View metadata, citation and similar papers at core.ac.uk
brought to you by CORE
provided by Publikations- und Dokumentenserver der Universitätsbibliothek Marburg

Zusammenhangsmessung zwischen Schusswaffen und Devianz

Inaugural-Dissertation
zur
Erlangung der wirtschaftswissenschaftlichen Doktorwürde
des Fachbereichs Wirtschaftswissenschaften
der Philipps-Universität Marburg

Eingereicht von:

Christian Westphal
Diplom-Volkswirt aus Elmshorn

Erstgutachter:	Prof. Dr. Karlheinz Fleischer
Zweitgutachter:	Prof. Dr. Bernd Hayo
Einreichungstermin:	30. Juli 2013
Prüfungstermin:	23. Oktober 2013
Erscheinungsort:	Marburg
Hochschulkennziffer:	1180

Inhaltliche Zusammenführung meiner Dissertation zum Thema "Zusammenhangsmessung zwischen Schusswaffen und Devianz"

Christian Westphal[a]

29. Juli 2013

1 Einleitung

Die staatliche Regulierung von Schusswaffen in Privatbesitz ist sowohl in der wissenschaftlichen Literatur als auch in der Gesellschaft ein intensiv diskutiertes Thema. Die Diskussion in der Öffentlichkeit wird nahezu ausschließlich auf Basis von Einzelbeobachtungen geführt. Interessenverbände und politische Parteien dominieren diese Diskussion. Herausragende Interessenverbände in den Vereinigten Staaten sind die *National Rifle Association* (NRA), die sich sehr erfolgreich [24] für ein freizügiges Waffenrecht in den Vereinigten Staaten einsetzt, und – mit entgegen gerichteter Zielsetzung – das auch global agierende *International Action Network on Small Arms* (IANSA). Eine in der Bedeutung mit der NRA vergleichbare Organisation, die ähnliche Ziele verfolgt, existiert in Deutschland nicht. Die Schießsport- und Jagdverbände sind hier die prominentesten Vertreter eines "pro"-Schusswaffen Standpunktes. Auf der anderen Seite werden insbesondere von der Partei *Die Grünen* in den letzten Jahren Gesetzesvorschläge unterbreitet (z.B. Bundestagsdrucksachen 17/2130 und 17/7732), die auf eine strengere Regulierung von privaten Schusswaffen in Deutschland abzielen.

In der öffentlichen und politischen Debatte werden vor allem drei Begründungen für Regulierung/Deregulierung von Schusswaffen angeführt. Dies sind zum

[a]University of Marburg, Faculty of Business Administration and Economics, Department of Statistics, christian.westphal@westphal.de,westphal@staff.uni-marburg.de

einen “school shootings.” In Deutschland fanden zwei besonders aufsehenerregende Taten 2002 in Erfurt und 2009 in Winnenden statt. Auf beide Ereignisse wurde von der Politik mit strengerer Schusswaffenregulierung durch Änderungen im deutschen Waffengesetz reagiert. In das Beratungsverfahren gingen auch wissenschaftliche Positionen ein [2]. Das school shooting vom Dezember 2012 in Newtown, Connecticut leitete in den USA eine intensive Diskussion um die dortigen Waffengesetze ein. Auch hieran beteiligte sich die Wissenschaft [11]. Dass die Tragik eines solchen Ereignisses von Interessenvertretern in ihrem Sinne ausgenutzt wird, ist nicht auszuschließen. Ebenso ist nicht garantiert, dass alle von Wissenschaftlern geäußerten Positionen neutral sind.

Des Weiteren wird in der Diskussion eine abstrakte allgemein erhöhte Gefährdung durch die Verbreitung von Schusswaffen angenommen, was sich in Deutschland auch in der Rechtsprechung (BVerwG 1 C 5.99) niederschlägt.

Ein dritter Diskussionspunkt sind Suizide. Sollte die Verfügbarkeit von Schusswaffen die Neigung einzelner sich selbst zu töten erhöhen, so ist es naheliegend, über Vermeidungsmöglichkeiten nachzudenken. Dass alle drei Punkte ökonomische Dimensionen besitzen, ist offensichtlich. Ebenso sind ökonomische Auswirkungen durch Regulierung/Deregulierung zu erwarten – und zwar abhängig sowohl von der Richtung und Größe eines eventuell bestehenden Zusammenhangs als auch vom direkten Effekt durch die Regulierung auf Markt und Komplementärmärkte für Schusswaffen sowie eventuellen individuellen Nutzen.

In der Wissenschaft wird – vermehrt seit [19], z.B. in [7], [4] und als aktuellen Beispielen [8], [16] und [15] – auch in der volkswirtschaftlichen Literatur mit quantitativen Methoden nach möglichen Zusammenhängen zwischen Schusswaffen und Devianz gesucht. Gibt es einen Zusammenhang zwischen unerwünschtem deviantem Verhalten und Schusswaffen, so ist – abhängig von der Richtung dieses Zusammenhanges – plausibel, dass die Verbreitung von Schusswaffen in der Bevölkerung sich auf diesem Wege positiv [3, 19, 18] oder negativ [4, 7] auf das Gemeinwohl auswirkt. Die Hypothese, dass die Verfügbarkeit von Schusswaffen mit school shootings zusammenhängt, ist in der qualitativen Literatur zu diesen Ereignissen verbreitet. Folgt man der Bezeichnung “school shooting”, so ist auf den ersten Blick ein perfekter Zusammenhang zu vermuten. Nach der Definition des Begriffs in [23] kann ein school shooting mit z.B. Hieb-, Stich-, Brandoder Sprengwaffen begangen werden. Derartige Fälle sind eingetreten, so dass der vormals offensichtlich perfekte Zusammenhang nicht besteht.

Bei allen drei oben beschriebenen Verhaltensweisen – school shootings, sonstige Gewaltkriminalität, Suizid – handelt es sich um von der gesellschaftlichen Norm abweichendes, also *deviantes*, Verhalten. Dieses soll – unter Berücksichtigung eventueller Trade-Offs – durch geeignete regulatorische Maßnahmen unterbunden werden. Aus wohlfahrtsökonomischer Sicht ist es hierbei wünschenswert, nicht opportun einer beliebigen Seite der Debatte stattzugeben. Vielmehr sollte eine Regulierung auf quantitativer Evidenz basieren. Solche ist, entgegen vielen qualitativen Ergebnissen, nicht durch einen persönlichen Bias beeinflussbar. Die wissenschaftliche Debatte zu dem Thema "Schusswaffen und Devianz" ist nicht abgeschlossen. Meine Arbeit trägt hierzu in drei Punkten bei: Zu school shootings zeige ich qualitativen Forschern eine einfach anzuwendende und leicht verständliche Methode auf und demonstriere, dass diese unter plausiblen Parametern zu aussagekräftigen Ergebnissen kommen kann. Zu Schusswaffen und Suiziden bestätige ich ein bekanntes Ergebnis: Mehr Schusswaffen hängen mit mehr Schusswaffensuiziden zusammen. Gleichzeitig berichtige ich ein in der Regulierungsdebatte verwendetes Ergebnis, das einen starken (Schein-)Zusammenhang zwischen Schusswaffen und Suiziden insgesamt behauptet [9]. Zur Thematik Schusswaffen und Gefährdung der Bevölkerung zeige ich, dass eine Regulierungsempfehlung [4] auf einem Scheinresultat aus einer Regressionsrechnung basiert. Hieraus ergibt sich gleichzeitig ein interessantes theoretisches Ergebnis zur möglichen Verzerrung von Ergebnissen in Log-Raten-Modellen.

2 School Shootings – Extrem seltene Ereignisse

Es existiert umfassende qualitative Literatur zu school shootings, z.B. [20, 21, 17, 6]. Aus dieser ergibt sich eine Vielzahl von Hypothesen über mögliche Einflussfaktoren, die zur Entscheidung, eine derartige Tat zu begehen, beitragen könnten. Ein solcher vermuteter Einflussfaktor ist die Verfügbarkeit von Schusswaffen. Ein quantitativer Untersuchungsansatz existiert meines Wissens nicht. Eine Einzelmeinung behauptet gar: "die seltenen Vorkommnisse verbieten ein quantitativ orientiertes Vorgehen" [1, S. 38].

In [23] findet sich ein theoretisches Modell, das die Entscheidung, ein school shooting zu begehen, als latentes Variablenmodell beschreibt. Die unbeobachtete Variable ist hierbei die "Fantasie" der Täter. Diese wird durch exogene Faktoren beeinflusst und kann zur binären Tatentscheidung führen. Mit dieser Motivation untersuche ich in [28] und aufbauend auf [13], ob eine quantitative Untersuchung von school shootings mit Hilfe einer Fall-Kontroll Studie und logistischer Regression erfolgversprechend dafür wäre, signifikante Zusammenhänge zu erkennen. Hierzu habe ich die Simulationssoftware [26] entwickelt und eine Simulationsstudie unter plausiblen Populationsparametern durchgeführt. Mein Ergebnis zeigt, dass unter

(i) geeigneter Definition der Grundgesamtheit,
(ii) der weltweit vorliegenden Fallzahl und
(iii) ggf. unter Zusammenfassung einzelner Einflussfaktoren in geeigneten Gruppen

mit signifikanten Ergebnissen zu rechnen ist – falls es tatsächlich Faktoren gibt, die mit einem erhöhten relativen Risiko, ein shool shooting zu begehen, zusammenhängen.

3 Suizid in Österreich und der *FSS*-Proxy

Aus zweierlei Motivation untersuche ich den Zusammenhang zwischen Suiziden und Schusswaffen mit österreichischen Registerdaten. Zum einen halte ich es für einen wichtigen Teil wissenschaftlicher Arbeit, bestehende Ergebnisse mit neuen Daten zu verifizieren. Zum anderen findet sich in [9] ein Ergebnis, das einen überraschend starken Zusammenhang zwischen Schusswaffenlizenzen und Suiziden aller Art für Österreich behauptet. In meiner Arbeit [27] zeige ich, dass die Zusammenhänge in [9] weitestgehend Scheinzusammenhänge sind, da der seit 1896 bekannte Effekt möglicher Scheinkorrelation zwischen Verhältnisvariablen ignoriert wurde [12, 22]. Einzig ein Zusammenhang zwischen Schusswaffenlizenzen und Schusswaffensuiziden lässt sich über diverse Modellspezifikationen bestätigen. Im bestpassenden der geschätzten Modelle findet sich mit schwacher Signifikanz (je nach verwendeten Standardfehlern) ein negativer Zusammenhang zwischen Schusswaffenlizenzen und anderen Suiziden, was auf einen Substitutionseffekt zwischen Methoden hindeuten könnte. Aus meiner Arbeit ergeben sich zwei Beiträge. Einerseits gilt es Scheinzusammenhänge aus der Regulierungsdebatte herauszuhalten, was in Bezug auf [9] geschehen ist. Andererseits ist die Bestätigung des positiven Zusammenhanges zwischen Schusswaffen und Schusswaffensuiziden von Bedeutung für die ökonomische Literatur. Der von Philip Cook [5] vorgeschlagene FS/S- oder FSS-Proxy (*Firearm Suicides / Suicides*) teilt die Schusswaffensuizide durch alle Suizide, um die Verbreitung von Schusswaffen in der Bevölkerung zu messen: Gibt es keine Schusswaffen, so nimmt dieser Proxy den Wert Null an. Hat jeder Suizidwillige Zugang zu Schusswaffen, ist die Verbreitung von Schusswaffen also maximal, so ist es plausibel, dass auch der Proxy sein Maximum annimmt. So stellt *FSS* die Verbreitung von Schusswaffen in der Bevölkerung dar.

4 Tötungsdelikte in den USA

In den Vereinigten Staaten sind Tötungsdelikte ein absolut und relativ größeres Problem als in Deutschland. In 2010 gab es dort 11,078 Tötungsdelikte (aus dem Bereich der Gewaltkriminalität) mit Schusswaffen bei 16,008 Tötungsdelikten insgesamt [25], was einer Tötungsdeliktrate von 5.2 auf 100,000 Einwohner und einer Tötungsdeliktrate mit Schusswaffen von 3.6 auf 100,000 Einwohner entspricht. In Deutschland gab es zum Vergleich 2,218 Tötungsdelikte, davon 147 mit Schusswaffenverwendung, was einer Rate von 2.7 und 0.2 auf 100,000 Einwohner entspricht [10]. Der Frage, ob sich bei diesem gegenüber Deutschland erhöhten Niveau beider Raten und unter Ausnutzung der regional unterschiedlichen Verbreitung von Schusswaffen [14] in den Vereinigten Staaten ein Zusammenhang zwischen Schusswaffen und Gewaltkriminalität finden lässt, sind Philip Cook und Jens Ludwig in ihrem Artikel "The social costs of gun ownership" [4] nachgegangen. Bei fragwürdiger Signifikanz finden die Autoren in dieser durch die *Joyce Foundation* finanzierten Studie einen positiven Zusammenhang zwischen der durch den FSS-Proxy gemessenen Schusswaffenverbreitung und Tötungsdelikten aller Art. Hieraus unterbreiten sie die Regulierungsempfehlung, den Besitz von Schusswaffen im Bereich von USD 100 bis USD 1,800 jährlich zu besteuern, um die Besitzer für die so vermeintlich nachgewiesenen sozialen Kosten aufkommen zu lassen.

Bei der schwachen und nur unter sehr bestimmten Bedingungen von den Autoren erlangten Signifikanz des Ergebnisses erschien es mir von Interesse, einige kleinere ökonometrische Probleme zu adressieren um die Ergebnisse auf Robustheit gegen diese zu prüfen. Da die Autoren auf meine Anfrage hin mir ihren Datensatz nicht zur Verfügung stellten, habe ich die Konstruktion des Datensatzes selbst vorgenommen, was einen erheblichen Aufwand bedeutete ($\sim$600 Seiten Programmcode), es gleichzeitig aber ermöglichte, fünf weitere Jahre in die Untersuchung aufzunehmen. Trotz großer Sorgfalt bei der Datenextraktion ist es mir zwar möglich die Ergebnisse qualitativ, jedoch nicht numerisch exakt, zu replizieren. Es stellt sich heraus, dass für die Signifikanz im Ergebnis von Cook und Ludwig lediglich eine ignorierte lineare Restriktion verantwortlich ist, auf die man bei einer gängigen Robustheitsüberprüfung stößt [30]. Berücksichtigt man diese, so verschwinden alle von den Autoren genannten Ergebnisse. Die Grundlage für die Regulierungsempfehlung ist somit hinfällig.

5 Schlussfolgerungen

Zu school shootings ist eine methodisch rigide quantitative Untersuchung wünschenswert, um zu überprüfen, ob sich so Evidenz für einzelne Behauptungen findet. Dass es hierfür mindestens eine geeignete quantitative Methode gibt, habe ich ausführlich dargelegt. Meines Wissens existiert jedoch kein Datensatz, der die hypothetisierten Einflussfaktoren für die bereits erfolgten Taten einheitlich erfasst.

Der FSS-Proxy kann als Maß für die Verbreitung von Schusswaffen verwendet werden. Ein behaupteter Zusammenhang zwischen Schusswaffenverbreitung und Suiziden aller Art lässt sich nicht messen.

Selbst bei der weiten und regional unterschiedlichen Verbreitung von Schusswaffen in den Vereinigten Staaten lässt sich anhand eines Panels von 200 Landkreisen und 25 Jahren kein Zusammenhang zwischen der Verbreitung von Schusswaffen und Gewaltkriminalität messen.

Literatur

[1] Britta Bannenberg. So genannte Amokfälle. *Bi Research – Forschungsmagazin der Universität Bielefeld*, 30:36–40, 2007.

[2] Britta Bannenberg. Anhörung von Sachverständigen "Änderung des Waffenrechts". Brief an den Innenausschuss des Bundestages, Juni 2009.

[3] Stephen G Bronars and John R Lott. Criminal deterrence, geographic spillovers, and the right to carry concealed handguns. *The American Economic Review*, 88(2):475–479, 1998.

[4] Philip J Cook and Jens Ludwig. The social costs of gun ownership. *Journal of Public Economics*, 90:379–391, 2006.

[5] P.J. Cook. The role of firearms in violent crime. *Criminal Violence, Sage Publishers: Newbury, NJ*, 1982.

[6] Dewey G Cornell and Herbert Scheithauer. *Columbine a Decade Later: The Prevention of Homicidal Violence in Schools*. Wiley, 2011.

[7] Mark Duggan. More guns, more crime. *The Journal of Political Economy*, 109(5):1086–1114, 2001.

[8] Mark Duggan, Randi Hjalmarsson, and Brian A Jacob. The short-term and localized effect of gun shows: Evidence from california and texas. *Review of Economics and Statistics*, 93(3):786–799, 2011.

[9] Elmar Etzersdorfer, Nestor D. Kapusta, and Gernot Sonneck. Suicide by shooting is correlated to rate of gun licenses in austrian counties. *Wiener Klinische Wochenschrift*, 118(15-16):464–468, 2006.

[10] Bundeskriminalamt (Hrsg.). *Polizeiliche Kriminalstatistik Bundesrepublik Deutschland – Berichtsjahr 2010*. Bundeskriminalamt Wiesbaden, 2010.

[11] Johns Hopkins Bloomberg School of Public Health, editor. *Summit on Reducing Gun Violence in America: Informing Policy with Evidence and Analysis*, 2013.

[12] Ji-Hyun Kim. Spurious correlation between ratios with a common divisor. *Statistics & Probability Letters*, 44(4):383–386, 1999.

[13] Gary King and Langche Zeng. Logistic regression in rare events data. *Political Analysis*, 9:137–163, 2001.

[14] Gary Kleck. Measures of gun ownership levels for macro-level crime and violence research. *Journal of Research in Crime and Delinquency*, 41(1):3–36, 2004.

[15] Matthew Lang. Do guns affect crime? evidence using a direct measure of firearms. *SSRN Working Paper*, 2195082, 2013.

[16] Matthew Lang. Firearm background checks and suicide. *The Economic Journal*, 2013.

[17] Peter Langman. *Why Kids Kill: Inside the Mind of School Shooters*. Palgrave, 2009.

[18] John R Lott. *More Guns, Less Crime: Understanding Crime and Gun Control Laws*. University of Chicago Press, 3rd edition, 2010.

[19] John R Lott and David B Mustard. Crime, deterrence, and right-to-carry concealed handguns. *The Journal of Legal Studies*, 26(1):1–68, 1997.

[20] Mark H. Moore, Carol V. Petrie, and Anthony A. Braga, editors. *Deadly Lessons: Understanding Lethal School Violence*. National Academy Press, 2002.

[21] Katherine Newman. *Rampage: The Social Roots of School Shootings*. Basic Books, New York, 2004.

[22] K. Pearson. Mathematical contributions to the theory of evolution. – on a form of spurious correlation which may arise when indices are used in the measurement of organs. *Proceedings of the Royal Society of London*, 60(359-367):489–498, 1896.

[23] Frank J. Robertz. *school shootings : Über die Relevanz der Phantasie für die Begehung von Mehrfachtötungen durch Jugendliche*. Verlag für Polizeiwissenschaft, 2004.

[24] Robert J. Spitzer. *The Politics of Gun Control*. CQ Press, 4th ed. edition, 2008.

[25] United States Department of Health and Human Services. Compressed mortality file (cmf) on CDC WONDER online database, 2010.

[26] Christian Westphal. *reccsim: Simulation of Rare Events Case-Control Studies*, 2012. R package version 0.9-1.

[27] Christian Westphal. Evidence for the "suicide by firearm" proxy for gun ownership from Austria. *MAGKS Joint Discussion Paper Series in Economics*, 32-2013, 2013.

[28] Christian Westphal. Logistic regression for extremely rare events: the case of school shootings. *MAGKS Joint Discussion Paper Series in Economics*, 35-2013, 2013.

[29] Christian Westphal. Personal website. `http://www.westphal.de`, 2013.

[30] Christian Westphal. The social costs of gun ownership: Spurious regression and unfounded public policy advocacy. *MAGKS Joint Discussion Paper Series in Economics*, 32-2013, 2013.

Package ‘reccsim’

February 15, 2013

Type Package

Title Simulation of Rare Events Case-Control Studies.

Version 0.9-1

Date 2012-04-20

Author Christian Westphal

Maintainer Christian Westphal <westphal@staff.uni-marburg.de>

Description All this package needs is some idea of a risk model and a population description and it will generate a random case-control-study from that risk model and population for you. You may repeat this to simulate powers of testing models and such.

License GPL (>= 2)

Repository CRAN

Date/Publication 2012-09-28 15:04:03

NeedsCompilation no

R topics documented:

`reccsim-package` *Simulate case-control studies.*

Description

This package allows you to simulate case control-studies from a known population with binary exogenous variables and a binary endogenous variable.

Details

Package:	reccsim
Type:	Package
Version:	0.9-1
Date:	2012-09-28
License:	GPL (>=2)

reccsim's functions are `rccs` and `build.population` as main workhorses and `interactive.population` to instruct users in how the `PopulationAtRisk` object has to be set up.

For simulating a case-control study you need to feed a `PopulationAtRisk` to `rccs` (for random case-control study). It will then return a case-control study which you may use for further analysis.

`interactive.population` will guide you through the construction of a PopulationAtRisk object, however for repeated simulation with different population parameters you will usually want to call `build.population` directly under specification of your population parameters.

Author(s)

Christian Westphal

Maintainer: Christian Westphal <westphal@staff.uni-marburg.de>

References

Breslow, N.E. (1996) *Statistics in Epidemiology: The Case-Control Study*. Journal of the American Statistical Association, Vol. 91 (433) pp. 14-28.

See Also

`interactive.population`

Examples

```
## Create a PopulationAtRisk manually from a risk formula
## where cancer is dependent on smoking and drinking:
## Try this with a population size of 500, 0.2 drinking
## 0.2 smoking and 0.1 smoking and drinking.
```

```
## Use 2 and 5 and their product 10 for relative risks in the respective
## groups.

## PaR <- interactive.population( cancer ~ smoking + drinking )

PaR <- build.population( cancer ~ smoking + drinking,
                         50000000,
                         .0001,
                         c(.2,.2,.1),
                         c(2,5,10)
                       )

## Now the PopulationAtRisk object stored in PaR may be used
## to construct a case control study, where we use five times
## as many controls as cases:

ccs <- rccs( PaR, ctc = 5)

## This randomized case control study from the PopulationAtRisk
## is now ready for further analysis.
## Using build.population() instead of interactive.population()
## will allow automatization for studying how for e.g. the logit
## model estimator behaves for different population parameters.

## Let us have a short summary of this cas-control study:

summary( ccs )
```

`absolute.risk` *Computes absolute risks from relative risks.*

Description

`absolute.risk` computes the absolute risks (probabilities) for different groups from relative risks and a population probability.

Usage

```
absolute.risk(pop.risk, pop.percentages, relative.risks)
```

Arguments

`pop.risk`	This is the probability of an event over the complete population.
`pop.percentages`	This is a vector of population percentages as in `build.population`.
`relative.risks`	This is a vector of relative risks as in `build.population`.

Author(s)

Christian Westphal

See Also

build.population

Examples

```
## This will tell you the risks in percentages for a baseline group
## (relative risk of one)
## and the three groups making up 20%, 20% and 10% of the population
## with relative risks of 2, 5 and 10.

absolute.risk( .0001, c(.2,.2,.1), c(2,5,10) )
```

---

add.cases *Computes how many additional cases a group generates.*

Description

Given a population risk and size, a population distribution across groups and relative group risks add.cases tells you how many additional cases a certain group generates compared with a comparison group.

Usage

```
add.cases(pop.size, pop.risk, pop.percentages, relative.risks)
```

Arguments

pop.size	An integer giving the population size.
pop.risk	A probability giving the overall probability across the whole population.
pop.percentages	The population distribution across some groups as in build.population.
relative.risks	The group's relative risks as in build.population.

Author(s)

Christian Westphal

See Also

build.population

Examples

```
## There ist a population of 50 million. Overall risk for a population
## member of becoming a case is 0.0001.

## There are four groups: Two groups each making up 20 percent of the
## population and one group making up 10 percent of the population.
## The fourth group is the baseline group with a relative risk of one
## and its 50 percent of the population are calculated automatically
## from the other group's percentages.

## The 20 percent groups are attributed with a relative risk of 2 and 5
## and the 10 percent group has a relative risk of 10.

## now add.cases will tell you, how many cases could be prevented if you
## were able to lower the overall risk by removing risk factors in the
## groups and thereby reducing a groups relative risk to one.

add.cases( 50000000, .0001, c(.2,.2,.1), c(2,5,10) )
```

---

build.population — *Builds a PopulationAtRisk object.*

Description

Given a known set of population parameters, `build.population` builds a PopulationAtRisk object which may be used in `rccs` for simulating a case control study.

Usage

```
build.population(formula, pop.size, pop.risk, pop.percentages, relative.risks)
```

Arguments

`formula`	A `formula` object describing the dependent binary variable (whatever the population is at risk for) and the independent variables (whatever is supsected/may influence the risk).
`pop.size`	The overall size of the population.
`pop.risk`	The overall population risk. I.e. the probability a random subject will exhibit the endogenous binary variable.
`pop.percentages`	The percentages of the risk groups (vector), i.e. the groups having any of the risk influencing factors. The percentage of the zero group (i.e. no factors) will be automatically computed.
`relative.risks`	The relative risks (vector) among the risk groups. Relative risk for the zero group is automatically set to one.

Author(s)

Christian Westphal

See Also

interactive.population, rccs

Examples

```
## We do have a population of 50 million people. Some (20%) consume steak,
## some (20%) consume beer, some (10%) consume both. Some consume neither.
## Those consuming steak do have twice the risk of getting cancer. Those
## who drink beer do have five times the risk of getting cancer. Those
## consuming both do have ten times (the product, which is approximately
## the equivalent to 'no interaction' for rare events) of getting cancer.

PaR <- build.population( cancer ~ steak + beer,
                         50000000,
                         .0001,
                         c(.2,.2,.1),
                         c(2,5,10)
                        )

PaR
```

---

expand *Formula expansion.*

Description

Expands some formula for all possible terms and interaction terms. This is function is not intended for the user.

Usage

```
expand(formula)
```

Arguments

formula

Author(s)

Christian Westphal

Examples

```
expand( cancer ~ smoking + drinking )
## will return the full model:
## cancer ~ smoking * drinking
```

`get.groups` *Extract population groups from a formula.*

Description

Gets all possible population groups (interactions) from a formula.

Usage

```
get.groups(formula)
```

Arguments

`formula`	This is an R formula

Author(s)

Christian Westphal

Examples

```
get.groups( cancer ~ smoking + drinking )
## will return the groups
## "smoking", "drinking" and "smoking:drinking"
```

`get.main` *Get main effects from an R* `formula` *object.*

Description

It will return all main effects from the `formula` (i.e. `y~a*b` expands to `y~ a+b+a:b`, main effects are `a` and `b`) as a character vector.

Usage

```
get.main(formula)
```

Arguments

`formula`	An R `formula` with operators "+", "*" , ":". Conditioning "|" is not implemented.

Value

An object of class `vector(mode="character", n)` where `n` is the number of main effects in the `formula`.

Author(s)

Christian Westphal

Examples

```
get.main( cancer ~ smoking + drinking + smoking:drinking )
## will return:
## "smoking", "drinking"
```

---

interactive.population *Build a population at risk interactively.*

Description

Helps you build a population at risk on at console.

Usage

```
interactive.population(formula)
```

Arguments

`formula`	The formula has to describe your risk model, i.e. some outcome, say `Y`, has to depend on some exogenous factors, say `A,B`. Then formulas like `Y~A+B, Y~A*B, Y~A+A:B` all are valid risk models.

Details

`interactive.population` is intended to teach the user about the creation of the `PopulationAtRisk` object used by `rccs` to generate random case-control studies. Usually for automatically trying different population parameters you will want to use the non-interactive function `build.population` to create a `PopulationAtRisk` object.

Value

An object of class `PopulationAtRisk`.

Author(s)

Christian Westphal

See Also

build.population, rccs

Examples

```
## Build a PopulationAtRisk with risk factors
## beer and steak and outcome cancer

## PaR <- interactive.population( cancer ~ beer + steak )
```

---

make.case.control.study

Construct a case-control-study data frame.

Description

Is used for combining a PopulationAtRisk object and a data frame of cases and a data frame of controls to a CaseControlStudy object. Usually not needed to call directly. Is used by rccs.

Usage

```
make.case.control.study(PaR, cases, controls)
```

Arguments

PaR	A PopulationAtRisk object as generated by build.population.
cases	A data frame of cases as generated by make.cases.
controls	A data frame of controls as generated by make.controls.

Author(s)

Christian Westphal

See Also

make.cases, make.controls, rccs

Examples

```
## Set up the population:
PaR <- build.population( cancer ~ smoking + drinking, 5000000, .0001,
c(.2,.2,.1), c(2,5,10) )

## Generate random cases:
my.cases <- make.cases( PaR, TRUE )

## Generate random controls:
my.controls <- make.controls( PaR, my.cases, 5, TRUE )
```

```
## Combine all three objects in a CaseControlStudy object
ccs <- make.case.control.study( PaR, my.cases, my.controls )

summary(ccs)
```

`make.cases` *Generate cases from a population at risk.*

Description

Generates a random data frame of cases based on the parameters held in a `PopulationAtRisk` object.

Usage

```
make.cases(PopulationAtRisk, requireAllGroups = FALSE)
```

Arguments

`PopulationAtRisk`

`requireAllGroups`
Setting this to `TRUE` forces cases for all population groups, i.e. the generation of cases is repeated until there are cases from all groups (chance for infinite loop).

Author(s)

Christian Westphal

See Also

`make.controls`, `make.case.control.study`, `rccs`

Examples

```
## First you generate a PopulationAtRisk named PaR with
## build.population or interactive.population.
## Then you run
## make.cases( PaR )
```

`make.controls` *Draw controls from a population at risk given observed cases.*

Description

Generates a random data frame of controls based on the parameters held in a `PopulationAtRisk` object and a data frame of cases. The latter is only used to determine the amount of controls to be drawn.

Usage

```
make.controls(PopulationAtRisk, cases, ctc, requireAllGroups
= FALSE )
```

Arguments

`PopulationAtRisk`

`cases` An object returned by `make.cases`

`ctc` cases-to-controls: How many controls shall be drawn for each case (integer)?

`requireAllGroups`
Setting this to `TRUE` forces controls for all population groups, i.e. the drawing of controls is repeated until there are controls from all groups (chance for infinite loop).

Author(s)

Christian Westphal

See Also

`make.cases`, `make.case.control.study`, `rccs`

Examples

```
## First you generate a PopulationAtRisk named PaR with
## build.population or interactive.population.
## Then you run
## cases <- make.cases( PaR )
## make.controls( PaR, controls, 5 )
```

rccs *Construct a random case-control-study from a population at risk.*

Description

This is a random case control study generator. Given a PopulationAtRisk object and a control factor cf it will create random cases based upon the population at risk and complement the cases with cf times as many random controls from the same population. It is reccsim's main workhorse.

Usage

```
rccs(PaR, ctc = 5, requireAllGroups.cases = FALSE,
requireAllGroups.controls = FALSE)
```

Arguments

PaR
: This is a PopulationAtRisk object constructed either interactively via interactive.population or manually from build.population.

ctc
: This has to be a positive integer telling rccs how many controls you want relative to the cases.

requireAllGroups.cases
: Setting this to TRUE ensures there will be cases in all groups. The random process may very well generate a set of cases where not all groups are represented (depending on the probabilities). If requireAllGroups.cases is set to TRUE and this happens, the result is rejected, the simulation gets repeated (chance for infinite loop) and a warning is issued.

requireAllGroups.controls
: Setting this to TRUE ensures there will be controls from all groups. The random process may very well draw a set of controls where not all groups are represented (depending on the probabilities). If requireAllGroups.controls is set to TRUE and this happens, the result is rejected, the simulation gets repeated (chance for infinite loop) and a warning is issued.

Details

Setting requireAllGroups.cases and requireAllGroups.controls to TRUE ensures the existence of the MLE in a binary regression setting as described by Silvapulle 1981.

Value

CaseControlStudy
: is of class CaseControlStudy (a list of length 2) containing the PopulationAtRisk at first and the case control study as data.frame at the second position.

Author(s)

Christian Westphal

References

Silvapulle, Mervyn J. (1981) *On the Existence of Maximum Likelihood Estimators for the Binomial Response Models*. Journal of the Royal Statistical Society B, Vol. 43 (3) pp. 310-313.

See Also

`summary`, `build.population`, `interactive.population`

Examples

```
## We do have a population of 50 million people. Some (20%) consume steak,
## some (20%) consume beer, some (10%) consume both. Some consume neither.
## Those consuming steak do have twice the risk of getting cancer. Those
## who drink beer do have five times the risk of getting cancer. Those
## consuming both do have ten times (the product, which is approximately
## the equivalent to 'no interaction' for rare events) of getting cancer.

PaR <- build.population( cancer ~ steak + beer, 50000000, .0001,
c(.2,.2,.1), c(2,5,10) )

## This will give a random case control study from the above population
## where you do get five times as many controls as cases.

ccs <- rccs( PaR, ctc = 5 )

summary( ccs )
```

---

risk.difference *Computes the absolute risk difference between groups.*

Description

Given a known population risk, a known population distribution across groups and known relative risks for these groups, `risk.difference` computes the absolute risk difference (probability differences) between the groups and a comparison groups with relative risk = 1.

Usage

```
risk.difference(pop.risk, pop.percentages, relative.risks)
```

Arguments

`pop.risk`	As in `build.population`.
`pop.percentages`	As in `build.population`.
`relative.risks`	As in `build.population`.

Author(s)

Christian Westphal

See Also

build.population

Examples

```
risk.difference( .0001, c(.2,.2,.1), c(2,5,10) )
```

---

summary.CaseControlStudy

Summarize a case-control-study.

Description

Summarize a case-control-study.

Usage

```
## S3 method for class 'CaseControlStudy'
summary( object, ... )
```

Arguments

object	A CaseControlStudy object as generated by make.case.control.study.
...	Generic arguments.

Author(s)

Christian Westphal

See Also

rccs

Examples

```
## Set up the PopulationAtRisk:
PaR <- build.population( cancer~smoking+drinking, 5000000, .0001,
c(.2,.2,.1), c(2,5,10) )

## Generate a random case-control study from it:
ccs <- rccs(PaR, 5, TRUE, TRUE)

## Summarize the case-control study:
summary(ccs)
```

Index

Logistic Regression for Extremely Rare Events: The Case of School Shootings

Christian Westphal[a]

This version: October 24, 2013

Abstract

School shootings are often used in public policy debate as a justification for increased regulation, based on qualitative arguments. However, to date, no effort has been made to find valid quantitative evidence for the claims bolstering the regulation recommendations. In defense of this absence of evidence, it is usually argued that the rarity of such events does not allow the employment of quantitative methods. This paper, using a simulation study, shows that, based on the number of school shootings in the United States and Germany combined, the well-known method of logistic regression can be applied to a case-control study, making it possible to at least test for an association between hypothesized influential variables and the occurrences. Moderate relative risks, explained by an observed variable, would lead to a high power of the appropriate test. A moderate numbers of cases generated by such a variable would suffice to show a significant association.

JEL Classifications: C25; C35; I18; K14
Keywords: Rare Events; Logistic Regression; Case-Control Studies; School Shootings

[a]University of Marburg, Faculty of Business Administration and Economics, Department of Statistics, `christian.westphal@westphal.de`, `westphal@staff.uni-marburg.de`

1 Introduction

The qualitative scientific literature from multiple fields contains a great many claims about what causally leads to the occurrence of school shootings, or, what is at least associated with the occurrence of such tragic events. Some of these claims are employed in public policy debate as a justification for increased regulatory action, and thereby have the potential to influence social welfare, even though these claims, while they may seem "obvious", are not backed up by quantitative evidence. A partial and compact overview of these claims is found in Kleck (1999: 2) and is quoted here in its entirety to illustrate the diversity of claims made:

> guns, "assault weapons", large-capacity ammunition magazines, lax regulation of gun shows; the failure of parents to secure guns, school cliques, and the exclusion of "outsiders"; bullying and taunting in schools, especially by high school athletes; inadequate school security, especially a lack of metal detectors, armed guards, locker searches, and so forth; excessively large high schools; inadequate monitoring of potentially violent students by schools; lazy, uninvolved Baby Boomer parents and correspondingly inadequate supervision of their children; young killers not being eligible for death penalty; a lack of religion, especially in schools; violent movies and television; violent video games; violent material and communications on the World Wide Web/Internet (including bomb-making instructions); anti-Semitism, neo-Nazi sentiments, and Hitler worship; "Industrial" music, Marilyn Manson's music, and other "dark" variants of rock music; Satanism; "Goth" culture among adolescents; and Southern culture.

All of these claims can be modeled as binary variables and the outcome, of course, is binary as well: a school shooting either happens or does not. For the quantitative analyst, it seems obvious to search for a significant association between the events and the hypothesized influencing variables. A theoretical model lending itself to this purpose is given in Robertz (2004) (an excellent book that is, unfortunately, not available in an English translation), where "fantasy" is considered a latent variable, influenced by exogenous variables, and, when pushed too hard, possibly leads to extremely deviant behavior, i.e., a school shooting. Then the "choice" of committing a school shooting depends on the influencing variables; hence we are dealing with a *choice model*, which can be modeled and estimated as a logistic model (see Manski and Lerman, 1977). In epidemiology, these models are called *incidence models* (see Prentice and Pyke, 1979). As King and Zeng (2001*b*) point out, when occurrence (or nonoccurrence) is rare, collecting a random sample with even one occurrence may become prohibitively expensive, which is clearly the case with school shootings as, fortunately, only very few students choose to kill their peers and teachers. Prentice and Pyke (1979) and Manski and Lerman (1977) show that collecting the occurrences and adding a random sample of nonoccurrences (or vice versa, depending on what is labeled as an occurrence) allows for consistent estimation of the logistic regression parameters from such a *case-control study*. A very good summary of these statistical methods in conjunction with case-control studies can be found in Breslow (1996).

For the problem at hand let us take as our population the "enrolled student years." I define an "enrolled student year" as each year an individual student is enrolled in school. I refrain from specifically stating what schools and which grades should be included; these choices will need to be made at the time of application. With this definition, we can easily measure the number of "enrolled student years that did not lead to a school shooting" and those that did. Following Robertz' (2004) definition of what constitutes a "school shooting," there were 72 cases from 1992 to 2009, committed by male students from 10 to 34 years old in the United States and Germany combined (Robertz and Wickenhäuser, 2010: 14). In the same time frame, there were around 500 million years of education provided to male students and around 1 billion years of education for both sexes, revealing the rarity, indeed, the extreme rarity, of school shootings. The goal of the case-control study is to find a statistically significant association, and better yet, causality, between the occurrence of school shootings and above-mentioned variables.

The method of case-control studies is examined by King and Zeng (2001*b*) (see also an intuitive explanation and application in King and Zeng, 2001*a*) for the case of *rare events*, which King and Zeng define as "dozens to thousands of times fewer ones ... than zeroes" (King and Zeng, 2001*b*: 138). From the numbers above, I am interested in how these methods perform in finite samples *when the occurrence is millions to tens of millions times more rare* than nonoccurrence; 72 in 500 million would be 1.44 occurrences in 10 million and 72 in 1 billion would be 0.72 occurrences in 10 million.

A viable way to draw a valid inference would be to construct a data set of all cases and controls, with the controls either randomly drawn from the population or artificial controls generated from known population parameters. The next step would be to group all hypothesized variables into two (or more) binary factors, assuming that none of the variables are negatively correlated and that none exhibit coefficients of opposed directions.[1] Next, check whether these factors have a statistically significant association with the outcome. Depending on how factors are constructed ("and" and "or" junctions come to mind), conclusions may be drawn from the test result, factor groups may be ruled out, and a stepwise search for individual variables may be constructed. Given this obvious arbitrary

[1] An assumption that is not contradicted anywhere in the qualitative literature.

interchangeability between individual variables and factors, the terms "variables" and "factors" are used interchangeably below.

This paper contributes to the literature by pointing out an easy-to-use quantitative method for measuring the association of (binary) factors with the occurrence of school shootings (in Section 2) and by examining via a simulation study what sort of relative risk a certain factor, for example, constructed as described above, would have to impose on individuals in order to show positive association in a logistic regression model (in Sections 3 and 4). My core findings are presented in Sections 4.3 and 4.4. A software package designed to repeat the simulation procedure for specific settings is provided, and its use is illustrated for an example setting. The main result shows that for plausible population sizes and overall probability of occurrence, only very few cases would need to be generated by an exogenous factor to find a significant association with the occurrences. Unfortunately, there is no data set, at least to my knowledge, that measures the above-mentioned variables for every school shooting that has ever occurred. Thus, putting the hypotheses to a meaningful test will require retrospectively collecting the data necessary for the cases and the control populations.

2 Methods

For a binary random variable $\mathbf{Y} = [y_1 \quad y_2 \quad \dots \quad y_T]'$ denoting the occurrence $y_t = 1$ or nonoccurrence $y_t = 0$ for sample member $t = 1, 2, \dots, T$ of an event influenced by some exogenous variables $\mathbf{x}_t = [x_{1,t} \quad x_{2,t} \quad \dots \quad x_{K,t}]$ and thereby $\mathbf{X} = [\mathbf{x}_1' \quad \mathbf{x}_2' \quad \dots \quad \mathbf{x}_T']'$, the logistic regression model

$$\pi_t = \Pr(y_t = 1|\mathbf{x}_t) = (1 + \exp\{-\mathbf{x}_t\boldsymbol{\beta}\})^{-1} \tag{1}$$

with $\boldsymbol{\beta} = [\beta_1 \quad \beta_2 \quad \dots \quad \beta_K]'$ can be used to estimate and test for the effects $\boldsymbol{\beta}$. Under random sampling from the *population at risk* – that is, every unit t that has a chance of becoming an occurrence – maximum likelihood methods allow for consistent and asymptotically normal estimation of $\boldsymbol{\beta}$ with the log-likelihood

$$\log L(\boldsymbol{\beta}|\mathbf{Y},\mathbf{X}) = -\sum_{t=1}^{T} \log\big(1 + \exp\{(1-2y_t)\mathbf{x}_t\boldsymbol{\beta}\}\big) \tag{2}$$

yielding the estimator $\hat{\boldsymbol{\beta}}$. It can be shown (see Prentice and Pyke, 1979; McCullagh and Nelder, 1989: 111–114) that maximizing the likelihood

$$L(\boldsymbol{\beta}|\mathbf{X},\mathbf{Y}) = \prod_{t=1}^{T} \Pr(\mathbf{x}_t|y_t) \tag{3}$$

of retrospective (choice-based) sampling yields the same estimator $\hat{\boldsymbol{\beta}}$, except for the intercept β_0. The intercept can be consistently estimated from this likelihood by

$$\hat{\beta}_0 - \log\Big[\Big(\frac{\bar{y}}{1-\bar{y}}\Big)\Big(\frac{1-\mathscr{E}[y_t]}{\mathscr{E}[y_t]}\Big)\Big] \tag{4}$$

a correction that, with knowledge of $\mathscr{E}[y_t]$, can and should be applied (see King and Zeng, 2001*b*: 144 and Section 6.2).

Using the corrected version of $\hat{\boldsymbol{\beta}}$ for estimating probabilities for some $\mathbf{x}_f$ via $(1+\exp\{-\mathbf{x}_f\hat{\boldsymbol{\beta}}\})^{-1}$ results in consistent but biased estimates due to two problems pointed out by King and Zeng (2001*b*: 145–150). First, there is a bias in $\hat{\boldsymbol{\beta}}$, which can be estimated using the following bias estimation from King and Zeng (2001*b*), which is based on McCullagh and Nelder (1989: 119–120,455–456):

$$\widehat{\mathrm{bias}}(\hat{\boldsymbol{\beta}}) = (\mathbf{X}'\mathbf{W}\mathbf{X})^{-1}\mathbf{X}'\mathbf{W}\boldsymbol{\xi} \tag{5}$$

with $\xi = 0.5\mathrm{tr}(Q)\big[(1+w_1)\hat{\pi}_t - w_1\big]$, tr being the trace operator, w_t being $w_1 = \mathscr{E}(y_t)/\bar{y}$ for cases, $w_0 = (1-\mathscr{E}(y_t))/(1-\bar{y})$ for non-case,s and $\hat{\pi}_t$ being the estimated probabilities of occurrence for unit t from $\hat{\boldsymbol{\beta}}$. $\mathbf{Q} = \mathbf{X}(\mathbf{X}'\mathbf{W}\mathbf{X})^{-1}\mathbf{X}'$ and $\mathbf{W}$ is the diagonal matrix constructed from the $\hat{\pi}_t(1-\hat{\pi}_t)w_t$. Applying this correction also reduces variance for the bias-corrected estimator $\tilde{\boldsymbol{\beta}} = \hat{\boldsymbol{\beta}} - \widehat{\mathrm{bias}}(\hat{\boldsymbol{\beta}})$ (see King and Zeng, 2001*b*: 147,161).

Second, when probabilities are then estimated from $\tilde{\boldsymbol{\beta}}$ via

$$\tilde{\pi}_f = \Pr(y_f = 1|\mathbf{x}_f, \tilde{\boldsymbol{\beta}}) = (1+\exp\{\mathbf{x}_f\tilde{\boldsymbol{\beta}}\})^{-1} \tag{6}$$

it must be kept in mind that changes in $\tilde{\boldsymbol{\beta}}$ usually do not affect $\tilde{\pi}_f$ symmetrically and hence do not cancel out. The probability calculation can be corrected for this problem by considering the distribution $f_{\tilde{\boldsymbol{\beta}}}$ of $\tilde{\boldsymbol{\beta}}$:

$$\Pr(y_f = 1|\mathbf{x}_f) = \int_{\mathbb{D}(\boldsymbol{\beta})} \Pr(y_f = 1|\mathbf{x}_f, \tilde{\boldsymbol{\beta}}) f_{\tilde{\boldsymbol{\beta}}}(\tilde{\boldsymbol{\beta}})\mathrm{d}\tilde{\boldsymbol{\beta}} \tag{7}$$

which can be estimated by using an estimation of the distribution $f_{\tilde{\boldsymbol{\beta}}}$ and can furthermore be approximated (see King and Zeng, 2001*b*: 149,161–162) by

$$\Pr(Y_f = 1|\mathbf{x}_f) \approx \tilde{\pi}_f + C_f \tag{8}$$

$$C_f = (0.5 - \tilde{\pi}_f)\tilde{\pi}_f(1-\tilde{\pi}_f)x_0\mathscr{V}(\tilde{\boldsymbol{\beta}})\mathbf{x}_0' \tag{9}$$

where $\mathbf{x}_0$ are the exogenous values for some arbitrarily chosen comparison group and $\mathscr{V}(\cdot)$ is the covariance matrix. Using the estimated distribution of $\tilde{\boldsymbol{\beta}}$, Equation (8) becomes a Bayesian estimator (see King and Zeng, 2001*b*: 149).

Equations (4) and (5) are implemented in Imai, King and Lau (2012). The correction in Equatin (9) is easily made by using, for example, the `R` function `fitted.values()`.

3 Simulation

3.1 Software

For the simulation, I wrote an `R`-package named `reccsim` (Westphal, 2012), standing for *rare events case-control study simulation*. The package's main functionalities are:

1. Building a `PopulationAtRisk` object. This object describes how the cases come to happen under a specific hypothesis and given a set of parameters, describing how factors/variables are distributed among the population.
2. Creating a pseudo-random case-control study `data.frame` from that `PopulationAtRisk` that then may be used for model estimation, for example with Imai, King and Lau (2012).

3.2 Parameters for Simulation

Assume an event's probability of occurrence to be 1 in 10 million, which is somewhere between the observed frequency of school shootings committed by "male enrolled student years" and "all student years," as set out in Section 1. Also consider two assumed factors, for example, an individual's access to "Guns" and an individual's consumption of violent computer "Games," influencing the individual probability $\Pr(y_t = 1|Guns, Games)$; note that for my analysis, it does not matter what two factors are assumed and, indeed, if one wishes to be as abstract as possible, a simple A and B will suffice. The issues that arise from these assumptions involve, first, that Equation (8) is not proven to be uniformly superior over the other estimators reported above. How do the bias corrections behave for extremely rare events and for different quantities of interest (QIs) discussed below? More importantly, what relative risk – given a population size and overall probability of occurrence – is needed to identify influencing factors? What happens when the model is not correctly specified? How does increasing the size of the control group relative to the case group affect the results? As shorthand for this last question, I will use the term *controls-to-case ratio* (as in Hennessy et al., 1999), abbreviated by CTC. To aid in answering these questions, I give an example distribution of the variables among the population in Table 1. The assumed factors of influence are two binary variables "Guns" and "Games." There is slight association between "Guns" and "Games." I will search for relative risks necessary to identify these variables' (factors') influence for population sizes of 100 million, 200 million, 500 million, and 1 billion, the latter two figures approximating the real-world setting (see the Introduction). Based on these populations, $10, 20, 50$, and 100 cases, respectively are expected from the aggregated binomial experiment. For multiple hypotheses testing, the type-I-error is set to 0.1 and a power of 0.98 for the test is considered sufficient. Note that the test's power requirement is specified very conservatively to protect my results from weak claims about necessary conditions for the method to function as intended. The marginal frequency for *Guns* in Table 1 is a very rough computation based on household gun ownership density in the United States and Germany under the assumption of independence between household gun ownership and school children. The marginal frequency for *Games* is simply a guess based on personal experience, and conveniently symmetrical to the marginal frequencies of gun availability. The joint frequency between both variables is, frankly, an arbitrary choice.

I will evaluate the correctly specified model – given here in R's `formula` notation – $Shooting \sim Guns + Games$, as well as the underspecified model $Shooting \sim Guns$, but leave the discussion of interaction effects to future research, seeing as the arguably necessary "explicit theory" in Berry, Meritt and Esarey (2010: 261-262) is yet to be posited.

$Guns/Games$	0	1	$\sum$
0	0.50	0.20	0.70
1	0.20	0.10	0.30
$\sum$	0.70	0.30	1.00

Table 1: Distribution of the population for simulation with assumed factors *Guns* and *Games*

Thus, the groups are as follows: "0" – the group having neither guns nor playing games; "Guns" – the group only having guns; "Games" – the group only playing violent games; and "Guns:Games" – the group having guns and playing violent video games.

I varied the relative risks rr_i as follows. $\pi_{Guns}/\pi_0 = rr_{Guns}$ from 1 to 10 in increments of 0.2. $\pi_{Games}/\pi_0 = rr_{Games}$ was $\in \{1, 2, 5, 10\}$ for each value of π_{Games}/π_0. Because for reasonably small probabilities, the odds ratio approximates the relative risk, we can compute

$$\begin{aligned} rr_{Guns,Games} &= \pi_{Guns,Games}/\pi_0 \approx OR_{Guns,Games} \\ &= \exp\{\beta_{Guns} + \beta_{Games}\} = OR_{Guns} \cdot OR_{Games} \approx \pi_{Guns}/\pi_0 \cdot \pi_{Games}/\pi_0, \end{aligned} \tag{10}$$

with π_i being the probability of occurrence in group i, when there is no interaction. p_i is group i's proportion of the population (notation as in King and Zeng, 2002). There was a restriction of

$$10^{-7} = \pi = p_0\pi_0 + p_{Guns}\pi_{Guns} + p_{Games}\pi_{Games} + p_{Guns,Games}\pi_{Guns,Games} \tag{11}$$

to account for the aforementioned occurrence of 1 in 10 million. For each set of parameters, the model estimation was repeated 10,000 times with a random case-control study generated each time. To ensure the existence of the maximum likelihood estimator (see Silvapulle, 1981), generated case-control studies with empty groups among either the cases or the controls were rejected. Therefore, my results are *estimations of theoretical properties of the estimators conditional on the nonexistence of empty groups*. This restriction can be easily satisfied in applications by restricting analysis to situations where cases are observed from all groups and increasing the CTC until there are controls from all groups, if necessary.

4 Results

In this section, I set out the simulation results. Unless otherwise noted, figures in the text refer to the population of 1 billion and a controls-to-cases ratio of $CTC = 5$. Results for different population sizes and different CTCs can be found in Tables 2, 3, and 4. Increasing the CTC does not change the results much. Varying the population size has a notable impact, as the number of cases generated varies. For a population of 100 million, the effects could not be found with a high enough power. The power of the test for β_{Guns} maxes out at 0.86 for a population of 100 million in the case of underspecification and at 0.79 for correct model specification. This is in accordance with the results of Peduzzi et al. (1996); there are simply not enough *events per variable*.[2] My requirements for the power are much stricter than the powers reported in Vittinghoff and McCulloch (2007: 715) and therefore my results, when interpreted in terms of *events per variable* (see Vittinghoff and McCulloch, 2007), differ, too.

4.1 Correctly Specified Model

4.1.1 Point Estimates

King and Zeng's theoretical results of $\tilde{\boldsymbol{\beta}}$ having less bias *and* less variance show in my results where $\tilde{\beta}_0$ has up to a 10% smaller RMSE[3] than $\hat{\beta}_0$ and $\tilde{\beta}_A$ has up to a 7% smaller RMSE[4] than $\hat{\beta}_A$. The RMSE ratios depending on rr_{Guns} are illustrated

[2] Also note how Westphal (2012) could easily be applied to re-study Peduzzi et al.'s topic.
[3] 18% and 24% for populations of 500 and 200 million, respectively.
[4] 14% and 21% for populations of 500 and 200 million, respectively.

in Figure 1. They look similar for different population sizes.

The *average absolute difference in bias* between both methods for all parameter sets is around eight times as high as the *average absolute difference in variance*.

My findings differ from King and Zeng when it comes to the estimation of probabilities. Using King's $\tilde{\boldsymbol{\beta}}$ increases the RMSE of $\tilde{\pi}_0$ up to 12% over simple prior correction.[5] Using King and Zeng's Bayesian method increases RMSE by 30%.[6] This increase of RMSE approaches zero for increasing $\pi_0$and likely will completely disappear or even reverse for larger π_0 than I simulated. Evidence for the latter conjecture is found in King and Zeng (2001*b*: Figure 6), where an X of 2.3 approximately represents a relative risk of 10 between the "groups" $X = 0$ and $X = 2.3$. That Figure clearly shows, that much higher relative risks are needed to find the Bayesian estimator superior. The same cannot be said about $\tilde{\pi}_{Guns}$. While the RMSE of $\tilde{\pi}_{Guns}$ itself seems to improve with increasing π_{Guns}, it becomes worse for the Bayesian estimator. It therefore appears that some caution is advisable when applying King's methods to extremely rare events in an effort to determine the probability estimations for the groups.

When estimating relative risks, using $\tilde{\boldsymbol{\beta}}$ shows huge improvement in variance and bias over using $\hat{\boldsymbol{\beta}}$ (see Figure 1 (a)–(d), population size: 1 billion, $CTC = 5$). Obvious improvement is achieved by using King and Zeng's Bayesian correction in mean squared error; however its magnitude seems to be negligible (maximum ratio observed: $\sqrt[2.5\cdot 10^7]{10}$).

Another quantity of interest is the power of the test. Due to its lower bias and variance, King and Zeng's estimator $\tilde{\boldsymbol{\beta}}$ is preferable to $\hat{\boldsymbol{\beta}}$ in terms of the test's power. The interesting section of the approximate power curve for the 1 billion population is shown in Figure 1 (e). Figure 1 (f) clearly shows that King and Zeng's estimator $\tilde{\boldsymbol{\beta}}$ is superior in specifity and sensitivity to $\hat{\boldsymbol{\beta}}$ in this setting.

4.1.2 Confidence Intervals

Confidence intervals for the quantities of interest (i) coefficients β_j where $j \in \{Guns, Games\}$, (ii) probabilities $\pi_i, i \in \{Guns, Games, (Guns, Games)\}$, and (iii) relative risks rr_i can be simulated. Imai, King and Lau (2012) provide the function `sim()` for conducting this simulation. Due to the number of simulations needed, I used the method described by King, Tomz and Wittenberg (2000: 349–350) and King and Zeng (2002: 1419) directly by using Genz et al. (2012), and the saved point estimates and estimated coefficients' covariance matrices from the output generated by Imai, King and Lau (2012) for simulating 1,000 draws from each of the $\boldsymbol{\beta}$ estimators' posteriors, mimicking `sim()`'s behavior. I set the nominal level of coverage at 90% for all simulations.

[5] 33% and 100% for populations of 500 and 200 million, respectively.

[6] 80% and 350% for populations of 500 and 200 million, respectively.

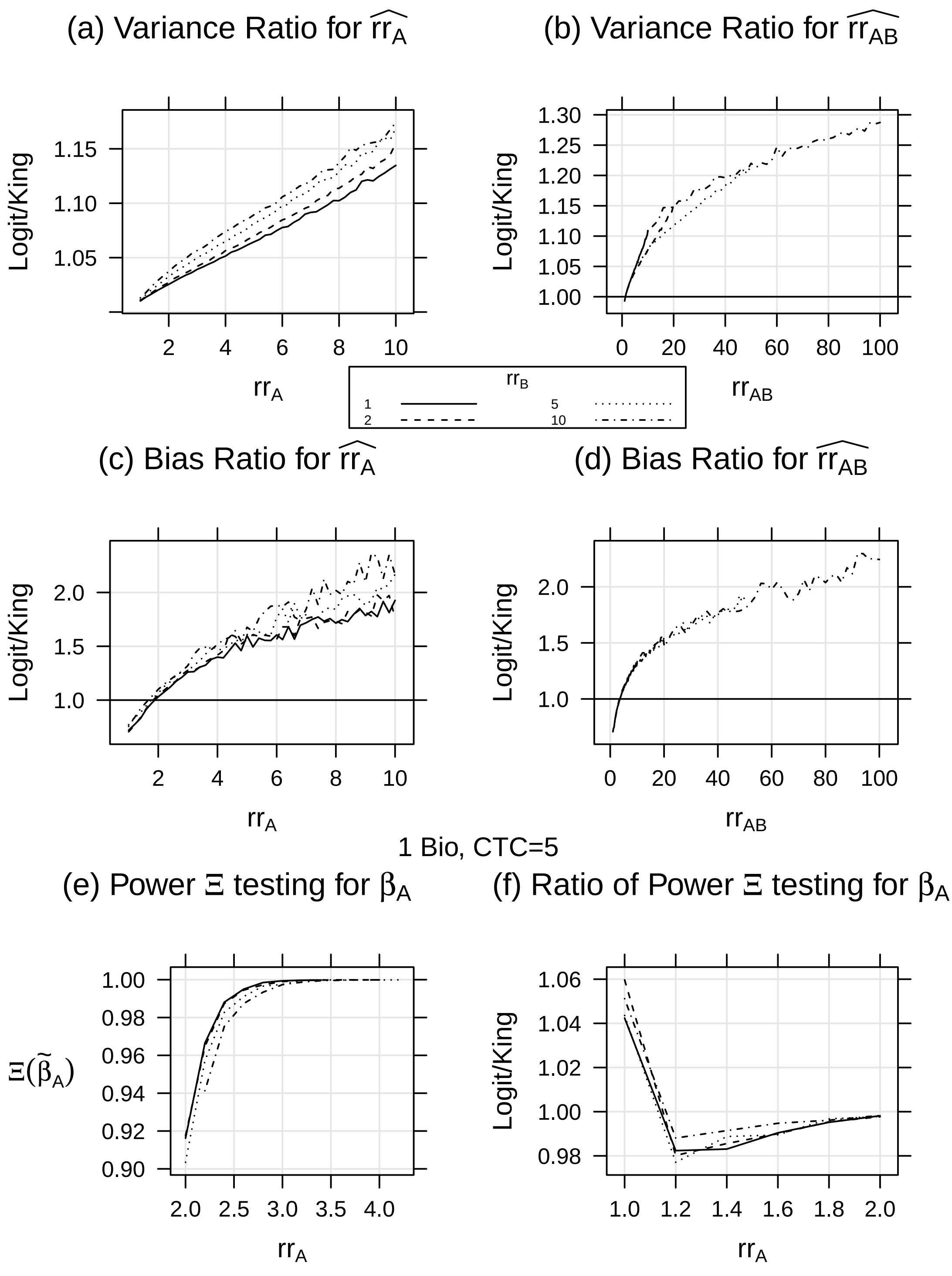

Figure 1: $\hat{\beta}$ vs. $\tilde{\beta}$, population 1 billion, $CTC = 5$

As to relative risks, Figure 2 (a) shows that neither the logit estimator with prior correction nor King's corrected estimator dominate when the model is specified correctly. When misspecified, however, King and Zeng's corrected estimator clearly beats the logit estimator with prior correction (Figure 2 (b)). Each point in Figure 2 represents one set of relative risks with rr_B indicated by the point's color. For the probability estimation, confidence interval coverage for both estimators is far too low (in the region of 40%) for the underspecification and way too high (starting at 93% and reaching up to 100%) for the correct specification.

4.2 Varying Population Size

Varying the population size from 100 million to 200 million, 500 million, or 1 billion does not change the direction of the results. The relative difference between the RMSEs of relative risk estimation appear to increase quadratically. Therefore, King and Zeng's correction is the more important the smaller the population/the rarer the event. The population size of 100 million did not yield high enough powers. For all other population sizes Table 2 shows some pivotal characteristics of the power of the test for $\tilde{\boldsymbol{\beta}}$.

4.3 Quantities of Interest

For the specific application of school shootings and possible contributing factors, there are multiple *quantities of interest*, set out for populations of 200 million, 500 million, and 1 billion in Table 2, 3, and 4. Below, I briefly discuss these quantities of interest.

The Maximum rr_{Guns} Needed to Reach a Power of 0.98. Which was the largest rr_{Guns}, unconditional on rr_{Games}, that yielded at least a simulated power of 0.98? Additionally, in the appropriate table rows, the value of rr_{Games} under which this value was found is given. The meaning of this value is that to achieve a test power of 0.98, and given all rr_{Games} I simulated, rr_{Guns} of the figure given in the table, or larger, will lead to a rather powerful test. The hypothesis test I conducted is two sided. Hence, a possible criticism is that, possibly, my power (i.e., the rejection of the null hypothesis) is being erroneously bolstered by a percentage of significant *negative* coefficient estimates. However, for relative risks ≥ 2.4, 2 out of 1.56 million simulation results exhibit this characteristic. Therefore, this potential problem seems of little concern.[7]

The Minimum rr_{Guns} Needed to Reach a Power of 0.98. Which was the smallest rr_{Guns}, unconditional on rr_{Games}, that yielded at least a simulated power of 0.98? In the appropriate table rows, the value of rr_{Games} under which this value was found is given. The meaning of this value is, that to achieve a test

[7]The figures are of similar negligible size for cases other than a population size of 1 billion, $CTC = 5$ and the respective relative risks reported in Tables 2 and 3.

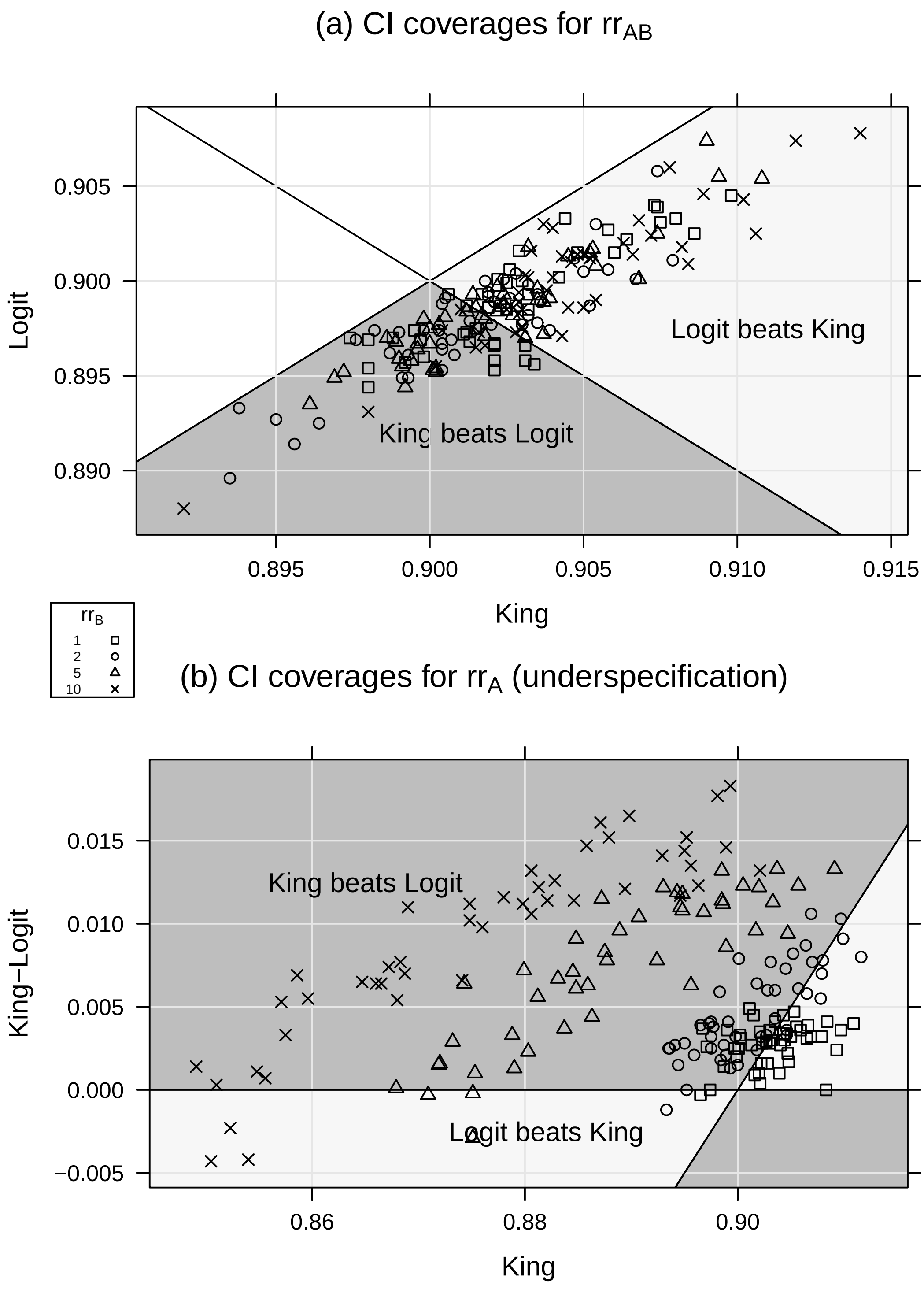

Figure 2: Comparison of confidence interval coverage for a nominal coverage of 0.9

power of 0.98, rr_{Guns} smaller than this value never resulted in a power of ≥ 0.98.

Cases Attributable to a Factor. The quantities of interest (QI) (c) and (d) in Tables 2 and 3 require some explanation: Given the different probabilities of occurrence in the (four) different groups there is a *baseline* probability of π_0 for units without exposure to risk-influencing factors. So if one could remove the probability increasing factors from the non-zero groups, these groups' π_i would switch to π_0. The groups would still generate cases, but at a lower probability. Therefore, the difference in probabilities between π_i and π_0 multiplied by the size of the subpopulation in group i tells us how many additional cases group i is responsible for, from now on called *cases attributable to group* i: CAG_i. Attributing CAG_i to a factor is easy when only one factor increases the relative risk of group i. In that situation, CAG_i is fully attributable to this factor and therefore can be written as *cases attributable to factor* j conditional on the group i: $CAF_{j|i}$. When $rr_{k \neq j} > 1, k \in \{Guns, Games\}$, $rr_{j,k}$ is computed as in Equation (10). Therefore, not all $CAG_{Guns,Games}$ can be attributed to a single factor. I split them between the factors using the weights of groups "Guns" and "Games" relative risks logarithm in the logarithm of the relative risk of group "Guns,Games":

$$CAG_{Guns,Games} = population \cdot p_{Guns,Games} \cdot (\pi_{Guns,Games} - \pi_0) \quad (12)$$

$$CAF_{j|Guns,Games} = CAG_{Guns,Games} \cdot \frac{\log(rr_j)}{\log(rr_{Guns,Games})} \quad (13)$$

This measure meets the following requirements for $a > 1, b \geq 1$: (i) For $a > b \quad \log(a)/\log(ab) > 0.5$. (ii) For $a < b \quad \log(a)/\log(ab) < 0.5$. (iii) For $a = b \quad \log(a)/\log(ab) = 0.5$. (iv) For $b = 1 \quad \log(a)/\log(ab) = 1$. In each case, a and b may be substituted by rr_{Guns} and rr_{Games}.

It is interesting that in a case where a second factor imposes a high relative risk, *fewer* cases are attributable to the first variable under the minimum identification requirement. I conjecture that the explanation for this can be found in Equation (10): under the assumption of no interaction between the linear terms, an additional variable with a relative risk > 1 leads to a multiplicative effect for the relative risk and therefore has an multiplicative effect on the number of cases exhibiting the factor relative to the number of cases *not* exhibiting the factor. The *CAG* can be computed by using the function `add.cases()` in Westphal (2012).

4.4 Increasing the Controls-to-Case Ratio

The original CTC was set at five times as many controls as cases (in accordance with Hennessy et al., 1999) in each case-control study. As King and Zeng (2001*b*: 141) state, for rare events, most information lies in the cases, and not in the controls. In my setting, initially there are no controls in the data and I use the

QI	Population size in million (Expected no. of cases)	200 (20)	500 (50)	1000 (100)
(a)	Max. rr_{Guns} needed to reach a power of 0.98 ($rr_{Games} = 10$)	NA	3.8	2.4
(b)	Min. rr_{Guns} needed to reach a power of 0.98 ($rr_{Games} = 1$)	8.2	3.4	2.4
(c)	$CAF_{Guns\|\cdot}$ is X out of Y (X/Y) expected cases given $rr_{Games} = 1$ and QI (b) from this table	14/20	22/50	30/100
(d)	$CAF_{Guns\|\cdot}$ is X out of Y (X/Y) expected cases given $rr_{Games} = 10$ and QI (a) from this table	NA	14/50	17/100

Table 2: Power of testing for β_A for different population sizes.

number of cases to determine the number of controls. Obviously, when there are very few cases compared to the population size, this method generates very few controls compared to the population size. King and Zeng (2001*b*: 153–157) undertake their analysis by *dropping* a percentage of controls from the data; I *add* some controls to the data. Hence, I approach the problem from the opposite direction: that is, King and Zeng start with 100% of controls, I start with none. Table 3 sets out the results for a range of "zeroes dropped," which is different from King and Zeng (2001*b*), who drop, at most, 90% of the non-cases.

As to be expected, adding more controls necessarily reduces variance. These effects are also shown in Table 3. Unfortunately, increasing the number of controls is costly in two ways. Obviously, research costs increase due to having to collect a larger control sample. Not so obviously, the cost of learning about the estimators' behavior increases because simulations take longer. The simulations I conducted for a single population size took about two days for a CTC of 5, about as long for a CTC of 10, twice as long for a CTC of 50 and would have taken around 60 days for a CTC of 500 on a state-of-the-art personal computer without any parallelization. Tables 2 and 3, QIs (a), (b), (e), and (f), show that the marginal returns measured in indentifying influential variables at lower relative risks depend on population size and the numbers of cases expected to be generated.

Pop. Size	QI	CTC (expected % of non-cases dropped) 5 (99.99994)	10 (99.999989)	50 (99.99949)
200 mio.	(a)	NA	8	6.2
	(b)	8.2	6.8	6.2
	(c)	14/20	13/20	12/20
	(d)	NA	9/20	8/20
	(e)	7.6	6.6	6.2
	(f)	6.4	6	5.4
	Max. MSE $\widetilde{rr}_{Guns}$	20.5	18.6	16.2
	(rr_{Guns}, rr_{Games})	(10,1)	(10,10)	(10,1)
	Max. MSE $\widetilde{rr}_{Guns,Games}$	4007	3681	2992
	(rr_{Guns}, rr_{Games})	(10,10)	(10,10)	(10,10)
500 mio.	(a)	3.8	3.4	3.0
	(b)	3.4	3.2	3.0
	(c)	22/50	20/50	19/50
	(d)	14/50	12/50	11/50
	(e)	3.4	3.2	3.0
	(f)	3.0	2.8	2.8
	Max. MSE $\widetilde{rr}_{Guns}$	21.3	20.0	18.8
	(rr_{Guns}, rr_{Games})	(10,2)	(10,2)	(10,2)
	Max. MSE $\widetilde{rr}_{Guns,Games}$	4804	4219	3334
	(rr_{Guns}, rr_{Games})	(9.6,10)	(9.8,10)	(9.8,10)
1 bio.	(a)	2.4	2.4	2.2
	(b)	2.4	2.4	2.2
	(c)	30/100	30/100	27/100
	(d)	17/100	17/100	15/100
	(e)	2.4	2.4	2.2
	(f)	2.2	2.0	2.0
	Max. MSE $\widetilde{rr}_{A}$	11.5	9.65	9.42
	(rr_A, rr_B)	(10,10)	(10,10)	(10,5)
	Max. MSE $\widetilde{rr}_{AB}$	2740	2270	1874
	(rr_A, rr_B)	(10,10)	(10,10)	(10,10)

Table 3: Effects of increasing the CTC for different quantities of interest (QI), (e) and (f) explained in table 4.

4.5 Increasing the Probability

My simulations did not vary overall probability of occurrence. However, it is easy to see that, given constant relative risks, increasing overall probability of occurrence necessarily increases probability of occurrence for all groups. From King and Zeng (2001*b*: Equation (6)), we know that variance for $\hat{\boldsymbol{\beta}}$ decreases with increasing π:

$$\begin{gathered} V(\hat{\boldsymbol{\beta}}) = \Big[\sum_{t=1}^{T} \pi_t(1-\pi_t)\mathbf{x}_t'\mathbf{x}_t\Big]^{-1} \\ \partial(\pi_t - \pi_t^2)/\partial\pi_t = 1 - 2\pi_t > 0 \forall \pi_t \in (0, 0.5) \\ \text{and thereby decreasing its inverse.} \end{gathered} \tag{14}$$

Therefore, under the assumption of $\hat{\boldsymbol{\beta}}$'s bias not increasing with π – for example, when doubling π and cutting population size in half – a lower relative risk will be needed to find the influence of a factor when the number of (expected) cases remains the same. The aforementioned assumption can be justified by Peduzzi et al. (1996: Figure 2) in combination with King and Zeng (2001*b*: Figure 4) and maybe shown from Equation (5).

4.6 Underspecified Model

Between *Guns* and *Games* there is a phi coefficient of $\phi = \frac{1}{21}$, i.e., a very weak association. Nevertheless, despite how weak this association seems to be, its effect when underspecifying the model as $Shooting \sim Guns$ is notable when looking at the power of the test in Table 4. Group "Guns"' effect is now found sooner for high relative risks in group "Games." Of course, while the test result is correct in a binary choice fashion, the improved power is not due to the test somehow becoming more sensitive but due to falsely loading explanatory power from "Games" onto "Guns" (see Lee, 1982: 207, Proposition 2). Finding a "Guns"' effect sooner for a non-influential "Games" when the model is specified as $Shooting \sim Guns$ is due to reduction in variance, which itself is due to, in this case correct, model building.

QI	Population size in million	200	500	1000
(e)	Max. rr_A needed to reach a power of 0.98 ($rr_B = 1$)	7.6	3.4	2.4
(f)	Min. rr_A needed to reach a power of 0.98 ($rr_B = 10$)	6.4	3.0	2.2

Table 4: Power for different population sizes, underspecified model, $CTC = 5$

Moreover, King and Zeng's coefficient bias correction now has the most influence on the relative risk bias when the influence of "Games" is lowest instead of highest. Apart from that, results change neither in direction nor (much) in effect size.

5 Conclusion

This paper shows that even for *extremely* rare events with binary exogenous variables, the logistic regression model is well worth to study in attempting (a) find association and (b) estimate relative risks when a serious effect from some factor is conjectured. Also note that for binary exogenous variables, no belief in the logistic form has to be held; it is simply an elaborate test for proportions.

This study revealed under what exogenous parameter settings confirmatory data analysis can be used to evaluate hypotheses derived from qualitative case studies of extremely rare events. King and Zeng's methods are very helpful, but must be applied selectively, depending on the researcher's quantities of interest. The reduction in mean squared error for the relative risk estimation compared to that achieved by the logistic regression maximum likelihood estimator is remarkable when used in the context of extremely rare events – even for a population size of 1 billion. When estimating relative risks or when searching for significance, there is no reason not to apply this correction (implemented in Imai, King and Lau, 2012) when dealing with case-control studies. Although its power does not improve dramatically, it will always offer some improvement due to the decreased bias and variance.

Based on the current paper and the work of King and Zeng (2001*b*,*a*), I suggest the following rules of thumb:

1. Effects can be found even for extremely rare events under moderate requirements for the relative risks imposed by the explanatory factors.
2. For different quantities of interest under different parameters, different methods have to be applied.
3. The more one factor's influence is hidden by another factor's influence, the more important become Equations (5) and (7).

Moreover, Westphal (2012) can be used to easily compare the methods described in Section 2 of this paper across plausible parameter sets, given a real world research problem. Indeed this should be a valid method for studying school shootings and, if properly conducted, may result in some actual quantitative evidence that may help society more effectively deal with this tragic problem.

References

Berry, William D., Jacqueline H.R. De Meritt, and Justin Esarey. 2010. "Testing for Interaction in Binary Logit and Probit Models: Is a Product Term Essential?"

American Journal of Political Science, 54(1): 248–266.

Breslow, Norman E. 1996. "Statistics in Epidemiology: The Case-Control Study." *Journal of the American Statistical Association*, 91(443): 14–28.

Genz, Alan, Frank Bretz, Tetsuhisa Miwa, Xuefei Mi, Friedrich Leisch, Fabian Scheipl, and Torsten Hothorn. 2012. "mvtnorm: Multivariate Normal and t Distributions." R package version 0.9-9992.

Hennessy, Sean, WarrenB. Bilker, Jesse A. Berlin, and Brian L. Storm. 1999. "Factors Influencing the optimal Control-to-Case Ratio in Matched Case-Control Studies." *American Journal of Epidemiology*, 149(2): 195–197.

Imai, Kosuke, Gary King, and Olivia Lau. 2012. "Zelig: Everyone's Statistical Software." R package version 3.5.3.

King, Gary, and Langche Zeng. 2001*a*. "Explaining Rare Events in International Relations." *International Organization*, 55(3): 693–715.

King, Gary, and Langche Zeng. 2001*b*. "Logistic Regression in Rare Events Data." *Political Analysis*, 9: 137–163.

King, Gary, and Langche Zeng. 2002. "Estimating risk and rate levels, ratios and differences in case-control studies." *Statistics in Medicine*, 21: 1409–1427.

King, Gary, Michael Tomz, and Jason Wittenberg. 2000. "Making the Most of Statistical Analyses: Improving Interpretation and Presentation." *American Journal of Political Science*, 44(2): 347–361.

Kleck, Gary. 1999. "There Are No Lessons to Be Learned from Littleton." *Criminal Justice Ethics*, 18: 60–63.

Lee, Lung-Fei. 1982. "Specification Error in Multinomial Logit Models." *Journal of Econometrics*, 20: 197–209.

Manski, Charles F., and Steven R. Lerman. 1977. "The Estimation of Choice Probabilities from Choice Based Samples." *Econometrica*, 45(8): 1977–1988.

McCullagh, P., and J.P. Nelder. 1989. *Generalized Linear Models.* . 2. ed., Chapman & Hall.

Peduzzi, Peter, John Concato, Elizabeth Kemper, Theodore R. Holford, and Alvan R. Feinstein. 1996. "A Simulation Study of the Number of Events per Variable in Logistic Regression Analysis." *Journal of Clinical Epidemiology*, 49(12): 1373–1379.

Prentice, R. L., and R. Pyke. 1979. "Logistic disease incidence models and case-control studies." *Biometrika*, 66(3): 403–411.

Robertz, Frank J. 2004. *school shootings : Über die Relevanz der Phantasie für die Begehung von Mehrfachtötungen durch Jugendliche.* Verlag für Polizeiwissenschaft.

Robertz, Frank J., and Ruben Wickenhäuser. 2010. *Der Riss in der Tafel.* Springer.

Silvapulle, Mervyn J. 1981. "On the Existence of Maximum Likelihood Estimators for the Binomial Response Models." *Journal of the Royal Statistical Society*, 43(3): 310–313.

Vittinghoff, Eric, and Charles E. McCulloch. 2007. "Relaxing the Rule of Ten Events per Variable in Logistic and Cox Regression." *American Journal of Epidemiology*, 165(6): 710–718.

Westphal, Christian. 2012. "reccsim: Simulation of Rare Events Case-Control Studies." R package version 0.9-1.

MAGKS

Göttingen Kassel Aachen Siegen Marburg Gießen

Joint Discussion Paper Series in Economics

by the Universities of

Aachen · Gießen · Göttingen
Kassel · Marburg · Siegen

ISSN 1867-3678

No. 32-2013

Christian Westphal

The Social Costs of Gun Ownership: Spurious Regression and Unfounded Public Policy Advocacy

This paper can be downloaded from
http://www.uni-marburg.de/fb02/makro/forschung/magkspapers/index_html%28magks%29

Coordination: Bernd Hayo • Philipps-University Marburg
Faculty of Business Administration and Economics • Universitätsstraße 24, D-35032 Marburg
Tel: +49-6421-2823091, Fax: +49-6421-2823088, e-mail: hayo@wiwi.uni-marburg.de

The Social Costs of Gun Ownership: Spurious Regression and Unfounded Public Policy Advocacy[a]

Christian Westphal[b]

This version: July 8, 2013

Abstract

In 2006, a study, published in the *Journal of Public Economics*, employing a panel regression of 200 U.S. counties across 20 years, found a significant elasticity of homicides with respect to firearms ownership. Based on this finding the authors made the public policy recommendation of taxing gun ownership. However that study fell prey to the ratio fallacy, a trap known since 1896. All the “explanatory power” (goodness-of-fit-wise and significance-wise) of the original analysis was due to regional and intertemporal differences and population being explained by itself. When the ratio fallacy is accounted for, all authors’ results can no longer be found. This is illustrated in this paper using a balanced panel from the data for 1980 to 2004. My findings are robust to (i) alternative specifications not subject to the ratio problem, (ii) using only data from 1980 to 1999 as in the original paper, (iii) using an unbalanced panel for 1980 to either 1999 or 2004, (iv) applying weighting as done by the original authors and (v) using data aggregated at the state level.

JEL Classifications: C51; H21; I18; K42
Keywords: Gun Ownership; Social Costs; Ratio Fallacy; Spurious Regression

[a]Thanks to participants of a Brown Bag Seminar held at Philipps-University Marburg and to participants of a Seminar on replication studies held at University of Göttingen both in which some good thoughts on the problem were had. Thanks to Bernd Hayo for contributing the ’moving averages’ solution for the noisy proxy mentioned in section 7 and to Florian Neumeier for contributing the growth model used in section 6.4.

[b]University of Marburg, Faculty of Business Administration and Economics, Department of Statistics, `christian.westphal@westphal.de,westphal@staff.uni-marburg.de`

Contents

1 Introduction

The association between guns and crime has been and continues to be a topic of intense debate in society at large and among social scientists. The debate intensified after Lott and Mustard (1997) published results showing that crime declined following the passing of shall-issue laws[1] for concealed carry handgun licenses. This finding sparked a furious academic debate across disciplines. Political scientists, legal scholars, criminologists, economists and scientists working in medical fields all added their voices to the discussion. From my perspective, the most noteworthy (for data and methods used, as well as results) econometric studies on guns and crime appearing after Lott and Mustard (1997) include Ludwig (1998); Duggan (2001); Leenaars and Lester (2001); Cook and Ludwig (2003, 2006); Cook et al. (2007) and Leigh and Neill (2010). Some works in this area bear strongly worded titles that rather clearly reflect their authors' perspectives: "Shooting Down More Guns, Less Crime" (Ayres and Donohue 2003*b*), "The Final Bullet in the Body of the More Guns Less Crime Hypothesis" (Donohue 2003) and "The Latest Misfires in Support of the More Guns, Less Crime Hypothesis" (Ayres and Donohue 2003*a*). These titles illustrate the intensity of the debate, which is also evident in Lott (2010: Chapter 7). Academic research results became increasingly important in the public and legal arenas. This can be seen in Fox and McDowall (2008: III.1.A), which begins with the bold statement

> "There is A Proven Correlation between the Availability of Handguns and Incidents of Violence."

and then goes on to draw on findings from Duggan (2001). In Fox and McDowall (III.1.C 2008), a result from (Cook et al. 2007: Section 4) is used to bolster their argument. Eventually, more refined econometric methods were applied to the issue. For example, Cook and Ludwig (2006), following the lead of Duggan (2001), apply advanced methods to very detailed data, taking into consideration many empirical problems.

I chose to revisit Cook and Ludwig (2006) (C&L hereafter) due to its rigor and the detailed description of the data sources from a preceding working paper (Cook and Ludwig 2004). The original objective was to address specialized econometric problems, such as the noisy proxy used and truncation of the data due to the logarithmic model, and to also possibly confirm the results with five more years of data. In this attempt, I made a surprising discovery: C&L ignored a statistical property of their data (ratios) leading to spurious results in regression analysis. Even more surprising is that this pitfall has been known about for more

[1] A "shall-issue" law forces a state to issue concealed carry licenses to any applicant. No reasons need to be given by the applicant; as long as he does not have any convictions or mental disorders the license must be issued.

than a century (Pearson 1896). This statistical property is the only reason C&L arrived at their result, based on which they advocated taxing gun ownership.

To make educated and welfare-maximizing decisions, public planners often rely on scientific findings. If these findings are biased or spurious, any public policy based on them may not have its intended effect and in the worst case could actually be harmful. To some, it may be "obvious" that externalities are imposed upon others by firearm possession. But even the "obvious" should be backed up by evidence if public policies, not to mention public funds, are going to be directed toward the issue and, unfortunately, C&L's results are not appropriate for this purpose. Their results are a statistical artifact of a well-known problem in regression analysis. This is my main finding and it is demonstrated in detail below. This work thus contributes to keeping spurious results out of the public policy debate. Furthermore the problem is illustrated in enough detail that other researchers may be alerted to this easy-to-miss problem and thus avoid it in their own empirical analyses.

This paper is organized as follows: C&L's original study is summarized and put into scientific context in Section 2. Section 3 describes the acquisition of the data necessary to repeat their analysis, and makes my analysis replicable by other researchers. Indeed, only those readers interested in such a replication need read Subsections 3.1 and 3.2. The results from Cook and Ludwig (2006) are repeated in Sections 3.3 and 4.3, with a sharp twist in the results in Section 4.4 nullifying C&L's original conclusion. Section 5 then shows how the results from Cook and Ludwig (2006) are spurious (mostly) due to ignoring the ratio fallacy – a problem known since Pearson's (1896) work and worked out in detail by Kronmal (1993). This finding is confirmed via a battery of robustness checks in Section 6, some of which also present possible fixes for the earlier model specification. None of the models yield significance for the parameter of interest.

2 "The Social Costs of Gun Ownership"

2.1 Summary

Cook and Ludwig (2006) appears to be a rigorous analysis of the relationship between guns and crime. The authors use the advanced method of panel analysis and analyze a comprehensive data set covering 200 U.S. counties and 20 years. The results are presented in a clear fashion and a specific public policy recommendation is made.

Framework: The analysis assumes that gun ownership may impose externalities on society (Cook and Ludwig 2006: 379–380), specifically that more guns may result in more homicides.

Measures: Due to a lack of administrative data on gun ownership, a proxy is used. That proxy is the fraction of suicides committed with a firearm (Cook and Ludwig 2006: 380), that is, "firearm suicides" divided by "suicides" (*FSS* or *FS/S*). This proxy

(a) seems reasonable as in a society with zero guns, FSS will be zero, and in a society where everyone has access to guns, all else equal, it very likely will achieve its maximum value.
(b) is confirmed to function in the way intended by other studies (Azrael, Cook and Miller 2004; Kleck 2004), at least for the cross-section.
(c) is supported by evidence that with increased availability of firearms the number of shooting suicides increases (see Klieve, Barnes and De Leo 2009; Klieve, Sveticic and De Leo 2009; Leenaars et al. 2003; Kellermann et al. 1992).
(d) continues to be valid if
    i. other methods of suicide are substituted by firearms suicide (as suggested by Klieve, Barnes and De Leo 2009) with increased availability of guns, or
    ii. the availability of guns increases the overall number of suicides through suicides by shooting (for which there is some evidence; see Leenaars et al. 2003; Klieve, Sveticic and De Leo 2009).[2]

For homicides, the numbers of homicides on the county level are used.

Data: The data used are a panel across the 200 U.S. counties with the largest populations (measured in 1990) and for the period 1980 to 1999. Statistics on population size and number of homicides and suicides, as well as some sociodemographic controls for each county, are available. From this information, a panel of ratios is computed with the appropriate numerators and denominators.

Methods: The panel of ratios is analyzed by a two-way (individual and time) fixed effects panel model on the logarithms. A variant of the estimating equation with a full description of the variables used can be found in Section 4.1. Different model specifications (different levels of aggregation, different sets of controls) are compared.

Main Result: From the logarithmic model an *elasticity* of the homicide rate with respect to the firearms ownership measure significant at the 5% level and on the order of 0.10 is estimated. From this result, an appropriate tax on gun ownership is calculated to be in the range of USD 100 to USD 1,800 depending on the local levels of gun ownership and homicides.

2.2 Criticism

Moody and Marvell (2010) note that C&L switch their sets of controls in the "crime equation" between Cook and Ludwig (2006), Cook and Ludwig (2002),

[2] I can find no literature discussing the case for no or a negative correlation between the number of firearms and the number of suicides (or suicides by shooting).

and Cook and Ludwig (2003) without giving any reasons for doing so. Kleck (2009) has several criticisms, including (a) that C&L's method of dealing with causal dependence is overly simple, (b) that the FSS proxy may not be valid for measuring trends in gun ownership, and, similar to Moody and Marvell's argument, (c) that the controls used are arbitrarily chosen and that some possible necessary controls are missing from the model. This last criticism is valid, but many sociodemographic controls can be substituted for each other and therefore I do not consider such a change in control variables – possibly attributable to data availability – to detract from the value of a study; also the fixed effects model will be able to capture any unobserved variables that do not change over time. Causality remains a problem and causal relationships have to be interpreted with care. Indeed, finding an *association* but stopping short of calling it *causality* seems prudent for the topic of guns and crime. Once an association is found, of course, it is worth trying to discover causal relationships.

3 Data Acquisition

3.1 Data Sources and Extraction

The aggregated data set used in Cook and Ludwig (2006) is not published and the authors chose not to share it with me. I thus acquired the data from the primary sources given in Cook and Ludwig (2004: Appendix 3). This allowed me to include five more years of data. The four data sources used are:

1. *CDC Wonder:* I used the CDC Wonder database[3] to obtain the *yearly population figure* for the counties. This database was also used to select the *200 counties with the largest population in 1990*[4] and the geographic *FIPS codes valid in 2009*. CDC Wonder cannot be used to extract statistics on number of homicides, firearm homicides, suicides, or firearm suicides as those numbers are suppressed in the later years for many of the 200 counties.
2. *Mortality Detail Data:* Cook and Ludwig (2004: 45) list the exact data sources used. These are ICPSR study data sets[5] 07632, 06798, 06799, 02201, 02392, 02702, 03085, 03306, 03473, 04640, 20540 and 20623, in chronological order. Each of these data sets contains micro data for approximately 2 million deaths in the United States for one year. No geographic codes are available

[3] United States Department of Health and Human Services (2010).

[4] The set of selected counties does not change if the 1990 census population from United States Census Bureau (1990) is used instead.

[5] United States Department of Health and Human Services. Centers for Disease Control and Prevention. National Center for Health Statistics (2010); United States Department of Health and Human Services. National Center for Health Statistics (1997, 2008*a*, *b*, 2009*a*, *b*, 2008*c*, 2007*a*, *b*, *c*, *d*, *e*)

for 2005,[6] and later data was not available to me.

3. *Sociodemographic Controls:* C&L had access to ICPSR study dataset 06054.[7] This data set was not available to me. I used the 1980, 1990, and 2000 censuses[8] directly, with the code also available from Westphal (2013).
   (a) For the *1980 census*, data were extracted from the summary tape file 3C. All data aggregated above or below county level were dropped. From Table 1, total population and rural population (used to compute urban population) are obtained. Table 10 gives the number of households. Table 12, cell 2 contains the number of blacks. Table 15, sum of cells 1–3 (males) and 28–30 (females) gives the population younger than five years (used to compute population aged five years and older). Table 20, cells 5–7 contains the number of female-headed households identifiable from the census data.[9] Table 34 contains the number of people who have been living in the same house for five years or more.
   (b) Summary tape file 3A was used for the *1990 census*. The total population is taken from Table P3; the respective number of households from P5. P6 gives the numbers of rural inhabitants. P8 contains the number of blacks. The sum of P13's first three cells yields the population below the age of five years. P17, in cells 10 and 11, gives the number of female household heads identifiable from this census. P43 contains the number of people who have lived in the same house for five years or more.
   (c) Summary file 3 for the *2000 census* – downloadable at the county level from the American Factfinder application[10] – in Table P005, cell VD01 contains the figure for total population. The same table, cell VD05 gives rural population. Table P006, cell VD03 yields the number of blacks. Table P008, cells VD03 – VD07 (males) and VD42 – VD46 (females), are the numbers of persons below five years of age. Table P009, cell VD21 contains the number of female-headed households identifiable from this census. Table P014, cell VD01 contains the number of households.
4. *Other Crime Data:* The FBI's Uniform Crime Reports are available via ICPSR study datasets[11] 08703, 08714, 09252, 09119, 09335, 09573, 09785, 06036,

[6] United States Department of Health and Human Services. National Center for Health Statistics (2008*d*).

[7] United States Department of Commerce. Bureau of the Census (1993).

[8] United States Census Bureau (1980, 1990, 2000). C&L did not name the 1980 census as a data source in Cook and Ludwig (2004), but it is mentioned in Cook and Ludwig (2006: Table 1). Relying only on the 1990 and 2000 censuses does not change any of the findings discussed below.

[9] The 2010 census gives the number of all female-headed households; it is remarkably higher than the number identifiable from the 1980, 1990 and 2000 censuses.

[10] For detailed download procedures see Westphal (2013).

[11] United States Department of Justice. Federal Bureau of Investigation (2006*a*, *b*, *r*, 2005, 2006*s*, *c*, *d*, *e*, *f*, *g*, *h*, *i*, *j*, *k*, *l*, *m*, *n*, *o*, *p*, *q*).

06316, 06669, 06850, 02389, 02764, 02910, 03167, 03451, 03721, 04009, 04360 and 04466. These contain reported crime numbers aggregated at the county level. Study dataset 06545[12] for the 1993 Uniform Crime Report data was not available for download at the time of writing.

For data extraction, I used R[13] with Grothendieck (2011), the latter very conveniently allowing SQL operations on R data frames. For the *mortality detail data*, the strategy is to read in the tab-separated file or fixed-width file for one year, and then drop all deaths occurring in counties not on the list of the 200 counties mentioned above. Next, count homicides, firearm homicides, suicides, and firearm suicides[14] – coded by either ICD9 or ICD10 – by county using SQL `count()`.[15] For the *control variables* and the *other crime data*, the data are already aggregated at the county level. The controls have to be interpolated/extrapolated[16] between/from the census years. The 2010 census was not used: the definition of female-household head was changed for that census and the number of people living in the same house for the last five years is missing from the 2010 summary file.

Different geographical coding schemes are found in the data: NCHS[17] coding and FIPS[18] coding. NCHS coding changes with each census and FIPS coding changes as counties are renamed or restructured. Changes relevant to the 200 largest counties between 1979 and 2004 are shown in Tables 1 and 2. These changes may lead to mismatched assignment of values if ignored during data extraction; thus each data source and each year had to be individually checked for such changes. There are many potential sources of error here. Detailed instructions and all code used can be found on my personal website (Westphal 2013) for individual use and critical review. To replicate Cook and Ludwig (2006), the five New York City counties are aggregated into one artificial county.[19]

3.2 Resulting Dataset

The resulting dataset shows 24 variables for $K = 196$ counties in $T = 26$ years (1979-2004). These variables include *five index variables*, namely, *year* as a time

[12] United States Department of Justice. Federal Bureau of Investigation (2008).

[13] R Development Core Team (2011).

[14] This includes homicides and suicides by explosives as these are not distinguishable in ICD9 coding.

[15] The numbers extracted this way were confirmed using the Stata files provided by ICPSR and repeating the data extraction using independent Stata code, also available from Westphal (2013).

[16] I used linear interpolation.

[17] National Center for Health Statistics.

[18] Federal Information Processing Standard.

[19] I assigned a “FIPS” code of 36998 to that artificial county.

State	County	NCHS 1970	NCHS 1980	FIPS 6-5 2009
Missouri	St. Louis City	26096	26097	29510
Missouri	St. Louis County	26095	26096	29189
Nevada	Clarc County	29002	29003	32003
New York	Kings County	33029	33029	36047
New York	Queens County	33029	33029	36081
New York	New York County	33029	33029	36061
New York	Bronx County	33029	33029	36005
New York	Richmond County	33029	33029	36085
Virginia	Norfolk City	47369	47088	51710
Virginia	Virginia Beach City	47402	47127	51810
Virginia	Fairfax County	47087	47040	51059

Table 1: NCHS code changes between 1970 and 1980 according to $http://www.nber.org/mortality/errata.txt$

Change notice	Year	Affects (out of 200 largest counties in 1990)
2	1992	none
3	1995	none
4	1999	none
5	1999	Dade County, FL changed its name to Miami-Dade County, FL. FIPS changed from 12025 to 12086.
6	2002	Parts of Adams County, CO and Jefferson County, CO now are part of Broomfield County, CO.
7	2001	none
8	2007	none
9	2008	none
10	2008	none

Table 2: FIPS 6-5 changes affecting the 200 largest counties in 1990 according to $http://www.census.gov/geo/www/ansi/changenotes.html$

index, and *nchs* and *fips*, and the tuple of *state* and *county* as interchangeable individual identifiers for the counties.

There are population numbers (per county, per year):

(1) *pop:* the population number from United States Department of Health and Human Services (2010),
(2) *total:* the (interpolated) population number from the censuses,
(3) *UCRpop:* the population number from the FBI's Uniform Crime Report,
(4) *total5plus:* the (interpolated) population number from the census for persons of five years and older,
(5) *households:* the (interpolated) number of households from the censuses,
(6) *deaths:* the number of all deaths (not used).

There are four numbers involving homicides and suicides (variables named by the respective ICD9 code):

(1) *E96:* homicides,
(2) *E965:* firearm homicides,
(3) *E95:* suicides,
(4) *E955:* firearm suicides.

The remaining nine variables are control variables:

(1) *resid5Yago:* number of residents not having moved in the last five years,
(2) *rural:* number of residents living in rural areas,
(3) *black:* number of black residents,
(4) *fhh:* number of female household heads,
(5) *UCRmurder:* number of murders from the UCR (not used),
(6) *UCRrape:* number of rapes from the UCR (not used),
(7) *UCRrobbery:* number of robberies from the UCR,
(8) *UCRassault:* number of assaults from the UCR (not used),
(9) *UCRburglary:* number of burglaries from the UCR.

I then used these numbers to calculate the percentages with the appropriate denominator. Usually, the denominator is *pop* except for the ratio of people not having moved in the last five years (denominator is *total5plus*) and the ratio of female household heads, where the denominator is the number of *households*. Switching the denominator to either $total$ or $UCRpop$ changes the results only marginally from those reported below; correlation between pop and $total$ is > 0.999 and correlation between pop and $UCRpop$ is > 0.989.

3.3 Comparison of Descriptives

There are detailed descriptives in Cook and Ludwig (2006: 382 and Table 1). I compare my data to those aggregates. The values computed from my dataset are found in Tables 3 and 4, with the values from the original article in parentheses. To avoid comparing different time periods, I restricted the comparison of descriptives to 1980–1999, the years used in the original study.

%	0	10	20	30	40	50	60	70	80	90	100
Quantile	4	28	35	41	47	55	65	80	106	156	1156
	(NA)	(27)	(NA)	(NA)	(NA)	(52)	(NA)	(NA)	(NA)	(142)	(NA)

Table 3: Quantiles of number of suicides for the 200 largest counties over 1980 to 1999, values from Cook and Ludwig (2006) in parentheses

C&L (p. 383) give all values "weighted by county population". This does not make any sense for values whose denominator is not county population. Therefore, I added a column *weight* to Table 4. This column allows comparing my data to those of C&L while at the same time giving the correct descriptives. Indiscriminately weighting by county population will not result in the sample mean if the variable does not have county population as a denominator. For example, for the average number of suicides per county in

$$\sum_{k=1}^{K}\sum_{t=1}^{T}\frac{E95_{k,t}pop_{k,t}}{KT\overline{pop}} \neq \overline{E95}, \tag{1}$$

where, from now on, k is the county index and t is the time index.

Note that the descriptives from Cook and Ludwig (2006: Table 1) and those from my dataset are very similar. Differences may be due to slightly different data sources,[20] a slightly different set of observations used for computation,[21] or, possibly, revised data.

4 Regression Analysis

4.1 Model

C&L used the logarithmic two-way fixed effects model with $t = 1, 2, \ldots, T$ indicating the years and $k = 1, 2, \ldots, K$ indicating the counties:

$$\ln Y_{k,t} = \beta_1 \ln FSS_{k,t-1} + X_{k,t}\beta_2 + d_k + d_t + \varepsilon_{k,t}. \tag{2}$$

$X_{k,t}$ contains the logarithmic values of the ratios used as controls, namely, (i) burglary rate, (ii) robbery rate, (iii) percentage black, (iv) percentage urban, (v) percentage 5+ year residents and (vi) percentage female-headed households. They include the proxy $FSS = E955/E95$ lagged by one year to circumvent possible reverse causation, i.e., people buying guns because of a higher homicide rate. The

[20] Remember, I had no access to the ICPSR study files on the censuses.

[21] I do not know if the values from Cook and Ludwig (2006) are calculated on the full data set or on their selection of 3,822 observations used for the regression analysis.

	weight	Full sample (largest 200)	Bottom quartile 1980 FSS	Top quartile 1980 FSS
Full period (1980-1999)				
FSS	$E95$	52.47	35.80	66.38
	pop	49.98	34.54	66.18
	pop	(49.9)	(34.6)	(66.9)
Homicide rate per 100'000	pop	11.30	11.00	14.27
	pop	(11.0)	(10.9)	(14.4)
Gun homicide rate per 100'000	pop	7.46	6.92	9.93
	pop	(7.3)	(6.9)	(10.1)
% Urban	pop	93.68	95.14	92.66
	pop	(92.6)	(94.7)	(91.8)
% Black	pop	14.33	16.75	18.64
	pop	(14.0)	(13.5)	(19.5)
% not moved in last 5 years	$total5plus$	57.76	59.43	48.35
% Female houshold head	$households$	17.36	18.60	16.40
	pop	17.18	18.40	16.28
	pop	(18.0)	(20.1)	(18.5)
Burglary rate	pop	1339	1218	1643
Robbery rate	pop	319	415	291
Avg. # Suicides per county	1	83.43	80.69	78.97
	pop	194.17	193.50	123.30
	pop	(195.8)	(192.5)	(120.0)
FSS in selected years				
1980	$E95$	49.91	29.28	72.72
	pop	48.01	29.20	73.11
	pop	(48.0)	(29.2)	(73.3)
1990	$E95$	54.93	37.87	68.36
	pop	52.57	36.78	68.48
	pop	(52.8)	(37.2)	(69.1)
1999	$E95$	50.56	35.71	60.01
	pop	48.18	34.93	59.81
	pop	(48.0)	(34.9)	(59.8)

Table 4: Descriptive statistics for county data revisited, values from Cook and Ludwig (2006: Table 1) in parentheses

dependent variable Y is the homicide rate $E96/pop$. Results for this and those in the following sections are qualitatively the same when the rate of firearms homicides $E965/pop$ is used. Results may be found at Westphal (2013). In their model, they include another constant term, β_0, but as we know this is either caught in the time and county dummies, or the model is overspecified, or we need to impose a restriction on one set of dummies[22], so I do not include it in in Equation (2).

4.2 Data for Analysis

Ratios taking a value of zero have to be excluded from the analysis as their logarithm is $-\infty$. There are several ways of excluding observations containing a ratio of zero: unbalance the panel or remove counties or years (whichever is less costly) in order to keep the panel balanced. For the remainder of this article I present results from a balanced panel. All observations from 1993 are removed as 1993 UCR data[23] were not available at the time of writing. I also removed all counties having a zero in either of the ratios' numerators (see Table 4). Results for this and those presented in the following sections are qualitatively the same and numerically close when using (various subsets of) the unbalanced panel.[24] The resulting balanced panel is 25 years long (1979 to 2004 without 1993) and 142 counties wide, i.e. there are 3,550 observations. Descriptive statistics do not differ much from those set out in Table 4. From those values, rates, logarithms of rates, and lagged values for FSS are added to the dataset; 3,408 values remain for 1980 to 2004 after balancing on the lags and differences.

4.3 Confirming results

Estimating Equation (2) from this balanced panel yields the estimation output in Table 5, column labeled "Equation (2)". The results are only slightly different from the results in Cook and Ludwig (2006: Table 2, final column). The sample used is different (five more years and balanced) and I did not apply weighting on the panel model, as this is rarely done in the econometric literature. The lack of efficiency can be dealt with after estimation by applying Driscoll and Kraay (1998) via Croissant and Millo's (2008) `vcovSCC` function.[25] Contrary to Cook

[22] An example would be $\sum_k d_k = 0$.

[23] United States Department of Justice. Federal Bureau of Investigation (2008).

[24] For the unbalanced panel weighting as used by C&L is needed to achieve significance. All results may be found at Westphal (2013).

[25] By applying weighting to account for heteroscedasticity (Cook and Ludwig 2006: 382) *and* calculating standard errors that are robust to heteroscedasticity (Cook and Ludwig 2006: 382), C&L basically "double correct" for heteroscedasticity. I did not find any econometric literature on this approach; however their weighting may be viewed as easily justifiable "importance weighting".

Years	1980-1999	1980-2004			
	Estimates by C&L	Equation (2) fixed effects		Equation (3) first differences	
	$N = 3822$	$N = 3408$		$N = 3266$	
Explanatories	Coef.	Coef.	SE	Coef.	p-val
$\ln FSS_{t-1}$	0.086^{*}	0.074^{*}	0.029	0.011	0.67
ln robbery rate	0.149^{***}	0.111^{**}	0.042	0.028	0.38
ln burglary rate	0.226^{***}	0.125^{***}	0.033	0.020	0.67
ln % black	$0.278^{\dagger}$	0.141^{**}	0.050	−0.071	0.80
ln % urban	-0.537^{***}	-0.490^{**}	0.160	−0.319	0.77
ln % same house 5 yr ago	−0.690	-0.467^{*}	0.235	−0.045	0.96
ln % female headed house	−0.303	0.650^{***}	0.186	0.914	0.31
R^2_{within}	NA	0.060		0.001	

Table 5: Estimation output, $^{\dagger}: p < 0.10$, $^{*}: p < 0.05$, $^{**}: p < 0.01$, $^{***}: p < 0.001$, robust standard errors according to Driscoll and Kraay (1998) computed with `vcovSCC` from Croissant and Millo (2008), R^2 adjusted

and Ludwig (2006: Table 3, model 3), who needed weighting to achieve significance on β_1, significance on the balanced panel is achieved without weighting.[26] This may be due to the large errors in the proxy, as noted by Cook and Ludwig (2006: 382), which bias the coefficient towards zero. Balancing the panel by excluding zeroes favors counties with less error in the proxy, that is, larger counties in terms of population, which therefore, all else equal, have a lesser chance of producing zeroes, are favored by the balanced panel.

The within R^2 reported in Table 5 is magnitudes smaller than the R^2 of around 0.9 reported by Cook and Ludwig (2006: Table 2) for all their models. They reported the R^2 from the least squares dummy variable estimation. That measure includes the fit from the dummies. The within R^2 only includes the fit from the ratios in $X_{k,t}$ and $FSS_{k,t-1}$. This tells us that much of the variation comes from regional and/or intertemporal differences, caught by the dummies. The coefficient on the female household heads changes sign between the original study and my estimation, but this does not affect the arguments in Sections 4.4, 5 or 6. For β_1, we can say the significant positive result from Cook and Ludwig (2006) is confirmed by my estimation.

[26] Notably despite having used the same data sources I cannot exactly replicate the results from Cook and Ludwig (2006). The following data have been updated since their work, but exactly what changes were made is not known: United States Department of Health and Human Services. Centers for Disease Control and Prevention. National Center for Health Statistics (2010: data sets 27, 28, 29).

4.4 Estimation on First Differences

Model (2) can be reformulated on the first differences, as is well known from reading any econometric textbook on panel analysis (e.g., Wooldridge 2002: Section 10.6). The individual fixed effects disappear from the model, the time fixed effects are transformed to the differences between the time fixed effects, and the errors are transformed.[27] The coefficients on the variables of interest remain the same mathematically, as can be seen from Equation (3).

$$\begin{aligned} \Delta \ln Y_{k,t} &= \ln Y_{k,t} - \ln Y_{k,t-1} \\ \Delta \ln Y_{k,t} &= \beta_1 \Delta \ln FSS_{k,t-1} + \Delta X_{k,t}\beta_2 + \delta_t + \nu_{k,t} \end{aligned} \tag{3}$$

Therefore, when estimating Equation (3) we would expect similar results in size and significance as achieved from estimating Equation (2). Looking at Table 5 reveals that *all significance has disappeared from the model.* This will be discussed in the next section.

5 Discussion

5.1 Ratio Fallacy

To understand what happens when we estimate the first difference model, the estimating equation (2) needs to be written out in full. In a first step we obtain:

$$\begin{aligned} \ln HomR_{k,t} = {}& \beta_1 \ln FSS_{k,t-1} + \beta_{2,1} \ln BurgR_{k,t} + \beta_{2,2} \ln RobR_{k,t} \\ & + \beta_{2,3} \ln BlackR_{k,t} + \beta_{2,4} \ln UrbR_{k,t} + \beta_{2,5} \ln Resid5R_{k,t} \\ & + \beta_{2,6} \ln FHHR_{k,t} + d_k + d_t + \varepsilon_{k,t} \end{aligned} \tag{4}$$

This equation still contains ratios, so it has to be written on the counts, yielding

$$\begin{aligned} \ln E96_{k,t} - \ln \mathbf{pop_{k,t}} = {}& \beta_1(\ln E955_{k,t} - \ln \mathbf{E95_{k,t}}) \\ & + \beta_{2,1}(\ln burglaries_{k,t} - \ln \mathbf{pop_{k,t}}) + \beta_{2,2}(\ln robberies_{k,t} - \ln \mathbf{pop_{k,t}}) \\ & + \beta_{2,3}(\ln blacks_{k,t} - \ln \mathbf{pop_{k,t}}) + \beta_{2,4}(\ln urbans_{k,t} - \ln \mathbf{pop_{k,t}}) \\ & + \beta_{2,5}(\ln 5yearResidents_{k,t} - \ln \mathbf{pop5plus_{k,t}}) \\ & + \beta_{2,6}(\ln fhh_{k,t} - \ln \mathbf{households_{k,t}}) + d_k + d_t + \varepsilon_{k,t}. \end{aligned} \tag{5}$$

One of the left-hand summands – $\ln \mathbf{pop_{k,t}}$ – repeats itself multiple times on the right-hand side. Basically, this model explains "population plus homicides" on the left-hand side by six different "population + something" terms on the right-hand side. Population is a perfect correlate of itself,[28] so as long as the added

[27] For when this is beneficial, see Wooldridge (2002: Section 10.7).

[28] It is nearly perfectly correlated with the population of those five years and older and the number of households; $r > 0.99$ for those variables.

values do not exhibit too much orthogonal variation to population itself, it *will* be able to explain itself. This is a variant of the ratio fallacy, first discovered by Pearson (1896) and discussed in detail by Kronmal (1993),[29] which here appears disguised in a logarithmic model. This fallacy can be seen more clearly in the logarithms of ratios, as now the variable responsible for the spurious results is linear in the terms on both sides. The FSS denominator is the number of all suicides ($E95$), i.e., it is not population. Why this does not affect the argument is shown in Section 5.3.

5.2 First Differences

I now demonstrate what happens when we compute the first differences of any of the ratios' logarithms using the example of the left-hand side of Equation (6) to understand why significance vanishes here.

$$\begin{aligned} \Delta Y_{k,t} &= \Delta(\ln E96_{k,t} - \ln pop_{k,t}) \\ &= (\ln E96_{k,t} - \ln pop_{k,t}) - (\ln E96_{k,t-1} - \ln pop_{k,t-1}) \\ &= \ln E96_{k,t} - \ln pop_{k,t} - \ln E96_{k,t-1} + \ln pop_{k,t-1} \\ &= \ln \frac{E96_{k,t}}{E96_{k,t-1}} - \underbrace{\ln \frac{pop_{k,t}}{pop_{k,t-1}}}_{=\Delta \ln pop_{k,t-1} \approx 0} \end{aligned} \tag{6}$$

Population changes relatively the slowest over time compared to the other values. Therefore, the second fraction is always very close to 1, meaning that the logarithm is always very close to 0. This is shown in Table 6 when comparing (i) the values on the diagonal and (ii) their (rescaled) squared deviations from zero (see the bottom row): the diagonal tells us $\Delta \ln pop$ has much less variation than any other value and the bottom shows us that the values on average are much closer to zero than all other values. Around sixty percent of the variance in the value based on population $\Delta \ln pop_{k,t}$ is variance between counties.[30] Close to 100% of all variance in all other values in Table 6 is variance within counties (over time).[31] Together with Table 6 this shows that all other values vary much more strongly over time than does the value based on population. Relative to the other values, the logarithm of the growth rate of the population can be considered constant, as it is depicted in Equation (6). Also, for the right-hand side term

[29]Further ample warning about these specifications is given in the methodological literature: Kuh and Meyer (1955); Madansky (1964); Belsley (1972); Casson (1973).

[30]Computed by analysis of variance decomposition of variance: *within*-county variance is variance over time, *between*-county variance is variance between counties.

[31]Actually, an analysis of variance decomposition shows negative between variance for $E95, E955$, and $E96$, which is rare but numerically possible and evidence for very low between variance.

of interest $\beta_1(\Delta \ln E955_{k,t-1} - \Delta \ln E95_{k,t-1})$, the term from the numerator is double the mean squared distance from zero and double the variance than the term from the denominator. This means that in *this specific data set*, taking the first differences *at least partially* removes the numbers causing spurious correlations between ratios. This does *not* mean, however, that taking first differences will solve this problem any time in any data set. Here, it basically removes population

	$\Delta \ln pop$	$\Delta \ln E96$	$\Delta \ln E95_{t-1}$	$\Delta \ln E955_{t-1}$
$\Delta \ln pop$	1.00	0.92	0.69	0.47
$\Delta \ln E96$	0.92	450.92	−0.22	2.81
$\Delta \ln E95_{t-1}$	0.69	−0.22	172.24	171.21
$\Delta \ln E955_{t-1}$	0.47	2.81	171.21	356.52
rescaled mean sum of squares	1.00	289.19	111.32	230.42

Table 6: Covariance matrix rescaled by $s^2_{\Delta \ln pop} = 0.0002409870$, mean sums of squares rescaled by $K^{-1}T^{-1}\sum_k \sum_t (\Delta \ln pop_{k,t})^2 = 0.0003727771$

from all the terms and only the (growth rates) of the numerators remain in the model after taking first differences. Once population is removed from both sides, the right-hand side is no longer able to explain the left hand-side.

5.3 But $E95$ is Not Population

One could now argue $E95_{k,t-1}$ is not population and therefore the results from Cook and Ludwig (2006) *are not* due to the ratio fallacy. When we look at the correlation matrix (Table 7) we immediately see that the correlation between suicides and population is far superior to any other correlation between the left-hand side and the right-hand side, at least in regard to the four variables shown in Table 7. Auxiliary panel regression results in Table 8 support the claim that it

	$\ln pop$	$\ln E96$	$\ln E95_{t-1}$	$\ln E955_{t-1}$
$\ln pop$	1.000	0.676	0.868	0.685
$\ln E96$	0.676	1.000	0.731	0.705
$\ln E95_{t-1}$	0.868	0.731	1.000	0.902
$\ln E955_{t-1}$	0.685	0.705	0.902	1.000

Table 7: Correlation matrix of population and different deaths

is $E95$ driving the results of the coefficient on the FSS proxy.

	Exogenous variable				
Dependent variable	$\ln E95_{k,t-1}$		$\ln E955_{k,t-1}$		R^2_{within}
$\ln pop_{k,t}$	0.24^{***}	(0.047)	-0.05^{***}	(0.010)	0.15
$\ln E96_{k,t}$	0.20^{***}	(0.056)	0.03	(0.029)	0.02

Table 8: Auxiliary two-way fixed effects panel regressions illustrating the explanatory power of the FSS denominator in the model, $^{*}: p < 0.05$, $^{**}: p < 0.01$, $^{***}: p < 0.001$, robust standard errors according to Driscoll and Kraay (1998) computed with `vcovSCC` from Croissant and Millo (2008) in parentheses, R^2 adjusted

5.4 Nonsense Regression Between Time Series

Regression between time series is known to produce spurious results in the following settings: trending or auto correlated time series (Granger and Newbold 1974), I(1) processes without drift (Phillips 1986), I(1) processes with further stationary regressors (Hassler 1996), stationary AR processes (Granger, Hyung and Jeon 2001), random walks with and without drift for fixed effects panel models (Entorf 1997), time-varying means (Hassler 2003), and stationary processes around linear trends (Kim, Lee and Newbold 2004), as well as in fixed effects (or first differences) estimations with weak variation in the time series (Choi 2011). It seems unlikely that none of these situations occurred in the original analysis, and thus there may be more sources for spurious results than just the ratio problem. As noted by C&L themselves (p. 383), there are heterogeneous trends for the dependent variable between counties. This is illustrated in Figure 1. Clearly, a single time dummy is incapable of detrending heterogeneous trends across counties. Therefore, not all trends will be accounted for in C&L's original model. Many of those time-series-related problems are automatically dealt with when taking first differences, while the single time dummy from model (2) is not able to detrend heterogeneous county trends.

5.5 Misspecification of the Original Model

5.5.1 Testing for Misspecification

We can look at the problem in C&L's model from the perspective of linear model theory. When we write out Equation (2) and rearrange the right-hand side of

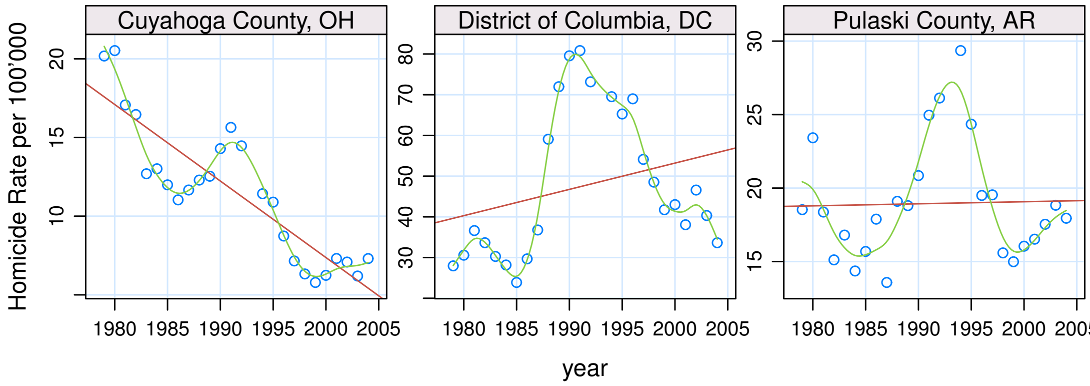

Figure 1: Illustration of heterogeneous time trends of the homicide rate between counties.

Equatin (7), we obtain

$$\begin{aligned}\ln HomR_{k,t} &= \beta_1(\ln E955_{k,t-1} - \ln E95_{k,t-1}) \\ &+ \beta_{2,1}(\ln burglaries_{k,t} - \ln pop_{k,t})\dots \\ &= \beta_{E955}\ln E955_{k,t-1} + \beta_{E95}\ln E95_{k,t-1}) + \beta_{2,1}\ln burglaries_{k,t} + \dots \\ &+ \underbrace{(-\beta_{2,1} - \beta_{2,2} - \beta_{2,3} - \beta_{2,4} - \beta_{2,5} - \beta_{2,6})}_{=\beta_{pop}} \ln pop_{k,t} + \varepsilon_{k,t}.\end{aligned} \tag{7}$$

We see a linear restriction of

$$\begin{bmatrix} \beta_{E955} + \beta_{E95} \\ \beta_{2,1} + \beta_{2,2} + \beta_{2,3} + \beta_{2,4} + \beta_{2,5} + \beta_{2,6} + \beta_{pop} \end{bmatrix} = \begin{bmatrix} 0 \\ 0 \end{bmatrix}, \tag{8}$$

which is rejected with a p-value of 5.372×10^{-14}. The number of households and the population of those five years and older are substituted for by population, as these are nearly perfect correlates.[32] Therefore, the original model seems to be misspecified. When estimation is performed on the first differences, the same restriction as in Equation (8) has to hold. In this case, the null hypothesis is not rejected (p-value of 0.90). This is further evidence that the differentiated model has *in this case* taken the ratio problem out of the data. However, this does not have to be the case for any dataset.

[32]Using the original denominators and testing all four linear hypotheses leads to an even more significant rejection of the null hypothesis.

5.5.2 Theoretical Bias

For a simple univariate linear model[33] on logarithms of ratios with a common denominator

$$\ln y_j - \ln z_j = b_0 + b_1(\ln x_j - \ln z_j) + \varepsilon_j \tag{9}$$
$$\Leftrightarrow \ln y_j - \ln z_j = b_0 + b_x \ln x_j + b_z \ln z_j + \varepsilon_j \text{ with } b_x = -b_z \tag{10}$$

with $j = 1, 2, \ldots, J$ and a linear restriction of $b_x + b_z = 0$ in Equation (10), the bias of the estimator of $b_x = b_1$ can be computed. The bias is known (see Judge et al. 1985: 53, Eq. (3.2.6)) to be

$$(G'G)^{-1}R'[R(G'G)^{-1}R']^{-1}(r - Rb) \tag{11}$$

where $G = \begin{bmatrix} 1_J & \ln X & \ln Z \end{bmatrix}$ is the usual matrix of independent variables in the least squares model and $1_J, \ln X$ and $\ln Z$ are column vectors of 1s, the $\ln x_j$, and the $\ln z_j$. R and r describe the linear restriction

$$\underbrace{\begin{bmatrix} 0 & 1 & 1 \end{bmatrix}}_{=R} \times \underbrace{\begin{bmatrix} b_0 \\ b_x \\ b_z \end{bmatrix}}_{=b} = \underbrace{\begin{bmatrix} 0 \end{bmatrix}}_{=r}. \tag{12}$$

Using

$$(G'G)^{-1} = \begin{bmatrix} c_{1,1} & c_{1,2} & c_{1,3} \\ c_{1,2} & c_{2,2} & c_{2,3} \\ c_{1,3} & c_{2,3} & c_{3,3} \end{bmatrix} \tag{13}$$

the bias for $\hat{b}_x$ given $b_x = 0$ can then be calculated:

$$\text{bias}(\hat{b}_x | b_x = 0) = -\frac{c_{2,2} + c_{2,3}}{c_{2,2} + 2c_{2,3} + c_{3,3}} b_z. \tag{14}$$

Considering all $c_{v,w}$ have the same denominator $\det(G'G)$, the fraction in Equation (14) becomes

$$-\frac{J\sum z^{*2} - (\sum z^*)^2 + J\sum x^* z^* - \sum x^* \sum z^*}{J\sum z^{*2} - (\sum z^*)^2 + 2(J\sum x^* z^* - \sum x^* \sum z^*) + J\sum x^{*2} - (\sum x^*)^2} \tag{15}$$

[33] I use different symbols here so as not to mislead the reader into thinking this is the same model as Equation (2). Furthermore the result can be directly applied to panel estimation. This can be seen from the appropriate transformations (e.g. Baltagi 2008: Sections 2.2, 3.2).

with $z_j^* = \ln z_j, x_j^* = \ln x_j$, and where each sum is on all j and the index has been omitted for readability. This, expanded by J^{-2} collapses to

$$F = -\frac{s_{z^*}^2 + s_{x^*,z^*}}{s_{z^*}^2 + 2s_{x^*,z^*} + s_{x^*}^2} \tag{16}$$

giving the overall expected bias of $b_x = b_1$ as Fb_z solely depending on the variances and covariance of $\ln X, \ln Z$ and the true b_z. This result is well in accordance with an upward biased estimate of β_1 in C&L's original model. Furthermore the implications are quite strong: If neither X nor Z contribute to the outcome Y and all variables are uncorrelated, then b_z takes a value of -1 and from estimating the restricted Model (9) we expect a positive estimate for b_1.

6 Alternative specifications

6.1 Controlling for Population

The first method for removing the spuriousness from C&L's model is given by Kronmal (1993: 390): include the inverse of the deflating variable as an explanatory variable on the right-hand side. In a logarithmic model, this means we can just add the logarithm of the deflating variable.[34] The model then becomes[35]

$$\ln Y_{k,t} = \beta_0 \ln pop_{k,t} + \beta_1 \ln FSS_{k,t-1} + X_{k,t}\beta_2 + d_k + d_t + \varepsilon_{k,t}. \tag{17}$$

The estimation result from this model is set out in the column labeled "Eq. (17)" in Table 9. Across the board, significance weakens considerably and completely disappears from the ratios computed from the best correlates of population in the numerator. This specification takes out the linear restriction on $\ln pop$ known from Equations (7) and (8). The linear restriction in (8) is now less restrictive:

$$\beta_{\ln E955_{k,t-1}} + \beta_{\ln E95_{k,t-1}} = 0. \tag{18}$$

The p-value for this null hypothesis is 0.002195. This still does not account for possibly spurious results due to time-series effects. It is likely that not all the left-hand side time series in the panel can be detrended by a single time dummy. Sections 6.2, 6.3, 6.4, and 6.5 discuss further model specifications.

[34] I know of only one other study on this topic that explicitly addresses this technique: Kleck and Patterson (1993).

[35] I use $\varepsilon_{k,t}$ for the error term multiple times in this section; however, I do not assume it to be identically distributed for all models. Also I reuse β for coefficients with different interpretations. These are not identical across models.

Underlying explanatories	Model			
	Eq. (17) controlling	Eq. (19) rearranged	Eq. (24) risk	Eq. (26) growth
population	−0.48 (0.000)	1.00 (restr.)	−0.000 (0.592)	1.079 (0.198)
suicide measures				
FSS_{t-1}	0.05 (0.082)	0.02 (0.516)	NA	NA
other suicides$_{t-1}$	NA	NA	0.120 (0.196)	−0.012 (0.784)
firearms suicides$_{t-1}$	NA	NA	0.017 (0.781)	−0.001 (0.979)
control variables				
robberies	0.13 (0.002)	0.16 (0.001)	0.002 (0.295)	0.028 (0.547)
burglaries	0.10 (0.005)	0.08 (0.076)	0.005 (0.000)	0.020 (0.677)
blacks	0.19 (0.000)	0.24 (0.000)	0.001 (0.011)	−0.067 (0.841)
urbans	−0.18 (0.314)	0.15 (0.477)	0.000 (0.238)	−0.210 (0.854)
same house 5 years ago	−0.11 (0.645)	0.29 (0.231)	−0.000 (0.287)	−0.349 (0.603)
female headed house	0.33 (0.089)	−0.02 (0.928)	−0.002 0.220	0.410 (0.371)
R^2_{within}	0.075	0.058	0.200	0.002

Table 9: Robustness of the null result to different model specifications eliminating the ratio fallacy. Cells give coefficient estimates and corresponding p-values in parentheses. Robust standard errors according to Driscoll and Kraay (1998) were computed with `vcovSCC` from Croissant and Millo (2008) for significance levels, R^2 adjusted

6.2 Algebraic Transformation of the Estimating Equation

Similar to the approach in Section 6.1 one may transform Equation (5) by adding $\ln pop_{k,t}$ on both sides. This results in

$$\ln E96_{k,t} = \ln pop_{k,t} + \beta_1 \ln FSS_{k,t-1} + X_{k,t}\beta_2 + d_k + d_t + \varepsilon_{k,t}. \quad (19)$$

$\ln pop_{k,t}$ now has a fixed coefficient of $\beta_{pop} = 1$. This technique is known from the Poisson regression model for the analysis of rates.[36] Any spurious correlation between the left-hand side and the right-hand side due to population appearing on both sides is no longer possible. Results are reported in the column labeled "Eq. (19)" in Table 9. There is no significance on FSS_{t-1}. Testing solely for the restriction $\beta_{pop} = 1$ rejects the null hypotheses.[37] Therefore the model appears to be misspecified. Time series problems are not accounted for.

6.3 Risk Model

Duggan (2003: 48–50) proposes a model[38] for explaining individual i's suicide decision[39]

$$\Pr(Suicide_i) = \alpha + X_i\theta + \gamma Gun_i + \lambda_i + \varepsilon_i \quad (20)$$

with X_i being individual observable controls, Gun_i a dummy for gun ownership, and λ_i individual i's unobserved individual propensity to commit suicide. Assume that a gun owner chooses suicide by firearm with a probability > 0. Then, as long as λ_i is not negatively correlated with gun ownership and as long as $\gamma \geq 0$, gun ownership will be associated with a higher probability of committing suicide at all or just with a higher probability of committing suicide by firearm.[40] For the limiting case of zero correlation between Gun_i and λ_i the relative risk of a gun owner becomes:[41]

$$RR_{Gun} = \frac{\alpha + X_i\theta + \gamma Gun_i + \varepsilon_i}{\alpha + X_i\theta + \varepsilon_i} \geq 1 \quad (21)$$

and the expected number of (firearm) suicides for a population of size pop will be

$$\mathcal{E}[E95] = pop \cdot (\alpha + \overline{X}\theta + \gamma \cdot \Pr(Gun)), \quad (22)$$

$$\mathcal{E}[E955] = pop \cdot \Pr(GunSuic|Suicide, Gun) \cdot \Pr(Suicide|Gun) \cdot \Pr(Gun) \quad (23)$$

[36] See Osgood (2000: 23–27).

[37] p-value of 8.22×10^{-12}.

[38] I slightly deviate from Duggan's notation without changing the model to keep my formulas simpler.

[39] This is a linear probability model. The parameters must satisfy the requirement of $0 \leq \Pr(Suicide_i) \leq 1 \forall i$. The following argument holds for other monotonous link functions as well.

[40] $\partial \Pr(GunSuic_i|Suicide_i)/\partial Gun_i > 0$.

[41] There is no evidence in the literature contradicting these assumptions.

under the simplifying assumption that only gun owners are able to commit suicide by firearm. When the X_i are dummies, $pop \cdot \overline{X}$ will become count data for those dummies (that is, numbers of people with certain characteristics). The relative risk notion in Equation (21) gives a very clear interpretation to the coefficients in this model.

I go in the opposite direction, and start at the macro level by proposing a "risk model" (coefficients do not match the identically named coefficients in equations (20), (21) and (22)) for *homicides* that can be estimated solely on differences in the counts, thus removing the potential for spuriousness due to time series:

$$\Delta E96_{k,t} = \beta_0 \Delta pop_{k,t} + \beta_{1,1} \Delta E955_{k,t-1} + \beta_{1,2} \Delta E95_{k,t-1} + \Delta X_{k,t} \beta_2 + \delta_t + \varepsilon_{k,t}, \quad (24)$$

where $X_{k,t}$ now contains the numerators' values of the control ratios used by C&L. $E955_{k,t-1}$ now is the gun proxy – which, given Duggan's (2003) model, should be positively correlated to the number of gun owners – and the number of non-firearm suicides is used as an additional control. Then $\beta_{1,1}$ should be positive if crime increases with more guns. Let us say β_0 is one person's baseline risk of becoming a victim/committing a homicide. Now attribute an additional risk to each gun owner,[42] then a relation of

$$\frac{\beta_0 + \beta_{1,1}}{\beta_0} \sim RR_{gunowner}, \quad (25)$$

exists, given the number of firearm suicides is somehow linked to the number of gun owners. Results are reported in the column labeled "Eq. (24)" of Table 9. There is no significance on the variable of interest ($E955$). Significance on the other variables must not be over interpreted. For example, for burglaries, it might just mean there are around 170 times as many burglaries as homicides. This model is susceptible to criticism for obvious heteroscedasticity across counties with different levels of population. Standardization would be helpful. Also multicollinearity might be an issue, as all numbers used are part of the population and therefore are in $pop_{k,t}$.

6.4 Growth Model

A way to standardize without using ratios is to use growth rates. Putting the (logarithm of) the growth rate of homicides on the left-hand side yields the following model:

$$\ln \frac{E96_{k,t}}{E96_{k,t-1}} = \beta_0 \ln \frac{pop_{k,t}}{pop_{k,t-1}} + \beta_1 \ln \frac{E955_{k,t-1}}{E955_{k,t-2}} + X_{k,t} \beta_2 + \varepsilon_{k,t}, \quad (26)$$

[42] Either as a victim or as the perpetrator or by imposing an externality upon the remaining population.

where $X_{k,t}$ contains the log growth rates for the controls. As in the risk model, non-firearm suicides can be added as a control. Results are reported in the column labeled “Eq. (26)” in Table 9; no significance is observed.[43]

6.5 Numerators and Denominators

To check where the explanatory power in C&L’s model comes from, a comparison of the following models seems appropriate:

$$
\begin{aligned}
\ln pop_{k,t} &= \beta_1 \ln E95_{k,t-1} + d_k + d_t + \varepsilon_{k,t} && (27)\\
\ln pop_{k,t} &= \beta_1 \ln E955_{k,t-1} + \beta_{2,1} \ln burglaries_{k,t} + \beta_{2,2} \ln robberies_{k,t} \\
&\quad + \beta_{2,3} \ln blacks_{k,t} + \beta_{2,4} \ln urbans_{k,t} + \beta_{2,5} \ln 5yearResidents_{k,t} && (28)\\
&\quad + \beta_{2,6} \ln fhh_{k,t} + d_k + d_t + \varepsilon_{k,t}. \\
\ln E96_{k,t} &= \beta_1 \ln E95_{k,t-1} + \beta_3 \ln pop_{k,t} + d_k + d_t + \varepsilon_{k,t} && (29)\\
\ln E96_{k,t} &= \beta_1 \ln E955_{k,t-1} + \beta_{2,1} \ln burglaries_{k,t} + \beta_{2,2} \ln robberies_{k,t} \\
&\quad + \beta_{2,3} \ln blacks_{k,t} + \beta_{2,4} \ln urbans_{k,t} + \beta_{2,5} \ln 5yearResidents_{k,t} && (30)\\
&\quad + \beta_{2,6} \ln fhh_{k,t} + d_k + d_t + \varepsilon_{k,t}.
\end{aligned}
$$

These models allow cross-checking whether the right-hand side numerators actually explain the left-hand side numerator as intended, or whether some other mechanism is driving the results. In Equation (27) there is only one right-hand side term. Due to nearly perfect correlation with $pop^{-1}_{k,t}$, I removed $pop5plus^{-1}_{k,t}$ and $households^{-1}_{k,t}$ from the model. Including $pop^{-1}_{k,t}$ on the right-hand side is obviously ridiculous. The results are given in Table 10. The comparison of within R^2s

	Model			
	lhs: pop		lhs: $E96$	
	Equation (27)	Equation (28)	Equation (29)	Equation (30)
R^2_{within}	0.1492	0.9542	0.0322	0.0818
	Modifications of equation (28)			
	lhs: pop		lhs: $E96/pop$	
rhs	only numerators	ratios	only numerators	ratios
R^2_{within}	0.9542	0.2790	0.0810	0.0635

Table 10: Diagnostics for finding the source of “explanatory power” in the original model, R^2 adjusted

clearly shows that Equations (27) and (28) each display a larger coefficient of determination than either Equation (29) or Equation (30). The very high within R^2

[43] For state-level data, this model yields a negative coefficient on the order of 0.3 on the gun proxy, significant at the 5% level. This is the only setting showing this result.

of 0.9542 for Equation (28) reduces to 0.0810 when the left-hand side is changed to $E96/pop$, to 0.2790 when the right-hand side is changed to ratios, and, finally, to 0.06 when lhs and rhs are changed to the full model from Equation (2) (Table 10, second row). Thus, from a goodness-of-fit point of view, the only thing C&L's full model does, is add a lot of noise to Equation (28). This adds additional support to the already strong theoretical and quantitative argument in Section 5 that the results of the original analysis were driven by the ratio fallacy. The coefficients of these estimations have no useful interpretation; this was purely an illustrative exercise to show what is driving the results in the original paper.

7 Conclusion

Other aspects of C&L's analysis could be addressed. Namely, (i) their data suffer from truncation for observations with zeroes in the numerators[44] and (ii) the FSS proxy is very noisy for smaller counties: Imagine a county in year $t-1$ having 20 suicides, one with a gun, and in t 20 suicides, eight with guns.[45] Surely gun ownership did not increase proportional to this increase in FSS. This problem could be addressed by using moving averages.[46] However, as their results are purely an effect of a technical property of the data, further criticism of the study seems unwarranted. Which model should they have chosen? This remains an open question but given that none yields significance or anything close to it for the parameter of interest, finding the "correct" model becomes somewhat of a moot point. Of much more relevance, especially when it comes to the sensitive topic of gun control, is to discover how many studies on this topic have ignored the ratio problem?

References

Ayres, Ian, and John J Donohue. 2003*a*. "The Latest Misfires in Support of the 'More Guns, Less Crime' Hypothesis." *Stanford Law Review*, 55: 1371.

Ayres, Ian, and John J Donohue. 2003*b*. "Shooting Down the "More Guns, Less Crime" Hypothesis." *Stanford Law Review*, 1193–1312.

Azrael, Deborah, Philip J Cook, and Matthew Miller. 2004. "State and Local Prevalence of Firearms Ownership Measurement, Structure, and Trends." *Journal of Quantitative Criminology*, 20(1): 43–62.

[44]This can be a severe problem for consistency as discussed in Hayashi (2000: Sections 5.3, 8.2).

[45]This is the case for Union County, NJ and $t = 1998$.

[46]I did, in fact, try this and all my findings remain robust to using moving averages; results, data, and code available upon request.

Baltagi, Badi. 2008. *Econometric Analysis of Panel Data.* Wiley.

Belsley, David A. 1972. "Specification with Deflated Variables and Specious Spurious Correlation." *Econometrica*, 923–927.

Casson, MC. 1973. "Linear regression with error in the deflating variable." *Econometrica*, 751–759.

Choi, In. 2011. "Spurious Fixed Effects Regression." *Oxford Bulletin of Economics and Statistics*.

Cook, Philip J, and Jens Ludwig. 2002. "The effects of Gun Prevalence on Burglary: Deterrence vs Inducement." *NBER Working Paper Series*, 8926.

Cook, Philip J, and Jens Ludwig. 2003. "Guns and Burglary." *Evaluating Gun Policy: Effects on Crime and Violence*, 74–106.

Cook, Philip J, and Jens Ludwig. 2006. "The social costs of gun ownership." *Journal of Public Economics*, 90: 379–391.

Cook, Philip J, Jens Ludwig, Sudhir Venkatesh, and Anthony A Braga. 2007. "Underground Gun Markets." *The Economic Journal*, 117(524): F558–F588.

Cook, Phillip J, and Jens Ludwig. 2004. "The social costs of gun ownership." *NBER Working Paper Series*, 10736.

Croissant, Yves, and Giovanni Millo. 2008. "Panel Data Econometrics in R: The plm Package." *Journal of Statistical Software*, 27(2).

Donohue, John J. 2003. "The Final Bullet in the Body of the More Guns, Less Crime Hypothesis." *Criminology & Public Policy*, 2(3): 397–410.

Driscoll, John C, and Aart C Kraay. 1998. "Consistent Covariance Matrix Estimation with Spatially Dependent Panel Data." *Review of Economics and Statistics*, 80(4): 549–560.

Duggan, Mark. 2001. "More Guns, More Crime." *The Journal of Political Economy*, 109(5): 1086–1114.

Duggan, Mark. 2003. "Guns and Suicide." *Evaluating Gun Policy: Effects on Crime and Violence*, 41–73.

Entorf, Horst. 1997. "Random walks with drifts: Nonsense regression and spurious fixed-effect estimation." *Journal of Econometrics*, 80(2): 287–296.

Fox, James Alan, and David McDowall. 2008. "Brief of Professors of Criminal Justice As Amici Curiae in Support of Petitioners." *Submitted to the Surpreme Court of the United States in District of Columbia and Adrian M. Fenty, Mayor of the District of Columbia v. Dick Anthony Heller*, No. 07-290, Jan. 11, 2008.

Granger, Clive WJ, and P Newbold. 1974. "Spurious Regressions in Econometrics." *Journal of Econometrics*, 2(2): 111–120.

Granger, Clive WJ, Namwon Hyung, and Yongil Jeon. 2001. "Spurious regressions with stationary series." *Applied Economics*, 33(7): 899–904.

Grothendieck, G. 2011. "sqldf: Perform SQL Selects on R Data Frames." R package version 0.4-6.1.

Hassler, Uwe. 1996. "Spurious regressions when stationary regressors are included." *Economics Letters*, 50(1): 25–31.

Hassler, Uwe. 2003. "Nonsense regressions due to neglected time-varying means." *Statistical Papers*, 44(2): 169–182.

Hayashi, Fumio. 2000. *Econometrics.* Princeton University Press.

Judge, George G, WE Griffiths, R Carter Hill, Helmut Lütkepohl, and Tsoung-Chao Lee. 1985. *The Theory and Practice of Econometrics.* New York:Wiley.

Kellermann, Arthur L, Frederick P Rivara, Grant Somes, Donald T Reay, Jerry Francisco, Joyce Gillentine Banton, Janice Prodzinski, Corinne Fligner, and Bela B Hackman. 1992. "Suicide in the home in relation to gun ownership." *New England Journal of Medicine*, 327(7): 467–472.

Kim, Tae-Hwan, Young-Sook Lee, and Paul Newbold. 2004. "Spurious regressions with stationary processes around linear trends." *Economics Letters*, 83(2): 257–262.

Kleck, Gary. 2004. "Measures of Gun Ownership Levels for Macro-level Crime and Violence Research." *Journal of Research in Crime and Delinquency*, 41(1): 3–36.

Kleck, Gary. 2009. "How Not to Study the Effect of Gun Levels on Violence Rates." *Journal on Firearms & Public Policy*, 21: 65.

Kleck, Gary, and E Britt Patterson. 1993. "The Impact of Gun Control and Gun Ownership Levels on Violence Rates." *Journal of Quantitative Criminology*, 9(3): 249–287.

Klieve, Helen, Jerneja Sveticic, and Diego De Leo. 2009. "Who uses firearms as a means of suicide? A population study exploring firearm accessibility and method choice." *BMC Medicine*, 7(1): 52.

Klieve, Helen, Michael Barnes, and Diego De Leo. 2009. "Controlling firearms use in Australia: has the 1996 gun law reform produced the decrease in rates of suicide with this method?" *Social Psychiatry and Psychiatric Epidemiology*, 44(4): 285–292.

Kronmal, Richard A. 1993. "Spurious Correlation and the Fallacy of the Ratio Standard Revisited." *Journal of the Royal Statistical Society. Series A (Statistics in Society)*, 156(3): 379–392.

Kuh, Edwin, and John R Meyer. 1955. "Correlation and Regression Estimates when the Data are Ratios." *Econometrica*, 400–416.

Leenaars, Antoon A, and David Lester. 2001. "The impact of gun control (Bill C-51) on homicide in Canada." *Journal of Criminal Justice*, 29(4): 287–294.

Leenaars, Antoon A, Ferenc Moksony, David Lester, and Susanne Wenckstern. 2003. "The Impact of Gun Control (Bill C-51) on Suicide in Canada." *Death Studies*, 27(2): 103–124.

Leigh, Andrew, and Christine Neill. 2010. "Do Gun Buybacks Save Lives? Evidence from Panel Data." *American Law and Economics Review*, 12(2): 462–508.

Lott, John R. 2010. *More Guns, Less Crime: Understanding Crime and Gun Control Laws.* . 3rd ed., University of Chicago Press.

Lott, John R, and David B Mustard. 1997. "Crime, Deterrence, and Right-to-carry Concealed Handguns." *The Journal of Legal Studies*, 26(1): 1–68.

Ludwig, Jens. 1998. "Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data." *International Review of Law and Economics*, 18(3): 239–254.

Madansky, Albert. 1964. "Spurious Correlation Due to Deflating Variables." *Econometrica*, 652–655.

Moody, Carlisle E, and Thomas B Marvell. 2010. "On the Choice of Control Variables in the Crime Equation." *Oxford Bulletin of Economics and Statistics*, 72(5): 696–715.

Osgood, D. Wayne. 2000. "Poisson-Based Regression Analysis of Aggregate Crime Rates." *Journal of Quantitative Criminology*, 16(1): 21–43.

Pearson, K. 1896. "Mathematical Contributions to the Theory of Evolution. – On a Form of Spurious Correlation Which May Arise When Indices Are Used in the Measurement of Organs." *Proceedings of the Royal Society of London*, 60(359-367): 489–498.

Phillips, Peter CB. 1986. "Understanding spurious regressions in econometrics." *Journal of Econometrics*, 33(3): 311–340.

R Development Core Team. 2011. "R: A Language and Environment for Statistical Computing." Vienna, Austria, R Foundation for Statistical Computing, ISBN 3-900051-07-0.

United States Census Bureau. 1980. "1980 Census of Population and Housing, Summary Tape File 3A."

United States Census Bureau. 1990. "1990 Census of Population and Housing, Summary Tape File 3A."

United States Census Bureau. 2000. "2000 Census of Population and Housing."

United States Department of Commerce. Bureau of the Census. 1993. *Census of Population and Housing, 1990 [United States]: Summary Tape File 3C.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR06054.v1.

United States Department of Health and Human Services. 2010. "Compressed Mortality File (CMF) on CDC WONDER Online Database."

United States Department of Health and Human Services. Centers for Disease Control and Prevention. National Center for Health Statistics. 2010. *Mortality Detail Files, 1968-1991.* Inter-University Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR07632.v4.

United States Department of Health and Human Services. National Center for Health Statistics. 1997. *Mortality Detail File, 1992.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR06798.v1.

United States Department of Health and Human Services. National Center for Health Statistics. 2007*a*. *Multiple Cause of Death, 1998.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR03306.v2.

United States Department of Health and Human Services. National Center for Health Statistics. 2007*b*. *Multiple Cause of Death, 1999.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR03473.v2.

United States Department of Health and Human Services. National Center for Health Statistics. 2007*c*. *Multiple Cause of Death Public Use Files, 2000-2002.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR04640.v1.

United States Department of Health and Human Services. National Center for Health Statistics. 2007*d*. *Multiple Cause of Death Public Use Files, 2003.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR20540.v1.

United States Department of Health and Human Services. National Center for Health Statistics. 2007*e*. *Multiple Cause of Death Public Use Files, 2004.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR20623.v1.

United States Department of Health and Human Services. National Center for Health Statistics. 2008*a*. *Multiple Cause of Death, 1993.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR06799.v1.

United States Department of Health and Human Services. National Center for Health Statistics. 2008*b*. *Multiple Cause of Death, 1994.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR02201.v2.

United States Department of Health and Human Services. National Center for Health Statistics. 2008*c*. *Multiple Cause of Death, 1997.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR03085.v2.

United States Department of Health and Human Services. National Center for Health Statistics. 2008*d*. *Multiple Cause of Death Public Use Files, 2005.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR22040.v1.

United States Department of Health and Human Services. National Center for Health Statistics. 2009*a*. *Multiple Cause of Death, 1995.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR02392.v2.

United States Department of Health and Human Services. National Center for Health Statistics. 2009*b*. *Multiple Cause of Death, 1996*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR02702.v2.

United States Department of Justice. Federal Bureau of Investigation. 2005. *Uniform Crime Reports: County Level Arrest and Offense Data, 1986*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR09119.v2.

United States Department of Justice. Federal Bureau of Investigation. 2006*a*. *Uniform Crime Reporting Program Data [United States]: County Level Arrest and Offenses Data, 1977-1983*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR08703.v1.

United States Department of Justice. Federal Bureau of Investigation. 2006*b*. *Uniform Crime Reporting Program Data [United States]: County Level Arrest and Offenses Data, 1984*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR08714.v2.

United States Department of Justice. Federal Bureau of Investigation. 2006*c*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 1989*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR09573.v1.

United States Department of Justice. Federal Bureau of Investigation. 2006*d*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 1990*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR09785.v1.

United States Department of Justice. Federal Bureau of Investigation. 2006*e*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 1991*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR06036.v1.

United States Department of Justice. Federal Bureau of Investigation. 2006*f*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 1992*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR06316.v1.

United States Department of Justice. Federal Bureau of Investigation. 2006*g*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 1994*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR06669.v3.

United States Department of Justice. Federal Bureau of Investigation. 2006*h*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 1995*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR06850.v2.

United States Department of Justice. Federal Bureau of Investigation. 2006*i*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 1996*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR02389.v3.

United States Department of Justice. Federal Bureau of Investigation. 2006*j*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 1997*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR02764.v2.

United States Department of Justice. Federal Bureau of Investigation. 2006*k*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 1998*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR02910.v2.

United States Department of Justice. Federal Bureau of Investigation. 2006*l*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 1999*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR03167.v4.

United States Department of Justice. Federal Bureau of Investigation. 2006*m*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 2000*. Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR03451.v4.

United States Department of Justice. Federal Bureau of Investigation. 2006*n*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 2001.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR03721.v2.

United States Department of Justice. Federal Bureau of Investigation. 2006*o*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 2002.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR04009.v2.

United States Department of Justice. Federal Bureau of Investigation. 2006*p*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 2003.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR04360.v2.

United States Department of Justice. Federal Bureau of Investigation. 2006*q*. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 2004.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR04466.v1.

United States Department of Justice. Federal Bureau of Investigation. 2006*r*. *Uniform Crime Reports: County Level Detailed Arrest and Offense Data, 1985 and 1987.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR09252.v1.

United States Department of Justice. Federal Bureau of Investigation. 2006*s*. *Uniform Crime Reports: County Level Detailed Arrest and Offense Data, 1988.* Inter-university Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR09335.v1.

United States Department of Justice. Federal Bureau of Investigation. 2008. *Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 1993.* Inter-University Consortium for Political and Social Research (ICPSR) [distributor]. http://dx.doi.org/10.3886/ICPSR06545.v1.

Westphal, Christian. 2013. “Personal Website.” `http://www.westphal.de`.

Wooldridge, Jeffrey M. 2002. *Econometric Analysis of Cross Section and Panel Data.* MIT Press.

The Social Costs of Gun Ownership Revisited

Bernd Hayo, Florian Neumeier, and Christian Westphal

Philipps-University Marburg

This version: May 15th, 2013

Corresponding author:

Bernd Hayo
Faculty of Business Administration and Economics
Philipps University Marburg
D-35032 Marburg
Germany
Phone: +49–6421–2823091
Email: hayo@wiwi.uni-marburg.de

* Thanks to participants of research seminars held in Göttingen and Marburg for their helpful comments on earlier versions of the paper. The usual disclaimer applies.

The Social Costs of Gun Ownership Revisited

Abstract

Cook and Ludwig (2006) use data on homicide rates and gun prevalence proxies from U.S. counties over the period 1980–1999 and, in their panel data analysis, find a positive and statistically significant association between both variables. We reexamine their analysis and show that their findings are driven by spurious correlations arising from the use of a common denominator (ratio fallacy) to deflate both dependent and independent variables as well as unaccounted county-level time trends. When we attempt to replicate their results accounting for these issues, we no longer find any evidence that gun ownership is linked to homicides.

JEL: H21; I18; K42

Keywords: Gun ownership; Social costs; Ratio fallacy; Spurious correlation

1. Introduction

Scientific research is become an increasingly important component of the gun debate in the United States (see, e.g., Fox and McDowall, 2008). In an influential paper, Cook and Ludwig (2006; C&L hereafter) find a significantly positive relationship between gun prevalence and homicides in a panel data analysis based on U.S. counties for the period from 1980 to 1999. They conclude that "an increase in gun prevalence causes an intensification of criminal violence—a shift toward greater lethality, and hence greater harm to the community" (Cook and Ludwig, 2006: 387). This study has been used as evidence in favor of gun control (Cook et al., 2008: Part I; Leigh and Neill, 2010: 514).

We argue that C&L's analysis suffers from a spurious correlation problem arising out of the use of ratios with common (or highly correlated) denominators on both sides of the regression equation (ratio fallacy). Moreover, their failure to account for county-level time trends is potentially problematic. In Section 2 of this comment, we conceptually explain the fallacies involved in C&L's empirical approach. Section 3 is concerned with replicating their results and Section 4 addresses the potential spurious correlation issue. Our empirical findings suggest that C&L's results are indeed spurious and that no policy recommendations involving gun control should be made based on their dataset.

2. Reconsidering Cook's and Ludwig's Approach

In their paper, C&L analyze the impact of gun prevalence on homicides in U.S. counties in order to evaluate the social costs associated with private gun ownership. They consider the following empirical model:

$$(1)\ \log\left(\frac{homicides_{i,t}}{population_{i,t}}\right) = \beta_0 + \beta_1 \log FSS_{i,t-1} + \beta_2 \log(X_{i,t}) + d_i + d_t + \varepsilon_{i,t}$$

The left-hand side variable is the number of homicides in county i at time t divided by population. C&L's main variable of interest, $FSS_{i,t-1}$, is defined as the share of suicides committed by firearms out of total number of suicides. It is employed as a proxy for gun prevalence. The vector of control variables X contains the prevalence of blacks, robberies, burglaries, the share of households headed by females, urban residence, and residents living in the same house five years ago, all in logs. Before taking logs, all control variables are divided by the respective county's population at time t. To account for heteroscedasticity, the observations are weighted by each county's population. The point estimate of β_1 reported by C&L is about 0.09 when the full set of controls is employed and statistically significant at the 5% level. However, the above empirical specification has several problems.

First, caution is required when ratios are used in regression analyses (Pearson, 1896). Kronmal (1993) proves that if the dependent variable and the independent variables are divided by a common denominator, then least squares estimates are biased. C&L's empirical specification is a special case, which can be seen after transforming Equation (1):

$$
\begin{aligned}
(1')\ \log\left(homicides_{i,t}\right) - \log\left(population_{i,t}\right) \\
&= \beta_0 + \beta_1[\log(firearm\ suicides_{i,t-1}) - \log(suicides_{i,t-1})] \\
&+ \beta_{2,1}[\log(robberies_{i,t}) - log(population_{i,t})] + \beta_{2,2}[\log(burglaries_{i,t}) \\
&- log(population_{i,t})] + \cdots + \varepsilon_{i,t}
\end{aligned}
$$

Due to the application of logarithms, one of the terms on the left-hand side appears multiple times on the right-hand side of the equation as a linear term. Thus, when estimating this model by OLS, the coefficient estimates will, at least partially, capture the effect of population being explained by itself.

In defense of C&L's approach, it could be pointed out that their main variable of interest, *FSS*, is standardized by the number of suicides in a county and not by population, so that their conclusions could still be valid even in the presence of a ratio fallacy. This is not a convincing argument, however, as (i) any source of endogeneity may affect all estimates (except under the empirically irrelevant condition of orthogonality) and (ii) population and the denominator of *FSS*, i.e., the number of suicides, are highly correlated (correlation coefficient from the pooled cross-section = 0.91). For these reasons, it is likely that C&L's estimate of β_1 is biased.

Second, C&L's empirical specification raises concerns about spurious regressions, which arise from the use of nonstationary time series. Although the data cover 20 years and deterministic and stochastic trends could be present, C&L do not address this concern. In fact—as the authors themselves point out—there is a convergence between high- and low-gun ownership areas during the sample period, indicating the existence of county-specific time trends (Cook and Ludwig, 2006: 381). Given that there are also heterogeneous trends in homicide rates, C&L's inclusion of county-invariant time fixed effects is not sufficient to rule out the potential for spurious regressions.

3. Replication of Cook's and Ludwig's Analysis

We commence our analysis by attempting to replicate C&L's findings and extract our data from the same sources employed by them (see Cook and Ludwig 2004: Appendix 3).[1] Unfortunately, data on burglaries and robberies, which C&L take from the FBI's Uniform

[1] Sociodemographic controls for 1990 are directly taken from the Census Bureau's 1990 census STF3A and not via the ICPSR study dataset 06054 as in C&L. Following C&L, we exclude the observation from Oklahoma County in 1995.

Crime Reports, are currently not available for the year 1993. Thus, the number of observations decreases when we employ these controls. We estimate Equation (1) with and without controls using exactly the same empirical specification as C&L do, i.e., we consider only the 200 largest U.S. counties, focus on the period from 1980 to 1999, take logs of all variables, include county and time fixed effects, weigh our estimates by each county's population, and report Huber-White standard errors.

The estimation results of Equation (1) are presented in Table 1. Columns (1a) and (1b) contain C&L's original estimates; the estimates reported in columns (1c) and (1d) are based on our own calculations. For whatever reason, we are not able to perfectly replicate C&L's findings.[2] However, in most cases, our estimates are reasonably close and the main variable of interest is statistically significant across both specifications.[3]

Table 1: Estimation Results for Equation (1).

	Estimates by C&L		**Own Estimations**	
	(1a)	**(1b)**	**(1c)**	**(1d)**
Main interest variables				
Firearm suicides (*t*–1)	0.100**	0.086**	0.071**	0.068*
	(0.044)	(0.038)	(0.036)	(0.036)
Control variables				
Robberies (*t*)		0.149***		0.041
		(0.042)		(0.033)
Burglaries (*t*)		0.226***		0.143***
		(0.072)		(0.056)
Blacks (*t*)		0.278*		0.156**
		(0.164)		(0.065)
Urbanization (*t*)		–0.537***		–0.432*
		(0.157)		(0.243)
Same house (*t*)		–0.690		–0.686***
		(0.419)		(0.227)
Female HH (*t*)		–0.303		0.741***
		(0.413)		(0.175)
R^2	0.915	0.923	0.912	0.916
N	3822	3822	3824	3630

Notes: Columns (1a) and (1b) contain the original estimates reported in Cook and Ludwig (2006), Table 2. Estimates in columns (1c) and (1d) are based on our own estimations. Huber-White standard errors are reported in parentheses. */**/*** indicate statistical significance at the 10%/5%/1% level.

[2] Unfortunately, we could not use Cook and Ludwig's original dataset and Stata codes. In our replication attempt, we modified our specification in several ways in order to achieve results matching those reported by C&L. For instance, we use population data from different sources for our analysis and apply different weights (e.g., average population, log population). However, our findings do not improve in this regard.

[3] Note that our sample contains two observations more than does C&L's. Our findings remain robust if we include the years from 2000 to 2004 in our sample and if we employ a balanced panel data set in which all counties with at least one missing observation are excluded.

4. Addressing Concerns Relating to Ratio Fallacy and Nonstationarity

In this section, we look at how C&L's findings are affected if the concerns regarding ratio fallacy and nonstationarity outlined in Section 2 are taken seriously.

To exemplify the problem that arises from using a common (or highly correlated) denominator, we estimate a model that is very similar to the one set up in Equation (1):

$$(2)\log(population_{i,t}) = \beta_0 + \beta_1 \log(suicides_{i,t-1}) + d_i + d_t + \varepsilon_{i,t}$$

The difference between the original C&L specification and Equation (2) is that we omit the numerators of the dependent variable and the gun prevalence proxy, i.e., the number of homicides and suicides committed by firearms, respectively. The results are presented in Model 2 of Table 2.[4] The point estimate of β_1 is 0.12, i.e., close to C&L's estimate, and significant at every reasonable level of significance. Moreover, model fit, as given by R^2, is notably higher than in models (1a) and (1c).[5] This result suggests that the significant coefficient of C&L's main variable of interest is primarily driven by the denominators, whereas the numerators only add estimation noise. Thus, our findings suggest that C&L's result is based on a ratio fallacy.

A straightforward way to avoid the problems associated with the use of a highly correlated denominator is to refrain from using ratios, as shown in Equation (3):

$$(3)\ \log(homicides_{i,t}) = \beta_0 + \beta_1 \log(fsuicides_{i,t-1}) + \beta_2 \log(pop_{i,t}) + d_i + d_t + \varepsilon_{i,t}$$

In this specification, it is necessary to control for population size so as to avoid an omitted variable bias: the number of homicides and suicides committed by firearms is higher in larger counties, which is why both variables will be positively correlated even if there is no direct association between them (see Section 2).[6] Note, however, that this specification does not eliminate potential stochastic and deterministic trends, which could still lead to a spurious regression. The most parsimonious way to address both ratio fallacy and spurious regression concerns at the same time is to compute log growth rates:

$$(4)\ \Delta \log(homicides_{i,t}) = \beta_0 + \beta_1 \Delta \log(firearm\ suicides_{i,t-1}) + d_t + \varepsilon_{i,t}$$

The estimation results for Equations (3) and (4) are presented in the second and third columns of Table 2.

Both modifications of the empirical specification have a large impact on estimates of the gun prevalence proxy. There is no longer a statistically significant association between the number

[4] We omitted all controls from the following specifications to make sure that our estimates are based on the same sample as employed by C&L. Note that our findings do not change when the full set of controls is employed. All omitted results are available on request.

[5] This difference in model fit is even more pronounced if within-R^2 is considered instead of total R^2. The within-R^2 is only 0.016 for model (1c) compared to 0.214 for model (2).

[6] Still, our results hardly change when population size is omitted from our specification.

of homicides and firearm suicides. The p-values corresponding to the estimates of β_1 in Models 3 and 4 are 0.50 and 0.52, respectively.[7]

Table 2: Estimation Results for Equations (2), (3), and (4).

	(2) Log Population	**(3) Log Homicides**	**(4) ΔLog Homicides**
Main interest variables			
Firearm suicides (*t*–1)		0.024	–0.013
		(0.036)	(0.019)
Suicides (*t*–1)	0.120***		
	(0.018)		
Control variables			
Population (*t*)		0.506***	
		(0.083)	
R^2	0.996	0.974	0.041
N	3824	3824	3714

Notes: In model (2), the dependent variable is log($population_t$) and the gun prevalence proxy is replaced by log($suicides_{t-1}$). In model (3), the dependent variable is log($homicides_t$) and the independent variable log($firearm\ suicides_{t-1}$). In model (4), log growth rates are computed. Huber-White standard errors are reported in parentheses. */**/*** indicate statistical significance at the 10%/5%/1% level.

In an alternative specification, C&L use the share of homicides committed by firearms as the dependent variable and again find a significant impact of the gun prevalence proxy. When we make this modification to our specification, we, too, replicate our previous results: once the ratio fallacy is addressed, no statistically significant positive association can be found.[8] In fact, in the case of log growth rates, we even find a significantly negative impact of gun prevalence on firearm homicides at the 5% level of significance. Thus, we believe that our analysis provides substantial evidence that the positive association between homicides and gun prevalence is a statistical artifact.

5. Conclusion

Cook and Ludwig's (2006) findings on the association between the number of homicides and gun prevalence in U.S. counties have not only been widely acknowledged in the scientific community, but are also used in policy discussion as an argument for more gun control. We suggest that their results are based on a misspecification of the empirical model and subject to ratio fallacy and spurious regression. After appropriately adjusting the empirical model to take

[7] Note that this finding is robust to omitting population from the regression.
[8] Results available on request.

these issues into account, we conclude that gun ownership has no significant impact on homicides in U.S. counties over the period 1980 to 2004.

References

Cook, P.J. and Ludwig, J. (2004). The social costs of gun ownership, *NBER Working Paper* No. 10736.

Cook, P. and Ludwig, J. (2006), The social costs of gun ownership, *Journal of Public Economics* 90, 379–391.

Cook, P. J., Ludwig, J., and Samaha, A. M. (2008), Gun control after *Heller*: Threats and sideshows from a social welfare perspective, *UCLA Law Review* 56, 1041–1094.

Fox, J. A. and McDowall, D. (2008), Brief of professors of criminal justice as amici curiae in support of petitioners, downloaded from: http://www.supremecourt.gov/opinions/07pdf/07-290.pdf (03/01/2013).

Kronmal, R. (1993). Spurious correlation and the fallacy of the ratio standard revisited, *Journal of the Royal Statistical Society Series A* 156(3), 379–392.

Leigh, A. and Neill, C. (2010), Do gun buybacks save lives? Evidence from panel data, *American Law and Economics Review* 12(2), 462–508.

Pearson, K. (1896), Mathematical contributions to the theory of evolution. On a form of spurious correlation which may arise when indices are used in the measurement of organs, *Proceedings of the Royal Society of London* 60, 489–498.

Evidence for the "Suicide by Firearm" Proxy for Gun Ownership from Austria[a]

Christian Westphal[b]

This version: July 29, 2013

Abstract

When attempting to measure gun ownership in the United States, the problem of missing administrative data arises, making it necessary to find a valid proxy. Several such proxies are employed in economic studies, one of which is the fraction of "suicides by firearm" of "all suicides" (*FSS*). My work validates this proxy from out-of-sample data, namely, Austrian administrative data on firearm licenses. I also reevaluate, with appropriate statistical methods, a result on firearms and suicide from the medical that is often used for public policy advocacy. This result is, unfortunately, heavily biased due to ignoring a well-known fallacy and thus can be only partially confirmed.

JEL Classifications: C15; C51; I18
Keywords: Gun Ownership; Suicide; Ratio Fallacy; Spurious Correlation

[a]Thanks to Matthew Lang for sharing his data on suicide and firearms in the United States with me. This allowed me to perfectly replicate his findings.

[b]University of Marburg, Faculty of Business Administration and Economics, Department of Statistics, `westphal@staff.uni-marburg.de`, `christian.westphal@westphal.de`

1 Introduction

The economic literature contains ample investigation into the relation between guns and crime. Seeing that in the United States there were 11,078 deadly assaults by firearm and 19,392 suicides by firearm in 2010,[1] a closer investigation of a possible association between firearms and suicide seems warranted. Two studies of Ireland (Kennelly, 2007; Yang and Lester, 2007) – with remarkably different outcomes – elaborate on the economic dimension of suicide in terms of cost. Furthermore, firearm suicide as a fraction of all suicides is believed to be a good proxy, at least in the cross-section, for gun ownership density (Azrael, Cook and Miller, 2004; Kleck, 2004). This association is exploited by Cook and Ludwig (2006) in a very detailed, albeit flawed (Westphal, 2013), analysis of the association between firearms and crime. Lang (2013) analyzes the association between firearms and suicide using U.S. National Instant Background Check data and confirms the validity of the FSS proxy.It seems valuable to investigate the interaction between firearms and suicide with high quality data from other than a U.S. sample, taking care to avoid methodological fallacies.

After World War II, European countries enacted much tighter firearms regulation than what exists in the United States. Therefore, much better administrative data are available. Austria has relatively low restrictions on the acquisition of firearms but has become increasingly concerned with monitoring legally purchased firearms. Austrian data on concealed carry licenses are available from 1982 to the present for all Austrian counties. This provides a reasonable, albeit imperfect, nationwide proxy for gun ownership taken directly from administrative data on firearm permits. These data have been used to compute correlations between firearm ownership rates and suicide rates in the medical literature (Etzersdorfer, Kapusta and Sonneck, 2006), and provide an intriguing starting point for possibly confirming, or not, the validity of the FSS proxy and at the same time further investigating the relationship between suicide and firearms.

Two questions are addressed in this paper: (1) Can the FSS proxy for gun ownership be confirmed from Austrian data on gun licenses? – and (2) What can be said about the relationship between firearms and suicide in Austria after a careful review of the methods used for analysis in former work? Answering these questions results in two main findings. First, I confirm the validity of the FSS proxy. An association between firearms and firearm suicides is persistent across all methods of analysis used and a variety of model specifications. If one prefers clustered standard errors over Driscoll-Kraay standard errors – a preference I do not advocate in my setting – a substitution between suicide methods

[1] ICD-10 codes X93, X94, and X95 used for "assault by firearm"; X72, X73, and X74 for "suicide by firearm." Values taken from United States Department of Health and Human Services (2010).

shows in the main model. Second, it is clear that, earlier correlation results in Etzersdorfer, Kapusta and Sonneck (2006) on the association between firearms and suicides are greatly overstated due to ignoring Pearson's (1896) finding on spurious correlations between ratio variables. Thus, the contributions of this paper include validation of earlier approaches to measuring gun ownership,[2] and a warning as to the hazards of using spurious results in public policy debate.

My paper is organised as follows. I revisit the literature on guns and suicide in Austria in Section 2. In Section 3.1, the results from Etzersdorfer, Kapusta and Sonneck (2006) are repeated. Sections 3.2 and 3.3 point out the statistical fallacy in Etzersdorfer, Kapusta and Sonneck (2006) and adjust for the problem using two approaches that both lead to numerically very close and qualitatively identical results. Section 4 motivates and estimates a fixed effects panel model based on a theoretical model from the economic literature. The main finding for the FSS proxy is found to be robust to several robustness checks in Section 4.2.

2 Former Analysis of Firearms and Suicides in Austria

Etzersdorfer, Kapusta and Sonneck (2006) (EKS hereafter) analyze correlations between *suicide rates* and *rates of firearm ownership*, proxied by the *rate of concealed carry licenses*, in all nine Austrian counties over the period from 1990 to 2000. Their results from a repeated cross-sectional analysis are *strong rank correlations between the firearms measure and firearm suicides, low-to-no rank correlations between firearms and other suicides, and weakly positive rank correlations between firearms and all suicides*. Based on these findings, their conclusion is to assume that overall suicides increase with more firearms, as depicted in Figure 1(a), as opposed to a substitution between suicide methods as shown in Figure 1(b). In EKS's (p. 468) opinion their findings "emphasise the need for political support" for stricter regulation on gun ownership in the interest of preventing suicide. Their finding is now propagated through the literature; for example, "it is a scientific fact ... that reducing the availability of guns ... will reduce deaths" (Leenaars, 2006, 439). There many references to similar studies[3] can be found.

[2] This measure is not without problems itself as can be seen in Westphal (2013).

[3] Notably similar to EKS of those mentioned are Markush and Bartolucci (1984), Killias (1993) and Leenaars et al. (2003).

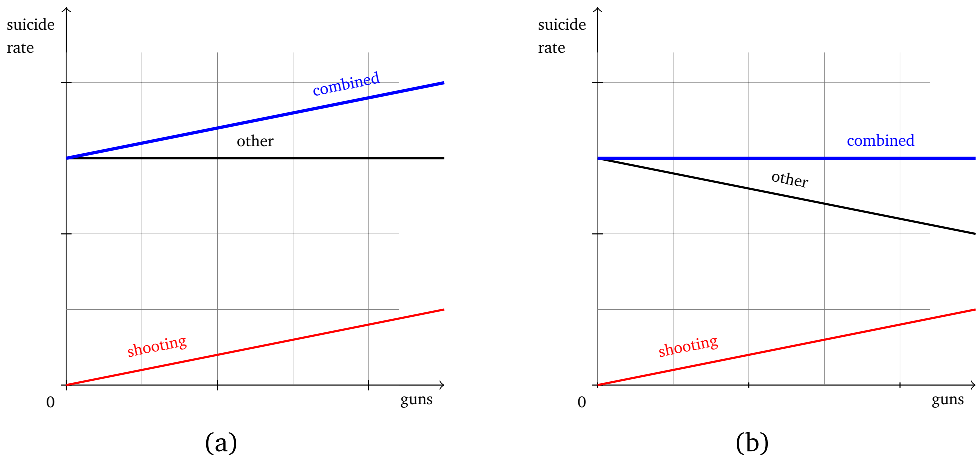

Figure 1: Competing models

3 Revisiting EKS's Results

3.1 Replication

In a first step, I replicate the results from EKS. EKS use four variables: *population size* (pop), *gun carry licenses*[4] (CCL), *suicides with firearms* ($E955$), and *all suicides* ($E95$). The latter two are based on their ICD-9[5] codes of the same name. The number of carry permits is used as a proxy for the number of gun owners. I obtained the data from their primary sources. Data on carry permits were obtained from the Austrian Interior Ministry, Department III/3. Statistik Austria provided population and suicide figures. Data for all variables were provided for the years $t = \{1982, 1985, 1987, 1990, 1992, 1994, 1995, \ldots, 2011\}$[6] and all $K = 9$ Austrian counties $k = 1, 2, \ldots, K$. I.e. $pop_{k,t}$ is population size in year t in county k. An overview of the variables is given in Table 1. These also are the variable names used in my program code and the data made available with this paper. Descriptives are set out in EKS (p. 464–465).

EKS compute rank correlations between *gun ownership rates* and *suicide rates*. Table 2 row I sets out my results for Spearman's rank correlation coefficient as

[4]"Waffenpaesse." CCL is the U.S. acronym for "concealed carry license." A CCL and a "Waffenpass" are not legally exactly identical, but are very similar and so I therefore use the acronym CCL based on its international recognition value.

[5]E955 includes suicides by explosives which cannot be distinguished from firearm suicides. However, for later years ICD-10 codes are available, which do differentiate between firearm and explosives suicides. The numbers indicate that there are very few suicides by explosives.

[6]Upon request, I could not be supplied with data for 1991 and 1993. Remarkable, EKS state results for these years.

Underlying	Absolute	Meaning	Rate	How computed?
Firearms	CCL	Number of carry permits	$CCLR$	CP/pop
Firearm suicides	$E955$	Number of fire-arm suicides	FSR	$E955/pop$
All suicides	$E95$	Number of *all* suicides	SR	$E95/pop$
Suicides *not* with firearms	$NE955$	Number of suicides *not* with firearms	OSR	$NE955/pop$
Population	pop	Number of inhabitants	NA	NA

Table 1: Variables and their meaning

well as Pearson's correlation coefficient, averaged over all years t.[7] Detailed result for individual years can be found in Table 4 in the Appendix. Neither small sample size nor non-normality of data, as claimed by EKS (p. 465), contradict the computation of Pearson's correlation coefficient. I therefore included these values in Table 2: values do not deviate much from the rank correlations. Tables 2 and 4 reveal the numerical and qualitative results from EKS are robust to the inclusion of years prior to and after their original period as well as to using either Pearson's or Spearman's method.

3.2 Accounting for Spurious Correlation between Ratios

Unfortunately, EKS fail to acknowledge Pearson's finding on correlations between ratios (Pearson, 1896): *using ratios for correlation analysis may lead to spurious results.*

Table 2, Row I shows there is little difference between rank correlations and Pearson's correlation for the data. Because of this and because of the availability of a theoretical result from Kim (1999), I now first use Pearson's correlation coefficient for illustration and examination of the ratio fallacy problem in EKS's results. A simulation study conducted in Section 3.3 shows that my findings do not change when using rank correlations.

Let there be three independent random variables X, Y, Z with known expected values and variances. To illustrate the problem at hand, let $X_{k,t}$ be the number of CCLs in county k in year t. $Y_{k,t}$ represents the corresponding number of suicides,

[7] As opposed to the *rank correlation of the average rates over time* (what is the meaning of that value aside from it being larger than the individual correlations in this setting?) as in EKS (Table 2).

Row	Firearm suicides Pearson	Firearm suicides Spearman	Other suicides Pearson	Other suicides Spearman	All suicides Pearson	All suicides Spearman
I	*Correlations between ratios...*					
	0.647	0.640	−0.053	0.044	0.157	0.222
IIa	*... rescaled by estimated reference points*					
	0.459	NA	−0.317	NA	−0.152	NA
IIb	*Reference points estimated by Equation* (1)					
	0.357	NA	0.397	NA	0.394	NA
IIIa	*... rescaled by simulated reference points*					
	0.433	0.444	−0.302	−0.190	−0.175	−0.089
IIIb	*Simulated reference points*					
	0.390	0.364	0.370	0.309	0.429	0.393

Table 2: Correlations between gun ownership rate and suicide rates and rescaling points. Both averaged over time

firearm suicides, or non-firearm suicides. $Z_{k,t}$ is the county's population in that year. In this setting, the coefficient of correlation $r_{X/Z,Y/Z}$ between X/Z and Y/Z in year t will not usually be zero, even if all three variables were truly uncorrelated. This *spurious correlation* is driven by the identical denominator common to both ratios.[8] The theoretical reference point for no correlation in this case is given in Kim (1999, Eq. (2.2)) as

$$r^0_{X/Z,Y/Z} = \frac{V^2_{1/Z}}{\sqrt{[V^2_X(1+V^2_{1/Z})+V^2_{1/Z}][V^2_Y(1+V^2_{1/Z})+V^2_{1/Z}]}} \tag{1}$$

for positive expected values of X, Y and when V_A is the coefficient of variation, i.e., $V_A = \sqrt{\mathscr{V}(A)}/\mathscr{E}(A)$. Positive expectation and finite variance is clearly fulfilled for population, suicides, and CCLs for all t. Therefore the empirical moments of population, carry permits and suicides will be used in Equation (1) to estimate the reference points for each year, as suggested by Kim (1999, 386). Using these yearly estimates to rescale the correlations based on Equations (2) and (3) gives us the rescaled correlations shown in Table 2, Row IIa with the estimated rescaling points in Row IIb. Detailed results for individual years are given in Table 5 in

[8] See Pearson (1896); Kronmal (1993); and Kim (1999).

the Appendix.

$$r^*_{X/Z,Y/Z} = \frac{r_{X/Z,Y/Z} - \hat{r}^0_{X/Z,Y/Z}}{1 + \hat{r}^0_{X/Z,Y/Z}} \quad \forall r_{X/Z,Y/Z} \leq r^0_{X/Z,Y/Z} \tag{2}$$

$$r^*_{X/Z,Y/Z} = \frac{r_{X/Z,Y/Z} - \hat{r}^0_{X/Z,Y/Z}}{1 - \hat{r}^0_{X/Z,Y/Z}} \quad \forall r_{X/Z,Y/Z} > r^0_{X/Z,Y/Z} \tag{3}$$

We obtain an average rescaled correlation of 0.46 between *firearms* and *firearm suicides*. This is neither a very strong nor a very weak correlation. Thus association between these two measures appears to persist after rescaling, albeit far more weakly than stated by EKS. Between *firearms* and *non-firearm suicides*, the average rescaled correlation over time takes a negative value of −0.32, which is rather weak. What is remarkable is the change in sign compared to the spurious results reported by EKS. Last, for *all suicides*, there is an average rescaled correlation of −0.15. This is hard to interpret without testing for significance, a problem addressed in Section 4. Without testing for significance, we have a not very strong, but clearly present, positive correlation between the measure for firearms and firearm suicides, a rather weak negative correlation between the measure for firearms and other suicides, and a negative correlation between the measure for firearms and all suicides too weak to base any findings on. However, it is still clear that rejecting Model (b) of Figure 1 in favor of Model (a), as done by EKS, is not advisable based on this empirical foundation.

3.3 Simulation Study

The results from Tables 2 (Rows IIa and IIb) and 5 while theoretically well founded are surprising given how much they change the initial results. Also my results report rescaled correlations for Pearson's method and not for Spearman's rank correlation: ranks are not ratios. So, do the results hold for ranked ratios? Because I could find no theoretical result for spurious correlation reference points for ranks of ratios, I conducted a simulation study.

I used a hotdeck simulation. For each year I repeatedly (10,000 times), randomly, and independently redistributed the observed numerators ($E95$, $E955$, CCL) across the counties, thus ensuring that, on average, there is no correlation between the numerators. Fortunately, $\max\{E95_{k,t}, E955_{k,t}, CCL_{k,t}\} < \min\{pop_{k,t}\}$ $\forall t$ so no ratios > 1 could occur. I next, for each repetition, computed the same ratios and ranks of ratios as done for the analysis in Sections 3.1 and 3.2. The random rank correlations of the numerators generated in this manner appear to be distributed around 0. (See Figure 2 in Appendix B for selected years.) The ratios' rank correlation distributions, on the other hand, are clearly shifted to the right and obviously skewed (Appendix B, Figure 3). The situation is persistent for all years and Pearson's correlations. The numerical results are well in accordance with the correction derived from Kim's (1999) theoretical result and Pearson's (1896) initial estimates of the problem size.

Year-wise simulated reference points can be calculated by computing the mean of the simulated (rank) correlations between the ratios. These values then can be used to rescale the results from the biased correlation analysis from Table 2, Row I, resulting in Rows IIIa, IIIb, and the detailed Tables 6 and 7 for rank correlations and Pearson's correlations found in Appendix B.

Results from Table 2, Row IIIa and Tables 6 and 7 are intriguing: after rescaling there remains, on average, a *negative* correlation between concealed carry licenses and all suicides. Interpreting such low correlation coefficients in favor of any side of any debate, however, is a tricky business. This question of valid inference is addressed in Section 4 of this paper.

4 Panel Regression

4.1 Model and Results

A more sophisticated method for analyzing the data seems appropriate. Panel regression, in contrast to EKS's repeated cross-sectional analysis, is capable of accounting for the contemporal and intertemporal structure of the data. Panel regression must be employed carefully as (a) either using ratios or (b) ignoring time series effects may cause spurious results. (a) can be dealt with by considering a risk model, as outlined below; (b) is solved by estimating the model on first differences.

The risk model is theoretically based on Duggan (2003).[9] A binary choice model is set up for individual i's suicide decision in the form of a linear probability model:

$$\Pr(Suicide_i) = \alpha + X_i\theta + \gamma Gun_i + \lambda_i + \varepsilon_i. \tag{4}$$

X_i represents individual characteristics, Gun_i is a dummy indicating if i owns a firearm, and the individual propensity λ_i tells us how strongly i is inclined to commit suicide. As Duggan notes, if the unobservable λ_i and Gun_i are not independent, we face a sample selection problem. Taking Duggan (2003, Eq. 2)

$$\lambda_i = \mu + \sigma Gun_i + \zeta_i \tag{5}$$

we see that unless $\sigma = 0$ in Equation (5), omitting λ_i introduces a bias into estimation of Equation (4)'s parameters. This problem can be overcome by using the data from above and imposing a risk model on the aggregate values. Let the

[9] I deviate slightly from Duggan's notation without changing the model to keep my formulas simple.

number of suicides (all, firearm, or non-firearm) have a conditional expectation of

$$\mathscr{E}[Y_{k,t}|\overline{X}_{k,t},\overline{Gun}_{k,t},\overline{\lambda}_{k,t},pop_{k,t}] = pop_{k,t}(\alpha + \overline{X}_{k,t}\theta + \gamma\overline{Gun}_{k,t} + \overline{\lambda}_{k,t}) \qquad (6)$$

where $\overline{X}_{k,t}, \overline{\lambda}_{k,t}$ are the averages of those values in county k in year t and $\overline{Gun}_{k,t}$ is the percentage of gun owners in k's population in year t. When we assume the average propensity and the average characteristics to be invariant over time, the (averages of) controls in $X_{k,t}$ and the propensities can be fully captured in a fixed effect model's county dummy. For those X_i that are individual characteristics, $pop\overline{X}$ become the count of persons with these characteristics.

Slightly relaxing the restriction of time-invariant unobserved variables, identical intertemporal changes in X and λ across all Austria can be captured in a time dummy. Then we arrive at a two-way fixed effects model of

$$y_{k,t} = \beta_0 pop_{k,t} + \beta_1 CCL_{k,t} + d_k + d_t + \varepsilon_{k,t}. \qquad (7)$$

Here the number of concealed carry licenses is used as a proxy[10] for the number of gun owners. Then β_0 can be interpreted identically to α from Equations (4) and (6) as the baseline risk of an individual committing suicide. β_1 will be related to γ by the relation between concealed carry licenses and gun owners. A gun owner's relative risk of committing suicide from Equation (4), ignoring propensity for illustrative purposes, will be related to the ratio of coefficients from Equation (7):

$$\frac{\alpha + X_i\theta + \gamma}{\alpha + X_i\theta} \sim \frac{\beta_0 + \beta_1}{\beta_0}. \qquad (8)$$

Given the nature of the data, i.e., a panel with time series, to rule out spurious results from time series effects (which may be numerous), Equation (7) is estimated on the first differences,[11] i.e.,

$$\Delta y_{k,t} = \beta_0 \Delta pop_{k,t} + \beta_1 \Delta CCL_{k,t} + \delta_t + \nu_{k,t}, \qquad (9)$$

which is a common technique for circumventing many time series problems. The null hypothesis of poolability of the data, conducting a Chow test for poolability across periods, is rejected for firearm suicides and all suicides as the dependent variable with p-values[12] < 0.05. Results are shown in Table 3. The association

[10]This will be a somewhat noisy proxy, of course, so we expect coefficient estimates to be biased downward (Baltagi, 2008, Section 10.1).

[11]Including individual fixed growth parameters, i.e. a county dummy in the first differenced model, leads to highly insignificant county fixed effects and to no qualitative change to the results set out in Tables 3 and 8.

[12]All standard errors are computed according to Driscoll and Kraay (1998); available for Stata via `xtscc` from Hoechle (2007). Employing clustered robust standard errors does not qualitatively change the results and – for all results reported to be significant on any level – uniformly yields smaller p-values.

between firearms and firearm suicides, well known from the extant literature, is confirmed at a reasonable level of significance. No association is found for firearms and overall suicides. There are three obvious explanations of this result.

(a) Too much noise may be added to the model by including other suicides, so that significance can no longer be attained.

(b) Variables that *cannot* be captured by the dummies are missing from the model, somehow causing the coefficient to be biased toward zero.

(c) The negative coefficient for non-firearm suicides shows very weak significance:[13] using one sided testing, the p-value is 0.08, which may imply substitution between methods in accordance with the findings of Klieve, Barnes and De Leo (2009) and Leenaars et al. (2003). Given that the opposed effect estimates are of nearly identical size, this argument is intriguing. However, the extremely weak significance of the negative coefficient must be considered, meaning that this finding should be viewed with caution. It is not replicated in the alternative model specifications in Section 4.2.[14] Repeating my analysis with more data from different countries would be interesting.

exogenous variable	Dependent variable, differenced: all suicides	firearm suicides	other suicides
Δpop	−0.000094	−0.000094	0.0000004
	(0.000173)	(0.000153)	(0.000159)
ΔCCL	0.000351	0.003517***	−0.0031670†
	(0.002080)	(0.001317)	(0.002235)
R^2_{within}	0.0008	0.0584	0.0097

Table 3: Estimation results for Model (9), standard errors in parentheses, $^{\dagger}/^{*}/^{**}/^{***}$ indicating two sided significance on 20/10/5/1% levels, robust standard errors according to Driscoll and Kraay (1998) computed with `vcovSCC` from Croissant and Millo (2008), $N = 198$ observations, R^2 adjusted

4.2 Robustness

The results from Section 4.1 do not exhibit strong significance, thus raising the question of their robustness to slight modifications[15] of the estimating equation. One possible modification is to standardise across counties by computing log

[13] With clustered robust standard errors, the coefficient becomes highly significant.

[14] All of those model specifications exhibit a worse fit. Thus they will all likely be further off the true underlying conditional expectation function.

[15] I use $\varepsilon_{k,t}$ for the error term multiple times in this section; however, I do not assume it to be identically distributed for all models. I also reuse β for coefficients with different interpretations. These are not identical across models.

growth rates on both sides of the estimating equation. This results in

$$\ln\Big(\frac{y_{k,t}}{y_{k,t-1}}\Big) = \alpha + \beta_0 \ln\Big(\frac{pop_{k,t}}{pop_{k,t-1}}\Big) + \beta_1 \ln\Big(\frac{CCL_{k,t}}{CCL_{k,t-1}}\Big) + \varepsilon_{k,t} \tag{10}$$

where once again $y_{k,t}$ may be either the number of all suicides, firearm suicides, or other suicides. The hypothesis of poolability across periods for Equation (10) is not rejected for any of the dependent variables. Thus the estimation is run without time dummies.[16] Results are found in Table 8 in the Appendix, in the row labeled "Log growth rates (pooled)." A moderately significant and positive estimate for β_1 is found when firearm suicides is the dependent variable.

Another feasible model specification is estimation directly on the ratios. Kronmal's (1993: 390) advice of including the inverse of the common denominator as an explanatory variable must be taken, however, or this model would fall prey to the same spuriousness found in EKS's results. Heterogeneous time trends in ratios are addressed by taking first differences. The estimating equation becomes

$$\Delta y_{k,t} = \beta_0 pop^{-1}_{k,t} + \beta_1 \Delta CCLR_{k,t} + \delta_t + \varepsilon_{k,t} \tag{11}$$

where $\Delta y_{k,t}$ for all suicides is computed as $SR_{k,t} - SR_{k,t-1}$; for the other suicides, the respective ratios are used. There is no "common denominator" per se for the left- and right-hand sides. Constructing a common denominator would result in population from t and $t-1$ also appearing in the numerator of both sides. Therefore, instead of the true denominator, I follow Kronmal's advice by assuming population to be constant from $t-1$ to t. This allows controlling for the denominator by using $pop^{-1}_{t,k}$ as an additional variable on the right-hand side. Testing rejects poolability across time, at least for "all suicides"; therefore, the results given in Table 8 of the Appendix include time fixed effects. Again, the estimate of Equation (11)'s β_1 when firearm suicides are the dependent variable is positive and moderately significant. The size of the estimate is notably similar to the result in Table 3 achieved by estimating Equation (9).

Following Duggan (2001) and Cook and Ludwig (2006), who run similar regressions for crime rates, we can also look for elasticity in suicide rates with respect to gun ownership rates by taking logarithms of the variables:

$$\Delta \ln y_{k,t} = \alpha + \beta_0 \Delta \ln pop_{k,t} + \beta_1 \Delta \ln CCLR_{k,t} + \varepsilon_{k,t}. \tag{12}$$

This model, in contrast to the models used by Duggan (2001) and Cook and Ludwig (2006), does account for spurious correlations between ratios by including the common denominator on the right-hand side. Note that this specification is

[16] Including time dummies does not change the results qualitatively; significance on the gun measure weakens, as is to be expected for an overspecified model.

very similar to Equation (10) model-wise, and as was the case for Equation (10), here, again, poolability is not rejected. Results for "firearm suicides" and "other suicides" in Table 8 in the Appendix are also very similar between these two models.[17] The estimate for β_1 when firearm suicides is the dependent variable is positive and moderately significant once again.

Thus, the results from the initial model in Section 4.1 hold up quite well to several modifications, which is what we would expect given an underlying – but, of course, unknown – conditional expectation function monotonous in the variables. From a goodness-of-fit point of view, Model (9) seems to be the best choice.

In light of this interesting result, a qualitative argument for causality can be made. Austrian firearm laws allow the purchase of firearms without need for a CCL. Therefore, persons intending to commit suicide by firearm do not need to acquire a CCL. This means the number of CCLs should not be driven by the number of firearm suicides. Firearm suicides, however, may very well be driven by the number of CCLs, given that those represent an underlying number of firearms owned by individuals.

5 Conclusion

I conclude this paper with two main findings.

(1) The "Fraction of Suicides by Firearms" (FSS) does indeed appear to be a valid proxy for gun ownership density. My results are in accordance with recent findings from U.S. data (Lang, 2013).[18]

(2) The correlation results from Etzersdorfer, Kapusta and Sonneck (2006) are greatly overstated because the authors fail to acknowledge spurious correlations between ratios.

Note, however, that finding (1), does not mean FSS should be used indiscriminately in regression analysis of models that contain firearms as an explanatory variable. FSS has its own problems, detailed in Westphal (2013), and may produce spurious results itself.

Finding (2) indicates that EKS's results cannot be used for public policy advocacy. Given that the journal that published Etzersdorfer, Kapusta and Sonneck (2006) is unwilling to acknowledge the partial spuriousness of the results,[19] EKS's article should be viewed with caution by the scientific and political community.

[17] An explanation for this can be found in Westphal (2013, Section 5.2).

[18] Lang's results are not affected by the ratio fallacy. The author unhesitatingly shared his data with me; running the usual specifications to check for spurious results due to ratio variables did not lead to different findings.

[19] The *Wiener Klinische Wochenschrift* rejected a short note on this problem for reasons unrelated to the scientific finding.

In conclusion, this paper demonstrates once again[20] that correlation and regression studies involving ratios need to take a close and careful look at the nature of the data and their possible implications. Recently, the ratio fallacy was demonstrated to occur in a prominent study on the association between guns and crime (Westphal, 2013), and it very well may be a problem in more analyses on that topic.

References

Azrael, Deborah, Philip J Cook, and Matthew Miller. 2004. "State and Local Prevalence of Firearms Ownership Measurement, Structure, and Trends." *Journal of Quantitative Criminology*, 20(1): 43–62.

Baltagi, Badi. 2008. *Econometric Analysis of Panel Data.* Wiley.

Cook, Philip J, and Jens Ludwig. 2006. "The social costs of gun ownership." *Journal of Public Economics*, 90: 379–391.

Croissant, Yves, and Giovanni Millo. 2008. "Panel Data Econometrics in R: The plm Package." *Journal of Statistical Software*, 27(2).

Driscoll, John C, and Aart C Kraay. 1998. "Consistent Covariance Matrix Estimation with Spatially Dependent Panel Data." *Review of Economics and Statistics*, 80(4): 549–560.

Duggan, Mark. 2001. "More Guns, More Crime." *The Journal of Political Economy*, 109(5): 1086–1114.

Duggan, Mark. 2003. "Guns and Suicide." *Evaluating Gun Policy: Effects on Crime and Violence*, 41–73.

Etzersdorfer, Elmar, Nestor D. Kapusta, and Gernot Sonneck. 2006. "Suicide by shooting is correlated to rate of gun licenses in Austrian counties." *Wiener Klinische Wochenschrift*, 118(15-16): 464–468.

Hoechle, Daniel. 2007. "Robust standard errors for panel regressions with cross-sectional dependence." *Stata Journal*, 7(3): 281–312.

Kennelly, Brendan. 2007. "The Economic Cost of Suicide in Ireland." *Crisis: The Journal of Crisis Intervention and Suicide Prevention*, 28(2): 89–94.

Killias, Martin. 1993. "International correlations between gun ownership and rates of homicide and suicide." *CMAJ: Canadian Medical Association Journal*, 148(10): 1721–1725.

[20] See Kronmal (1993) for more examples.

Kim, Ji-Hyun. 1999. "Spurious correlation between ratios with a common divisor." *Statistics & Probability Letters*, 44(4): 383–386.

Kleck, Gary. 2004. "Measures of Gun Ownership Levels for Macro-level Crime and Violence Research." *Journal of Research in Crime and Delinquency*, 41(1): 3–36.

Klieve, Helen, Michael Barnes, and Diego De Leo. 2009. "Controlling firearms use in Australia: has the 1996 gun law reform produced the decrease in rates of suicide with this method?" *Social Psychiatry and Psychiatric Epidemiology*, 44(4): 285–292.

Kronmal, Richard A. 1993. "Spurious Correlation and the Fallacy of the Ratio Standard Revisited." *Journal of the Royal Statistical Society. Series A (Statistics in Society)*, 156(3): 379–392.

Lang, Matthew. 2013. "Firearm Background Checks and Suicide." *The Economic Journal*.

Leenaars, Antoon A. 2006. "Suicide by shooting and gun ownership (licenses) in Austria: would gun restriction help?" *Wiener Klinische Wochenschrift*, 118(15): 439–441.

Leenaars, Antoon A, Ferenc Moksony, David Lester, and Susanne Wenckstern. 2003. "The Impact of Gun Control (Bill C-51) on Suicide in Canada." *Death Studies*, 27(2): 103–124.

Markush, Robert E, and Alfred A Bartolucci. 1984. "Firearms and Suicide in the United States." *American Journal of Public Health*, 74(2): 123–127.

Pearson, K. 1896. "Mathematical Contributions to the Theory of Evolution. – On a Form of Spurious Correlation Which May Arise When Indices Are Used in the Measurement of Organs." *Proceedings of the Royal Society of London*, 60(359-367): 489–498.

United States Department of Health and Human Services. 2010. "Compressed Mortality File (CMF) on CDC WONDER Online Database."

Westphal, Christian. 2013. "The Social Costs of Gun Ownership: Spurious Regression and Unfounded Public Policy Advocacy." *MAGKS Joint Discussion Paper Series in Economics*, 32-2013.

Yang, Bijou, and David Lester. 2007. "Recalculating the Economic Cost of Suicide." *Death Studies*, 31(4): 351–361.

A Correlation Tables, Repeated Cross-Section

	Rates					
	Firearm suicides		Other suicides		All suicides	
Year	Pearson	Spearman	Pearson	Spearman	Pearson	Spearman
1982	0.265	0.25	0.375	0.317	0.363	0.317
1985	0.734	0.667	0.131	0.233	0.331	0.200
1987	0.568	0.617	0.507	0.400	0.622	0.533
1990	0.870	0.867	0.014	−0.083	0.345	0.300
1992	0.658	0.517	0.006	0.200	0.209	0.183
1994	0.756	0.733	−0.111	0.083	0.144	0.283
1995	0.651	0.700	−0.038	−0.117	0.235	0.267
1996	0.381	0.300	−0.118	−0.067	0.044	0.200
1997	0.768	0.617	−0.181	−0.033	0.144	0.250
1998	0.512	0.417	0.215	0.300	0.353	0.400
1999	0.762	0.867	0.090	0.183	0.421	0.567
2000	0.745	0.817	−0.072	−0.083	0.155	0.250
2001	0.643	0.667	0.184	0.500	0.358	0.717
2002	0.527	0.583	0.049	0.167	0.196	0.133
2003	0.727	0.667	−0.156	−0.333	0.086	−0.267
2004	0.816	0.983	−0.152	−0.083	0.046	0.250
2005	0.213	0.367	−0.513	−0.383	−0.410	−0.267
2006	0.669	0.733	0.003	0.233	0.203	0.283
2007	0.463	0.300	−0.619	−0.400	−0.549	−0.317
2008	0.817	0.767	0.231	0.417	0.586	0.583
2009	0.767	0.700	−0.298	−0.200	−0.072	−0.033
2010	0.856	0.850	−0.252	−0.317	0.086	0.100
2011	0.712	0.733	−0.521	0.083	−0.296	0.167
Average over time	0.647	0.640	−0.053	0.044	0.157	0.222

Table 4: Correlations between gun ownership rate and suicide rates

	firearms suicides		other suicides		all suicides	
	estimated	rescaled	estimated	rescaled	estimated	rescaled
Year	r^0	r^*	r^0	r^*	r^0	r^*
1982	0.366	−0.074	0.378	−0.002	0.377	−0.010
1985	0.380	0.571	0.390	−0.186	0.391	−0.043
1987	0.358	0.327	0.378	0.207	0.377	0.393
1990	0.357	0.797	0.399	−0.275	0.392	−0.034
1992	0.364	0.462	0.388	−0.275	0.387	−0.128
1994	0.359	0.619	0.387	−0.359	0.384	−0.173
1995	0.353	0.460	0.404	−0.315	0.396	−0.115
1996	0.336	0.068	0.392	−0.366	0.383	−0.245
1997	0.375	0.629	0.393	−0.412	0.391	−0.177
1998	0.324	0.278	0.394	−0.128	0.382	−0.021
1999	0.338	0.641	0.401	−0.222	0.393	0.047
2000	0.384	0.586	0.414	−0.344	0.414	−0.183
2001	0.364	0.439	0.418	−0.165	0.412	−0.038
2002	0.383	0.234	0.394	−0.247	0.399	−0.145
2003	0.403	0.543	0.393	−0.394	0.396	−0.222
2004	0.391	0.698	0.407	−0.397	0.408	−0.257
2005	0.333	−0.090	0.412	−0.655	0.399	−0.578
2006	0.344	0.495	0.400	−0.284	0.391	−0.135
2007	0.359	0.163	0.402	−0.729	0.403	−0.678
2008	0.331	0.726	0.410	−0.127	0.406	0.303
2009	0.366	0.633	0.392	−0.496	0.395	−0.334
2010	0.340	0.782	0.420	−0.473	0.412	−0.231
2011	0.324	0.575	0.372	−0.651	0.367	−0.485
Average over time	0.357	0.459	0.397	−0.317	0.394	−0.152

Table 5: Correlations rescaled (r^*) with reference points of no correlation (r^0) between carry permit rates and suicide rates; reference points estimated by Equation (1)

B Simulation Results

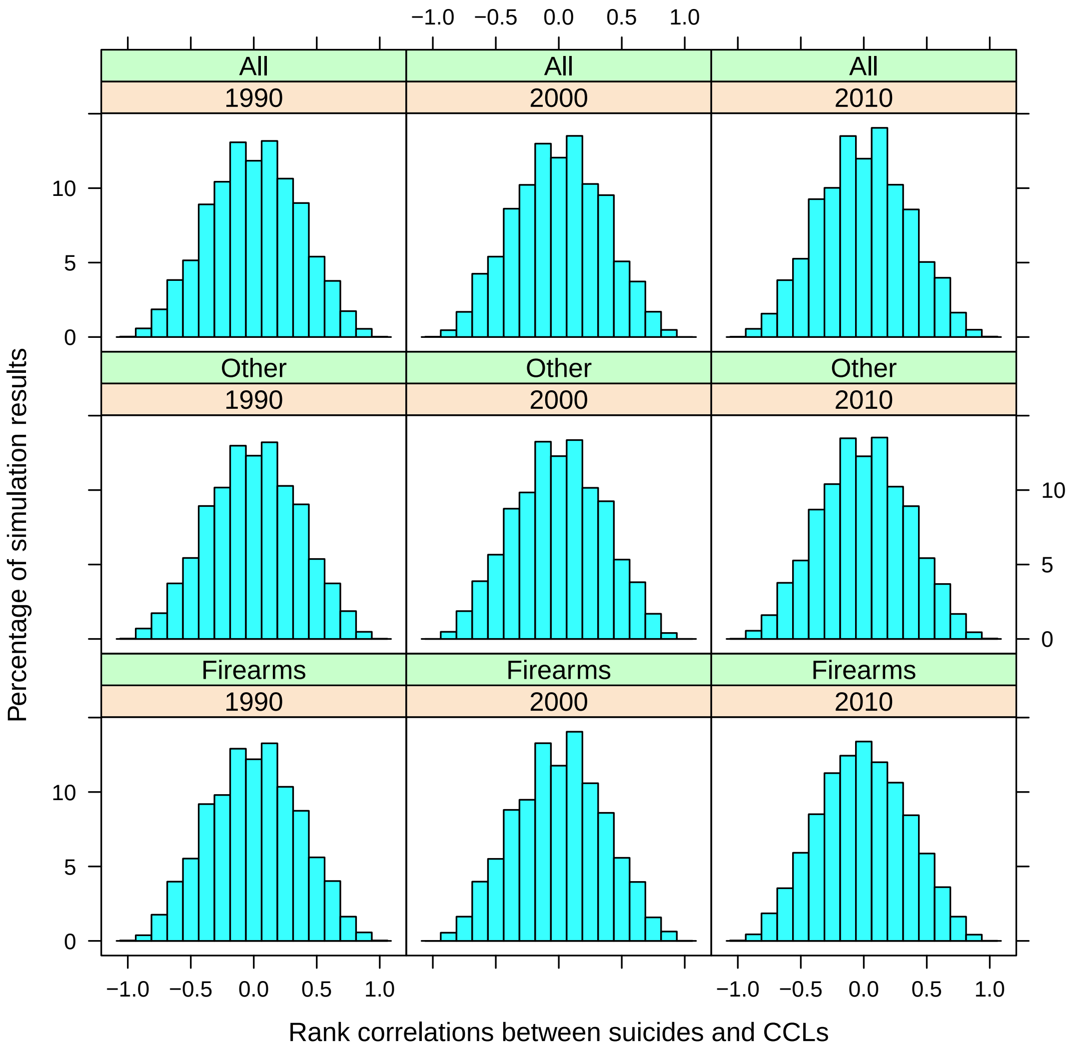

Figure 2: Histograms of simulated rank correlations between uncorrelated carry permits and suicides.

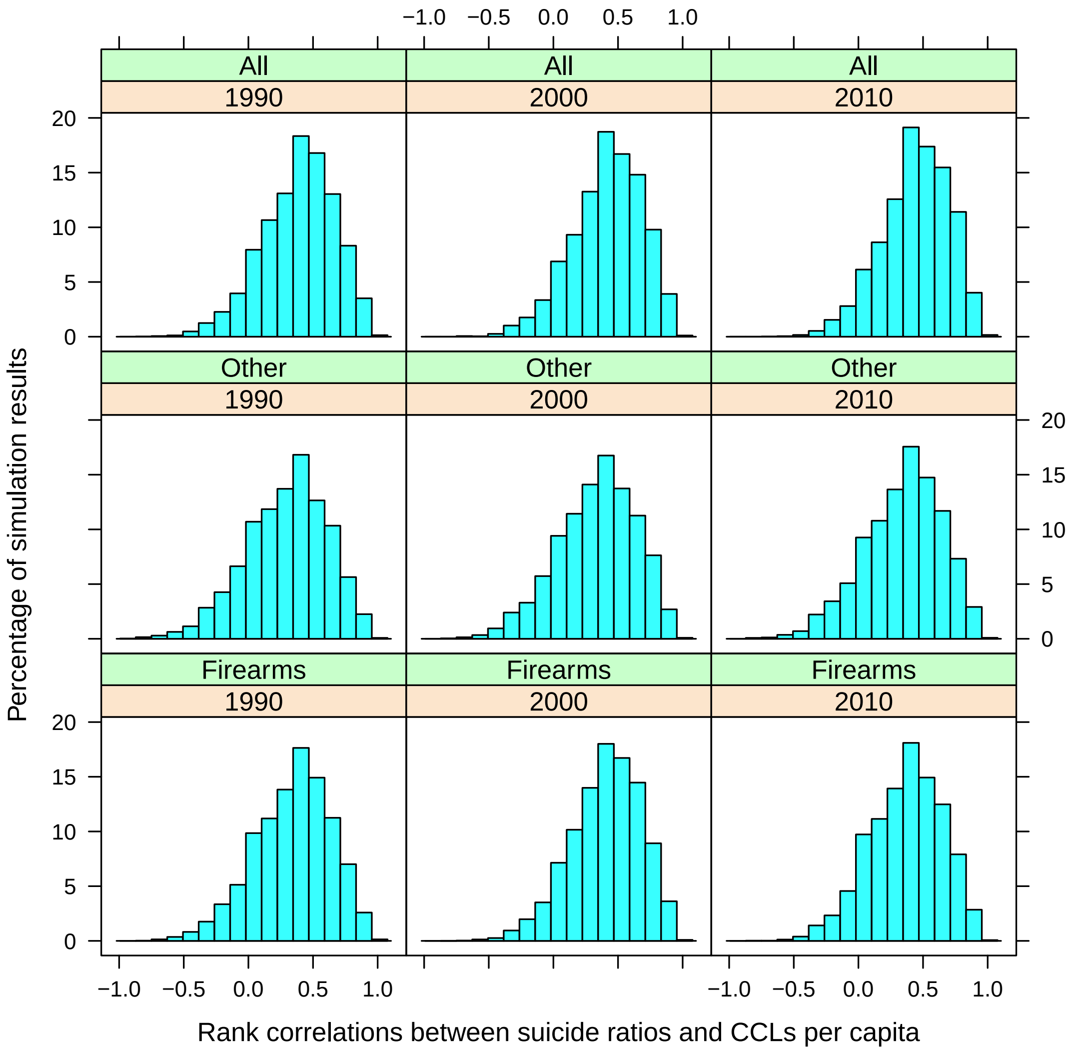

Figure 3: Histograms of simulated rank correlations between uncorrelated carry permits and suicide *rates*.

	firearms suicides		other suicides		all suicides	
	simulated	rescaled	simulated	rescaled	simulated	rescaled
year t	r^0	rg r^*	r^0	rg r^*	r^0	rg r^*
1982	0.346	−0.072	0.277	0.055	0.362	−0.033
1985	0.398	0.447	0.321	−0.066	0.399	−0.142
1987	0.376	0.386	0.291	0.154	0.369	0.260
1990	0.342	0.797	0.295	−0.292	0.389	−0.064
1992	0.380	0.220	0.297	−0.075	0.382	−0.144
1994	0.343	0.594	0.278	−0.152	0.358	−0.055
1995	0.339	0.546	0.300	−0.320	0.397	−0.093
1996	0.313	−0.010	0.277	−0.269	0.358	−0.116
1997	0.343	0.417	0.280	−0.245	0.368	−0.087
1998	0.298	0.169	0.285	0.021	0.362	0.060
1999	0.322	0.803	0.302	−0.091	0.388	0.292
2000	0.406	0.691	0.335	−0.314	0.417	−0.118
2001	0.409	0.436	0.339	0.244	0.433	0.501
2002	0.432	0.266	0.316	−0.113	0.396	−0.188
2003	0.419	0.426	0.328	−0.498	0.414	−0.481
2004	0.416	0.971	0.330	−0.311	0.405	−0.110
2005	0.300	0.095	0.314	−0.531	0.391	−0.473
2006	0.329	0.603	0.299	−0.051	0.385	−0.074
2007	0.393	−0.067	0.331	−0.549	0.405	−0.514
2008	0.375	0.627	0.367	0.078	0.457	0.232
2009	0.418	0.484	0.327	−0.397	0.414	−0.316
2010	0.368	0.763	0.345	−0.492	0.441	−0.236
2011	0.317	0.609	0.277	−0.152	0.358	−0.141
average over time	0.364	0.444	0.309	−0.190	0.393	−0.089

Table 6: Rank correlations rescaled (rg r^*) with simulated reference points of no correlation (r^0) between carry permit rates and suicide rates

Year	firearms suicides simulated r^0	firearms suicides rescaled r^*	other suicides simulated r^0	other suicides rescaled r^*	all suicides simulated r^0	all suicides rescaled r^*
1982	0.383	−0.086	0.342	0.050	0.392	−0.021
1985	0.408	0.551	0.367	−0.173	0.417	−0.061
1987	0.383	0.299	0.356	0.235	0.406	0.364
1990	0.374	0.792	0.358	−0.254	0.420	−0.053
1992	0.402	0.427	0.355	−0.258	0.416	−0.146
1994	0.381	0.605	0.347	−0.340	0.409	−0.188
1995	0.377	0.439	0.364	−0.295	0.431	−0.137
1996	0.359	0.034	0.349	−0.346	0.417	−0.263
1997	0.403	0.611	0.359	−0.397	0.426	−0.198
1998	0.340	0.260	0.356	−0.104	0.422	−0.048
1999	0.373	0.620	0.363	−0.200	0.428	−0.004
2000	0.439	0.546	0.399	−0.337	0.458	−0.208
2001	0.412	0.393	0.398	−0.153	0.460	−0.070
2002	0.436	0.163	0.383	−0.242	0.439	−0.169
2003	0.442	0.512	0.385	−0.390	0.439	−0.245
2004	0.429	0.678	0.393	−0.391	0.445	−0.276
2005	0.353	−0.103	0.375	−0.646	0.435	−0.588
2006	0.368	0.476	0.368	−0.266	0.432	−0.160
2007	0.409	0.092	0.390	−0.726	0.443	−0.678
2008	0.368	0.710	0.401	−0.121	0.456	0.240
2009	0.410	0.605	0.383	−0.493	0.439	−0.355
2010	0.378	0.769	0.393	−0.463	0.456	−0.254
2011	0.346	0.560	0.337	−0.642	0.394	−0.495
average over time	0.390	0.433	0.370	−0.302	0.429	−0.175

Table 7: Correlations rescaled (r^*) with simulated reference points of no correlation (r^0) between carry permit rates and suicide rates

C Further Regression Analysis Results

The results for the different models need to be viewed with caution. Joint significance is weak to nonexistent for all of them. The value of reporting the results lies in the robustness of the positive coefficient on the various transformations of the firearms proxy.

The time-pooled models are estimated with a constant as shown in Equations (10) and (12). In theory, this constant should be zero. However, for some of the pooled models, this constant tests weakly significant. This could indicate an ignored time effect, thereby contradicting the result from the respective Chow tests for timepoolability. Results for these models do not differ much when they are estimated without time pooling.

exogenous variable	Dependent variable all suicides	firearm suicides	other suicides
Log growth rates (pooled), Equation (10)			
$\ln(pop_t/pop_{t-1})$	0.1442	$-2.2778^{\dagger}$	0.5507
	(0.590)	(1.572)	(0.633)
$\ln(CCL_t/CCL_{t-1})$	-0.0044	0.7981^{**}	-0.1516
	(0.209)	(0.312)	(0.178)
R^2_{pooled}	0.0000	0.01411	0.0024
Estimation on ratios, Equation (11)			
pop^{-1}	0.4916	0.1870	0.3046
	(2.001)	(0.744)	(1.483)
$\Delta CCLR$	$0.0038^{\dagger}$	0.0030^{**}	0.0008
	(0.003)	(0.001)	(0.002)
R^2_{within}	0.0078	0.0254	0.0006
Elasticity estimation (pooled), Equation (12)			
$\Delta \ln pop$	$-0.8602^{\dagger}$	-2.4800^{*}	-0.6009
	(0.620)	(1.427)	(0.633)
$\Delta \ln CCLR$	-0.0044	0.7981^{**}	-0.1516
	(0.148)	(0.312)	(0.178)
R^2_{pooled}	0.0015	0.0152	0.0121

Table 8: Estimation results for panel models, standard errors in parentheses, $^{\dagger}/^{*}/^{**}/^{***}$ indicating two sided significance on 20/10/5/1% levels, robust standard errors according to Driscoll and Kraay (1998) computed with `vcovSCC` from Croissant and Millo (2008), $N = 198$ observations, R^2 adjusted

Lebenslauf aus Datenschutzgründen nicht in der elektronischen Version enthalten.

C H R I S T I A N W E S T P H A L
D I P L O M - V O L K S W I R T

Christian Westphal · Am Redder 22 · 25336 Klein Nordende

Prof. Dr. Sascha H. Mölls
c/o: Dekanat
Universitätsstraße 25

35037 Marburg

Ihre Zeichen, Ihre Nachricht vom	Meine Zeichen	Telefon (04121)	Klein Nordende
	b/cw00228	4915581	2013-07-30

Eidesstattliche Erklärung

Sehr geehrter Herr Professor Mölls,

hiermit versichere ich an Eides statt, dass ich die vorgelegte Dissertation selbst und ohne fremde Hilfe verfasst habe, eventuelle Beiträge von Ko- Autoren dokumentiert habe, nicht andere als die in ihr angegebenen Quellen oder Hilfsmittel benutzt habe, alle vollständig oder sinngemäß übernommenen Zitate als solche gekennzeichnet sowie die Dissertation in der vorliegenden oder einer ähnlichen Form noch bei keiner anderen in- oder ausländischen Hochschule anlässlich eines Promotionsgesuchs oder zu anderen Prüfungszwecken eingereicht habe.

Mit freundlichen Grüßen

Am Redder 22
25336 Klein Nordende
Tel.: +49 4121 4915581
Fax: +49 4121 4915585
E-Mail: christian.westphal@westphal.de

Sparkasse Elmshorn (BLZ 221 500 00)
Konto-Nr.: 101 257 088

PGP-Schlüssel: 0x3FFF6951, 0x73C0BB28
Please encrypt to both keys.