Online Supplement to "Handgun Divestment and Suicide Risk."

PART A

eTable 1. Number of eligible persons per person-month by divestment status.

	No divestment arm	Divestment arm
January 2005	650	0
February 2005	2432	0
March 2005	4613	5
April 2005	6419	6
May 2005	8482	8
June 2005	10762	18
July 2005	12715	21
August 2005	14540	20
September 2005	16411	30
October 2005	18317	20
November 2005	18789	18
December 2005	21063	20
January 2006	23058	26
February 2006	25094	30
March 2006	27568	41
April 2006	29546	44
May 2006	31658	24
June 2006	34312	31
July 2006	36671	36
August 2006	38652	39
September 2006	40458	42
October 2006	42142	41
November 2006	41184	51
December 2006	42771	47
January 2007	44460	47
February 2007	46227	49
March 2007	48545	59
April 2007	50403	57
May 2007	52490	45
June 2007	54818	62

July 2007	56902	50
August 2007	58997	46
September 2007	60999	67
October 2007	62713	75
November 2007	64517	68
December 2007	66136	61
January 2008	67612	92
February 2008	64814	79
March 2008	66947	66
April 2008	68875	64
May 2008	71035	57
June 2008	72125	88
July 2008	74311	78
August 2008	76442	98
September 2008	78581	78
October 2008	80681	100
November 2008	80869	106
December 2008	82252	99
January 2009	84516	83
February 2009	88770	92
March 2009	92946	100
April 2009	96178	111
May 2009	99380	104
June 2009	103738	113
July 2009	107359	110
August 2009	110430	139
September 2009	112976	107
October 2009	115321	108
November 2009	117815	105
December 2009	119785	128
January 2010	121787	127
February 2010	123697	125
March 2010	126255	143
April 2010	128378	123

May 2010	130887	113
June 2010	125632	129
July 2010	128293	109
August 2010	130969	114
September 2010	133127	120
October 2010	135290	126
November 2010	135207	113
December 2010	137223	143
January 2011	139065	138
February 2011	141048	126
March 2011	143983	150
April 2011	146686	143
May 2011	149898	125
June 2011	154137	133
July 2011	157417	152
August 2011	160248	160
September 2011	162697	150
October 2011	164819	155
November 2011	167296	153
December 2011	169812	184
January 2012	172133	176
February 2012	174761	161
March 2012	178542	177
April 2012	181272	176
May 2012	185136	147
June 2012	178561	146
July 2012	182243	148
August 2012	185260	179
September 2012	188002	174
October 2012	191024	203
November 2012	191220	183
December 2012	193581	194
January 2013	195447	213
February 2013	199754	226

March 2013	209958	242
April 2013	219878	220
May 2013	225150	197
June 2013	230131	175
July 2013	234616	182
August 2013	238204	208
September 2013	240957	220
October 2013	243754	207
November 2013	246601	191
December 2013	248660	204
January 2014	251077	192
February 2014	253769	151
March 2014	256422	203
April 2014	258526	174
May 2014	262368	189
June 2014	248904	166
July 2014	251791	154
August 2014	254446	198
September 2014	256578	170
October 2014	258660	179
November 2014	255648	173
December 2014	257317	154
January 2015	258744	171
February 2015	262399	161
March 2015	268422	204
April 2015	271426	197
May 2015	274679	180
June 2015	277898	194
July 2015	280348	171
August 2015	282881	194
September 2015	285185	181
October 2015	287715	201
November 2015	290159	177
December 2015	291772	211

January 2016	292473	151
February 2016	295665	167

eTable 2. Risk per 10,000 individuals of suicide death within five years by divestment status, stratified by sex.

	Divestment, Risk (95% CI)	No divestment, Risk (95% CI)	Risk ratio (95% CI)	Risk difference (95% CI)
Any suicide death				
Men	28.8 (16.0, 43.2)	16.3 (14.3, 19.6)	1.77 (0.99, 2.61)	12.6 (-0.2, 26.0)
Women	10.7 (0.0, 24.8)	8.1 (5.7, 13.4)	1.32 (0.00, 3.32)	2.6 (-8.8, 16.8)
Firearm suicide death				
Men	8.4 (2.6, 14.9)	14.5 (12, 17.1)	0.58 (0.18, 1.07)	-6.0 (-12.3, 1.0)
Women	0.0 (0.0, 0.0)	6.8 (4.0, 9.9)	0.00 (0.00, 0.01)	-6.7 (-9.9, -4.0)

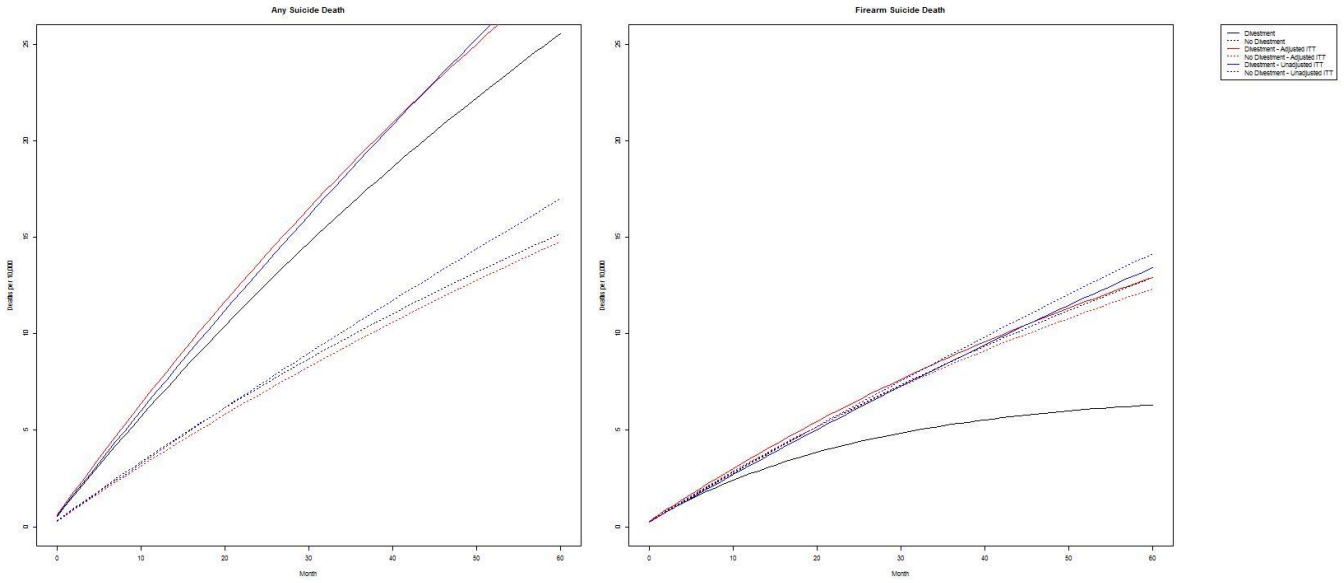
eTable 3. Sensitivity analyses for adjusted 5-year risk per 10,000 individuals of suicide death by divestment status, by varying divestment timing measurement strategies for eligible divestment types and restricting the outcome to only deaths occurring in the home.

	Divestment, Risk (95% CI)	No divestment, Risk (95% CI)	Risk ratio (95% CI)	Risk difference (95% CI)
Any suicide death				
Assuming no lag	26.2 (14.3, 37.8)	15.6 (13.1, 17.2)	1.68 (0.96, 2.57)	10.6 (-0.6, 23.2)
Assuming 4-month lag	24.0 (13.6, 36.0)	14.7 (13.2, 17.2)	1.63 (0.86, 2.39)	9.3 (-2.1, 20.5)
Assuming 6-month lag	24.2 (13.5, 35.5)	15.6 (13.1, 17.4)	1.56 (0.89, 2.31)	8.7 (-1.8, 20.4)
Death in home	14.5 (7.2, 23.3)	11.0 (9.4, 13.1)	1.32 (0.67, 2.09)	3.5 (-3.9, 12.2)
Firearm suicide death				
Assuming no lag	6.3 (1.5, 12.3)	12.2 (10.8, 15.0)	0.51 (0.10, 0.97)	-6.0 (-11.6, -0.4)
Assuming 4-month lag	6.2 (1.6, 11.9)	12.5 (10.9, 14.6)	0.49 (0.12, 0.94)	-6.3 (-11.7, -0.8)
Assuming 6-month lag	6.4 (2.0, 12.0)	12.3 (11.0, 14.9)	0.52 (0.16, 0.95)	-5.9 (-11.3, -0.6)
Death in home	5.4 (1.3, 10.2)	9.7 (7.9, 11.5)	0.55 (0.13, 1.11)	-4.3 (-8.7, 1.1)

eTable 4. “Intention-to-treat” sensitivity analyses for adjusted 5-year risk per 10,000 individuals of suicide death by divestment status at baseline. Adjusted “intention-to-treat” analyses use the same pooled logistic regression model as described in the primary analysis, but do not censor (or adjust for censoring with weights) when deviating from the protocol.

	Divestment, Risk (95% CI)	**No divestment, Risk (95% CI)**	**Risk ratio (95% CI)**	**Risk difference (95% CI)**
Any suicide death	28.8 (19.8, 40.2)	14.8 (13.3, 17.9)	1.94 (1.28, 2.59)	14.0 (4.5, 24.1)
Firearm suicide death	12.9 (6.9, 20.7)	12.3 (10.8, 14.7)	1.04 (0.55, 1.70)	0.6 (-5.8, 7.6)

eFigure 1. Cumulative incidence of suicide death and firearm suicide death by divestment status estimated using adjusted and unadjusted "intention-to-treat" (ITT) analyses compared to the primary "per-protocol" estimates. Adjusted "intention-to-treat" analyses use the same pooled logistic regression model as described in the primary analysis, but do not censor (or adjust for censoring with weights) when deviating from the protocol. Unadjusted "intention-to-treat" analyses do not include baseline covariates in the pooled logistic regression model so as to illuminate the extent to which adjustment for measured baseline confounding affects estimates. "Per-protocol" analyses are the primary analyses as described (and reproduced from) the main article.



PART B

Supplemental Discussion of Competing Events

The analyses presented throughout censor when individuals under follow-up die by causes other than the outcome of interest in a particular analysis (i.e., suicide in the primary analyses). Usually, this censoring implies that the causal interpretation of our results would be under elimination of these other causes, which can be viewed as a controlled direct effect with an ill-defined intervention preventing the competing events.[1] For example, the primary results would be interpreted as the effect of divestment had nobody died of other causes throughout follow-up. The meaningfulness of the controlled direct for decision-making purposes has been debated at length in the methodologic literature; many argue the "total" effect of divestment on suicide – including through possible effects on other causes of death – has a more useful interpretation. Moreover, the controlled direct effect can only be identified under an assumption that we can appropriately measure and adjust for shared causes of suicide and non-suicide deaths.

Here, we argue that our results can reasonably be interpreted as an *attempt* to estimate the total effect on suicide, albeit nonetheless confounded as described in the main text. This is based on two key conjectures, both incorporated into the simplified causal diagram below.

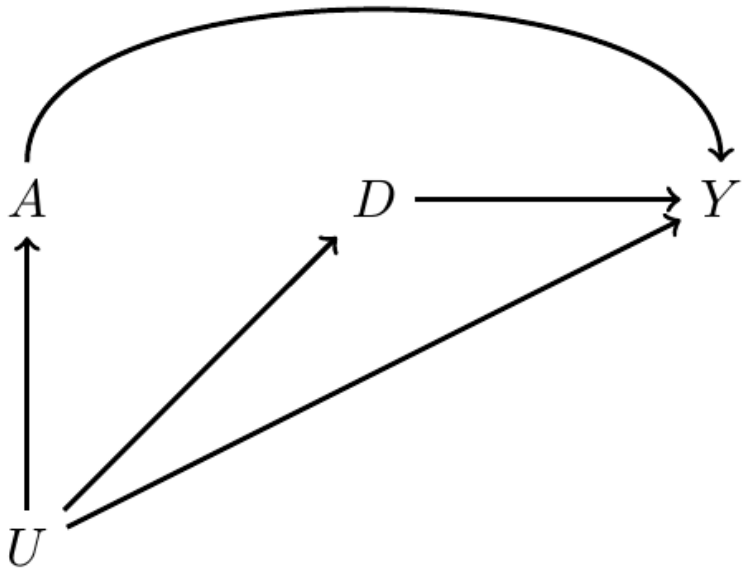
One, the causal effect of divestment directly on other causes of death is plausibly close to null, or at least is comparably small. Divestment (of lawfully acquired firearms) might directly affect other firearm-related deaths (homicide; accidental deaths) but these deaths are rare in comparison to suicide; all other causes of death should only be affected insofar that divestment first affects the risk of firearm-specific deaths. In the causal diagram, this is depicted by the absence of an arrow from divestment to non-suicide deaths, acknowledging that the arrow may in fact be present but represent a weak effect via homicide or accidental deaths.

Two, there is substantial unmeasured confounding between divestment and other causes of death. This is supported by the implausible effects estimated in our negative control analyses presented in the main text. In the causal diagram, this is depicted by the shared cause of divestment and other causes of death, *U*.

Together, this suggests the total effect does not functionally include pathways through other causes of death at all (the only effect of divestment on suicide in the causal diagram is direct) *and* that identifying this total effect would require blocking the backdoor paths *A*-*U*-*Y* and *A*-*U*-*D*-*Y*. Censoring by other causes of death blocks one of these paths (*A*-*U*-*Y* cannot be blocked without measuring *U*).

Though censoring can also open up a new backdoor path if there are further unmeasured causes of *D* and *Y* (as *D* could act then as a collider), we justify our choice as mitigating the identified source of bias at risk of opening up hypothetical other sources of bias. This justification is in part motivated by having no *a priori* strong causes of suicide and non-suicide deaths in mind that are not also part of our conjectured explanation of the negative control analyses. That is, mental and physical health shared causes of *D* and *Y* are also likely linked to *A* (as discussed in the main text) and thus it is unclear what new bias exist beyond the general unmeasured confounding via *U* that is unavoidable with or without censoring. Future studies that include data on plausible shared causes of *A*, *D*, and *Y* (e.g., via linkage to healthcare records; via qualitative and quantitative studies of divesters assessing the reasons for and context surrounding their choice to divest) may shed new light on this topic.

eFigure 2. A causal directed acyclic graph indicating inferred relationships between divestment (*A*), non-suicide deaths (*D*), suicide death (*Y*), and unmeasured causes of each (*U*). For explanatory purposes, this causal diagram is simplified so as not to index how each node can also vary over time, and does not include further measured covariates.



PART C

Supplemental Discussion of Quantifying Bias

As discussed in the main text, we interpret our collective estimates as evidence of the *existence* of bias in our primary analyses. Here were take two approaches to try to quantify the *magnitude* of bias and therefore explore what might be a plausible range of bias-adjusted estimates. We focus here on residual baseline confounding – i.e., failure to emulate randomization from a target trial – and utilize the following bias formulas[2-4] for risk ratios in the presence of a binary unmeasured confounder:

$$\frac{RR_k^{est}}{RR_k^{true}} = \frac{1 + (\gamma_k - 1)\Pr\left[U = 1 | A = 1, L = l\right]}{1 + (\gamma_k - 1)\Pr\left[U = 1 | A = 0, L = l\right]}$$

such that *k* indexes that this formula is specific to a particular outcome Y_k, *A* denotes divestment status, *L* denotes the measured variables adjusted for in the current study, *U* denotes a binary unmeasured confounder, and $\gamma_k$is defined as follows

$$\gamma_k = \frac{\mathrm{E}[Y_k | A = a, L = l, U = 1]}{\mathrm{E}[Y_k | A = a, L = l, U = 0]}$$

and is assumed to be the same for levels of baseline divestment status. In the conjectures below, we consider scenarios for suicide, firearm suicide, non-firearm suicide, and unintentional overdose deaths, which we index with *k* = *s*, *fs*, *nfs*, *overd*, respectively.

Conjecture 1: Consider an arbitrary binary unmeasured confounder such that the conditional relative risk of the confounder with non-firearm, firearm, and overall suicide is constant (i.e., $\gamma_s = \gamma_{fs} = \gamma_{nfs}$). If the true effect of divestment on non-firearm suicide risk is null (i.e., $RR_{nfs}^{true} = 1$; this would occur if there was no lethal means substitution), and failing to adjust for this confounder fully explains the non-null estimate, then the effect of divestment on firearm and overall suicide risk would be $\frac{RR_{fs}^{est}}{RR_{nfs}^{est}}$ and $\frac{RR_{s}^{est}}{RR_{nfs}^{est}}$, respectively.

Proof: Assuming $\gamma_s = \gamma_{fs} = \gamma_{nfs}$ trivially implies that the following ratios are equal to one another and equal to the estimated relative risk for non-firearm suicide when RR_{nfs}^{true}=1:

$$\frac{RR_s^{est}}{RR_s^{true}} = \frac{RR_{fs}^{est}}{RR_{fs}^{true}} = \frac{RR_{nfs}^{est}}{RR_{nfs}^{true}} = RR_{nfs}^{est}$$

Rearranging terms implies:

$$RR_{fs}^{true} = \frac{RR_{fs}^{est}}{RR_{nfs}^{est}}$$

and

$$RR_{s}^{true} = \frac{RR_{s}^{est}}{RR_{nfs}^{est}}$$

■

Thus, under these assumptions, our point estimates imply that the bias-corrected 5-year risk ratios for firearm and overall suicide would be 0.07 and 0.23, respectively.

Conjecture 2: Suppose that the true effect of divestment on unintentional overdose death is null (i.e., $RR_{overd}^{true} = 1$) and that the estimated effect could be explained by an arbitrary binary unmeasured confounder for a given value γ_{overd} and $p_{ndiv} = \Pr[U = 1|A = 0, L = l]$. Then, if that same confounder was related to firearm or overall suicide risk with specified values γ_{fs} and γ_s, this implies the effect of divestment on firearm and overall suicide risk would be $RR_{fs}^{est} \Big/ \frac{1+(\gamma_{fs}-1)p_{div}}{1+(\gamma_{fs}-1)p_{ndiv}}$ and $RR_{s}^{est} \Big/ \frac{1+(\gamma_{s}-1)p_{div}}{1+(\gamma_{s}-1)p_{ndiv}}$, respectively, with $\Pr[U = 1|A = 1, L = l] = p_{div} = \frac{(RR_{overd}^{est})(1+(\gamma_{overd}-1)p_{ndiv})}{RR_{overd}^{true}} - 1 \Big/ \gamma_{overd} - 1$.

Proof: We begin with the bias formula for unintentional overdose death risk:

$$\frac{RR_{overd}^{est}}{RR_{overd}^{true}} = \frac{1 + (\gamma_{overd} - 1)p_{div}}{1 + (\gamma_{overd} - 1)p_{ndiv}}$$

Rearranging terms, we can solve for p_{div}:

$$p_{div} = \frac{(RR_{overd}^{est})(1 + (\gamma_{overd} - 1)p_{ndiv})}{RR_{overd}^{true}} - 1 \Big/ \gamma_{overd} - 1$$

Next considering the bias formulas for firearm and overall suicide, we can rearrange terms to solve for the bias-corrected risk ratios:

$$\frac{RR_{fs}^{est}}{RR_{fs}^{true}} = \frac{1 + (\gamma_{fs} - 1)p_{div}}{1 + (\gamma_{fs} - 1)p_{ndiv}}$$

$$\rightarrow RR_{fs}^{true} = RR_{fs}^{est} \Big/ \frac{1 + (\gamma_{fs} - 1)p_{div}}{1 + (\gamma_{fs} - 1)p_{ndiv}}$$

and

$$\frac{RR_{s}^{est}}{RR_{s}^{true}} = \frac{1 + (\gamma_{s} - 1)p_{div}}{1 + (\gamma_{s} - 1)p_{ndiv}}$$

$$\rightarrow RR_{s}^{true} = RR_{s}^{est} \Big/ \frac{1 + (\gamma_{s} - 1)p_{div}}{1 + (\gamma_{s} - 1)p_{ndiv}}$$

with p_{div} taking the value derived above.

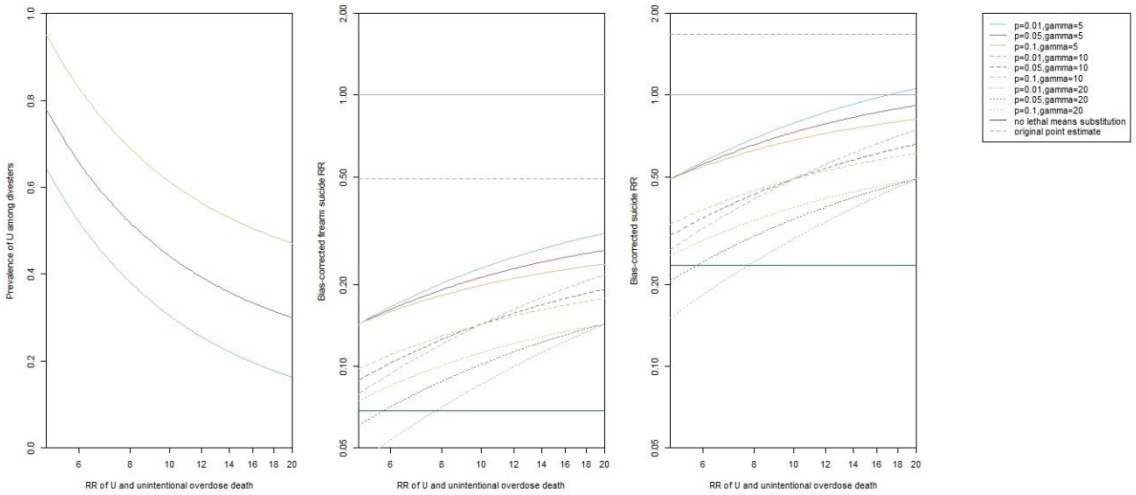
■

Thus, under these assumptions, our point estimates imply that the bias-corrected 5-year risk ratios for firearm and overall suicide would take the values presented in eFigure 3 for the range and combinations of values of $p_{ndiv}, \gamma_{overd}, \gamma_{fs}, \gamma_s$ we consider.

To put this range of estimates into context, let's consider plausible bias parameters for one of the strongest risk factors for both death by unintentional overdose and by suicide: past-year emergency department visits for acute alcohol intoxication, alcohol dependence, or alcohol abuse. Standardized mortality ratios for this medical history have been estimated to be 15 for deaths by overdose and 7 for deaths by suicide; we can take these are approximations for our bias parameters of relative risks (i.e., $\gamma_{overd} = 15$ and $\gamma_{fs} = \gamma_s = 7$).[5] If we assume that 1% of handgun owners who did not divest had this medical history at time zero, this would imply the prevalence of this medical history among those who did divest was 21%. The bias-corrected 5-year risk ratio for divestment on firearm suicide would be 0.23. The bias-corrected 5-year risk ratio for divestment on overall suicide would be 0.79.

Because our interest is in sustained treatment strategies, we note that using this bias formula developed for a point intervention misses the nuance of how *U* may affect time-varying confounding (e.g., staying divested once divested). Using a binary *U* also is overly simplistic. When more empirical evidence is gathered about the motivations for and context surrounding divestment, including remaining divested, these bias analytic approaches could be updated to reflect such acquired knowledge.

eFigure 3. Prevalence of an unmeasured confounder among divesters and range of bias-corrected 5-year risk ratios for firearm and overall suicide under a range of assumed permutations of bias parameters.



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