

# Handgun Divestment and Risk of Suicide

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**Background:** Firearm ownership is strongly related to suicide risk, yet little is known about how much risk declines when ownership ends (“divestment”).

**Methods:** Using data from 523,182 handgun owners, we estimated the effect of divesting and remaining divested versus never divesting on the risk of suicide and firearm-specific suicide. We used pooled logistic regression with inverse probability weighting, adjusting for demographic and area-level measures.

**Results:** The 5-year risk of suicide death was 25.6 (95% confidence interval [CI] = 15.1, 37.2) per 10,000 persons with divestment and 15.2 (95% CI = 13.2, 17.3) per 10,000 persons with no divestment, corresponding to a risk difference of 10.4 (95% CI = 0.7, 21.1) per 10,000 persons. The 5-year risk of firearm-specific suicide death was 6.3 (95% CI = 1.4, 11.9) per 10,000 persons with divestment and 12.9 (95% CI = 11.0, 14.6) per 10,000 persons with no divestment, corresponding to a risk difference of -6.6 (95% CI = -11.4, -0.1) per 10,000 persons. Comparing divestment to no divestment, risks were elevated for deaths due to other causes proposed as negative control outcomes; we incorporated these estimates into a series of bias derivations to better understand the magnitude of unmeasured confounding.

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The study data was formed by linking confidential datasets held by three state agencies in California (Department of Justice, Secretary of State, Department of Public Health). The three agencies have prescribed processes for making their data available to researchers. Availability of supporting code: All analyses were conducted using the SAS initiators macro available here: <https://www.hsph.harvard.edu/causal/software/>.

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**Conclusions:** Collectively, these estimates suggest that divestment reduces firearm suicide risk by 50% or more and likely reduces overall suicide risk as well, although future data collection is needed to fully understand the extent of biases such as unmeasured confounding.

**Keywords:** divestment; firearm; handgun; suicide

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Access to firearms has consistently been shown to be strongly associated with risk of suicide.<sup>1–11</sup> This evidence base consists of ecologic, case-control, and cohort studies of individuals who own firearms and of households that contain them. Little is known, however, about the extent to which firearm owners who voluntarily divest their stock may experience a reduction in suicide risk. Many promising public health approaches to reducing firearm violence—including red flag laws, buyback programs, and lethal means counseling—hinge on divestment. Thus, directly estimating the effect of divestment on suicide deaths could better inform the rationale for and expected benefits from these prevention efforts.

Studying the effect of divestment complements prior studies of the causal relationship between gun access and suicide risk. Most notably, it mitigates the potential for “reverse causation” to inflate effect estimates: that is, when studying the effect of purchasing (rather than divesting), suicide intent may motivate some people to purchase firearms. While this form of bias does not easily explain published estimates for long-term suicide risk,<sup>10</sup> let alone estimates obtained in studies of children or in studies of adults who were not themselves owners of the firearms in their home,<sup>3,12,13</sup> it is nonetheless a recognized weakness of existing access studies. Unmeasured acute suicide intent could, of course, confound the effect of divestment on suicide risk, but the direction of confounding bias might act in the opposite direction if, for example, people divest because they, or their loved ones, recognize their vulnerability to suicide. Because so little is known about the reasons people divest of their firearms,<sup>14,15</sup> the extent to which confounding by suicide intent, or, for that matter, other shared causes of divestment and suicide, might introduce bias in assessing the effect of divestment on suicide risk is unknown. Indeed, what is known about why gun owners divest comes from a single national survey conducted in 2015, in which, remarkably, fewer than 10% of those who had sold or otherwise gotten rid of all household guns reported safety concerns as prompting their change in firearm access.<sup>14</sup>

Here, we aim to estimate the effect of handgun divestment on handgun owners' risk of suicide. Study data come from the Longitudinal Study of Handgun Ownership and Transfers (LongSHOT), a cohort study of registered voters in California between 2004 and 2016.<sup>11,16</sup>

## METHODS

### LongSHOT

The study was approved by the institutional review board at Stanford University. Design of LongSHOT has been described at length elsewhere.<sup>11,16</sup> In brief, LongSHOT was formed by linking information on handgun transfers and all-cause mortality among adults in California to a series of 13 historical extracts of the Statewide Voter Registration Database (SVRD). The extracts were spaced approximately 1 year apart, spanning 18 October 2004, through 23 May 2016. The SVRD includes all registered voters in the state, creating a large sample of adults known to be alive and residing in California.

Virtually all lawful transfers of firearms in California must be transacted through a licensed firearms dealer.<sup>17</sup> Dealers are required to relay details of the transfers to the California Department of Justice, which archives this information in the Dealer Record of Sale (DROS) database. This has been done with handgun transfers for decades and for long-gun transfers since 2014.

We linked handgun transfers archived in the DROS database between 1 January 1985, and 29 February 2016, to the individuals in the SVRD files. (Other LongSHOT studies<sup>11,16</sup> have extended to 31 December 2016, using transactions measured in the Automated Firearm System updates in the remainder of 2016.<sup>15</sup>) This historic database on handgun transfers allowed us to identify handgun acquisitions, de-acquisitions, and “divestment”—defined as de-acquisition of the handgun (or last of the handguns, in the case of multi-gun purchasers) a person was observed to have acquired during the historical period. Finally, we obtained vital status on all people named in the SVRD files via linkage with the California Death Statistical Master Files.

In addition to time-varying information on the number of handguns each cohort member owned, their vital status, and whether they continued to reside in California, we obtained demographic characteristics using information from the SVRD files. Age was available in the SVRD; sex (male, female) was available but incomplete, with missing values imputed using a procedure based on predictions from historical data of newborn names by year of birth;<sup>18</sup> racial or ethnic group (non-Hispanic White, Hispanic, non-Hispanic Black, Asian, other, unknown) was imputed using a procedure with inputs of census block, surname, sex, and age.<sup>19,20</sup> Details of how these imputation procedures were implemented in LongSHOT are provided in the appendix of Studdert et al.<sup>11</sup> (Part A XIII and XIV). We further obtained four area-level

variables based upon residential address: socioeconomic status index; total violent and property crime rates; and urbanicity. We computed the Agency for Healthcare Research and Quality socioeconomic status index based on calendar year and census tract.<sup>21–23</sup> We calculated total violent and property crime rates (per 10,000 persons per year) at the county and calendar year level using official state statistics.<sup>24,25</sup> We categorized urbanicity according to the Rural Urban Commuting Area codes classification system,<sup>26,27</sup> linking from the 2010 census to individuals' residential census tract.

### The Protocol of the Target Trial

In this subsection, we describe the protocol of a target trial to estimate the effect of handgun divestment on suicide risk, and how our methodology emulates each element of this protocol (Table 1). It is not feasible for the target trial described here to be conducted as a real randomized trial, given ethical and practical constraints. However, discussing how our approach emulates such a trial illustrates, supports, and adds transparency to the analytic strategy we used to estimate causal effects and interrogate our estimates.

To be eligible, persons must be registered voters residing in California (as verified by the most recent SVRD extract); be 21 years old or older; have purchased their first handgun on or after 18 October 2004; owned one handgun for at least 3 consecutive months; and not have owned multiple handguns simultaneously. The eligibility criteria regarding first purchase timing, duration of ownership, and single handgun ownership are included in the target trial to increase confidence in an accurate measurement of handgun ownership at the time of eligibility. Specifically, restricting to those whose first purchases were in 2004 onwards and who owned at most one handgun minimizes misclassification accrual, while requiring ownership for at least 3 consecutive months helps to exclude those whose handgun purchase was directly motivated by acute suicide intentions.

Two treatment strategies are considered: (1) divest handguns at baseline and remain divested throughout follow-up or (2) continue to own at least one handgun (i.e., refrain from divestment at baseline and throughout follow-up). Outcomes of interest are suicide death, firearm suicide death, and non-firearm suicide death. Note firearm suicide deaths may be by any type of firearm and ownership of the firearm is not specified. Individuals are followed from baseline (randomization) until loss to follow-up, death, or 29 February 2016, whichever occurs earliest. Given prior work on LongSHOT suggesting that sustained handgun ownership is the norm and that divesters frequently reacquire handguns,<sup>15</sup> the causal contrast of primary interest from this target trial is the per-protocol effect—specifically, the effect of divestment and remaining divested compared to refraining from divesting throughout the study period. The effect among women and men separately is also of interest, as sex has been identified as a potential modifier in prior firearm access-suicide studies.<sup>11</sup>

**TABLE 1.** A Brief Overview of the Target Trial and How It Is Emulated in LongSHOT

Protocol Elements	The Target Trial	Emulation in LongSHOT
Eligibility criteria	Handgun owners 21 years old or older who are registered voters in California (as measured by the prior SVRD extract), purchased a first handgun on or after 18 October 2004, have owned a handgun at least 3 consecutive months, and have no history of owning multiple handguns concurrently	Same, assessed on the first of each month throughout LongSHOT <sup>a</sup>
Treatment strategies	(1) Divest and remain divested indefinitely and (2) never divest	Same
Treatment assignment	Randomly assigned at baseline	Assumed randomly assigned conditional on sex, race–ethnicity, age, cumulative months of handgun ownership, calendar year, area-level SES, area-level total property and violent crime rates, urbanicity
Follow-up period	Followed until death by any cause, 29 February 2016, or loss to follow-up (defined by leaving the voter files)	Same
Outcome	Suicide death, firearm-specific suicide death	Same, plus the following additional outcomes to explore vulnerabilities to key biases: death from any cause; liver disease death; automobile accident death; lung cancer death; myocardial infarction death; unintentional overdose death; suicide death outcomes specifically in the home
Causal contrast	Per-protocol effect (“divest and remain divested vs. never divest”), with subgroup analyses by sex	Same
Analysis plan	See main text	See main text

<sup>a</sup>Note a person may meet eligibility criteria in multiple months. SES indicates socioeconomic status.

## Emulating the Target Trial With LongSHOT

To emulate the target trial described above using LongSHOT data, we consider enrollment into a series of 134 “trials” in each month of observation between January 2005 and February 2016. We assessed the same eligibility criteria as described above for the target trial on the first day of each calendar month. We classified eligible individuals as divesters if they divested during the index month and as nondivesters otherwise. We make the assumption that we can emulate conditional randomization of treatment assignment conditional on the baseline variables listed in the analysis plan below. The outcome ascertainment, follow-up period, and causal contrasts are the same as are described for the target trial.

Divestment timing is measured indirectly by the acquisition of a handgun known to have been owned by a LongSHOT cohort member in a subsequent transaction. This approach means that the exact date of the cohort member’s divestment is well measured for some types of transfers (e.g., private party transfers, in which the cohort member is the transferor) while it is unknown for other types of transfers (e.g., dealer sales, in which the dealer is the transferor, the transferee is the next owner of the cohort member’s handgun). In our primary analyses, we estimate the date of divestment to be 2 months before the date of the new transfer for transaction types without well-measured dates. We chose this 2-month lag period on the basis of information obtained in interviews with two dealers and a Bureau of Firearms staff member and represented their best estimate of the average time to resale.<sup>11</sup> Private party transfers,

which have well-measured divestment dates, constitute the majority of divestment transfers in LongSHOT.<sup>15</sup>

## Analysis Plan

Using data on all eligible divesters and a 10% random sample of eligible nondivesters each month, we fit a weighted pooled logistic regression model for suicide death during a calendar month of follow-up to approximate a discrete hazards model. The model included a time-varying intercept (restricted cubic splines), an indicator for divestment status, a product term between divestment status and follow-up time, sex, racial or ethnic group, age, calendar year, cumulative months of handgun ownership, and the area-level measures of urbanicity, socioeconomic status, and crime rates (violent and property). We censor individuals when they deviate from their assigned strategy, meaning we censor when a divester reacquires a handgun or when a nondivester divests, and use time-varying inverse probability weights to mitigate the selection bias due to this censoring. These stabilized weights were functions of time-updating area-level measures (urbanicity, socioeconomic status, total violent and property crime rates), age, sex, and racial or ethnic group. Individuals were also censored in the first month they no longer appeared in the SVRD (loss to follow-up) or died from other causes, and time-varying inverse probability of censoring weights were fit as functions of the same covariates described for treatment weights plus divestment status. Weights were truncated at the 99th percentile.

To produce estimates for risk differences, risk ratios, and standardized survival curves by divestment strategy, we use predicted probabilities from the weighted pooled logistic regression model. For all analyses, 95% confidence intervals (CIs) were computed using nonparametric bootstrapping based on 500 resamples. Analyses were completed in SAS 9.4 (SAS Institute, Cary, NC) using the `initiators` macro (<https://www.hsph.harvard.edu/causal/software/>).<sup>28</sup>

We repeated this analysis procedure with firearm suicide and nonfirearm suicide as outcomes. To estimate effects among men and women separately, we fit the models for the weights using all individuals but fit the pooled logistic outcome model only within the relevant sex subgroup.

To investigate potential bias, we repeated the primary analysis procedure substituting as outcomes all-cause mortality and deaths due to liver disease, automobile accidents, lung cancer, unintentional overdose, and myocardial infarction. These five negative control cause-specific outcomes were chosen to study the potential for confounding or selection biases due to substance use, other psychiatric disorders, chronic and acute stress, and risk-taking behaviors—factors known to be strongly associated with suicide risk but that, to date, have not been studied in relationship with divestment. In addition, because outcome ascertainment is dependent on maintaining residency in California, and differential rates of institutionalization by divestment status are possible yet unobserved, we repeated the primary analysis procedure restricting the outcome to suicide deaths that occurred in the home. We determined suicide locations using information available in the death certificates. To investigate the sensitivity of our results to imperfect measurement of divestment dates of divestment, we repeated the primary analyses with varying lag periods from 0 to 6 months for specifying estimated dates. Finally, to contextualize how our findings are affected by nonadherence and our ability to adjust for time-varying selection bias induced by censoring for nonadherence, we also estimate the analog of a modified

intention-to-treat effect (the effect of divesting compared to not divesting at time zero regardless of subsequent acquisitions and de-acquisitions).

## RESULTS

Our analytic sample consisted of 18.7 million eligible person-months across 523,182 unique persons (Figure 1 and eTable 1; <http://links.lww.com/EDE/B970>). Table 2 shows baseline characteristics. During follow-up for each of the (nonunique) 16,075 “person-trials” in the divestment arm, there were 31 suicide deaths, with 263 (1.6%) deaths from other causes and 3,045 (18.9%) lost to follow-up before the administrative end of follow-up. During follow-up for each of the (nonunique) 18.7 million “person-trials” in the nondivestment arm, there were 19,895 suicide deaths, with 257,973 (1.4%) deaths from other causes and 2,105,563 (11.3%) lost to follow-up before the administrative end of follow-up. Suicide deaths by firearm accounted for 83.9% of the suicide deaths in the nondivestment arm and 45.2% in the divestment arm.

The estimated 5-year risk of suicide death was 25.6 (95% CI = 15.1, 37.2) per 10,000 persons with divestment and 15.2 (95% CI = 13.2, 17.3) per 10,000 persons with no divestment, corresponding to a risk difference of 10.4 (95% CI = 0.7, 21.1) per 10,000 persons (Table 3; Figure 2). The estimated 5-year risk of firearm-specific suicide death was 6.3 (95% CI = 1.4, 11.9) per 10,000 persons with divestment and 12.9 (95% CI = 11.0, 14.6) per 10,000 persons with no divestment, corresponding to a risk difference of −6.6 (95% CI = −11.4, −0.1) per 10,000 persons. Among men, the 5-year risk difference for suicide death was 12.6 (95% CI = −0.2, 26.0) per 10,000 persons; the 5-year risk difference for firearm-specific suicide death was −6.0 (95% CI = −12.3, 1.0) per 10,000 persons (eTable 2; <http://links.lww.com/EDE/B970>). Among women, the 5-year risk difference for suicide death was 2.6 (95% CI = −8.8, 16.8) per 10,000 persons; the 5-year risk difference for firearm-specific suicide death was −6.7 (95% CI = −9.9, −4.0) per 10,000 persons.

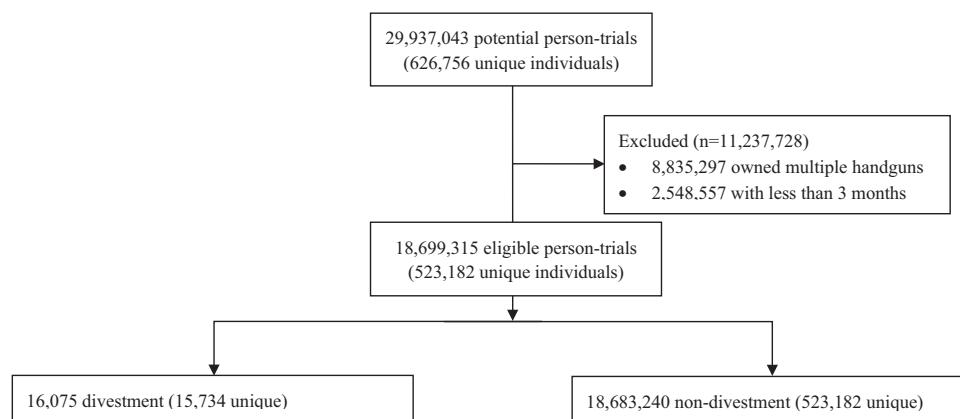


FIGURE 1. Flowchart of handgun acquirers in the LongSHOT into divestment arms, 2005–2016.

The point estimates for the 5-year risks for all-cause mortality and death by each of the negative control outcomes were all greater with divestment than without (Table 4). For example, the 5-year risk difference for all-cause mortality was 150.1 (95% CI = 97.9, 197.7) deaths per 10,000 persons; the 5-year risk difference for unintentional overdose was 11.6 (95% CI = 3.6, 20.0) per 10,000 persons. Sensitivity analyses varying the estimated divestment date and restricting to suicide deaths occurring in the home produced effect sizes similar to those estimated in the primary analyses (eTable 3; <http://links.lww.com/EDE/B970>). Compared with the primary per-protocol analysis, the modified intention-to-treat analysis estimated a bigger increased risk in overall suicide and close to no difference in risk in firearm suicide (eTable 4; <http://links.lww.com/EDE/B970>), with absolute firearm suicide risks changing

the most under divestment (eFigure 1; <http://links.lww.com/EDE/B970>).

In light of the estimates for nonfirearm suicide risk and negative control outcomes, we further derived the bias that would arise in effect estimates on our primary outcomes from an arbitrary unmeasured confounder that could conceivably explain our estimates of substantial increases in unintentional overdose and nonfirearm suicide deaths associated with divestment. The premises<sup>29–31</sup> and mathematical proofs for these bias derivations are provided in the eAppendix (eFigure 3; <http://links.lww.com/EDE/B970>), along with bias-corrected 5-year risk ratios for firearm and overall suicide estimated from scenarios varying mathematically possible confounder attributes. Bias-corrected 5-year risk ratios ranged from 0.04 to 0.31 for firearm suicide (compared with our estimated 5-year risk ratio of 0.49 in primary analyses) and from 0.15 to 1.06 for overall suicide (compared with our estimated 5-year risk ratio of 1.68 in primary analyses). The only scenarios we considered in which divestment was not protective against overall suicide was when an unmeasured confounder was exceedingly strongly associated with unintentional overdose (e.g., conditional risk ratio of 20) but much less so with suicide (e.g., conditional risk ratio of 5).

**TABLE 2.** Baseline Characteristics of Eligible Individuals by Divestment Status

Characteristic	Divestment Arm	No Divestment Arm
N person-trials	16,075	18,683,240
N unique persons	15,734	523,182
Sex, %		
Male	76.6	78.3
Female	23.2	21.5
Other/unknown	0.2	0.1
Race-ethnicity, %		
White	68.6	75.5
Hispanic	20.6	15.2
Black	5.7	4.7
Asian	4.5	4.0
Other	0.2	0.1
Unknown	0.4	0.5
Age, years, %		
21–34	50.1	34.5
35–49	25.0	28.7
50–64	17.5	25.9
65+	7.4	10.9
Urbanicity, %		
Urban core	84.3	82.3
Suburban	10.5	11.8
Large rural town	3.2	3.4
Small town isolated rural	1.9	2.5
Neighborhood socioeconomic status index, mean	68.8	70.5
Total property crime rate, mean	2,725.0	2,675.8
Total violent crime rate, mean	431.4	426.0
Cumulative months of handgun ownership, mean	27.0	33.8
Transaction type, %		
Private party transfer	57.4	N/A
Pawn redemption	1.0	N/A
Other	41.5	N/A

N/A indicates not available.

## DISCUSSION

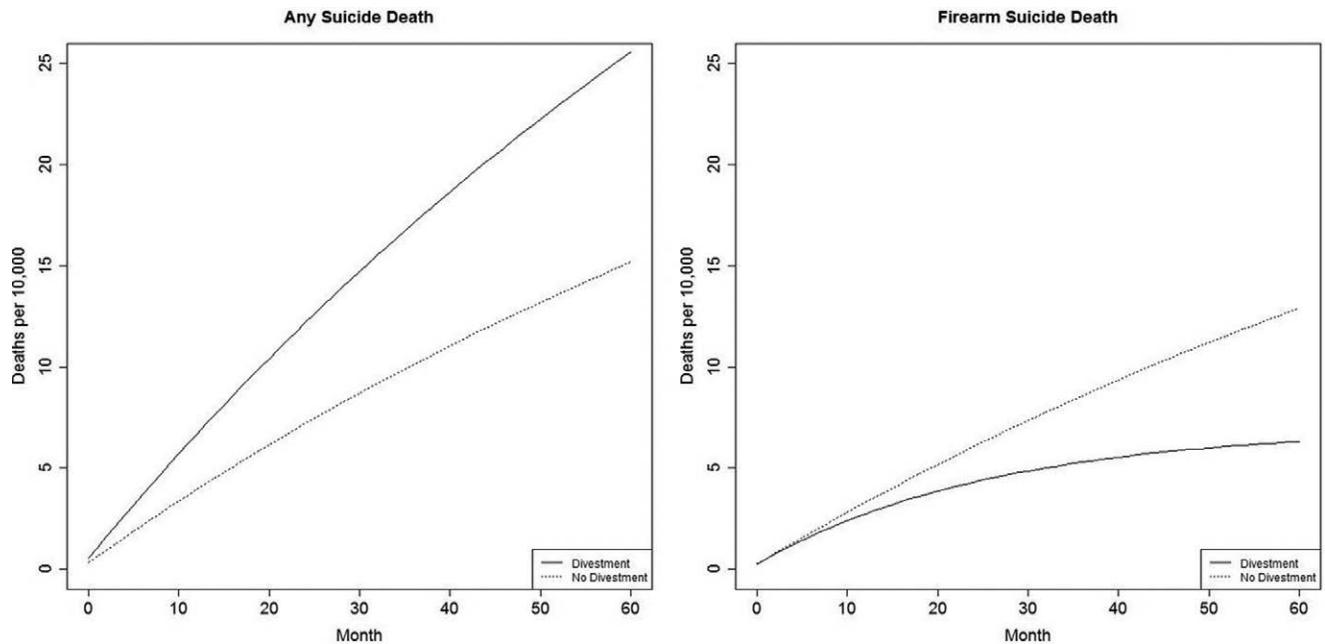
In this cohort study of registered voters in California, we estimated that the risk of firearm-specific suicide decreases substantially after handgun divestment. However, we also estimated that the risk of overall suicide, all-cause mortality, and several specific causes of death chosen as negative control outcomes increased after divestment. Because it is not reasonable to expect a harmful effect of divestment on these outcomes, we interpret our findings as implying that unmeasured confounding or other sources of bias are substantial. Through discussing and quantifying these possible sources of bias, we argue these results collectively suggest that the reduction in firearm suicide deaths under divestment is 50% or more, and that our results are consistent with a protective, and conceivably strongly protective, effect of divestment on overall suicide risk.

Given the limited literature on circumstances surrounding divestment,<sup>15</sup> our conjectures about the sources of substantial confounding are based primarily on our negative control outcome findings. That unintentional overdose deaths were three-fold higher after divestment suggests that the well-established risk factors shared by overdose death and suicide—e.g., mental health and substance use disorders—are likely strongly associated with divestment among handgun owners. Confounding by acute crisis periods (financial, health, or other) may also be a factor, especially considering the frequency with which divesters subsequently reacquire handguns.<sup>15</sup> In addition, voluntary divestment may be more common among those who know or suspect their death is imminent, perhaps as an action of reducing possessions,

**TABLE 3.** Adjusted 5-year Risk per 10,000 Individuals of Suicide Death by Divestment Status

Outcome	Divestment, Risk (95% CI)	No Divestment, Risk (95% CI)	Risk Ratio (95% CI)	Risk Difference (95% CI)
Any suicide death	25.6 (15.1, 37.2)	15.2 (13.2, 17.3)	1.68 (1.05, 2.40)	10.4 (0.7, 21.1)
Firearm suicide death	6.3 (1.4, 11.9)	12.9 (11.0, 14.6)	0.49 (0.11, 0.99)	-6.6 (-11.4, -0.1)
Nonfirearm suicide death <sup>a</sup>	16.8 (8.4, 26.3)	2.4 (1.6, 3.3)	7.16 (3.69, 12.11)	14.5 (5.9, 24.0)

<sup>a</sup>Estimated risks of firearm and nonfirearm suicide deaths do not sum to the estimated risk of any suicide death because each were modeled separately.

**FIGURE 2.** Cumulative incidence of suicide death and firearm suicide death by divestment status.

which might explain the elevated risk of all-cause mortality and suicide risk.<sup>32</sup>

In quantifying the bias that would arise in our primary outcome analyses from an arbitrary unmeasured confounder that would explain our estimates of substantially increased risk of unintentional overdose deaths associated with divestment, we found that divestment was protective against overall suicide for nearly any mathematically possible combination of confounder attributes (eAppendix; <http://links.lww.com/EDE/B970>). As prior literature indicates that lethal means substitution is minimal,<sup>7</sup> it is noteworthy that much of the range of plausible bias-corrected estimates from the negative control

analyses is close to the bias-corrected estimates obtained if we assume divestment has no effect on nonfirearm suicide. Moreover, the more plausible ranges of these bias-corrected protective effect estimates (of divestment on suicide risk) were a near-mirror image of the harmful effects that prior work had found accompanied purchasing a handgun.<sup>11</sup> As more is learned about the motivation for and circumstances surrounding voluntary divestment, and therefore what sources of unmeasured confounding can be named rather than conjectured, these types of bias analyses can be updated to produce a more empirically informed bias-corrected estimate of the effect of divestment on suicide risk.

**TABLE 4.** Adjusted 5-year Risk per 10,000 Individuals of All Deaths and “Negative Outcomes” Death by Divestment Status

Outcome	Divestment, Risk (95% CI)	No Divestment, Risk (95% CI)	Risk Ratio (95% CI)	Risk Difference (95% CI)
All death	384.4 (332.2, 431.9)	234.2 (223.9, 244.0)	1.64 (1.42, 1.86)	150.1 (97.9, 197.7)
Liver disease	7.6 (1.4, 15.5)	4.4 (3.2, 5.9)	1.71 (0.31, 3.77)	3.1 (-3.2, 11.5)
Automobile accident	4.6 (0.8, 10.2)	2.8 (1.8, 4.0)	1.64 (0.31, 3.88)	1.8 (-1.9, 7.1)
Lung cancer	22.6 (10.2, 36.6)	17.4 (14.5, 20.8)	1.30 (0.58, 2.16)	5.2 (-7.5, 18.8)
Unintentional overdose	16.4 (8.1, 24.9)	4.8 (3.2, 6.1)	3.43 (1.75, 5.70)	11.6 (3.6, 20.0)
Myocardial infarction	19.4 (6.8, 33.5)	15.9 (12.4, 18.8)	1.23 (0.42, 2.09)	3.6 (-8.9, 16.4)

Selection biases conceivably play an important role, as loss to follow-up, deaths by other causes, and “non-adherence” were all more common in the divestment arm. Comparing our modified intention-to-treat analysis to our primary per-protocol analysis suggests that “non-adherence,” if anything, creates an opportunity for bias in the opposite direction of the net bias inferred. Specifically, divesters frequently reacquired handguns, and when they did were more likely to die by firearm suicide, making the absolute firearm suicide risk of baseline divestment substantially higher than the risk estimated under sustained divestment, while the risk under nondivestment compared to sustained ownership were relatively similar. Although the number of suicide deaths is small, the comparison of baseline divestment versus sustained divestment aligns with what we would expect given prior studies on firearm access or handgun purchasing, that is, reacquiring a handgun increases suicide risk. In the eAppendix (eFigure 2; <http://links.lww.com/EDE/B970>), we further explore the nature of the competing event by other causes of death in relation to not just possible selection bias but also interpretation.<sup>33</sup> It is unclear what direction or magnitude of bias might be explained by the differential changes in remaining a resident and registered voter (i.e., loss to follow-up), although the standardized survival curves suggest there is nonetheless net bias in the first year or two of follow-up when loss to follow-up is lesser overall.

Our results should to be interpreted in light of our divestment measure, which captures only lawful transfers and only handguns owned by the individual. Unlawfully owned firearms, lawfully owned long guns, and guns owned by other members of the household are not accounted for and may be sources of confounding if differentially distributed by divestment status. Even if these other sources of firearm access are not associated with divestment status, their prevalence can still imply that the estimated effect on firearm-specific suicide death is attenuated relative to the effect that may have been observed if the research question was divestment of all types of firearms at the household level. Put another way, our analysis has misclassification biases relative to the question of studying household divestment of all firearms, in contrast to the narrower question of personal divestment of lawfully acquired handguns. This narrowed question nonetheless has policy salience, because lawfully owned guns are more likely to be the target of existing interventions like buyback programs or court-ordered removal of firearms that pose a danger to self or others. Another measurement limitation is that the precise timing of divestment is not known for some transfer types (e.g., dealer sales) comprising 41.5% of divestment transfers (Table 2), although sensitivity analyses were reassuring here because estimates were largely consistent across various methods for dating such divestments.

Although our intention was to estimate the causal effect of divestment, an unexpected finding that stems from our identification of probable residual confounding is that divestment may be a strong predictor of suicide. Indeed, the observed

(rather than adjusted) rate of suicide death among the 15,734 first-time eligible divesters was 61.6 (95% CI = 43.6, 84.6) per 100,000 person-years, over four-fold higher than the 13.6 per 100,000 person-years in the Californian general populations ages 21 and older in the same calendar years, and three-fold higher than the 21.2 per 100,000 person-years among Californian men ages 21 and older in the same calendar years.<sup>34</sup> This puts divestment on par with depression and other psychiatric disorders in terms of its relative strength as a predictor of suicide.<sup>35</sup> Thus, perhaps an actionable result for suicide prevention under current divestment patterns would be to consider episodes of divestment as an opportunity to provide suicide prevention services. Buyback programs, firearm dealerships, pawn shops, estate lawyer practices, and other places of divestment might explore incorporating such resources.

A focus on understanding the effects of divestment is a step closer to an understudied but practical and promising public health initiative: encouraging gun owners, especially those at risk of misusing firearms to hurt themselves or others, to give up their guns. While some of the current strategies for preventing firearm-related injuries focus on interventions at the point of purchase (e.g., background checks; waiting periods), such efforts have limited empirical support<sup>36</sup> and, importantly, do not address the risk posed by the enormous existing stock of weapons. Indeed, there are an estimated 300 million firearms in private hands such that more than one-fifth of US adults and one-third of US households currently possess one or more firearms.<sup>37,38</sup> Collectively and especially in light of our investigations of residual confounding, the current study suggests handgun divestment may reduce the risk of suicide deaths, with a reduction in firearm-specific suicide deaths of 50% or more for the handgun owners themselves. A fuller account of the public health impact of divestment would also need to reflect how divestment affects the risk of others in the household.

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