

EXHIBIT 3

MICHELLE FLANAGAN vs CALIFORNIA ATTORNEY GENERAL XAVIER BECERRA Kleck, Gary

1 UNITED STATES DISTRICT COURT

2 CENTRAL DISTRICT OF CALIFORNIA

3

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5 MICHELLE FLANAGAN, SAMUEL)
6 GOLDEN, DOMINIC NARDONE, JACOB)
7 PERKIO, and THE CALIFORNIA)
8 RIFLE & PISTOL ASSOCIATION,)
9 Plaintiffs,)

10 v.) NO. 2:16-cv-06164-JAK-AS

11 CALIFORNIA ATTORNEY GENERAL)
12 XAVIER BECERRA, in his official)
13 capacity as Attorney General of)
14 the State of California, et al.,)
15 Defendants.)

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19 DEPOSITION OF GARY KLECK,

20 taken on behalf of the Defendant Xavier Becerra,

21 Attorney General of the State of California,

22 at 180 East Ocean Boulevard, Suite 200,

23 Long Beach, California, commencing at

24 9:30 a.m. on Tuesday, July 25, 2017,

25 reported by MARCENA M. MUNGUIA, CSR No. 10420,

a Certified Shorthand Reporter in and for

the State of California, pursuant to Notice.

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MICHELLE FLANAGAN vs CALIFORNIA ATTORNEY GENERAL XAVIER BECERRA Kleck, Gary

1 APPEARANCES:

2 For the PLAINTIFFS: MICHEL & ASSOCIATES, P.C.
3 BY: SEAN A. BRADY
4 180 East Ocean Boulevard
Suite 200
Long Beach, California
90802
5

6 For the DEFENDANT JONATHAN M. EISENBERG
XAVIER BECERRA, Deputy Attorney General
7 ATTORNEY GENERAL OF Department of Justice
THE STATE OF OFFICE OF THE ATTORNEY
8 CALIFORNIA: GENERAL
State of California
9 300 South Spring Street
Suite 1702
10 Los Angeles, California
90013
11

12 Also Present: Matthew Nguyen
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3 - "Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data and a State-Level Synthetic Controls Analysis," dated 5/23/17 13

4 - NBER Working Papers Series: "Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data and a State-Level Synthetic Controls Analysis," dated June 2017 14

5 - Gary Kleck's Rebuttal of Expert Report of John J. Donohue, dated 6/29/17 15

6 - The Impact of "Shall-Issue Concealed Handgun Laws on Violent Crime Rates" 68

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1 Long Beach, California, Tuesday, July 25, 2017

2 9:30 a.m.

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4
5 GARY KLECK,

6 called as a Witness, and having been first duly sworn by
7 the Certified Shorthand Reporter, was examined and
8 testified as follows:

9 THE WITNESS: I do.

10
11 EXAMINATION

12 BY MR. EISENBERG:

13 Q Good morning. Please state your name for the
14 record.

15 A Gary Kleck.

16 Q Do you have a name that's preferred to go by
17 such as Mr. Kleck, Professor Kleck, Gary?

18 A Professor Kleck.

19 Q All right. I will refer to you that way. I'm
20 Jonathan Eisenberg. I'm a Deputy Attorney General for
21 the State of California. We're here, as I believe you
22 know, for the deposition of Flanagan v. Becerra case. I
23 wanted to present the first exhibit which has been
24 premarked, which is a deposition notice and ask you just
25 a couple of questions about it, and then I'll go over

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1 first year that you testified as an expert witness?

2 A 1983.

3 Q And have the cases that you've testified about
4 all concerned firearms in one way or another?

5 A Yes.

6 Q You're aware of course that -- I think what you
7 called the Great American Gun Debate, that there is a
8 side that's sort of known as the gun control or anti-gun
9 side and another side that's known as the pro-gun side,
10 so to speak? You're aware of those terms?

11 A Yes.

12 Q Have you testified for the gun control or
13 so-called anti-gun side in any of the 14 cases?

14 MR. BRADY: Objection; calls for speculation, is
15 vague as to "gun control side."

16 THE WITNESS: No.

17 BY MR. EISENBERG:

18 Q Have you ever given testimony in litigation that
19 was in favor of a gun law becoming more restrictive?

20 A Can you repeat the question, please?

21 Q Okay. Have you ever testified in litigation
22 about a gun law -- sorry -- in support of a gun law
23 becoming more restrictive?

24 A No.

25 Q Other than at today's deposition, you're being

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1 years that you understood the organization to be
2 advocating for more restrictive firearm laws?

3 A Yes.

4 Q And was that one of the reasons that you were a
5 member of Common Cause?

6 MR. BRADY: Objection; right to privacy.

7 THE WITNESS: No.

8 BY MR. EISENBERG:

9 Q I'd like to ask you about the page three of your
10 report, which is the articles published. The first
11 question I want to ask you is about your article in the
12 Journal of Contemporary Criminal Justice, "City-level
13 Characteristics and Individual Handgun Ownership:
14 Effects of Collective Security and Homicide," which is
15 six down from the top. Do you see that reference there?

16 A Yes.

17 Q It lists you as having a coauthor whose name
18 I'll probably mispronounce, Tomislav Kovandzic?

19 A That's correct.

20 Q That's great. See, I wish we could somehow note
21 that on the deposition transcript that I got that right,
22 but will just have to be his affirmation.

23 Was Mr. Kovandzic ever a student of yours --

24 A Yes.

25 Q -- at Florida State?

MICHELLE FLANAGAN vs CALIFORNIA ATTORNEY GENERAL XAVIER BECERRA Kleck, Gary

1 A Yes.

2 Q And what is your -- what field of study is your
3 professorship in? In other words, how would you answer
4 the -- how would you make the statement, "I am a
5 professor of" blank. How would you fill in the blank?

6 A Criminology.

7 Q And did Mr. Kovandzic take an advanced degree
8 under your supervision?

9 A Not exactly. That would imply I was his major
10 professor. I was a professor but not his major
11 professor.

12 Q Okay. And so you've worked with him on a couple
13 of papers at least; right? Because there's two listed
14 here. There's the one that we just referred to and then
15 three down of that is a 2011 article in the Journal of
16 Criminal Justice. The statement there that he is a
17 coauthor of that paper is accurate of course? Yes?

18 A Yes.

19 MR. BRADY: Object- -- well --

20 BY MR. EISENBERG:

21 Q Have you written any other papers that were
22 published where Kovandzic was a coauthor; in other words,
23 things not listed here, but they do exist?

24 A None that come to mind right now prior to 2007.

25 Q Have you served as an editor on any of -- any

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1 A Yes.

2 Q The competition of the group of researchers
3 changes from article to article; correct?

4 A Usually.

5 Q Is there any journal in this field where the
6 reviewers are always the same people?

7 A Not to my knowledge.

8 Q Have you published an academic article in an
9 economics journal?

10 MR. BRADY: Objection; ambiguous as to "economics
11 journal."

12 THE WITNESS: I don't think so.

13 BY MR. EISENBERG:

14 Q Do you have an opinion about the quality of peer
15 reviewing in economics journals?

16 A No.

17 Q I'll ask the same questions for statistics
18 journals. Have you had an academic article published in
19 a statistics journal?

20 A Not that I recall.

21 Q And do you have an opinion on the quality of
22 peer reviewing in statistics journals?

23 A No.

24 Q All right. I'd like to direct you to page six
25 of your rebuttal report in the -- I'm going to be just

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1 injury is something short of death?

2 MR. BRADY: Objection.

3 BY MR. EISENBERG:

4 Q Do you understand my question?

5 MR. BRADY: Objection; calls for speculation,
6 confusing, compound.

7 THE WITNESS: I do understand it.

8 BY MR. EISENBERG:

9 Q Please answer it.

10 A No, it does not increase the violence generally.

11 Q When you said a moment ago that it increases the
12 lethality of violence across the board, what were you
13 referring to by "across the board"?

14 A I meant the entire category of violent attacks
15 without respect to the intent of the aggressor, which is
16 what your question had specifically pertained to.

17 Q Right. Okay. Are you aware that since
18 Professor Donohue put out the NBER working paper that's
19 Exhibit 2 at this deposition, Exhibit B to his report,
20 that he disaggregated the results or reran the results
21 from that paper to show what they were for each of the
22 crime categories?

23 A No.

24 MR. BRADY: Objection; assumes facts not in evidence.

25 THE WITNESS: No.

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1 BY MR. EISENBERG:

2 Q Have you heard or been informed of anything to
3 that effect?

4 A He had done an earlier study in which he did
5 that and I was aware of that earlier study.

6 Q Are you referring to the 2014 study where the
7 person named Aneja is the lead author?

8 A Yes.

9 Q But you're not aware that since you submitted
10 your rebuttal report that we're talking about right now,
11 Professor Donohue reran the data to break it out by each
12 of the four crime categories?

13 A No.

14 Q If I represent to you that Donohue did that,
15 would you consider that to be an improvement on the prior
16 work?

17 A Yes.

18 Q And if the results that came out showed at a
19 level of statistical significance that aggravated
20 assaults went up uniformly across the models studied,
21 et cetera, would you consider that to be valid evidence
22 that right-to-carry laws lead to increases in aggravated
23 assaults?

24 MR. BRADY: Objection; incomplete hypothetical,
25 assumes facts not in evidence, confusing, compound.

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1 diligence in writing out those reports?

2 MR. BRADY: Objection; calls for speculation,
3 incomplete hypothetical, beyond the scope of what the
4 witness was called to testify about.

5 THE WITNESS: That's certainly possible, but of
6 course it would apply to data aggregated up to any level.
7 I mean, it's the point of origin. It's the data as they
8 originally gathered any police-based crime statistics.

9 BY MR. EISENBERG:

10 Q So why again is county data less accurate in
11 your mind?

12 A Because you can have entire local agencies that
13 fail to report their crime statistics and thus get, you
14 know, grossly understated numbers of crimes reported to
15 the police simply as an artificial product of this
16 failure of the agency to submit Uniform Crime Reports to
17 the FBI or rather technically to the state Uniform Crime
18 Reporting agency.

19 Q Which then goes up to the FBI crime report?

20 A Correct.

21 Q Okay. Thanks. You are familiar with another
22 scholar named John Lott?

23 A Yes.

24 Q Are you aware that John Lott promotes an idea
25 that has been summarized as more guns, less crime?

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1 A Yes.

2 Q Is the theory essentially that if there are more
3 people armed with firearms, that has the effect -- net
4 effect of reducing crime in the area?

5 A Yes.

6 Q Do you agree that that thesis is correct?

7 A No.

8 Q It's true that you have criticized John Lott's
9 work in writing before; correct?

10 A Yes.

11 Q Have you criticized John Lott for doing studies
12 involving crime data where the crime is this county crime
13 data that you've described as inaccurate?

14 A Yes, although I'm not sure if it's in a
15 published report of any kind. I couldn't swear to that.
16 Certainly I've made the oral comments at professional
17 meetings, noting this same problem, and I even had the
18 conversations directly with Professor Lott saying exactly
19 that, that the county-level data are problematic for that
20 reason.

21 Q Have you ever said words to the effect that no
22 credible criminologist believes that the data shows that
23 increasing the number of guns that people are carrying
24 has the effect of reducing crime?

25 A No, I don't recall significant saying anything

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1 that are so seriously flawed, no conclusion at all can be
2 derived.

3 Q Would you say that in the Donohue study, which
4 is Exhibit 3 here, that the data is so bad that the
5 studies aren't even worth being done on that data?

6 A For aggravated assault, yes; and to the extent
7 that aggravated assault constitutes most of the violent
8 crime rate as he refers to it, yes. I think there is
9 little to be gained by analyzing that crime.

10 Q Have you ever said words to the effect that, I
11 do not believe that truth is determined by majority vote.
12 It is not the most popular conclusion that is most likely
13 to be correct. It is the one supported by the
14 methodologically strongest research, no matter how
15 numerous or rare the technically stronger studies may be?

16 A Yes.

17 Q Is that a view that you hold today?

18 A Yes.

19 Q Is that a view that you've held for a long time,
20 let's say at least ten years?

21 A Yes.

22 Q Okay. Have you ever said words to the effect
23 of, Do I know of anybody who specifically believes with
24 more guns there are less crimes and they're a credible
25 criminologist? No.

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1 (Exhibit 6 was marked for identification by .
2 the Certified Shorthand Reporter and is attached
3 hereto.)

4 BY MR. EISENBERG:

5 Q Wow. Your stack is all neat. My stack is a
6 mess. Have you ever seen this report before?

7 A Yes.

8 Q When you've referred to studies on this general
9 question of the effect of right-to-carry laws and said
10 that a Kovandzic study was the best available or words to
11 that effect, were you referring to this study?

12 A I do think this is the best available study,
13 yes.

14 Q Now, what makes this study the best available
15 study on this topic?

16 A It relies on city-level data and thus is not
17 subject to the problem of crime counts being aggregated
18 up inappropriately. The crime counts are based on what a
19 single agency counted up; that is, the local city police.

20 Q And do you think that source of data is more
21 accurate than the state-level data?

22 A Almost certainly, because even with the
23 state-level data, there's a problem with aggregating up
24 to the state level because even the FBI has to estimate
25 the effect of missing or nonreporting agencies and it is

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1 adjustment for missing nonreporting agencies.

2 Q And that adjustment's done by the FBI?

3 A Yes.

4 Q And so the FBI-adjusted data that you're
5 referring to, is it less accurate than city-level data as
6 used in the Kovandzic report here?

7 A It's necessarily subject to additional sources
8 of error because it's -- there's error in the estimation
9 process.

10 Q Are there any other -- strike that.

11 Is the city-level data more reliable, therefore,
12 than the FBI-adjusted state data?

13 A Yes.

14 Q So is it your understanding that the Kovandzic
15 report supports your belief that there is no net effect
16 on crime rates from the passage of right-to-carry laws?

17 A Yes.

18 Q And is this --

19 A Although if you want to quibble about the
20 distinction between "no" and "little," I guess, you know,
21 the way they phrase it is that there's little support for
22 that proposition.

23 Q What's the difference between "little" and "no"
24 there in a --

25 A They're being careful when they say "little"

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1 because there is hundreds of ways you can analyze a body
2 of data and some of the ways you analyze it might be
3 better than others, although you don't always know for
4 sure which way those are and they can yield different
5 results not necessarily because, you know, you're
6 studying different phenomena, but rather you're studying
7 it in different ways, some of which are better than
8 others, and so you get a variation in results, and
9 Kovandzic, Marvell, and Vieraitis are careful researchers
10 in that they at least concede that some ways of
11 impacting -- the right-to-carry laws indicate some
12 impact.

13 Q Is that true for any specific categories of
14 crimes?

15 A Is what true? That findings can vary because of
16 different methods being used?

17 Q Let me -- let me strike the question and
18 rephrase it.

19 Are you aware that the Kovandzic study found
20 that there's a statistically significant correlation
21 between I think what they called shall-issue laws and
22 aggregated assault?

23 MR. BRADY: Objection; assumes facts not in evidence,
24 calls for speculation.

25 THE WITNESS: I'm aware that some of their results

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1 indicated that and others of their results did not and
2 that's why they're being cautious and using that phrase,
3 "There's little evidence of" blah, blah, blah. You know,
4 it's a reflection of the fact that there are varying
5 results even within their study.

6 And by the way, even for individual crime types,
7 even when you're not comparing one crime type to another,
8 even within one crime type, they would get a variety of
9 findings.

10 BY MR. EISENBERG:

11 Q And if I could have you turn to page 307 of the
12 report, please, I'm going to ask you if you agree with
13 one of the statements there. I'm going to read the
14 sentence that's the last one on page 307 and it follows
15 to page 309 because there's a chart on page 308, and I'm
16 going to leave out the stuff in parentheses and then I'm
17 going to ask you if you agree with the statement:

18 "The results in table one provide no
19 support for Lott and Mustard's and Lott's
20 thesis that the longer SI laws are in
21 place" -- and I think they mean shall-issue
22 laws -- "the greater their deterrent effect
23 on violent crime."

24 Do you agree with that statement?

25 A Yes.

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1 Q So the next statement says that:

2 "The coefficient on the aggregate SI
3 law time-trend variable is in the unexpected
4 positive direction for each of the four
5 violent crime regressions and is significant
6 in the positive direction for aggravated
7 assault."

8 Do you see that?

9 A I do.

10 Q Do you understand that that statement about the
11 significance at the statistically significant level for
12 aggravated assault is contradicted by other data that's
13 reported in the same study?

14 A Could you repeat the question?

15 Q Okay. So let me rephrase. Earlier, I believe
16 you said that the Kovandzic study gets some results that
17 are statistically significant for aggravated assault but
18 other results where there's no statistical significance
19 for aggravated assault; correct?

20 A Correct.

21 Q So where in the study is the -- are the results
22 that show no statistically significant relationship
23 between shall-issue laws and aggravated assault?

24 A Well, the summary of all of the findings is in
25 Table 3. So that's a whole bunch of different estimates

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1 BY MR. EISENBERG:

2 Q So do you remember the question?

3 A Yes.

4 Q Okay. Are you able to answer?

5 A I have no position on it. I don't think there's
6 any relevant empirical evidence on it, so I have no
7 direct basis for an opinion.

8 Q Well, have you ever stated or written that if a
9 jurisdiction goes from no public carrying of firearms to
10 public carrying of firearms, you get a big increase in
11 the number of people trying to get permits -- sorry. Let
12 me strike the question.

13 If you go from a regime that's so-called "may
14 issue" to "shall issue" for public carrying permits -- do
15 you understand what I'm talking about --

16 A Yes.

17 Q -- by the terms? Okay. So let's -- have you
18 ever written something to the effect that going from a
19 regime that has may issue for public carry permits to
20 shall issue for public carry permits results in a large
21 number of people applying for permits?

22 A Yes. I think that does happen.

23 Q And isn't -- well, wouldn't it be the law
24 enforcement authorities that process those applications?

25 A It would.

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1 Q Wouldn't that work constitute a drain on police
2 resources?

3 MR. BRADY: Objection; argumentative, calls for
4 speculation, beyond the scope of what the expert was
5 called to testify about, ambiguous as to which regime
6 you're talking about.

7 THE WITNESS: Yes. To be specific, the fact that
8 they had to process a lot of carry permit applications
9 and, in particular, the fingerprinting process, which is
10 usually a part of that application process, would take up
11 police time and would cut into some kind of other
12 activities in which the police engage and I think the
13 most likely one would be recordkeeping. It would be, you
14 know -- it would result in many agencies deciding, well,
15 we're going to have to give up some kind of activity in
16 order to be able to have officers doing all these
17 fingerprinting activities and the likeliest thing they'd
18 give up -- it wouldn't be something crucial in the way of
19 crime control. It would be in the way of the kind of
20 paperwork they don't have much respect for anyway.

21 BY MR. EISENBERG:

22 Q Do you have empirical evidence to support the
23 idea that the work that would be allowed to slip would be
24 the recordkeeping?

25 A No.

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1 Q Why are you positing that as the thing that
2 would slip then?

3 A I'm merely saying it's a low-priority activity
4 for police.

5 Q How do you know it's a low-priority activity?

6 A It would be common knowledge that I don't think
7 any police officer would dispute. It's just widely
8 believed and known that they dislike paperwork.

9 Q How do you -- I mean, when you say it's widely
10 believed, is that based on surveys that you've either
11 conducted or reviewed?

12 A Not based on surveys I've conducted, but it's
13 certainly based on what people -- scholars have learned
14 from talking to police officers, whether in the context
15 of surveys or not.

16 Q Has it -- there been an attempt to quantify the
17 evidence on that question?

18 A Not to my knowledge.

19 Q Couldn't you say then that what the scholars
20 have done is just gathered anecdotes on that topic?

21 MR. BRADY: Objection; argumentative.

22 THE WITNESS: I guess I really don't understand the
23 intent of the question. I mean, it's information
24 gathered by asking police officers what they think and
25 police officers telling them what they think. It's --

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1 used to generate that result?

2 A I haven't addressed it because it was just a
3 side issue.

4 Q Well, doesn't this chart suggest that when
5 right-to-carry laws are enacted, there's a lot more --
6 there are a -- there is a statistically significant
7 increase in the number of police who are employed and
8 that statistical significance is to the 99th percent?

9 MR. BRADY: Objection; argumentative, assumes facts
10 not in evidence.

11 THE WITNESS: Well, that's what his results are
12 indicating. But, you know, I would have passed over this
13 as basically irrelevant to the main issue of how do
14 right-to-carry laws affect crime rates? Since the police
15 employment or police officers per 100,000 rate doesn't
16 affect crime rates, it doesn't matter one bit whether you
17 control for them or not and it doesn't matter whether or
18 not right-to-carry laws increase those police strength
19 units because they, in turn, will not have any net effect
20 on crime rates.

21 BY MR. EISENBERG:

22 Q If you believed otherwise about the effect of
23 police strength on crime rates, wouldn't your answer have
24 to be different?

25 A Are you saying --

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1 Q So as a scholar, you would think if I am doing
2 an analysis of the effect of what we'll call either shall
3 issue or right-to-carry laws on violent crimes in a city,
4 you would come up with one set of control variables, but
5 it wouldn't necessarily be the same as the control
6 variables that you would come up with if you were doing a
7 study of the effect of shall issue or right-to-carry laws
8 on violent crime rates statewide?

9 A That's correct.

10 Q Okay. Are you aware of any studies that have
11 attempted to show the relationship between open-carry
12 laws and violent crime rates?

13 A No.

14 MR. BRADY: John, before you proceed, I'm sorry. I
15 don't mean to interrupt you. It's about to be 1:00. I
16 just wanted to check with you, do you anticipate us
17 having a break for lunch or are you going to power
18 through?

19 MR. EISENBERG: I can go either way.

20 MR. BRADY: Well, what do you -- maybe we should
21 ask --

22 MR. EISENBERG: Yeah. Do you want to go off the
23 record and talk about lunch? Do you want to do this on
24 the record? I don't care.

25 MR. BRADY: Let's go off the record.

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1 THE WITNESS: "Robustness" means more than you just
2 get the same results because you did it a number of
3 different ways. There's an assumption that you're doing
4 it in multiple superior or arguably superior ways,
5 methodologically sound ways, but if you do it in eight
6 different incompetent ways and you get the same result,
7 it's possible you're getting the same screwed-up result
8 because of the same deficiencies in the methods. And in
9 this case, you know, Donohue tries out four different
10 sets of predictors, all of which are terrible selections
11 of predictors. I mean, he's -- they're very poor sets of
12 confound -- allegedly confounding variables, and the same
13 is true of the other three models in addition to the DAW
14 model that they prefer. I mean, that's what we were
15 talking about before --

16 BY MR. EISENBERG:

17 Q Right.

18 A -- that the DAW model only has two variables
19 that might be confounders and we don't even know that
20 they are confounders and it's basically just as bad for
21 the other three models they use. So they're all
22 inadequate. They're all failing to control for other
23 factors that affect crime. They're just failing to
24 control for slightly different sets of variables.

25 Q So John Lott is the -- one of the two authors of

1 the Lott and Mustard model and all of your criticisms
2 apply to his and his coscholar's choice of control
3 variables as well?

4 A Yes.

5 Q And then the other panel is Marvell and Moody?

6 A Uh-huh.

7 Q The other panel data analysis from the Marvell
8 and Moody model?

9 A Right.

10 Q And you consider the control variables selected
11 to that -- for that model to be poor?

12 A Yes. All of them are poor, and there's only
13 slight differences in terms of how poor they are.

14 Q The discussion so far has been on the panel data
15 analysis, but there's another approach used in the study
16 that's called Synthetic Controls.

17 A Uh-huh.

18 Q What's your understanding of what the synthetic
19 control tool is in statistical analysis?

20 A Well, it used to be in the old days that if you
21 wanted to know if passing a right-to-carry law increases
22 or decreases crime, you might compare one state that had
23 it with one other state that didn't enact a new
24 right-to-carry law, but everyone understood that was kind
25 of limited because there's lots of different control

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1 you have no basis for picking out which states are most
2 similar to the treatment state.

3 Q But if the trend lines are similar between the
4 control state and the state that you're studying, isn't
5 that all you need?

6 A Nope, not at all. That's a very weak foundation
7 for producing the result that you're trying to produce,
8 which is getting at what the crime rates in the
9 right-to-carry states would have been if they didn't pass
10 the right-to-carry law, the so-called counter-factual
11 situation.

12 Well, if you're not producing a set of control
13 states that have similar trends with regard to whatever
14 would have produced those different levels of post-law
15 crime rate, then it'll be sheer luck if they happen to be
16 good control variables for approximating what trends
17 would have occurred in crime in the right-to-carry states
18 if they had not passed the right-to-carry laws.

19 Q Do you consider synthetic controls analysis, if
20 properly implemented, to be a sound statistical
21 methodology?

22 A Yes.

23 Q Have you written a paper in which you used
24 synthetic controls analysis as part of your analysis?

25 A No, not exactly. Once upon a time years ago, I

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1 THE WITNESS: I wouldn't have the slightest idea.

2 BY MR. EISENBERG:

3 Q So you don't know that his notion of NRA-backed
4 secrecy laws is only laws affecting police and private
5 investigators, as opposed to the general public?

6 A I didn't really understand that to be his
7 position. I -- I think he was vague as to exactly why it
8 was that NRA-backed secrecy laws would somehow prevent us
9 from discovering the misconduct of carry permit holders.
10 I don't recall him being that specific as to why that
11 worked out that way. In fact, it kind of stood out how
12 vague and unspecific he was, because he didn't explicitly
13 say, "Well, I think NRA-banned secrecy laws prevent
14 police from discovering the fact that a permit -- an
15 arrestee had a carry permit."

16 Q All right. So you criticize the gun -- I'm
17 sorry -- the concealedcarrykiller's.com website for
18 including reports about people who commit suicide as
19 related to those people's permits; correct?

20 A Correct.

21 Q And you also criticize in the same way a report
22 that would be about a premeditated murder; correct?

23 A Correct.

24 Q What's -- what is it about a premeditated murder
25 as opposed to a regular murder that would be less

1 relevance to the question of the effect of the concealed
2 carry permit?

3 A The relevance is that a person who is intending
4 to commit a murder is therefore by implication intending
5 to do something that places them at risk of, depending on
6 what state you're talking about, suffering either the
7 death penalty or a very long prison sentence and,
8 therefore, that's a person who doesn't care about whether
9 they might be arrested and charged with the relatively
10 minor offense of carrying without a permit.

11 So having a permit or not having a permit on the
12 way to committing a premeditated murder is irrelevant,
13 whereas if it were an unpremeditated murder, it's sort of
14 a murder that arises out of circumstances you hadn't
15 anticipated, like you got into a, you know, traffic
16 dispute or an argument in a bar. Then you may not have
17 wanted to carry without a permit because that's a
18 relatively serious offense with a relatively serious
19 penalty compared to nothing at all, but it's trivial
20 compared to the penalty a premeditated murderer
21 anticipates risking by committing a planned murder.

22 Q All of the testimony that you gave there, it's
23 really theorizing. It's not backed up by empirical
24 evidence, correct?

25 MR. BRADY: Objection; argumentative.

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1 THE WITNESS: Well, it's -- it's theory that's been
2 widely agreed upon by scholars. I don't know of anybody
3 who would dispute the fact that people who commit
4 premeditated murders are more likely to be considering
5 the consequences of that action, including lethal
6 penalties, versus those who didn't intend or plan to
7 commit a murder. It's a common topic of discussion in
8 connection with the effect of the death penalty and
9 nobody's ever disputed that premise. Scholars of
10 homicide and the capital punishment debate in particular
11 have never quibbled over that and I certainly have no
12 reason to do it.

13 So, you know, there's a great deal of what is
14 widely accepted consensus knowledge within the field that
15 doesn't -- isn't thought to require any empirical
16 confirmation. Nobody goes out and does research to
17 confirm that two and two equals four. It's widely
18 accepted.

19 BY MR. EISENBERG:

20 Q Right, but there you're talking about the death
21 penalty; right? You're talking about permits to carry
22 weapons.

23 A Precisely.

24 Q So you're saying that the scholarly consensus on
25 the effect of the premeditation element of the murder is

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1 firearm and thereby inspire the person to go ahead and
2 get a firearm?

3 MR. BRADY: Objection; incomplete hypothetical and
4 calls for speculation.

5 THE WITNESS: I would regard it as possible but
6 unlikely.

7 BY MR. EISENBERG:

8 Q Why do you say "possible but unlikely"?

9 A Because those who own guns think there's ample
10 utility without the ability to legally carry them, either
11 because they don't care about carrying them in public or
12 they do and they're willing to do it illegally without
13 benefit of the permit.

14 Q You've written before that you believe that a
15 lot of illegal public carrying of firearms goes on
16 compared to the number of people who are legally carrying
17 because they're permitted. Is that a fair general
18 summary of work that you've done before?

19 A It is.

20 Q Have you studied the question of what happens to
21 the people who had been illegally carrying firearms in
22 public if their jurisdiction suddenly makes it legal?

23 A Not exactly, but there's indirectly relevant
24 evidence about what happens when, for whatever reason,
25 people get a permit.

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1 Q Right.

2 A And one reason of course would be, Well, now the
3 state has made permits more widely available, more easily
4 available. And the answer is it doesn't seem to make any
5 difference in their frequency of carrying rate. Permit
6 holders who are asked retrospectively about their
7 frequency of carrying before they got the permit, so it's
8 a permit versus no-permit comparison, basically say it
9 didn't change, it didn't affect them. That is to say,
10 they were carrying just as often before they got the
11 permit as they did later; and of the minority who said
12 that they had changed, there was no significant
13 difference between the percent who said they went up in
14 frequency when they got the permit and the percent who
15 said they went down in frequency when they got the
16 permit. So the implication is the aggregate net effect
17 of getting permits on frequency of carrying is zero. It
18 just canceled out.

19 Q So are you suggesting that people who have --
20 some significant fraction of people who have lawful --
21 who lawfully have permits to carry used to be carrying
22 illegally?

23 MR. BRADY: Objection; calls for speculation.

24 BY MR. EISENBERG:

25 Q It sounds like what you're saying. Is that what

MICHELLE FLANAGAN vs CALIFORNIA ATTORNEY GENERAL XAVIER BECERRA Kleck, Gary

1 you're saying?

2 A Yes.

3 Q Do you know what the fraction is?

4 MR. BRADY: Same objection; calls for speculation.

5 THE WITNESS: When we did our national self-defense

6 survey, among other things, we asked people, about

7 whether they carry guns for self-protection outside the

8 home and we didn't ask them about whether they had a

9 permit. We figured it was fruitless, but there were

10 national estimates of how many people did have permits.

11 So we could take our estimate of what fraction of the

12 U.S. adult population was carrying with or without a

13 permit and then compare that to the number of known

14 permits that are out there, based on the information

15 provided by state governments for the most part, and it

16 works out to maybe on the order of one sixth of all of

17 the permit holders -- all the carriers had a permit at

18 that time. And we're talking like '93 or '94, around

19 there.

20 BY MR. EISENBERG:

21 Q Isn't, you know, an obvious implication of that

22 study that there are lots and lots of permit holders who,

23 in fact, were not law abiding in the past?

24 MR. BRADY: Objection; misstates testimony,

25 argumentative.

1 THE WITNESS: Everybody is not law abiding.

2 MR. BRADY: Calls for a legal question.

3 THE WITNESS: Everybody is not law abiding. The
4 fraction of the population who violates the criminal law
5 is 100 percent as far as -- I mean, virtually 100 percent
6 admit to it. They'll freely confess to one of a number
7 of criminal acts and that's like an ancient fact known to
8 criminologists for a good 70 years or so.

9 So if you're asking do permit holders also have
10 this attribute of having violated criminal laws, the
11 answer is yes, like everybody else. But if you're asking
12 do they do it any more than people who are permit
13 holders, then the answer is no.

14 BY MR. EISENBERG:

15 Q Is there a term in criminology, "law-abiding
16 person"?

17 A Yeah.

18 Q What does it mean if you say everybody violates
19 the criminal laws?

20 A In practice, they really mean relatively law
21 abiding. There's no such thing as absolute law abiding.
22 It's just shorthand for relatively law abiding.

23 Q So what makes somebody relatively law abiding --

24 A They're doing --

25 Q Let me finish -- as opposed to not relatively

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1 exhibit next in order, it's a paper called "Policy
2 Lessons from Recent Gun Control Research."

3 THE REPORTER: Exhibit 7.

4 (Exhibit 7 was marked for identification by
5 the Certified Shorthand Reporter and is attached
6 hereto.)

7 BY MR. EISENBERG:

8 Q This appears to be one of your scholarly papers.
9 Is that, in fact, what it is?

10 A Yes.

11 Q And it was -- it has a copyright date of 1986.
12 Is that an accurate date for the publication of this
13 paper?

14 A Yes.

15 Q And was it, in fact, published in a journal
16 called Law and Contemporary Problems?

17 A Yes.

18 Q Is that a law journal?

19 A It's a law and social science journal.

20 Q Is it affiliated with a particular university?

21 A I think it's Duke.

22 Q So if I could turn your attention to page 39 of
23 the paper, the last sentence that starts on that page not
24 including the footnotes says:

25 "Evidence is consistent with the idea

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1 that guns facilitate some assaults and thus
2 gun availability could conceivably increase
3 assault frequency."

4 Do you see that sentence?

5 A Yes.

6 Q You wrote those words?

7 A Yes.

8 Q And at the time you wrote them, did you believe
9 them to be true?

10 A Yes.

11 Q As we sit here today, do you believe that
12 statement to be true?

13 A Yes.

14 Q What evidence are you speaking of in that
15 sentence?

16 A The best evidence pertains to robbery and the
17 notion that, you know, a robbery-linked assault would be
18 facilitated. It is supported by the fact that the more
19 of the intimidating or powerful reality applies to the
20 victim, the more -- the stronger they are, the bigger
21 they are, the fact that they're male rather than female,
22 the fact that they're in the vital ages of relatively
23 young adulthood rather than being very young or very old,
24 basically the more powerful the victim is, the more
25 likely it is that the aggressor was using a gun. And in

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1 A That was outside the scope of the statement,
2 although if you consider a robbery a property crime in
3 addition to a violent crime, then yeah, it encompassed
4 robberies. We actually know more about gun involvement
5 in robberies than any other crime other than murder.

6 Q If I could move you to page 59, please, we've
7 got the section that's Roman Numeral IX, "Summary of the
8 Policy Lessons." In the third enumerated point, you
9 reference the varying need for gun control among the
10 states. By that statement, did you mean that different
11 states may have more -- you know, optimal levels of gun
12 control that are different from each other, other states?

13 A Yes.

14 Q Is that something that you believed when you
15 wrote this article?

16 A Yes.

17 Q Is that something that you believe still today?

18 A Yes.

19 Q What's the basis for believing that there are
20 different needs for gun control among different states?

21 A Crime statistics that indicate the rate of gun
22 crime is higher in some states than others and data based
23 on surveys for the most part, but also to a lesser extent
24 suicide data that indicates levels of gun ownership
25 widely vary among states. So in short, some states have

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1 a problem with gun violence and some don't.

2 Q And the way to address the varying rates of gun
3 violence is to have more or less gun control?

4 MR. BRADY: Objection; misstates testimony, calls for
5 speculation, beyond the scope of what the expert's called
6 to testify about.

7 THE WITNESS: Well, taken in context with the other
8 things I say, you don't -- you're trying to implement the
9 least restrictive policies that will least impair
10 legitimate uses of firearms in order to advance the
11 legitimate public interest in reducing gun violence.

12 Well, if you don't have any gun violence to speak of in
13 your state, then there isn't much justification for
14 imposing those restrictions which might limit legitimate
15 uses of firearms, whether it's something relatively, you
16 know, minor like hunting and target shooting or something
17 gravely serious like armed self-defense.

18 BY MR. EISENBERG:

19 Q Do you believe that certain crimes are
20 correlated with family economic conditions?

21 A Yes.

22 Q And what I meant there was by crime rates for
23 particular crimes. It's correlated with family economic
24 conditions. Does that change your answer?

25 A I don't see the distinction.

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1 not an issue.

2 BY MR. EISENBERG:

3 Q Is response bias a problem in most surveys?

4 A There's always some response bias.

5 Q Do you believe it's true that criminals often
6 steal guns?

7 A Yes.

8 Q Do you believe that the theft of guns by --
9 well, the theft of guns is not a premeditated act and
10 it's more an opportunistic act?

11 MR. BRADY: Objection; calls for speculation, beyond
12 the scope of what the expert's assignment was.

13 THE WITNESS: Yes.

14 BY MR. EISENBERG:

15 Q Have you looked at the question of whether the
16 prevalence of guns in public places leads to increases in
17 gun theft?

18 A No.

19 Q Do you have any opinion on that topic?

20 MR. BRADY: Objection; calls for speculation. It is
21 beyond the scope of what the expert was called to testify
22 about.

23 THE WITNESS: Could you repeat the question, please?

24 BY MR. EISENBERG:

25 Q Do you have any professional opinion about what

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1 "Although a minority of the laws," is that a fair summary
2 of the findings of the paper?

3 A Yes.

4 Q Is it fair to say that a lot of the studies that
5 you have done looking at whether there is a relationship,
6 statistically significant relationship, between firearms
7 and crime shows that you really can't answer the question
8 one way or another?

9 A I would say I couldn't agree to that statement
10 as phrased because it implies I'm not willing to draw a
11 conclusion or I didn't think the evidence was good enough
12 to merit a conclusion, and I disagree with that. I don't
13 think that's the way I think of it at all. Rather, at
14 any one point, I draw conclusions on the basis of what I
15 think is the technically best evidence, the evidence that
16 most closely follows the rules of how to do correct
17 scientific research as laid down by research methodology
18 textbooks, and so the evidence is never perfect.

19 I'm basing the conclusions -- the best available
20 evidence is not perfect evidence and so what I'm saying
21 here is the best available evidence indicates that some
22 gun laws seem to make things better and some seem to make
23 things worse, so the results are consistent with both
24 kinds of effects. And of course we can all think of
25 reasons why they might have both good and bad effects,

1 both intended and unintended effects.

2 Do I believe these absolutely and will never,

3 ever change if better evidence comes along? No,

4 absolutely not. So my conclusions are always tentative

5 based on what I consider to be the best available

6 evidence.

7 Q Right. Right. Yeah, I think you may have
8 thought that I was implying something negative and I
9 really wasn't. I just want to make that clear. I wasn't
10 implying that you are afraid to draw conclusions. I was
11 just saying that it seems that the results of many of
12 your studies show that there is some positive effects,
13 some negative effects, and the result is that there is
14 not -- that the net often just nets out to something I
15 wouldn't say meaningless but something close to zero. Is
16 that fair? Is that a fair summary of some -- of a lot of
17 your papers?

18 A Yes. I consider it a nuanced view. I mean,
19 it's an even-handed view which considers the possibility
20 of both good and bad effects of guns being out there.

21 Q So the Lott studies assert a more affirmative
22 relationship between the factors of public carrying of
23 firearms and crime rates than you have found to be
24 warranted; correct?

25 A Correct.

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1 engages that literature in that sense, but that's not
2 what I was objecting to. That's a separate point.

3 BY MR. EISENBERG:

4 Q Okay. Is it meaningful for a scholar in your
5 field what is in the bibliography of a published study?

6 A Oh, you expect whatever was discussed in the
7 text and cited in the text has a corresponding listing in
8 the bibliography and when it's published in a journal,
9 usually there's a copy editor who will tell you either,
10 A, you cited something in the text, but you don't have it
11 in the bibliography or you listed something in the
12 bibliography that you never cited in the text and that's
13 generally considered to be a no-no.

14 Q For a scholar in your field, does the placement
15 or the reference to some other study in the bibliography
16 mean that you are endorsing the -- that study as valid?

17 A No.

18 Q It just means that you may have reference to the
19 study?

20 A Yes.

21 Q So if -- well, I'll move away from the
22 hypothetical. If you look at the Donohue study, which is
23 the one that's Exhibit 3, Exhibit B, he makes reference
24 to some work by a scholar named Zimmerman. Is Zimmerman
25 a professor or a scholar that you know at least by

Corrections Need to Transcript of Deposition of Gary Kleck in *Flanagan v. Becerra*

All of the following corrections were made because of typographical errors.

<u>Page</u>	<u>Line</u>	<u>Correction Needed</u>
32	1	in Public → and Public
32	9	leadership → readership
35	2	competition → composition
37	14	opposite side → opposite sign
42	2	criteria of → criterion
74	14	key efficiency → coefficient
81	19	deemed → obtained
100	15	variability → variable
120	21	one → other
123	3	trends and → trends in
123	18	Francis Bacon → Taylor and Francis
124	13	possibility → possible
127	23	and → in
134	22	percent for urban → percent urban
137	22	muliplanarity → multicollinearity
141	15	will → we'll
141	24	and → in
151	2	goes → makes
155	2	caused → committed
155	20	irrelevant → is irrelevant
168	3	them → whom
177	12	who are → who are not
178	14	articulate → articulation

185 2 have → have no
187 7 generalized ability → generalizability
192 21 survey → surveys
196 25 in context → in a context
236 3 sampling → sample

MICHELLE FLANAGAN vs CALIFORNIA ATTORNEY GENERAL XAVIER BECERRA Kleck, Gary

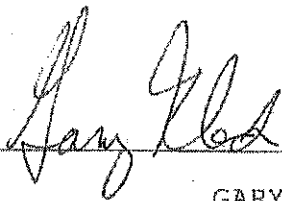
1 STATE OF CALIFORNIA

2
3 COUNTY OF LOS ANGELES
4

5 I am the witness in the foregoing deposition.
6 I have read the deposition. Having made changes and
7 corrections as I desire, I certify that the same is true
8 of my own knowledge, except as to those matters which are
9 therein stated upon my information or belief, and as to
10 those matters, I believe it to be true.

11 I declare under penalty of perjury under the laws of
12 the State of California that the foregoing is true and
13 correct.
14
15

16 Executed on September 4, 2017
17 at 489 Teenie Court, Tallahassee, FL 32312 (Address)
18
19
20

21
22 
23

GARY KLECK

The Impact of "Shall-Issue" Concealed Handgun Laws on Violent Crime Rates

Evidence From Panel Data for Large Urban Cities

TOMISLAV V. KOVANDZIC

University of Alabama at Birmingham

THOMAS B. MARVELL

Justec Research

LYNNE M. VIERAITIS

University of Alabama at Birmingham

What happens when states ease access to permits to carry concealed handguns in public places? Supporters maintain the laws can reduce violent crime rates by raising the expected costs of crime, because of criminals anticipating greater risks of injury and lower rates of success completing their crimes. Opponents argue that the laws are likely to increase violent crime, especially homicide, as heated disputes involving permit holders are more likely to turn deadly because of the greater lethality of firearms. This study uses panel data for all U.S. cities with a 1990 population of at least 100,000 for 1980 to 2000 to examine the impact of "shall-issue" concealed handgun laws on violent crime rates. The authors measure the effects of the laws using a time-trend variable for the number of years after the law has been in effect, as opposed to the dummy variable approach used in prior research. They also address many of the methodological problems encountered in previous studies. The results provide no evidence that the laws reduce or increase rates of violent crime.

Keywords: gun control; right-to-carry laws; homicide; violent crime; concealed-carry laws; handguns

By 2001, at least 33 states had adopted "right-to-carry" or "shall-issue" concealed firearms laws (SI laws), which require authorities to issue concealed handgun permits to adult residents meeting specified objective criteria (U.S. Bureau of Justice Statistics, 2001, pp. 94-95). The laws replaced earlier locally administered, highly discretionary, "may issue" carry permit laws in which

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Deposition of Gary Klock
Date 7/25/17
Exhibit 10
For Identification
Marcena Munquia, CSR 10420
Eisenberg Decl. Ex. 3 -047

local authorities could issue a carry license but were not required by law to do so. Supporters of SI laws maintain that allowing citizens to carry guns legally reduces crime, especially those committed in public places such as robbery, because prospective criminals fear encountering armed victims (Lott, 1998a, 1998b, 2000; Lott & Mustard, 1997). This position is based on theories of economic choice which posit that "a person commits an offense if the expected utility to him exceeds the utility he can get by using his time and other resources at other activities" (Becker, 1968). Specifically, proponents argue that SI laws can reduce levels of violence by deterring prospective criminals from even attempting crimes, presumably because would-be criminals perceive an increased risk of injury to themselves and a reduction in the rate of success in completing crimes (Lott, 1998a, 1998b, 2000; Lott & Mustard, 1997).

SI laws, however, do not automatically increase criminals' fear that victims might be armed. They might not know about the law. The actual increase in self-protection gun carrying might be, or might be perceived as, slight in comparison with normal rates of self-protection gun carrying, most of which is probably done in violation of concealed weapons carrying laws (Kleck, 1997; Kovandzic & Marvell, 2003). And some newly licensed gun carriers probably carried illegally before the new laws (Kleck, 1997; Kovandzic & Marvell, 2003; Lott, 1998; Ludwig, 1998).

Opponents of SI laws argue that "threatening situations" (when someone is attacked or fears an attack) are more likely to turn fatal when more people carry guns (Cook, 1991; Ludwig, 1998; McDowall, Loftin, & Wiersema, 1995b; Webster, Vernick, Ludwig, & Lester, 1997; Zimring, 1968).¹ Other critics speculate that higher levels of self-protection gun carrying by permit holders might prompt criminals to carry guns more often (Ayres & Donohue, 2003a; Cook, 1991; Green, 1987; Ludwig, 1998; McDowall et al., 1995a; but see Kleck, 1997, pp. 204-205).

The present study examines the impact of SI laws on the four major forms of violent crime, using panel data from 1980 to 2000 for U.S. cities with a 1990 population of 100,000 or more. In the next section of the article, we examine the extensive prior research on SI laws and suggest procedures to mitigate methodological problems encountered there. We then describe our data and methods and present our results. In the final section, we consider the theoretical and policy implications of our findings.

PREVIOUS RESEARCH²

The first evaluation of SI laws was Kleck and Patterson (1993), using cross-sectional data for 170 U.S. cities with a population greater than 100,000 in 1980. They separately assessed the effects of 19 different types of state and city gun controls, including those SI laws passed before the post-1986 wave of SI laws on rates of homicide, robbery, assault, rape, suicide, and fatal gun accidents, as well as the impact on gun ownership levels. The authors found no evidence that cities in states with SI laws have lower or higher rates of violence compared to cities in states without SI laws. There was also no evidence of higher rates of gun ownership in cities that reside in SI states, undercutting the idea by many that SI laws might lead to increases in gun ownership levels (Ayres & Donohue, 2003a; Lott, 2000). Because few SI laws existed in 1980, however, this evaluation is incomplete.

The next study (McDowall et al., 1995a) used ARIMA time-series analyses with monthly homicide mortality data (during 1973 to 1992) from five counties in Mississippi, Oregon, and Florida. They found positive, and usually significant, impacts on gun homicides, whereas the impacts on nongun homicide were mixed. The authors concluded that, at the least, there was no evidence that SI laws reduce homicide. Several have criticized this study for failing to justify the selection of the five counties (Kleck 1997; Polsby, 1995). In response to Polsby's (1995) criticism that deterrence theory suggests that nongun homicides are also likely to be reduced by more gun carrying, McDowall et al. (1995a) examined annual total homicide data for all of Florida and found an overall decline following the passage of Florida's SI law (see second panel of their Table 2).

The most publicized and controversial study of SI legislation is by Lott and Mustard (1997) in the *Journal of Legal Studies* and subsequent follow-ups to that work, especially two books by Lott titled *More Guns, Less Crime* (Lott, 1998b, 2000). The initial study by Lott and Mustard (1997) evaluated SI laws in 10 states using county panel data for 1977 to 1992. The SI laws were entered as before-after dummy variables scored 1 starting the year after a law went into effect and 0 otherwise. Control variables included age structure, economic trends, and arrest rates. They conducted numerous alternate analyses, such as with differenced variables,

with individual state trends, and with laws represented by linear and nonlinear trends and permits issued in a single year. In general, they concluded that SI laws reduce violent crime, including homicide, by some 4% to 7%, but increased property crimes. Follow-up studies by Lott (1998a, 1998b, 2000), which added later years of data and new SI laws, largely confirmed the negative correlations between enactment of SI laws and violent crimes observed in the original Lott and Mustard (1997) study.

Given the obvious policy implications of Lott and Mustard's findings for the regulation of concealed gun carrying in public places, numerous academics have reanalyzed the Lott and Mustard data, at least 15 by our count. Of these 15 studies, 8 of them found SI laws to be significantly and negatively correlated with violent crime in at least half of the model specifications presented (Benson & Mast, 2001; Bronars & Lott, 1998; Donohue, 2003; Duggan, 2001; Marvell, 1999; Moody, 2001; Olson & Maltz, 2001; Plassmann & Tideman, 2001; Plassmann & Whitley, 2003). Five studies generally found nonexistent effects of SI laws on violent crime rates (Black & Nagin, 1998; Dezhbakhsh & Rubin, 1998; Harrison, Kennison, & Macedon, 2000; Marvell, 2001), whereas the remaining three studies generally found SI laws in more than half of all model specifications presented to be, if anything, positively related to violent crime rates (Ayres & Donohue, 2003a, 2003b; Ludwig, 1998).

Especially important is Black and Nagin (1998), who relaxed the assumption of uniform effects in the Lott and Mustard (1997) model by entering separate dummy variables for each state SI law. With respect to homicide and rape, the number of negative coefficients, significant and nonsignificant, only slightly outnumbered their positive counterparts. Florida's large negative coefficients stood out, and without Florida the apparent impact of the laws when using an aggregate law dummy disappeared for murder and rape.

Another reanalysis of Lott and Mustard's (1997) data was conducted by Ludwig (1998). Ludwig suggests Lott and Mustard's results may be attributed to omitted variable bias because the fixed-effects approach cannot control for unobserved factors (e.g., crack markets, gang activity, poverty) that influence county crime rates but are not fixed across time. Ludwig argues that these factors may have influenced SI and non-SI states differently,

resulting in spurious or partially spurious findings for the SI law variable. To address the problem of omitted variable bias, Ludwig uses the difference-in-difference-in-difference (DDD) estimator, which takes advantage of the fact that juveniles cannot obtain carry permits because of minimum age requirements. Ludwig argues juveniles serve as a natural control group for estimating the impact of SI laws on adult homicide victimization rates (i.e., the treatment group). According to Ludwig, the difference between the change in the adult and juvenile homicide victimization rate eliminates the effects of both fixed and time-varying factors that cause both homicide series to vary across time and isolates those factors that impact the difference between adult and juvenile homicide victimizations. Ludwig also accounts for the possibility that nationwide factors may have influenced changes in adult and juvenile homicide victimization rates differently by comparing differences in the adult-juvenile trends in SI states with the difference in adult-juvenile homicide rates in non-SI states. As a result, the DDD estimator is able to isolate those factors that are unique to states passing SI laws that will cause adult homicide rates to increase or decrease compared to juvenile homicide rates. Using state panel data for 1977 through 1994, Ludwig found that adult homicide rates have increased, albeit nonsignificantly, in states passing SI laws. More specifically, Ludwig reports an increase of .16 homicides per 100,000 adults, implying an increase in adult homicide rates in SI states of roughly 1.4%. Consistent with the findings of Black and Nagin (1998), Ludwig also finds Florida to be a key player in the SI-crime debate. When excluding Florida from the sample, the estimated impact of SI laws on adult homicide rates become even greater in the positive direction (.76 homicides per 100,000 adult population, which equates to a 6.8% increase in the adult homicide rate in SI states).³

The most recent analysis of the Lott and Mustard (1997) data is by Ayres and Donohue (2003a, 2003b). Similar to Black and Nagin (1998), the authors found SI laws to be negatively and significantly related to most violent crimes when using an aggregated "hybrid model," which includes a dummy variable and a linear trend variable in the model specifications to capture any immediate and long-term effects of the laws on crime (see Tables 10 and 11 in Black & Nagin, 1998). However, when the authors used a separate dummy and time-trend variable for each state to estimate a

state-specific effect for each of the 24 adopting states, they found every crime type in more states where SI laws were positively and significantly related to crime than in more states where SI laws were negatively and significantly related to crime. Of the 216 estimated impacts reported (24 states by 9 crime types), 150 were in the positive direction and 59 of them were statistically significant, whereas only 17 were statistically significant in the negative direction. More important, there were 6 states which witnessed a statistically significant increase in violent crime, whereas only one state (Florida) experienced a statistically significant decrease. The authors attributed the differences between the aggregated and disaggregated hybrid models to two factors. First, weighting the regressions by population in the aggregated hybrid model gives undue influence to states with a large number of high population counties like Florida and Texas—both of which witnessed statistically significant decreases in crime after they passed SI laws. Second, the aggregated model gives early-adopting states greater impact in the estimation than late-passing states. Because early- and large-passing states such as Florida and Georgia witnessed drops in crime following the passage of SI laws, they had a greater impact on the estimated aggregate impact.

A study not based on the Lott and Mustard (1997) data set is by Kovandzic and Marvell (2003). It evaluated Florida's SI law's impact using county-level Uniform Crime Report (UCR) data from Florida authorities. As discussed above, previous studies of SI laws have suggested that Florida plays a pivotal role in the SI law-crime debate. McDowall et al. (1995b) found that the Florida law, if anything, is associated with more gun homicides, whereas Ayres and Donahue (2003a), Lott and Mustard (1997), Lott (1998b, 2000), and Ludwig (1998) found that it reduced homicides. More important, Black and Nagin (1998) and Marvell (1999) argue that the Lott and Mustard (1997) and Lott (1998b, 2000) results for homicide and rape are entirely driven by the inclusion of Florida in their sample. Kovandzic and Marvell (2003) used panel data for 58 Florida counties from 1980 to 2000. The impact of SI laws on violent crime was measured using data on carry permits issued per 100,000 population rather than the dummy variable and time-trend variable approach used in earlier evaluations. They controlled for numerous confounding factors including age structure, economic deprivation, and prison population. The authors

also addressed potential simultaneity problems between permit issuance rates and violent crime using the Granger causality test. The authors found little evidence of a relationship between permit-issuing rates and violent crime. They also found no evidence of a deterrent or homicide-promoting effect of permit rate growth when using homicide victimization data from the Centers for Disease and Control (CDC) or when modeling UCR and CDC homicide victimization rates as a Poisson distribution. Results from the Granger causality test also found little evidence that increases in violent crime lead to increases in permit-issuance rates.

Methodological Shortcomings of Previous Research

Although previous evaluations of SI laws and crime have attempted to address the various methodological shortcomings typically associated with macro-level evaluations of policy interventions, they have done so in a piecemeal fashion. It is important that research address all these shortcomings at once. We believe the major methodological deficiencies are the following: (a) the use of dummy variables to measure the treatment effects of SI laws on crime; (b) the use of aggregate law variables, which assume that SI law impacts are similar in all states; (c) the inability to address potential simultaneity problems between passage of SI laws and crime; (d) measurement problems surrounding the dates of passage of state SI laws; (e) the use of county-level UCR data, which is unreliable because of incomplete crime reporting and inadequate procedures to impute missing crime data; and (f) the overestimation of significance levels in county-level studies because of "clustering" of error terms at the state level. We discuss each of these problems below and discuss how we attempt to address them in our research.

Using dummy variables to measure the treatment effects of shall-issue laws. With several exceptions (e.g., Ayres & Donohue, 2003a, 2003b; Kovandzic & Marvell, 2003; Lott, 1998a, 1998b, 2000; Lott & Mustard, 1997), analysts have relied solely on before-after dummy variables to measure the "treatment effects" of SI laws on violent crime. This assumes unrealistically that SI laws have a once-and-for-all impact on crime. More specifically, this dummy variable approach implies that criminals know when SI laws go

into effect, do not forget about them, and believe the chance of encountering an armed victim varies little across time. Although it is entirely plausible that the mere passage of a SI law could lead to immediate reductions in crime because of publicity campaigns and news coverage attendant to the passage of the laws (often referred to as *announcement effects*), it is unlikely that such effects would remain static across time (Ayres & Donohue, 2003a; Kovandzic & Marvell, 2003). Perhaps crime levels would have to return to normal as publicity fades. Perhaps the crime-reduction impact of SI laws is lagged for a year or so as the criminal population learns about the laws via word of mouth (Kleck, 1997; Kovandzic & Marvell, 2003). Quite likely the laws act as a deterrent according to the extent they increase the number of permits and adults carrying guns (Kovandzic & Marvell, 2003; Lott, 1998a, 1998b, 2000; Lott & Mustard, 1997). Because the number of adults with carry permits grows in approximately a linear fashion (Kovandzic & Marvell, 2003, p. 377; Lott, 2000, p.75), one might expect any deterrent impacts of SI laws on violent crime to increase across time as criminals respond to the increased risk of coming into contact with armed victims (Lott, 1998a, 1998b, 2000; Lott & Mustard, 1997).

Data on the number of persons with carry permits is only available in a few states such as Florida (see Kovandzic & Marvell, 2003), therefore we rely primarily on time trend variables to model the impact of the laws. This procedure is not without precedent. Lott and Mustard (1997), for example, presented results using time and time-squared variables for the number of years before and after the law went into effect, and the results suggest that deterrent effects of SI laws increase across time, presumably because of increased self-protection carrying by prospective victims. Ayres and Donohue (2003a) also found evidence of growing deterrent effects of SI laws on violent crime when using an aggregated time-trend model (referred to as the Lott-spline model) and the hybrid model which we described earlier, but they discount these results because they are not based on their preferred model with disaggregated SI law variables. Black and Nagin (1998) also examined whether SI laws become more effective over time. They used a series of dummy variables indicating the number of years before and after the enactment of a SI law. Results indicated that homicide, rape, and assault were declining in counties residing in

SI states prior to the adoption of the SI law and continued to decline thereafter. With respect to robbery, they found increases prior to and after of the adoption of a SI law, although the postintervention increase was at a much slower rate (Black & Nagin, 1998, p. 215).

Assuming uniform effects of SI laws on violent crime. A second problem is that most studies assume that SI law effects are homogeneous. As noted above, Black and Nagin (1998), Marvell (1999), Ayres and Donohue (2003a) found substantial differences between states when the SI law variable is disaggregated and a tendency for positive coefficients to outnumber negative ones. This work is consistent with recent econometric research by Pesaran and Smith (1995) and Baltagi and Griffin (1997), which concludes that the assumption in panel studies of homogeneous impacts across jurisdictions is probably not justified. In the present analysis, we conduct the main analysis with an aggregated SI variable, and then use state-specific SI law variables to see if the results are consistent.

Simultaneity problems. With the possible exception of Kovandzic and Marvell (2003), previous studies of SI laws have not adequately addressed simultaneity problems, which might arise because growing crime rates might prompt states to pass SI laws and prompt citizens to obtain permits. Such an effect would bias the SI law coefficients in a positive direction, understating any deterrent effect. Lott and Mustard (1997) and Lott (1998b, 2000) address potential simultaneity bias using two-stage least squares regressions but do not present the results of any standard diagnostic tests to ensure their excluded instrumental variables are reliable (i.e., the excluded instruments are correlated with the endogenous explanatory variable, passage of SI laws) and valid (i.e. the excluded instruments are uncorrelated with the error terms in the violent crime equations). Davidson and Mackinnon (1993) maintain that "tests of overidentifying restrictions should be calculated routinely whenever one computes 2SLS estimates" (p. 236). Sargan takes it a step further and argues that studies using 2SLS regression procedures without testing for overidentifying restrictions is a "pious fraud" (as cited in Godfrey, 1988). In this article, we follow the lead of Kovandzic and Marvell (2003)

and use the Granger causality test to address the possible reciprocal relationship between the passage of SI laws and violent crime.

Incorrect dates for passage of SI laws. Lott and Mustard (1997) coded the effective dates of SI laws based on a compilation of passage dates provided in Cramer and Kopel (1995). As Kleck (1997) notes, relying on a single source of information for coding of gun laws often leads to measurement error for the gun law variables. In Lott and Mustard's case, they used the incorrect effective date for 5 of the 10 laws studied. The correct effective dates of the laws are given in Marvell (2001, p. 707; see also Vernick & Hepburn, 2003).

County-level UCR data problems. Most research on SI laws uses county-level UCR data, archived and produced by the National Archive of Criminal Justice Data (NACJD). These data are highly suspect because reporting is spotty, especially in small counties, and attempts by NACJD to estimate missing data are incomplete and change across time (Maltz & Targonski, 2002; Marvell, 1999). NACJD obtains from the FBI the raw UCR figures that are sent by police agencies to the FBI, and it combines agencies within each county to develop county-level crime data. However, NACJD has to deal with missing data to make reasonable county level estimates of crime and permit year-to-year comparisons in crime. NACJD imputed crime data for counties during the years 1977 to 1993 as follows: Within each county, any agency submitting less than 6 monthly reports is excluded when calculating the county's total crime and population counts. If, however, the agency submitted 6 to 11 monthly reports, the crime data were weighted to produce 12 monthly equivalents. As a result, crime rate calculations derived from the NACJD county crime dataset implicitly assumes that excluded law enforcement agencies have a crime rate that is identical to the rest of the county (Maltz & Targonski, 2002, p. 308). Lott and Mustard (1997), moreover, did not rely on population figures from NACJD when calculating county crime rates, instead using countywide population counts from the U.S. Census Bureau, such that they assume that agencies with missing data have no crime.⁴

In the present study, we use cities as our unit of analysis, and UCR city data does not suffer from the data-reporting problems

described above for county-level crime data. Because the FBI only reports crime counts for a particular city in their annual report if the individual law enforcement agency responsible for that jurisdiction submits 12 complete monthly reports, there is no need to impute missing crime data because of incomplete agency reporting. In addition, cities exhibit greater per-capita variation in crime rates than do large urban counties or states, which is exactly what SI law-crime research is trying to explain. Finally, cities are more internally homogenous than counties or states and thus are less likely to be susceptible to aggregation bias (see also Lott, 2000, p. 30-33).

Overestimation of significance levels. Finally, Lott and Mustard (1997), Lott (1998a, 1998b, 2000), and those revisiting the SI law-crime question using county-level data have overestimated the statistical significance of their findings because of correlation of variables within states (Harrison, Kennison, & Macedon, 2000; Moody, 2001). In such a situation, standard errors can be seriously biased downward, leading to inflated *t* ratios for the SI law variable (Greenwald, 1983; Moulton, 1990). Using Lott and Mustard's county-level data and robust Huber-White standard errors, which do not require independence of observations within "clusters" (i.e., SI states), both Harrison et al. (2000) and Moody (2001) found that the robust standard errors for the SI law dummy variables in the homicide regressions were much larger than the conventional standard errors. Coefficients on the dummy variables in the homicide regressions were rarely significant at the .05 level.

DATA AND METHOD

Research Design and Sample

The present study examines the potential deterrent effects of SI laws using panel data for the period 1980 to 2000 from 189 cities with a population of 100,000 or more in 1990 for which there were Uniform Crime Reports data. Of the 189 cities with populations greater than 100,000 in 1990, 77 resided in states passing SI laws between 1980 and 2000. If SI laws have any deterrent impact, it is most likely to show up in cities, because the cities had more

restrictive permit practices under pre-SI laws than rural areas, such that the SI laws probably had a larger impact on self-protection gun carrying (Lott, 1998b, 2000; Lott & Mustard, 1997).

Panel data have distinct advantages over more commonly used time-series or cross-sectional data. The most important is the ability to enter proxy variables for omitted variables that cause crime rates to vary across time and space. The proxy variables, which number more than 200 here, are discussed further below. Second, the high number of degrees of freedom provides greater statistical power and permits numerous control variables, which gives us more confidence that nonsignificant coefficients indicate the absence of an impact.

Methods for Panel Data

We follow conventional strategies for the statistical modeling of panel data by using a fixed-effects model, in which there is a dummy variable for each city and year, except the first year and city to avoid perfect collinearity (Hsiao, 1986, p. 41-58; Pindyck & Rubinfeld, 1991, p. 224-226).⁵ Specifically, the city dummies control for unobserved (and unmeasurable) city-specific factors whose values remained approximately stable during the study period (i.e., time-invariant factors) that caused rates of violent crime to differ across cities (Hsiao, 1986). Examples of these factors might include demographic characteristics, political orientation of city, urbanity, climate, drug and gang-related activities, and deeply embedded cultural and social norms. The city dummies also control for differences in city-level crime reporting practices that remained approximately stable during the study period. The year dummies control for unobserved time-varying factors that could affect all cities in a given year in the same fashion. An example of a national event that may have affected violent crime throughout the nation would be the 1994 Crime Control and Law Enforcement Act, which contained several major crime-reduction programs including truth-in-sentencing, the federal version of a three-strikes law; funds for 100,000 new officers; expansion of the death penalty; ban on possession of guns by juveniles; and enhanced penalties for drug offenses and for using firearms in crimes. Because the analysis includes fixed effects for both years and cities, the coefficient estimates for the SI law time-trend

variable and specific control variables (discussed below) are based solely on within-city changes across time. Finally, we follow the recommendation of Ayres and Donohue (2003a) and Marvell and Moody (1996, 2001) and include separate linear trend variables for each city.⁶ These control for unobserved factors that affect the time-series behavior of crime that can differ from city to city and depart from the nationwide trends captured by the year dummies. Without them, the coefficient on the SI law time-trend variable would simply measure whether crime rates are higher or lower for years after the law (relative to national trends captured by the year dummies), even if the change occurred before or well after the law went into effect.

Right-to-Carry Law Variables

Between 1980 and 2000, 24 states switched to a nondiscretionary permit system allowing applicants, who meet certain objective criteria, to obtain a permit to carry a concealed handgun. The 24 states and the years they began issuing permits on a nondiscretionary basis were obtained through statutory research conducted by Marvell (2001). They are as follows: Alaska (1994), Arizona (1994), Arkansas (1995), Florida (1987), Georgia (1989), Idaho (1990), Kentucky (1996), Louisiana (1996), Maine (1980), Mississippi (1990), Montana (1991), Nevada (1995), New Hampshire (1994), North Carolina (1995), Oklahoma (1995), Oregon (1990), Pennsylvania (1989), South Carolina (1996), Tennessee (1994), Virginia (1995), Texas (1995), Utah (1995), West Virginia (1988), and Wyoming (1994). Seven states had SI laws or their equivalents prior to 1980 (Alabama, Connecticut, Indiana, North Dakota, South Dakota, Vermont, and Washington).⁷ The SI laws include only those that did not give local authorities discretion to reject applications; they do not include laws that state that authorities "shall issue" permits but then proceed to give the issuing authority discretion to reject the application because, for example, the authority deems the applicant to lack "good moral character."

As discussed above, the impact of SI laws on violent crime are measured using a time-trend variable, which is coded as zeroes for all the years up to and including the year the SI law was passed in each particular city and the values 1, 2, 3, and so forth for the

following years. For example, consider a city located in Florida, which passed its SI law in 1987. In this case, in 1990, the time-trend variable is equal to 3. Again, measuring the effects of SI laws in this manner allows us to test whether the impacts of the laws are more closely linked to the number of people carrying guns in public, which grows across time as more people obtain permits. Because it is possible, albeit unlikely, that the full deterrent impacts of the laws occur immediately (if prospective shooters quickly learn about the laws through "announcement effects" discussed earlier), we also present results of estimations in which the effects of SI laws are measured using a before-after dummy variable. Similar to prior SI law studies (e.g., Lott & Mustard, 1997), the dummy variable is scored 1 the year after a law went into effect and 0 otherwise.⁸

Violent Crime

Violent crime is measured by the four offenses in the UCR Crime Index involving force or threat of force: homicide, forcible rape, robbery, and aggravated assault (Federal Bureau of Investigation, 1981-2001). Rape and assault data are probably less reliable than homicide and robbery data, because reporting rates for assault and rape have changed within the past couple of decades because new laws encourage women to report domestic violence and because police are more likely to record assaults (Reiss & Roth, 1993, pp. 407-414). To the extent these reporting changes occurred nationwide, they would be captured by the year dummies, but we cannot be sure that is the case. Consequently, results for these two crimes should be interpreted with caution. Seven cities were dropped from the sample because they failed to report crime data to the FBI for more than half of the years studied: Moreno Valley, CA; Rancho Cucamonga, CA; Santa Clarita, CA; Overland Park, KS; Kansas City, KS; Cedar Rapids, IA; and Lowell, MA.

Specific control variables. In addition to the year dummies, city dummies, and city-trend variables, we include eight specific control variables. These are selected based on a review of previous macro-level studies linking violence rates to the structural charac-

teristics of geographical units (Byrne, 1986; Kovandzic, Vieraitis, & Yeisley, 1998; Land, McCall, & Cohen, 1990; Parker, McCall, & Land, 1999; Sampson, 1986; Vieraitis, 2000, and the studies reviewed therein); they are percentage African American; percentage Hispanic; percentage ages 18 to 24 and 25 to 44; percentage households headed by females; percentage persons living below the poverty line, per-capita income; percentage population living alone, per-capita income; and percentage state prison population. Data for the first six are from the U.S. Census Bureau (1983, 1994), except that 2000 data were obtained from the U.S. Census Bureau Web site using American Fact Finder. These measures are only available for decennial census years, and we estimate data between decennial census years via linear interpolation. Given the small changes in these variables between decennial census years, a linear trend is justified. Income data for 1980 to 2000 are from the U.S. Bureau of Economic Analysis Web site. The income data are county-level estimates, and we use these values as imperfect substitutes for city-level income. Personal income data are converted from a current dollar estimate to a constant dollar 1967 basis by dividing personal income by the consumer price index. Prison population is the number of inmates sentenced to state institutions for more than a year, available annually at the state level,⁹ using data from the U.S. Bureau of Justice Statistics Web site. Because prison populations are year-end estimates, we take the average of the current year and prior year to estimate mid-year prison population.

Continuous variables are expressed as natural logs to reduce the impact of outliers. Heteroscedasticity was detected using the Breusch-Pagan test, mainly because violent crime rate variation is greater across time in the smaller cities. To avoid inefficient and biased estimated variances for the parameter estimates, we weighted the violence regressions by amounts determined by the test. Panel unit root tests (Levin & Lin, 1992; Wu, 1996) indicate that the violent crime data are stationary (i.e., the unit root hypothesis is rejected, suggesting that the analysis be conducted in levels and not first differences). Autocorrelation is mitigated by including a 1-year lag of the dependent variable in each violent crime regression (Hendry, 1995). The lagged dependent variable also has the added benefit of controlling for omitted lagged effects

(Moody, 2001; Wooldridge, 2000). Examination of collinearity diagnostics developed by Belsley, Kuh, and Welsh (1980) revealed no serious collinearity problems for the SI law time-trend variable. Although there were collinearity problems among the proxy variables, they did not substantively alter the coefficients or the statistical significance of the SI law time-trend variable, and we only measured the significance of proxy variables as groups using the *F* test. Perfect collinearity among each set of proxy variables was avoided by dropping one year dummy (i.e., 1980), one city dummy (Birmingham, AL), and one city trend variable (Birmingham, AL).

RESULTS

Table 1 presents the results for each violent crime type, using regression procedures described above. Specifically, we estimate the aggregate impact of SI laws on violent crime with the following model:

$$y_{it} = \alpha_i \text{year}_t + \Phi_i D_i + \gamma (\text{Shall}_{it} * \text{trend}) + \Psi_i (D_i * \text{trend}) + \beta x_{it} + u_{it}$$

where y_{it} is the natural logarithm of a particular violent crime per 100,000 people in city i in year t , year_t is a vector of year dummies, D_i is a vector of city dummies, $D_i * \text{trend}$ is a vector of individual city trends (equal to 1 in 1980, 2 in 1981, and 21 in 2000), x_{it} is a vector of demographic and economic controls and u_{it} is an error term. The variable $\text{Shall}_{it} * \text{trend}$ is a time-trend variable equal to the number of years after the law had been in effect and equal to 0 for the years before the law had been in effect. Additional analyses explore potential simultaneity bias problems using the Granger causality test and potential "announcement effects" of SI laws on violent crime using the dummy variable approach.

The Aggregate Impact of Shall-Issue Laws on Violent Crime

The results in Table 1 provide no support for Lott and Mustard's (1997) and Lott's (1998a, 1998b, 2000) thesis that the longer SI laws are in place, the greater their deterrent effect on violent

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TABLE 1
The Estimated Impact of Shall-Issue Laws on Violent Crime

Target Independent Variable	Dependent Variable: Natural Log of the Corresponding Violent Crime Type Per 100,000 Resident Population							
	Homicide		Robbery		Assault		Rape	
	Coefficient	t ratio	Coefficient	t ratio	Coefficient	t ratio	Coefficient	t ratio
SI law time-trend variable	.011	0.80	.010	0.91	.019	2.59	.012	1.33
Control variables (in natural logs)								
Percentage 18 to 24 years old	1.55	4.13	.532	2.22	-.333	-1.59	-.097	-0.39
Percentage 25 to 44 years old	-.867	-0.58	-.086	-0.17	-.379	-0.84	.824	1.59
Percentage Black	.264	1.18	.276	2.45	.042	0.47	.071	0.30
Percentage Hispanic	.085	0.97	.045	0.86	-.008	-0.15	-.105	-2.03
Percentage female-headed households	.311	2.68	-.030	-0.58	.028	0.46	.005	0.05
Percentage persons < poverty line	-.033	-0.11	.014	0.09	-.190	-1.22	.335	2.23
Percentage persons living alone	-.737	-1.28	-.670	-2.48	.189	0.70	.558	1.09
Per-capita income, county	.753	1.98	.177	0.92	-.008	-0.06	.479	3.56
Prison population, state	-.298	-3.57	-.212	-3.78	.013	0.29	-.074	-1.39
Violent crime type, 1-year lag	.070	1.97	.558	23.61	.565	17.77	.409	7.62
Sample size	3,863		3,863		3,863		3,773	
Adjusted R ²	.897		.971		.941		.907	

NOTE: The violent crime regressions encompass 189 cities (in 43 states) during 1980 to 2000. The dependent variables are listed at the top of each column. To conserve space results for city dummies, year dummies and city trend variables are not shown. The shall-issue law is represented by a time-trend variable as described above. All continuous variables are divided by population and logged. All regressions are weighted by a function of population as determined by the Breusch-Pagan Test. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

crime. The coefficient on the aggregate SI law time-trend variable is in the unexpected positive direction for each of the four violent crime regressions and is significant in the positive direction for aggravated assault. The *t* ratio for aggravated assault, however, is somewhat small given the large sample size and, as discussed above, the assault data are somewhat suspect. In any event, the results for the aggregate SI law time-trend variable imply an average increase of 0.2% in aggravated assault for each additional year SI laws are in effect, for a net effect of 1% higher aggravated assault rates after 5 years. Perhaps the most damaging finding in Table 1 to the more guns-less crime thesis, however, is the fact that robbery is not reduced by the increased presence of SI laws. If prospective criminals afraid of encountering armed victims in public places are deterred from even attempting crimes in the first place, then robbery should be the crime most likely to decline because it is committed in public more than homicide, rape, and assault.

Examining Robustness of Findings Using Alternate Model Specifications

Additional analyses, which are not reported in the interest of space, indicate that the lack of deterrent effects of SI laws on violent crime rates revealed in Table 1 do not appear to be sensitive to model specification.¹⁰ The results are similar with a distributed lag (a trend that plateaus after 5 years), with first-differenced variables, dropping the city trend variables, without logging variables, without weighting the regressions, and without the lagged dependent variables. In contrast to Table 1, the SI law coefficient is not significant in the assault regressions. When we reestimated the regressions in Table 1 using robust standard errors without clustering by state, *t* ratios were greater than 2 in the robbery, assault, and rape regressions.

Addressing Potential Simultaneity Problems

One possible explanation for the lack of a negative and significant coefficient for the SI law variables is simultaneity, which can happen if citizens respond to increases in violent crime by applying for and obtaining permits to carry guns or if state governments enact SI laws in response to high-crime rates. It does not

help to lag the independent variable because serial correlation between current and prior year crime rates can lead to simultaneity with the lagged dependent variable. If there is simultaneity, the SI variable coefficient might be biased in the positive direction—the opposite of any deterrent impact on violent crime. We explore this issue in two ways. The first is the Granger causality test, which entails regressing the SI law time-trend variable on one and 2-year lags of itself and 1- and 2-year lags of violent crime (Granger, 1969; Pindyck & Rubinfeld, 1991). The Granger test has a drawback in that it misses purely contemporaneous (same year) causation (Wooldridge, 2000, p. 98). In the present situation, however, if violent crime has a contemporaneous impact on permit laws and permit use, it must also have an impact lagged 1-year, because it takes time for legislatures and citizens to learn of crime trends and act on them. In addition, serial correlation of current and lagged crime rates would probably produce a significant coefficient on the lagged crime variable even if causation is completely contemporaneous. Thus, the absence of a lagged impact implies the absence of a current-year impact. The results of the Granger test showed no evidence of reverse causation. The lagged homicide variables in the SI time-trend variable regression were far from significant, small in size, and in the unexpected negative direction.

The second procedure, which only addresses possible simultaneity involved in enacting the law (i.e., that the legislature might act in response to high crimes rates, as opposed to simultaneity because of citizens getting more permits), is to drop from the analysis observations occurring just before and just after the law was passed (i.e., three observations for each state with SI laws). This analysis produces results very similar to those in Table 1. In sum, there is no evidence that individuals respond to increases in violent crime by acquiring concealed carry permits and, presumably, begin lawfully carrying guns in public for purposes of self-protection.

Models With Shall-Issue Law Dummy Variable

As discussed above, estimating the impact of SI laws on homicide by the number of years the law is in existence might miss an

impact that is due solely to the existence of the law or to "announcement" effects when the law went into effect. This is the traditional before-and-after model, operationalized by a dummy variable scored 1 for all years after the law went into effect. Although the coefficients on this SI law dummy variable are generally in the negative direction, they are extremely small and far from significant (homicide, $b = -.001$, $t = -.03$; robbery, $b = .009$, $t = .30$; assault, $b = -.021$, $t = -.94$; rape, $b = -.005$, $t = -0.23$). The results do not differ substantially when using the alternate regression procedures listed above in reference to the regressions with SI trend variables. These "null" results for the SI law dummy variables differ from much previous work, which generally find a deterrent effect (e.g., Lott, 1998b, 2000; Lott & Mustard, 1997) or "homicide promoting effect" (e.g., McDowall et al., 1995b) for SI laws.

To test the possibility of announcement effects (i.e., a short term impact resulting from publicity given the law when first enacted), we constructed a dummy variable that is scored one only in the first 2 years after a SI law is enacted. Again, coefficients are small and far from significant, with the exception of the assault regression, where the coefficient is $-.041$ ($t = -2.71$). Although this suggests a small announcement effect that deters assaults, it is not evidence that SI laws reduce assault because in the long run, SI laws appear to increase assault (see Table 1).

Estimating the State-Specific Impacts of Shall-Issue Laws on Violent Crime

Based on the results in Table 1, there is no evidence to support the thesis that the longer SI laws are in place, the greater their deterrent effect on violent crime. However, the regressions in Table 1 estimated an aggregated effect for the laws across all cities residing in adopting SI states. If, for example, the impact of the laws on violent crime rates varies significantly across states then the models in Table 1 are misspecified. Moreover, as noted above, the dangers of estimating a single aggregated effect are particularly acute because of differences in (a) permit fees and training requirements for a concealed handgun permit and where concealed handguns can be taken (Lott, 2000), (b) publicity and news

coverage surrounding passage of the laws, and (c) the number of persons in the adult population with concealed handgun permits.

We address this problem by using separate SI law variables for each state. The variable is a postlaw trend for cities in a particular state and 0 for cities elsewhere. Table 2 presents these estimates for all four violent crime categories and shows that the coefficients on the SI law time-trend variable for each of the 19 states that switched to a nondiscretionary carry permit system between 1980 and 2000—a total of 76 estimates.

Similar to Ayres and Donohue (2003a), we are leery of the more constrained specifications of the aggregate regressions, which implicitly assumed that the impact of SI laws is uniform across states. Indeed, for each violent crime type, we were able to reject the hypothesis that the 19 SI law time-trend variables were jointly equal. But this heterogeneity does not lead us to revise the Table 1 results because for each violent crime category, there are more states where passage of SI laws lead to statistically significant increases in violent crime rates than states with statistically significant decreases. For example, although there are two states that experienced significant declines in homicide, five states experienced significant increases. Of the 76 estimated impacts of SI laws on violent crime rates presented in Table 2, 13 exhibited statistically significant decreases in violent crime upon passage of the laws, whereas 23 exhibited significant increases. Overall, Table 2 shows 33 decreases in violent crime and 43 increases. In sum, the results of the state-specific effects of SI law suggests that for most states, the passage of SI laws are positively associated with violent crime rates.

Examination of the SI law time-trend variables for individual states reveals that cities in two states (Arkansas and Louisiana) show a statistically significant decrease in at least three violent categories without showing a significant increase in any category. This result differs from Ayres and Donohue (2003a), who found a positive association between passage of SI laws and violent crime rates in these states. On the other hand, the significant increases for cities in Pennsylvania and Nevada are similar to Ayres and Donohue's findings. Perhaps the most important finding in Table 2 is the lack of a significant relationship between passage of SI laws and homicide rates in Florida. As noted above, the

TABLE 2
The State-Specific Impact of Shall-Issue Laws on Violent Crime

State	Dependent Variable: Natural Log of the Corresponding Violent Crime Type Per 100,000 Resident Population							
	Homicide		Robbery		Assault		Rape	
	Coefficient	t ratio	Coefficient	t ratio	Coefficient	t ratio	Coefficient	t ratio
Alaska	-.021	-1.31	-.042	-4.42	-.001	-0.06	.009	0.51
Arizona	.042	3.66	.022	2.91	.015	1.92	.032	3.34
Arkansas	-.046	-3.21	-.049	-5.98	-.065	-6.07	-.009	-0.75
Florida	-.008	-0.81	-.020	-3.26	.013	2.10	.017	2.76
Georgia	.020	1.37	.011	1.29	.034	5.61	-.005	-0.61
Idaho	-.010	-0.59	.070	5.38	.058	8.60	.017	1.85
Kentucky	.052	2.84	.017	1.43	-.016	-2.17	-.040	-4.10
Louisiana	-.045	-2.06	-.041	-3.60	-.050	-5.96	.001	0.12
Mississippi	-.023	-1.94	-.007	-0.91	-.002	-0.33	.038	4.78
Nevada	.116	8.19	.078	9.08	.023	4.13	.064	8.35
North Carolina	.010	0.58	.002	0.23	.022	2.15	-.004	-0.31
Oklahoma	-.014	-0.97	-.027	-3.07	-.010	-1.61	-.020	-2.14
Oregon	-.007	-0.56	.002	0.35	.047	9.55	-.001	-0.26
Pennsylvania	.060	4.83	.035	4.33	.058	7.22	.045	6.55
South Carolina	-.032	-0.96	.019	1.25	-.019	-1.08	-.107	-4.13
Tennessee	.032	2.30	.019	1.82	.001	0.15	.016	1.75
Texas	-.014	-0.96	.026	2.93	.006	0.94	-.003	-0.43
Utah	.004	0.07	.035	1.71	.072	2.79	.009	0.19
Virginia	-.024	-0.83	.044	2.48	.034	1.96	-.009	-0.50

(continued)

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TABLE 2 (continued)

State	<i>Dependent Variable: Natural Log of the Corresponding Violent Crime Type Per 100,000 Resident Population</i>							
	<i>Homicide</i>		<i>Robbery</i>		<i>Assault</i>		<i>Rape</i>	
	<i>Coefficient</i>	<i>t ratio</i>	<i>Coefficient</i>	<i>t ratio</i>	<i>Coefficient</i>	<i>t ratio</i>	<i>Coefficient</i>	<i>t ratio</i>
Summary								
Negative and significant	2		5		3		3	
Negative and not significant	9		1		4		6	
Positive and significant	5		6		7		5	
Positive and not significant	3		7		5		5	

NOTE: This table presents violent crime regressions similar to those reported in Table 1 except that state-specific SI law time-trend variables are entered instead of the aggregate SI law time-trend variable. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

disaggregated SI law analyses conducted by several researchers (e.g., Ayres & Donohue, 2003a; Black & Nagin, 1998; Marvell, 1999) revealed large drops in homicide rates for Florida counties after its SI law, and they concluded that Florida is largely responsible for the negative correlations observed between passage of SI laws and homicide when using aggregate law variables. The reason for the disparate findings between those and the present study might be because there was a decline limited to rural areas or because of problems with the NACJD county data.

Results for Specific Control Variables in Table 1

Finally, the results for the control variables in Table 1 yield several key findings for future macro-level studies attempting to explain temporal variation in violent crime. First, increases in the number of African Americans and persons living below the poverty line do not appear to increase violent crime, except that the former may increase robbery and the latter may increase rape. These results contradict the findings of most cross-sectional studies, which typically find both of these structural covariates to be positively associated to violent crime rates, especially homicide (Kovandzic et al., 1998; Land et al., 1990; Parker et al., 1999). The most likely explanation for the disparate findings is that cross-sectional studies are reproducing cross-sectional variation patterns established at some point in the distant past. That is, at some point in time increases in the size of the African American and the number of persons living in poverty lead to increases in violent crime rates, and a subsequent pattern of cross-sectional variation was established, but this pattern was established well before the study period examined here. Second, increases in state imprisonment rates are associated with lower homicide and robbery, although the elasticities are somewhat smaller than those found in state- and national-level studies (Levitt, 1996; Marvell & Moody, 1997). As expected, increases in the number of persons between ages 18 to 24 are systematically related to increase in homicide and robbery. Finally, the number of families headed by females appears to be positively related to homicide rates. Although a common finding in macro-level cross-sectional studies, to our knowledge, this is the first time this variable has been related to cross temporal changes in homicide rates.

SUMMARY AND DISCUSSION

Our results provide little support for the findings by Lott and Mustard (1997) and Lott (1998b, 2000) that SI laws reduce violent crime. This does not automatically refute the theory that criminals are deterred by a greater possibility that victims are armed, because it is possible that this occurs but is counterbalanced by the theorized criminogenic effects of increased gun carrying that we discussed earlier. It seems unlikely, however, that the two would happen to balance so precisely for most violent crimes. More likely there is no deterrent effect. A likely reason is that the laws do not significantly alter rates of civilian gun carrying for self-protection and thus do not increase actual risks to criminals (Kleck, 1997, p. 372; Kovandzic & Marvell, 2003). Only about 1% of the adult population has concealed handgun permits (Kovandzic & Marvell, 2003), whereas survey research, such as the National Self-Defense Survey (Kleck & Gertz, 1998), indicate that at least 8% of adults carry a gun for protection each year. This suggests that upward of 90% of all self-protection carrying is done in violation of concealed weapon laws. To the extent that jurisdictions with higher levels of permitted gun carrying also have higher rates of total self-protection carrying, it seems unlikely that such a modest increase in the number of prospective victims carrying guns in public places is perceptible to criminals (Kleck, 1997, p. 372). Also, the National Gun Policy Survey found that 73% of adult gun carriers with permits reported no change in their level of gun carrying after they obtained a carry permit (Smith, 2001, p. 15). Most of the permits issued under SI laws, therefore, do not represent additional gun carrying. It is important to stress, however, that the essential factor, according to the deterrence hypothesis, is criminals' perception of the laws' impacts. To our knowledge, there is no information on this topic, and it is a prime candidate for further research.

Although the problems with prior research on SI laws have largely been methodological, the impetus for increasing support for such laws is based on a simplistic view of criminal behavior. Proponents of SI laws have relied on early versions of rational choice theory, put forth by economists, but contemporary versions posit more complex explanations for criminal behavior. The basic idea that criminals make choices based on an analysis of

the perceived costs and benefits remains; however, we recognize that offenders' rationality is "bounded" or "limited" (Clarke & Cornish, 2002, p. 25). Offenders do not simply add and subtract the perceived costs and benefits of crime as efficiently as economic theory suggests. The context in which they make their choices, including background factors and situational opportunities, is given greater consideration and specification in contemporary rational choice theories.

In addition, although economic theories of choice assume individuals use similar cost-benefit analyses, criminological rational choice theories consider a wider range of costs and benefits and explore in greater detail individual differences in the criminal decision-making process (Cornish & Clarke, 1986; Paternoster & Bachman, 2002; Tittle, 2000). Even if criminals have timely information regarding the passage of SI laws and the number of people lawfully carrying guns in public, such information is unlikely to have a significant impact on their behavior and violent crime rates. According to ethnographic research on active offenders, most crime is opportunistic and does not involve elaborate planning and potential costs are given relatively little consideration (Jacobs, 2000; Jacobs, Topalli, & Wright, 2003; Shover, 1996; Wright & Decker, 1994, 1997). Even when offenders do calculate the costs, they also factor in their ability to manage or eliminate these potential costs (Hochstetler & Copes, 2003; Miller & Jacobs, 1998). Research suggests that criminals are extremely confident about their abilities to control a situation and deal with whatever may arise, including encountering an armed victim (Jacobs, 2000; Wright & Decker, 1997).

Although the focus of the rational choice perspective as delineated by Cornish and Clarke (1986) concentrates on the impact of decision making on individual criminal behavior, the perspective has also been applied at the macro level. Routine activity theory explains variations in crime rates over time and place. Cohen and Felson (1979) contend that crime rates will be higher in the presence of motivated offenders, suitable targets, and in the absence of capable guardians and that the convergence of these three elements is dependent on the routine activities of persons in everyday life. The presence of motivated offenders is assumed to be a constant; but the number of young males, particularly those residing in poor urban areas, is probably a better measure of the

number of motivated offenders. Depending on the type of crime to be studied, definitions of "suitable" targets vary, but for violent crime, the profile of victims mirrors that of offenders (i.e., young, poor, non-White males residing in urban areas). Guardianship concerns any measure—human or nonhuman—which would make a target difficult if not impossible to access. In this case, a gun serves as a capable guardian over a person. Theoretically, violent crime rates should decline with an increase in guardianship (i.e., potential targets are armed), regardless of levels of motivated offenders or suitable targets. However, because the ability of everyday routines to impact violent crime rates is dependent on the convergence of all three elements in time and space, it is unlikely that the passage of SI laws would significantly reduce violent crime rates because permit acquisition, much like gun ownership in general, is higher among Whites, middle-aged persons, richer people, and in rural and suburban areas—patterns that are all the reverse of the way in which criminal victimization is distributed (Hood & Neeley, 2000).

We should point out, however, that neither the present study nor previous evaluations of SI laws have explicitly measured total rates of civilian gun carrying. Consequently, conclusions regarding the net effect of civilian gun carrying on violent crime rates based on this body of research are not warranted.¹¹ That is, the lack of a negative correlation between passage of SI laws and violent crime rates observed in the present study tells us nothing about the broad effects of civilian gun carrying rates on violent crime, especially homicide. Moreover, if "citizens arming" did reduce violent crime, much of the effect may have nothing to do with gun-carrying rates. The best documented effect of citizen arming on crime is the effect of actual defensive use of guns on whether crime victims are injured. Because homicide, by definition, requires that a victim be injured, anything that reduces injury is very likely to also reduce fatal injury. The evidence on the effects of actual defensive gun use uniformly indicates that it significantly reduces the likelihood of victim injury (see Kleck, 1997, chap. 5, for a review of the literature). Neither the possible, albeit undocumented, effects of civilian gun carrying rates nor the documented effects of actual defensive gun use in any way require that states adopt SI laws for these effects to occur.

NOTES

1. Analysis of revocation data by Lott (2000, p. 221-222) provides little support for the Zimring-Cook hypothesis (i.e., gun violence among permit holders is nearly nonexistent), with less than 0.5% of permits issued being revoked for any type of firearms-related violations.

2. A summary of macro-level studies examining the impact of SI laws on crime rates by Kovandzic and Marvell (2003) can be found on the Internet at <http://www.mmarvell.com/data.html>. Studies examining the impact of SI laws on mass public shootings (Duwe, Kovandzic, & Moody, 2002) and police deaths (Mustard, 2001) are not included.

3. Lott and Mustard (1997) also examined the possibility that passage of SI laws would have differential effects on homicide rates for adults and juveniles. They find that passage of SI laws leads to reductions in homicide rates for both adults and juveniles. The authors argue that this evidence is not contradictory to the SI law efficacy hypothesis because (a) criminals may leave areas where adults carry concealed handguns, and thus all age groups benefit from the increase in permitted gun carrying by adults, and (b) gun-carrying adults can protect juveniles in violent confrontations when they are physically present. We are not persuaded by either of these claims.

4. An extensive examination of the county-level crime datasets by Marvell (1999) also revealed extreme measurement problems with the county-level crime datasets produced by the NACJD. When comparing the sum of the county crime data in states as compiled by the NACJD to the state totals reported in the FBI's *Crime in the United States*, which adjusts estimates when agencies fail to report, Marvell found the NACJD totals in 16 states to be off by at least 50% from 1982 to 1985 and off by 25% after 1985.

5. Because the coefficients for the city and year dummies are uninterpretable (i.e., they merely denote the presence of some unobserved time-stable feature of cities or unobserved factors affecting all cities equally in a given year), we do not include them in Table 1.

6. Each city has its own trend variable, which equals 1 in 1980, 2 in 1981, and 20 in 2000.

7. Because Maine, Montana, New Hampshire, West Virginia, and Wyoming did not have a city with a population of 100,000 or more in 1990, these laws were not evaluated.

8. The seven states that had SI laws or their equivalent prior to 1980 were coded 0 because the effect of the law is captured by the city dummy variable.

9. We realize that some readers might be uncomfortable with including prison population in the homicide regression because it induces simultaneity bias—that is, homicide rates might affect prison population levels and be affected by them. As Marvell and Moody (2001) note, however, this is unlikely to be the case because murderers make up only 14.6% of the overall prison population (U.S. Bureau of Justice Statistics, 2003). In any event, deleting prison population from the homicide regressions has no impact on the results presented in Table 1.

10. Results of these alternate model specifications are available upon request from the senior author.

11. We thank one of the anonymous reviewers for pointing this out to us.

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Tomislav V. Kovandzic is an associate professor in the Department of Justice Sciences at the University of Alabama at Birmingham. He received his Ph.D. in criminology from Florida State University in 1999.

Thomas B. Marvell is a lawyer-sociologist and is director of Justec Research.

Lynne M. Vieraitis is an assistant professor in the Department of Justice Sciences at the University of Alabama at Birmingham. She received her Ph.D. in criminology from Florida State University in 1999.