott, Jr., More Guns, Less Crime: Understanding Crime and Gun Control Laws, University of Chicago Press, 3rd edition, 2010, pp. 312-318. Crime Prevention Research Center, "Updated: Murder and homicide rates before and after gun bans," Crime Prevention Research Center, April 16, 2016 (https://crimeresearch.org/2016/04/murder-and-homicide-rates-before-and-after-gun-bans/).

[9] *Ibid*.

frequently.[10] There is even evidence that total deaths rose. With vaccines or mask mandates for COVID-19, people engaged in fewer safety precautions, such as not washing their hands as often or rejecting social distancing.[11] To the extent that people overestimated

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[10] Peltzman, Sam (1975). The effects of automobile safety regulation. Journal of Political Economy 83: 677-726. Steven Peterson, George Hoffer, and Edward Millner, "Are Drivers of Air-Bag-Equipped Cars More Aggressive? A Test of the Offsetting-Behavior Hypothesis," Journal of Law and Economics 38 (Oct. 1995): 251–64. Antonio Nicita and Simona Benedettini, "Does the Enforcement Design Exacerbate the 'Peltzman Effect'? Evidence from Driver Record Point and Road Safety in Italy," University of Siena Working Paper, March 2010 (https://papers.ssrn.com/sol3/papers.cfm?abstract_id=1568095).

[11] Katharina Henk, Florian Rosing, Fabian Wolff, Svenja B. Frenzel, Rolf van Dick, Valerie A. Erkens, Jan A. Häusser, Andreas Mojzisch, and Diana Boer, "An examination and extension of the Peltzman effect during the Covid-19 pandemic," Curr Res Ecol Soc Psychol. 2023; 4: 100091. Published online 2023 Jan 26. (https://www.ncbi.nlm.nih.gov/pmc/articles/PMC9888029/).
Hahab Falahi, Jasem Mohamadi, Hojjat Sayyadi, Iraj Pakzad, Ayoub Rashidi, Razi Naserifar, Jahangir Abdil, Azra Kenarkoohi, "COVID-19 Vaccination, Peltzman Effect and Possible Increase in Highrisk Behaviors: A Growing Concern Related to Risk Compensation and Reduced Compliance to Public Health Protective Measures after Vaccines Rollout," Infect Disord Drug Targets. 2022;22(8):8-12 (https://pubmed.ncbi.nlm.nih.gov/35440338/). Brit Trogen and Arthur Caplan, "Risk Compensation and COVID-19 Vaccines," Ann Intern Med. 2021 Mar 2: M20-8251 (https://www.ncbi.nlm.nih.gov/pmc/articles/PMC7983310/).
Arielle Kaim, Mor Saban, "Are we suffering from the Peltzman effect? Risk perception among recovered and vaccinated people during the COVID-19 pandemic in Israel," Public Health, Vol. 209, August 2022, pp. 19-22

the effectiveness of vaccines or masks in preventing the spread of the virus, these mandates could actually increase the spread of the disease. The observation has also been applied to protective gear in sports, cyber security, and crime.[12] We are not arguing that people overestimate the effectiveness of domestic violence protection orders nor that total acts of violence will increase, but that this effect by itself could neutralize some or all of the benefit from the law.

We look at the impact of § 922(g)(8) on the number of domestic murders, domestic gun murders, domestic femicides, and domestic gun femicides. There is no statistically significant evidence of benefits from this law in any of those areas. Indeed, there is evidence that domestic gun murder and domestic gun femicide increased.

(https://www.sciencedirect.com/science/article/pii/S0033350622001457).

[12] Alberto Chong and Pascual Restrepo, "Regulatory Protective Measures and Risky Behavior: Should We Be Saved from Ourselves?" University of Ottawa Working paper, March 2014 (https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2405494). Rui Zhang and Quanyan Zhu, "Attack-Aware Cyber Insurance of Interdependent Computer Networks," Net Institute Working Paper, October 11, 2016 (https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2848576). Eberhard Feess, "On the interplay of public and private law enforcement with multiple victims," European Journal of Law and Economics, (2015), Vol 39, pp. 79-95.

1. Literature Review

A review of the literature cited by amicus briefs filed as of September 24, 2023 reveals several major shortcomings. The first is an issue of causation. Many murders are committed with firearms, but that doesn't mean that any particular law will stop them from getting weapons or that they won't be able to commit murder in other ways. In this instance, Rahimi obtained a gun despite being barred by the protective order.

The second is that none of the cited studies address the effect of 18 U.S.C. § 922(g)(8) on domestic murders directly. Instead, the most often cited study by Diez et al. that addresses the effect of prohibiting persons subject to domestic violence protective orders from possessing firearms is concerned with the effectiveness of *state* firearm surrender laws on domestic murders.[13] The argument is that since these laws are similar to § 922(g)(8), therefore § 922(g)(8) also reduces domestic violence. But it is an empirical question how § 922(g)(8) affects domestic violence.

Finally, and most significantly, these public health studies are poorly done. They use a single dummy variable to measure the effect of the state surrender laws on domestic murder, even though the laws vary widely in their application and severity. But

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[13] Carolina Diez et al., State Intimate Partner Violence-Related Firearm Laws and Intimate Partner Homicide Rates in the United States, 1991 to 2005, Annals of Internal Med. (2017), https://www.acpjournals.org/doi/full/10.7326/M16-2849

research shows such studies can be seriously biased.[14] Nor do the various authors use clustered standard errors, which results in overestimated levels of statistical significance. In addition, the most frequently cited study by Diez et al. uses imputed data to replace missing data values and does not subject the replacement to a robustness test to show that it did not affect the results. The study used only three control variables after listing 24 potentially relevant variables without testing whether the discarded variables were significant as a group (a test for possible omitted variable bias).

The Amicus Brief of Public Health Researchers and Lawyers cites two papers on the 1996 Lautenberg Amendment, not 18 U.S.C. § 922(g)(8).[15] But more importantly, the two studies also do not take into account the differences in existing state laws and just use a simple dummy variable for the Lautenberg Amendment.

While all the researchers cited by the literature cited by amicus briefs rely on public health studies, it is important to recognize how public health research

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[14] Clément de Chaisemartin and Xavier D'Haultfoeuille, Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects Am. Econ. Rev. 2020, 110, 2964-2996.

[15] Wallin MA, Holliday CN, Zeoli AM. The Association of Federal and State-level Firearm Restriction Policies With Intimate Partner Homicide: A Re-analysis by Race of the Victim. J Interpers Violence. 2022 Sep;37(17-18):NP16509-NP16533. doi: 10.1177/08862605211021988. Epub 2021 Jun 18. PMID: 34144667 and Raissian, K.M. (2016), Hold Your Fire: Did the 1996 Federal Gun Control Act Expansion Reduce Domestic Homicides?. J. Pol. Anal. Manage., 35: 67-93.

differs dramatically from research by criminologists and economists who are much more skeptical of gun control. For example, while the largest-ever survey of 120 academics on gun control found that academics were overall quite skeptical that gun control lowered murder rates or the frequency of mass public shootings, the differences between criminologists, economists, and public health researchers were stark.[16] Outside of getting rid of gun-free zones or concealed handgun laws, there is no gun control regulation that criminologists and economists think would reduce murders or mass public shootings. By contrast, public health researchers are more likely to support barring sales to convicted stalkers or people convicted of violent misdemeanors and strongly support gun-free zones.

2. Methodology

We measure the strength of state domestic violence protective orders (DVPO) by the duration of the order. Federal law 18 U.S.C. § 922(g) was passed in 1994. It has no effect in the absence of a state protection order. We create a dummy variable to measure its effect, taking the value zero for years before 1994 and the value one for the remaining years. We then create two test variables by multiplying the temporary order duration and the final order duration variables by the dummy variable for 1994 and later.

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[16] Arthur Z. Berg, John R. Lott, Jr., and Gary A. Mauser, "Expert Views on Gun Laws." Regulation, Winter 2019-2020, Vol. 42, No. 4, pp. 40-47.

Therefore, we are estimating the additional effect of § 922(g)(8) given an existing state protection order.

This methodology allows us to estimate a two-way fixed-effects (TWFE) panel data model where we include state and year dummies. The year dummies control for events such as recessions, the passage of other federal laws, and anything else that affects all states in a given year. This is the most commonly used econometric methodology for policy analysis. The de Chaisemartin and D'Haultfoeuille (op.cit.) criticism does not apply to our model because the test variables are not simple dummies, instead they are the product of a continuous variable (state order duration) and a dummy for 1994.

A DVPO is intended to protect the applicant against potential violence committed by a domestic partner. While a person of any gender can request such an order, women are the primary users. As such, we investigate several different outcomes: domestic homicides, domestic homicides of women, domestic homicides committed by firearms, and femicides by a domestic partner using firearms. These are count variables which only take integer values and are the result of counting (as opposed to ranking). The appropriate statistical model for such data is the negative binomial model.[17] The negative binomial is a generalization of the Poisson model in that it does not require the mean and variance of the dependent variable to be equal. The typical case is that the variance is greater than the mean, which is called

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[17] Cameron, A.C. and Trivedi, P.K. (1998). *Regression Analysis of Count Data*. Cambridge, UK.

overdispersion. We tested for overdispersion in each of the models reported below. There was significant overdispersion in every case, justifying the negative binomial model.

The choice of control variables to include in the model is crucial because states are continuously changing the number of police, the number of incarcerated persons, other laws, etc. If we leave out a potentially relevant control variable, the omission biases all estimates including the estimated coefficients on the test variables. This bias could result in a spurious regression. On the other hand, including an irrelevant variable whose true coefficient is zero, does not bias the estimates, but leads to imprecise estimates. This imprecision could be so bad that the estimate of interest, e.g., the effect of DVPO's on domestic murders appears to be insignificant when it is in fact significant.

We address this modeling problem by estimating two models. The first is a general model that includes many potentially relevant control variables. Using many control variables allows us to avoid omitted variable bias but may cause our estimates to be imprecise. To avoid the imprecision problem, we derive a so-called "specific" model with only the significant control variables. This "general to specific" modeling methodology has been used successfully in many applications.[18]

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[18] See: Hendry, D. F. (1995) *Dynamic Econometrics*. Oxford University Press, Oxford, UK; Owen, D. and Weatherston, C.R. (2004). Uncertainty of outcome and super 12 attendance: application of a general-to-specific modeling strategy,' *Journal of*

3. Data

Data on the dependent variables come from Jacob Kaplan's Supplementary Homicide Reports (SHR) produced by the FBI for the years 1976-2020.[19] SHR data for 1980-1983 are missing and most of the data for the control variables end in 2018. Also, many of the control variables have missing data for the District of Columbia. As a result, our sample consists of the 50 states for the years 1976-1980 and 1984-2018. Because of the missing observations for 1981-83, this is an unbalanced panel. In the robustness section, we restrict the dataset to 1984-2018 to form a balanced panel.

We define a domestic homicide as one in which the first victim was related to the first offender as a husband, wife, boyfriend, girlfriend, common-law husband, common-law wife, ex-husband, ex-wife, or in a homosexual relationship. We then determined if the victim was female and if the offender used a firearm.

Sports Economics 5, 347-370; Muelbauer, J. and Nunziata, L. (2004). Forecasting (and explaining) US Business Cycles, CEPR Discussion Papers 4584; Rao, B.B. and Singh, R. (2006). Demand for money for fiji with PcGets. *Applied Economics Letters* 13, 987-991; Reade, J. J. (2007). Modeling and forecasting football attendances. *Oxonomics* 2, pp. 27–32.

[19] Kaplan, Jacob. Jacob Kaplan's Concatenated Files: Uniform Crime Reporting (UCR) Program Data: Supplementary Homicide Reports (SHR), 1976-2020. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2021-09-22.
https://www.openicpsr.org/openicpsr/project/100699/version/V11/view

For the policy variables, we examined the law codes of all fifty states from 1970 to 2023. There are two types of protective orders. Temporary or emergency orders are usually issued without the participation of the respondent. These are usually short, averaging 27 days (median 14 days), but they can be as long as two years. The final order, which replaces any temporary order, is longer,

averaging four years with a median of one year. However, many states issue permanent orders, which can be modified or dissolved. To translate these into numerical values, we had to make several assumptions. For example, since we don't know the respondent's age or the probability of the order being appealed or otherwise reduced or rescinded, we assume that a permanent order lasts 25 years. Since the numerical values are necessarily arbitrary, we did robustness checks with these long-duration order values doubled and halved to see if the results changed. (The results are not sensitive to these assumptions.) The control variables are listed in Table 1 along with the means of the dependent variables and the policy variables of interest.

Table 1: Means and other statistics.

Variable	N	Mean	St. Dev	Min	Max
Domestic murders	1587	33.73	43.53	0	344
Domestic femicides	1587	23.88	30.18	0	217
Dom gun murders	1587	19.59	27.24	0	241

Dom gun femicides	1587	14.37	19.02	0	141
Final order, years	1587	3.98	7.40	0.05	42.48
Temp order, days	1587	27.13	73.38	0.28	730
Final*§ 922(g)(8) dummy	1587	3.55	7.26	0	42.48
Temp*§ 922(g)(8) dummy	1587	21.19	68.85	0	730
Prisoners per capita	1587	333.90	154.32	32.84	871.62
Police per capita	1587	277.60	50.41	161.67	522.74
Abortion	1587	1.51	1.31	0	6.02
Unemployment rate	1587	5.65	1.88	2.30	14.80
Employment per capita	1587	57.90	5.33	37.99	73.07
Military per capita	1587	1.01	0.90	0.27	6.65
Construction per capita	1587	3.33	0.75	1.36	6.90
Alcohol per capita	1587	1.91	0.45	0.89	4.04
Crack	1587	1.41	1.10	-0.70	7.78
Density	1587	4.31	13.54	0.08	111.34
Income per capita	1587	16.80	3.48	9.16	30.45
Poverty rate	1587	12.89	3.71	2.90	27.20

Welfare per capita	1587	249.58	89.65	0.33	548.53
Percent gun suicide	1587	61.10	12.73	0	100
Executions per capita	1587	0.87	3.08	0	40
Percent population 15-34	1587	28.92	2.72	23.37	39.81
Percent black men 15-34	1587	1.66	1.44	0.07	5.90

Police, prison, and the death penalty all have deterrent roles. Prison and executions also have incapacitation effects. These variables are all in per capita rates. We also lag them to avoid possible simultaneity. The effective abortion rate for murder is included because there is substantial evidence that legalized abortion significantly affects crime rates. [20] Unfortunately, the Donohue and Levitt (2020) sample ends in 2014 and computation of the effective abortion rate is complicated. However, it is easy to extrapolate the series since it is a smooth trend, and we only need to extrapolate four years.

We include the proportion of the population between 15 and 34 and the proportion of black males 15-34 because relatively young men largely commit murder. We have three employment variables: total employment per capita, indicating the amount of legitimate employment available; military

[20] Donohue, J.J. and Levitt, S.D. (2020). The Impact of Legalized Abortion on Crime over the Last Two Decades. *American Law and Economics Review* 22: 241-302.

employment, since the military concentrates many young men in certain areas but also sends many young men out of the country; and construction employment per capita because the construction industry also concentrates large numbers of young men. The denser the population, the more interactions among the inhabitants, some of which could cause domestic murder. For this reason, we include population density as a control variable. We include per capita consumption of alcohol because alcohol can reduce inhibitions. We include the unemployment rate, real personal income per capita, the poverty rate, and real welfare payments per capita, any of which could affect domestic murder by altering the stresses associated with domestic life.

A possibly important factor in any crime equation using historical data is the emergence of crack cocaine in the 1980's. The resulting large increase in the supply of cocaine caused turf battles among drug gangs, increasing murder rates. We control for the crack cocaine epidemic by including the Fryer crack index, a combination of indicators of cocaine use compiled by Fryer and his colleagues for the period of the crack epidemic.[21] There are continuous values for 1981 to 2000 for each state. We set pre-1981 values at the 1981 levels and post-2000 values at the 2000 levels. We also include the most widely used proxy for gun availability -- the proportion of suicides committed by firearms -- to control for

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[21] Fryer, R.G., Heaton, P.S., Levitt, S.D., Murphy, K.M. (2013). Measuring Crack Cocaine and Its Impact. *Economic Inquiry* 51:1651-1681.

ambient gun ownership. Finally, we also include the population level since more people can be expected to be associated with more domestic violence.

4. Results

The next two tables present the primary results. Table 2 shows the effects of 18 U.S.C. § 922(g)(8) on domestic murder and domestic femicide. The coefficients are presented as incident rate ratios indicating the percent change in the dependent variable for a one-unit change in the explanatory variable. A value of 1 indicates no change, a value greater than one such as 1.01 indicates that domestic murders would increase by one percent for a one-year increase in the average final order. A value less than one indicates that a one-year increase in final restraining orders would decrease domestic murders by one minus the coefficient. For example, the coefficient on final order years in the first column of Table 2 is .993 indicating that a one-year increase in the length of the order would reduce domestic murders by .007 percent. The corresponding t-statistic is reported in parentheses below the coefficient. T-statistics greater than two in absolute value are considered significant (using the standard five percent significance level).[22] Significant t-statistics are also indicated by asterisks.

Table 2 shows that state temporary and final domestic violence protection orders by themselves do

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[22] Federal Judicial Center, *Reference Manual on Scientific Evidence* (3rd Edition). Washington, DC: National Academies Press, p.251.

not significantly affect domestic murders or domestic femicides. The enhancement provided by 18 U.S.C. § 922(g)(8) also has no significant effect on domestic murders or domestic femicides.

Table 3 shows the effects of 18 U.S.C. § 922(g)(8) on domestic gun murder and domestic gun femicide. We again find that neither temporary nor final domestic violence protection orders significantly reduce domestic gun murders or domestic gun femicides. The effect of the § 922(g)(8) working through temporary orders is a small, but significant, increase in domestic gun murders. The estimates for domestic gun femicides show a similarly small effect but are not consistently significant. The effect of § 922(g)(8) working through final orders shows a very small and insignificant decline in both domestic gun murders and domestic gun femicides. Overall, 18 U.S.C. § 922(g)(8) has not significantly reduced domestic murders, domestic femicides, domestic gun murders, or domestic gun femicides.

Table 2: Results for domestic murder and domestic femicide

	Domestic Murder		Domestic Femicide	
Variables	General	Specific	General	Specific
Temporary order, days	1.000	1.001	1.001	1.001
	(0.602)	(0.862)	(1.031)	(1.364)
Final order, years	0.993	0.991	0.993	0.991
	(-1.183)	(-1.250)	(-1.101)	(-1.165)

§ 922(g)(8) dummy * temp days	1.000	1.000	1.000	1.000
	(0.556)	(1.350)	(-0.432)	(0.121)
§ 922(g)(8) dummy*final years	0.997	0.994	0.998	0.994
	(-0.651)	(-1.103)	(-0.423)	(-1.115)
Number of prisoners per capita lagged	1.000	1.000	1.000	
	(-1.356)	(-1.338)	(-0.653)	
Number of police per capita lagged	1.000		1.000	
	(-0.302)		(0.0198)	
Abortion	0.984		0.958	
	(-0.282)		(-0.788)	
Unemploy-ment	1.010		1.014	
	(0.772)		(1.125)	
Employment	0.993		0.995	
	(-0.460)		(-0.300)	
Military	1.007		1.039	
	(0.0536)		(0.305)	
Construction	1.103	1.078	1.087	1.041
	(1.798)	(1.760)	(1.506)	(1.044)
Alcohol	1.503*	1.446**	1.335	1.386*
	(2.172)	(2.810)	(1.541)	(2.551)
Crack	1.042	1.076	1.042	1.079
	(1.411)	(1.673)	(1.376)	(1.666)
Density	0.995		0.985	0.982*
	(-0.485)		(-1.540)	(-2.135)
Income	1.004		1.007	

	(0.192)		(0.345)	
Poverty	1.011	1.014	1.007	
	(1.221)	(1.665)	(0.779)	
Welfare	1.000		1.000	
	(-0.666)		(-0.0126)	
Percent gun suicide lagged	0.997	0.997	0.997	0.998
	(-1.671)	(-1.450)	(-1.406)	(-1.072)
Executions per capita lagged	1.000		1.001	
	(-0.0873)		(0.414)	
Pct pop 15-34	0.982		0.976	
	(-0.707)		(-0.941)	
Pct black men 15-34	1.751***	1.613***	1.558***	1.471**
	(3.813)	(3.378)	(3.357)	(3.014)

Note: ***p<.001, p<**.01, p<*.05; negative binomial model; coefficients are incidence rate ratios; t-ratios in parentheses; standard errors clustered on states; state and year effects estimates are suppressed.

Table 3: Domestic gun murder, domestic gun femicide

	Domestic gun murder		Domestic gun femicide	
Variables	General	Specific	General	Specific
Temporary order, days	1.000	1.001	1.000	1.000
	(0.274)	(0.703)	(0.323)	(0.486)
Final order, years	0.994	0.992	0.991	0.991
	(-0.965)	(-1.104)	(-1.402)	(-1.351)

§ 922(g)(8) dummy * temp days	1.001*	1.001**	1.001*	1.001
	(2.083)	(2.588)	(2.258)	(1.839)
§ 922(g)(8) dummy *final order years	0.993	0.991	0.996	0.996
	(-1.329)	(-1.654)	(-0.786)	(-0.826)
Number of prisoners per capita lagged	1.000		1.000	
	(-0.316)		(0.400)	
Number of police per capita lagged	0.999		0.999	
	(-0.737)		(-0.434)	
Abortion	0.962		0.956	
	(-0.614)		(-0.826)	
Unemploy-ment	1.013		1.006	
	(0.808)		(0.411)	
Employment	0.990		0.997	
	(-0.461)		(-0.157)	
Military	0.974		0.978	
	(-0.179)		(-0.171)	
Construction	1.106		1.088	
	(1.562)		(1.360)	
Alcohol	1.648*	1.664***	1.470	1.589***
	(2.317)	(3.373)	(1.850)	(3.524)
Crack	1.072*	1.109*	1.084*	1.091**
	(2.041)	(2.242)	(2.314)	(2.768)

Density	0.997		0.991	0.984
	(-0.270)		(-1.075)	(-1.537)
Income	0.994		0.987	
	(-0.294)		(-0.698)	
Poverty	1.015	1.016*	1.010	
	(1.602)	(1.995)	(1.143)	
Welfare	1.000		1.000	
	(-0.243)		(0.376)	
Percent gun suicide lagged	0.997		0.998	
	(-1.112)		(-0.995)	
Executions per capita lagged	0.996	0.995	0.998	
	(-1.356)	(-1.889)	(-0.727)	
Pct pop 15-34	0.979		0.968	0.972
	(-0.660)		(-1.045)	(-1.032)
Pct black men 15-34	1.697**	1.525*	1.542**	1.571**
	(3.192)	(2.458)	(2.843)	(2.997)

Note: ***p<.001, p<**.01, p<*.05; negative binomial model; coefficients are incidence rate ratios; t-ratios in parentheses; standard errors clustered on states; state and year effects estimates are suppressed.

5. Robustness checks

An alternative method for estimating the impact of a crime policy is to investigate the policy's effect on the per capita domestic murder rate. For this analysis, we apply the standard two-way fixed-effects model to the rate of domestic murders per one million people. Table 4 presents the results. The estimates are

very similar to those shown in Tables 2 and 3. 18 U.S.C. § 922(g)(8) does not significantly affect domestic murder or domestic femicide. The small positive effect for domestic gun murders which was significant in Table 3 is not significant in the per capita fixed-effects model. There is a small but significant positive effect on domestic gun femicide for temporary protective orders, but only in the more precise specific model.

Table 4: Estimates using per capita data

	Domestic murder		Domestic femicide	
	General	Specific	General	Specific
Temporary order, days	0.034	0.038	0.011	0.0071
	(0.87)	(1.03)	(0.40)	(0.25)
Final order, years	-0.36	-0.37	-0.18	-0.19
	(-1.08)	(-1.19)	(-0.85)	(-0.82)
§ 922(g)(8) dummy * temp days	-0.015	-0.016	-0.0061	0.0046
	(-1.13)	(-1.33)	(-0.59)	(0.50)
§ 922(g)(8) dummy *final order years	0.033	0.028	0.049	-0.075
	(0.14)	(0.12)	(0.31)	(-0.44)

	Domestic gun murder		Domestic gun femicide	
	General	Specific	General	Specific
Temporary order, days	0.026	0.032	0.0044	-0.0025
	(1.05)	(1.31)	(0.24)	(-0.14)
Final order, years	-0.21	-0.27	-0.24	-0.22
	(-0.79)	(-1.01)	(-1.41)	(-1.16)
§ 922(g)(8) dummy * temp days	0.00031	-0.0012	0.011	0.027**
	(0.032)	(-0.13)	(1.39)	(3.46)
§ 922(g)(8) dummy *final order years	-0.16	-0.12	0.0034	-0.070
	(-0.78)	(-0.62)	(0.027)	(-0.49)

Note: ***p<.001, p<**.01, p<*.05; two-way fixed-effects model; control variables and state and year effects estimates are suppressed; standard errors clustered on states.

As noted above, there is some uncertainty as to how to translate the duration of indefinite or permanent final orders into numbers that can be entered into a dataset. We investigate the sensitivity of the results to these arbitrary values by doubling and halving the values of any orders over 10 years and re-

running the negative binomial regressions. The results are reported in Table 5.

Table 5: Doubling and halving long sentences

	2X Domestic murder		Half X Domestic murder	
	General	Specific	General	Specific
Temporary order, days	1.00 (0.57)	1.00 (0.84)	1.00 (0.65)	1.00 (0.89)
Final order, years	1.00 (-0.88)	1.00 (-1.07)	0.98* (-2.05)	0.99 (-1.70)
§ 922(g)(8) dummy * temp days	1.00 (0.61)	1.00 (1.42)	1.00 (0.44)	1.00 (1.24)
§ 922(g)(8) dummy *final order years	1.00 (-0.87)	0.99 (-1.27)	1.00 (-0.40)	0.99 (-0.89)
	Domestic femicide		Domestic femicide	
	General	Specific	General	Specific
Temporary order, days	1.00 (1.00)	1.00 (1.34)	1.00 (1.10)	1.00 (1.42)
Final order, years	1.00 (-0.82)	1.00 (-1.02)	0.98 (-1.86)	0.99 (-1.55)

§ 922(g)(8) dummy * temp days	1.00 (-0.39)	1.00 (0.19)	1.00 (-0.55)	1.00 (0.0044)
§ 922(g)(8) dummy *final order years	1.00 (-0.65)	0.99 (-1.27)	1.00 (-0.22)	0.99 (-0.92)
	Domestic femicide		Domestic femicide	
	General	Specific	Gen-eral	Spec-ific
Temporary order, days	1.00 (0.23)	1.00 (0.67)	1.00 (0.35)	1.00 (0.76)
Final order, years	1.00 (-0.55)	1.00 (-0.87)	0.98 (-1.82)	0.98 (-1.77)
§ 922(g)(8) dummy * temp days	1.00* (2.14)	1.00** (2.66)	1.00 (1.95)	1.00* (2.46)
§ 922(g)(8) dummy *final order years	0.99 (-1.52)	0.99 (-1.86)	1.00 (-0.72)	0.99 (-1.14)
	Domestic gun femicide		Domestic gun femicide	
	General	Specific	Gen-eral	Spec-ific
Temporary order, days	1.00	1.00	1.00	1.00

	(0.28)	(0.44)	(0.38)	(0.55)
Final order, years	1.00	1.00	0.98*	0.98*
	(-1.04)	(-1.01)	(-2.23)	(-2.24)
§ 922(g)(8) dummy * temp days	1.00*	1.00	1.00*	1.00
	(2.34)	(1.91)	(2.08)	(1.71)
§ 922(g)(8) dummy *final order years	0.99	0.99	1.00	1.00
	(-1.02)	(-1.04)	(-0.46)	(-0.52)

Note: ***p<.001, p<**.01, p<*.05; indefinite long sentences for final orders are doubled in the first two columns and halved in columns three and four; negative binomial model; coefficients are incidence rate ratios; t-ratios in parentheses; standard errors clustered on states; control variables, state and year effects estimates are suppressed.

The results are virtually the same as those reported in Tables 2 and 3 and they are almost identical for doubling and halving the order length. The results appear to be robust to assumptions concerning final order duration.

The SHR data has missing values for all states from 1980-1983. The resulting panel is unbalanced. To get a balanced panel, we estimated the negative binomial model using data from 1984-2018. The results are reported in Table 6.

Table 6: Using balanced panel, 1984-2018

	Domestic murder		Domestic femicide	
	General	Specific	Gen-eral	Specific
Temporary order, days	1.000 (0.457)	1.000 (0.445)	1.001 (0.875)	1.001 (0.944)
Final order, years	0.992 (-1.255)	0.992 (-1.192)	0.992 (-1.224)	0.992 (-1.094)
§ 922(g)(8) dummy * temp days	1.000 (0.530)	1.000 (1.306)	1.000 (-0.454)	1.000 (-0.0547)
§ 922(g)(8) dummy *final order years	0.997 (-0.660)	0.996 (-0.819)	0.999 (-0.345)	0.997 (-0.749)
	Domestic gun murder		Domestic gun femicide	
	General	Specific	Gen-eral	Specific
Temporary order, days	1.000 (0.140)	1.000 (0.224)	1.000 (0.222)	1.000 (0.400)

Final order, years	0.994	0.993	0.990	0.991
	(-1.040)	(-1.016)	(-1.520)	(-1.447)
§ 922(g)(8) dummy * temp days	1.001	1.001*	1.001*	1.001
	(1.948)	(2.453)	(2.145)	(1.702)
§ 922(g)(8) dummy *final order years	0.993	0.993	0.997	0.996
	(-1.357)	(-1.451)	(-0.707)	(-0.838)

Note: ***p<.001, p<**.01, p<*.05; indefinite long sentences for final orders are doubled in the first two columns and halved in columns three and four; negative binomial model; coefficients are incidence rate ratios; t-ratios in parentheses; standard errors clustered on states; control variables, state and year effects estimates are suppressed.

The results are almost identical to those reported in Tables 2 and 3.

CONCLUSION

We have estimated the enhancement effect of federal law 18 U.S.C. § 922(g)(8) using a panel of fifty states over 38 years. The results are remarkably robust. We find that 18 U.S.C. § 922(g)(8) has no statistically significant beneficial impact on domestic murder, domestic femicide, domestic gun murder, or domestic gun femicide. Additional penalties for illegally obtaining a gun or violating a protective order

are unlikely to provide marginal deterrence. In the past, some courts have traded off protection of rights versus prevention of crime. The debate in the past has focused on violent versus non-violent felons. Presumably, on average, individuals going through a civil process are less likely to engage in crime with a firearm than those convicted of a felony. Even when hearings occur, the lower standard of proof in civil matters means a higher probability of false positives and thus may inadvertently disarm innocent people. The statute not only fails to withstand Constitutional analysis, it fails to accomplish its policy goal. Therefore, we urge this Court to affirm the decision below.

Respectfully submitted,

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