The Social Costs of Suicide

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Abstract

Suicide is a leading cause of death, yet its costs are conventionally counted as if they end with the person who dies. We use population-wide Swedish administrative data to provide quasi-experimental estimates of the costs that a suicide imposes on the surrounding social network. To address selection into suicidal behavior, we compare the families of individuals whose first suicide attempt is fatal to the families of individuals whose first attempt is non-fatal, leveraging elements of chance in whether an attempt proves fatal. A family member's suicide worsens relatives' mental health, reduces their earnings, and raises their mortality. Strikingly, it also transmits suicide itself: relatives' own suicide mortality rises in every direction we measure—from children to parents, from parents to children, and across siblings, partners, and co-workers. Benchmarking against fatal accidents shows that bereavement accounts for the deterioration in mental health and earnings, while the transmission of suicide appears only after suicide. Our estimates imply that every 100 suicide deaths cause roughly 0.4 to 0.8 additional suicide deaths and three to six additional deaths among close relatives within five years. Thus, an accounting that counts only the decedent understates the mortality cost of suicide and the benefits of prevention by at least 3 to 6 percent.

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1 Introduction

More than 700,000 people die by suicide each year—727,000 in 2021, the most recent year for which global estimates are available—making it a leading cause of death worldwide and, among the young, one of the leading causes of death overall: globally, suicide is the third leading cause of death among 15- to 29-year-olds (World Health Organization 2025). In the United States, suicides outnumber homicides roughly two to one (National Center for Health Statistics 2025), and firearm suicides reached an all-time high of more than 28,000 deaths in 2025 (The Trace 2026). The way societies count the costs of these deaths, however, treats suicide as a cost borne by the person who dies. In public-health budgeting, cost-benefit analyses of prevention policy, and the economic literature, a suicide is typically counted as a life lost and valued as a statistical life. Public discussion often goes further, framing suicide as a death that, whatever its tragedy, "at least harmed no one else." Whether this accounting is even approximately right depends on an empirical question about which there is remarkably little causal evidence: what does a suicide do to the people left behind?

This paper provides, to our knowledge, the first quasi-experimental accounting of the social costs of suicide across both family and workplace networks. We trace the effects of the same event through the deceased's parents, siblings, partner, adult children, and co-workers, and distinguish the general costs of bereavement from suicide-specific costs. We find that these costs are large, that they span mental health, mortality, and economic outcomes, and that a distinct component of them is the propagation of suicide itself: a family member's or co-worker's death by suicide causally raises survivors' own suicide mortality, in every direction of the family tree and beyond it into the workplace. A suicide is not the end of a tragedy borne alone; it is, with meaningful probability, the beginning of another.

Estimating these effects requires confronting a fundamental selection problem. Families affected by suicide differ from other families long before the event: mental illness, economic distress, and suicidal behavior itself run in families, through genes and through shared environment. A comparison of bereaved families to unaffected families—the design underlying most existing evidence on suicide bereavement—conflates the effect of the death with everything that made the death more likely. The observational literature is well aware of the problem. A recent meta-analysis of social transmission *outside* the family pools 153 effect sizes from 59 studies covering more than one million individuals. It concludes that suicidality appears transmissible between friends and peers, while cautioning that nearly all underlying studies are observational and at high risk of bias (Gavan et al. 2026). For transmission *within* the family, where selection on shared genetic and environmental liability is most severe, no comparable synthesis of causal evidence exists.

Our research design addresses this selection problem by conditioning on the suicide attempt itself. Using population-wide Swedish administrative data, we identify all individuals whose first suicide attempt occurs between 2001 and 2019, more than 120,000 individuals. We do so by combining hospitalization records carrying codes for intentional self-harm with cause-

of-death records. We then compare the outcomes of relatives of individuals whose first attempt was *fatal* to the outcomes of relatives of individuals whose first attempt was *non-fatal*. Every family in our comparison has experienced a suicide attempt, but what differs is whether the attempt was survived. Our design leverages elements of chance in this outcome together with a parallel-trends assumption. Whether an attempt is fatal depends in part on circumstances outside the control of the family, such as whether someone comes home in time or the minutes an ambulance takes. Relatives of fatal and non-fatal attempters are on parallel trajectories in mental health, earnings, and mortality for years before the event, and diverge only at it. Because we find that suicidal behavior propagates within families in all directions, our preferred sample restricts to focal individuals who are the first in their family to experience a suicide event. This makes the event we study an unambiguous first exposure for the family unit.

We also construct a parallel quasi-experiment for *sudden accidental death*: fatal versus non-fatal traffic accidents, processed through an identical pipeline and reweighted to match the suicide sample's age-at-event distribution. This benchmark allows us to decompose each estimate into the effect of losing a family member suddenly and the additional effect of losing them *to suicide*.

Our analysis yields three sets of results. First, we quantify the costs a suicide imposes on the family. In the five years following a fatal (relative to a non-fatal) attempt, the probability that a parent fills a psychotropic drug prescription rises by 5.0 percentage points—a 25% increase over the pre-event base—with comparable increases for partners and smaller ones for siblings and adult children (Table 2). Mothers' labor earnings decline by roughly 7%, and partners' by roughly 4%, with no recovery over the horizon we observe. Parents' and partners' all-cause mortality rises by 1.4 and 2.0 percentage points, respectively, over five years. These effects are largest where the deceased is youngest: when the person who dies is under 25, parents' psychotropic drug use rises by 58% and mothers' earnings decline by roughly 11% (Table 3). Against the backdrop of the ongoing debate over youth mental health (Currie 2025), these results underscore that youth suicide is a shock absorbed by an entire family. One cost we do not find: couple dissolution. Divorce among bereaved parents *falls* following a fatal attempt, and continues to fall in the second five years after the event.

Second, and most strikingly, we find that suicide propagates. Over the five years following a fatal attempt, the probability that a surviving family member *dies by suicide* rises by 0.43 percentage points for partners, 0.07 percentage points for parents, 0.08 percentage points for siblings, and 0.03 percentage points for adult children. These are large proportional effects relative to near-zero baselines (Table 2, column (8)). The transmission runs in every direction we can measure: not only from parents to children, the direction emphasized by the literature on intergenerational transmission, but also upward from children to parents, laterally across siblings, and between partners. The partner relationship is particularly informative because it involves no genetic link and therefore rules out a purely hereditary explanation. The transmission also extends beyond the family. Linking focal individuals to their establishments, pre-

liminarily, we also find that a co-worker's fatal (versus non-fatal) attempt raises the five-year suicide mortality of demographically similar workplace peers by 0.05 percentage points.

Third, the traffic-accident benchmark decomposes these costs into bereavement and contagion. The mental-health and economic effects of losing a family member to a fatal accident resemble those of losing a family member to suicide: psychotropic drug use rises, earnings fall, divorce declines. Sudden bereavement, whatever its cause, is costly. But surviving relatives' own suicide mortality moves only after a suicide: in the accident benchmark it is a precisely estimated zero for parents, siblings, and children, and an estimated zero for partners against 0.43 percentage points after a partner's suicide (Tables 4 and 5). Nor do we find displacement into other causes of death. Bereavement is general; the transmission of suicide is not.

Taken together, the results imply that the conventional accounting of suicide's costs substantially understates its consequences for families and social networks. Each suicide imposes measurable health and economic costs on those around the deceased and raises the probability of subsequent suicide. Combining our transmission estimates with the observed sizes of family and workplace networks, we calculate that every 100 suicide deaths cause roughly 0.4 to 0.8 additional suicide deaths within five years. This range brackets, under a forward-looking causal design, the 0.69% of suicides that Pitman et al. (2022) attribute to suicide bereavement in Danish registry data. Counting deaths from all causes, every 100 suicide deaths generate an additional three to six deaths among close family members within five years. Preventing 100 suicides therefore saves not 100 lives but roughly 103 to 106 lives within five years alone, before accounting for induced suicide attempts, effects over longer time horizons, morbidity, or lost earnings.

Two policy implications follow. First, the benefits of preventing suicide exceed the direct mortality benefit by at least 3–6%, equivalent to roughly $0.4 to $0.8 million per suicide at the value of a statistical life used in US policy analysis. Second, "postvention"—support for the bereaved—should be viewed not only as support after a suicide but also as suicide prevention in the strict sense, because our estimates show that bereaved relatives face causally elevated suicide risk.

This paper contributes to four literatures. First, we contribute to the economics of suicide. Since Hamermesh and Soss (1974) modeled suicide as a forward-looking choice, economists have studied the determinants of suicide—economic conditions, access to means and to treatment, and the strategic element of attempts (Cutler et al. 2001; Ludwig et al. 2009)—and suicide figures centrally in the account of rising "deaths of despair" among less-educated Americans (Case and Deaton 2020, 2022). This literature concerns the causes of suicide; its costs have been left to a per-decedent accounting. We provide the missing demand side of prevention policy: causal estimates of what is saved—beyond the decedent's own life—when a suicide is prevented. Our results also engage the youth mental-health debate (Currie 2025; Blanchflower et al. 2024): the families of the young are where we find the largest spillovers, so whatever is driving youth suicide, its social costs are larger than its death counts suggest.

Second, we contribute to a large multidisciplinary literature on suicide contagion and suicide bereavement. Since Phillips (1974) documented suicide spikes following front-page suicide coverage—the "Werther effect"—a substantial body of work has linked media portrayals to subsequent suicides (Niederkrotenthaler et al. 2020), and registry-based studies have documented elevated suicide risk among the bereaved: in offspring after parental suicide, in parents after a child's suicide, in bereaved spouses, and among exposed co-workers (Agerbo 2005; Hedström et al. 2008; Erlangsen et al. 2017), as reviewed in Pitman et al. (2014). Thus the epidemiological literature has observed associations in several directions of the network; what it has not been able to do, as syntheses of the nonfamilial evidence acknowledge (Gavan et al. 2026) and as applies with still greater force within the family, is separate transmission from selection, because exposed and unexposed families differ in precisely the characteristics—including shared genetic liability—that predict suicide. Our contribution is a design that conditions on the attempt, an accident benchmark that isolates what is specific to suicide, and a unified framework that estimates transmission in all directions and in two networks at once, alongside the economic outcomes this literature does not observe. Two contemporaneous papers bring quasi-experimental designs to this question, and our work is complementary to both. Cuddy et al. (2026) find, in a matched difference-in-differences design on South Carolina school records, that a schoolmate's suicide raises adolescents' self-harm diagnoses by nearly 50% over two years, with no comparable effect of deaths from assault or traffic accidents; Halla and Schmidpeter (2024) find in Austrian data that a co-worker's suicide raises own suicide risk over twenty years, with no effect of a co-worker's fatal car accident. The three papers follow different networks: schoolmates at one stage of life, co-workers, and, here, the family across the entire age range of the person who attempts, together with the workplace. Ours differs also in what it measures—survivors' own suicide and all-cause mortality alongside mental health and earnings—and in its identifying variation, whether an attempt proved fatal, which holds fixed selection into the event itself. Our contribution relative to both is the family: the same event followed through every direction of the family tree, with the bereavement/contagion decomposition.

Third, we contribute to the quasi-experimental literature on family spillovers of severe health shocks and death. Economists have traced the effects of spousal health shocks and bereavement on health behaviors, labor supply, and mortality (Fadlon and Nielsen 2019, 2021; Espinosa and Evans 2008), of losing a child (van den Berg et al. 2017), of losing a parent (Böckerman et al. 2023; Jensen and Zhang 2026), of fetal exposure to maternal bereavement (Persson and Rossin-Slater 2018), and of exposure to violent death in schools and communities (Rossin-Slater et al. 2020; Bharadwaj et al. 2021). Relative to this literature we make two contributions. Methodologically, the fatal/non-fatal contrast within attempters addresses a selection margin that bereavement designs typically cannot. The sharpest existing designs compare among the bereaved, using the timing of a relative's death—during pregnancy versus shortly after birth—for identification (Persson and Rossin-Slater 2018); our design holds fixed not only exposure to

the event but selection into the event itself, comparing families all of whom stood at the same precipice. Substantively, we show that the *cause* of death matters. The bereavement components of our estimates—mental health, earnings, mortality—resemble what this literature finds for other sudden deaths; the transmission of suicide is the component unique to suicide, and it is the component that compounds, because each induced death seeds the network anew. Our finding of causal child-to-parent and partner-to-partner transmission also complements the literature on the intergenerational transmission of mental health (Bütikofer et al. 2024), which has focused on the parent-to-child direction.

Fourth, we contribute to the study of the workplace as a site of social influence. Co-workers have been shown to shape one another's retirement, effort, and parental leave decisions (Duflo and Saez 2003; Mas and Moretti 2009; Dahl et al. 2014); co-worker deaths affect survivors' wages through the substitutability channel (Jäger et al. 2024); and violence and harassment between colleagues carry lasting labor-market consequences (Folke and Rickne 2022; Adams-Prassl et al. 2024). We show that the workplace also transmits the most extreme of health behaviors. The juxtaposition with Jäger et al. (2024) is instructive: whatever small pecuniary gains accrue to incumbent workers through labor-market adjustment after a co-worker's death, a co-worker's *suicide* leaves behind an elevated risk of death among the survivors themselves.

The remainder of the paper proceeds as follows. Section 2 describes the setting, the data, the construction of the event and linked samples, and the descriptive facts that motivate the analysis. Section 3 presents the empirical strategy: the source of the identifying variation, the specification, what the design requires and what it does not, why we lead with the reduced form, and the traffic-accident benchmark. Section 4 presents the results. Section 5 aggregates them into a suicide multiplier and a per-suicide mortality cost. Section 6 concludes.

2 Setting, Data, and Samples

2.1 Setting

Two institutional features of the Swedish setting are particularly important for our analysis. First, healthcare is universal and essentially free, so an individual who survives a suicide attempt is very likely to appear in the inpatient register, irrespective of income or insurance status. Hence, the population of *surviving* attempters we observe is not selected by the ability to pay for care. Second, Sweden's personal identity number allows individuals to be linked across health and labor market registers, as well as across family networks. We use these links to connect a person who attempts suicide to their parents, siblings, partner, children, and co-workers, and to follow them before and after the event.

2.2 Data

The backbone of our data is an extract from the Swedish Population Register covering all individuals residing in Sweden from 2000 onward. We merge these data with Statistics Sweden's longitudinal database of individuals (LISA), which contains demographic and socioeconomic information drawn from various administrative records (Statistics Sweden 2026). We link individuals to their spouses using marital records and, from 2011, to cohabiting partners using information on shared addresses; hereafter, we refer to a person's spouse or cohabiting partner simply as their partner. We further link individuals to their parents, siblings, and children using the Statistics Sweden multigenerational register, and to the establishment where they work each year using LISA. To construct measures of suicidal behavior and health outcomes, we merge in health records from the National Board of Health and Welfare (Socialstyrelsen 2019). Two registers are central to our analysis.

The Cause of Death Register. We observe the universe of deaths among Swedish residents from 2001, including the exact date of death and the underlying cause coded according to ICD-10 (Brooke et al. 2017). Cause of death is based on the death certificate, which is completed by a physician in Sweden. We use the underlying-cause variable to classify each death as intentional self-harm (ICD-10 codes X60–X84), an event of undetermined intent (Y10–Y34), a land traffic accident (V01–V19 and V30–V89), or another cause. Our *narrow* definition of a suicide death is X60–X84; our *broad* definition additionally includes Y10–Y34. The distinction between a death classified as intentional self-harm and one classified as an event of undetermined intent rests on the certifying physician's assessment of intent, which can be noisy. Intuitively, the broad definition includes deaths that may have resulted from intentional self-harm but for which the certifying physician could not establish this with certainty.[1] We observe cause of death through 2019 for suicide and through 2017 for traffic accidents. Appendix B.2 details the underlying data deliveries and the resulting coverage limitations.

The National Patient Register. We observe the universe of inpatient hospital admissions, with the date of admission, the date of discharge, the discharge destination, the main and secondary diagnosis codes, and—importantly—the external cause codes that record the mechanism of an injury (Ludvigsson et al. 2011). We identify a *non-fatal suicide attempt* as an unplanned inpatient admission for which any external cause code indicates intentional self-harm or an event of undetermined intent.[2] The external cause variable is coded differently across data deliveries, ranging from Swedish-text categories to explicit ICD codes. Appendix B.3 doc-

[1] Every table reports relatives' own suicide mortality under both the narrow and the broad measure (Appendix D.6). The underlying-cause codes are complete over 2001–2016 and allow a decomposition of suicide deaths by method (self-poisoning X60–X69, hanging X70, drowning X71, firearms X72–X74, other X75–X84), which we use in Section 3.1.

[2] We exclude admissions flagged as planned by the register, which typically represent scheduled follow-up care rather than the attempt itself.

uments the mapping we use in each period, as well as the ICD-9 equivalents (E950–E959 and E980–E989) that allow us to identify attempts as far back as 1987.

We make two measurement choices, detailed in Appendix B.4. First, a single suicide attempt frequently generates several inpatient records because patients are transferred between hospitals or readmitted on the day of discharge. We collapse overlapping, nested, and same-day-contiguous spells into a single attempt. Second, we determine whether an attempt is fatal exclusively by matching individuals to the death registers. We classify an attempt as fatal if and only if the individual's date of death coincides with the date of discharge, rather than using the hospital's recorded discharge destination, which is less reliable.

Other health data. We observe the universe of prescription drug fills dispensed by outpatient pharmacies from July 2005 through 2019, including the date of the fill and the drug's Anatomical Therapeutic Chemical (ATC) code. The data cover fills regardless of whether the purchase is covered by public health insurance.[3] We use these data to construct outcomes that measure mental health.

2.3 Events, Fatality, and the First-in-Family Restriction

We combine the National Patient Register and the Cause of Death Register into a single event dataset. Formally, an individual i *attempts suicide* on date d if either (i) i is admitted on an unplanned basis to an inpatient ward on date d with an external cause code indicating self-harm or undetermined intent, or (ii) i dies on date d with an underlying cause coded X60–X84 or Y10–Y34. An attempt is *fatal* if it satisfies (ii), or if it satisfies (i) and the individual's recorded date of death equals the date of discharge from that admission. All other attempts are *non-fatal*. Note that (ii) picks up the empirically important case of a fatal first attempt with no preceding hospitalization—an individual who dies without ever reaching a hospital.

For each individual attempting suicide, we record their *first* suicide attempt occurring between 2001 and 2019, using hospitalization records dating back to 1987 to verify that it is truly their first.[4] We refer to the individual who attempts as the *focal individual*, and to the date of their first attempt as the *event date*. We restrict to focal individuals aged at least 12 at the event date.

First attempt in the family. Our preferred sample applies one further restriction, which we label *first-in-family*: we drop focal individuals for whom any linked relative—parent, child, sibling, or partner—has a suicide attempt dated strictly before the focal individual's own first attempt. The reason is substantive rather than technical. One of the central findings of this paper is that suicidal behavior propagates within families, and in all directions. If we take each

[3] This is pharmacy transaction data for prescription drug fills, not insurance claims data.

[4] The lookback period matters: among individuals who eventually die by suicide during 2001–2019, 46% have a single attempt when attempts are counted over 2001–2019, compared with only 28% when attempts are traced back to 1987 (Appendix Table A11).

individual's first attempt without regard to their family, then the "first" attempt of a person whose sibling attempted three years earlier is not a first exposure, and the estimated effect on the rest of the family conflates the focal individual's attempt with the earlier one. Taking the first attempt in the *family* makes the event we study an unambiguous first exposure for the family unit. The restriction is not innocuous—it selects on the family's suicide history, which is the very margin the paper is about—so we report the corresponding results for the sample of all first attempts throughout (Appendix Table A7); they are similar. Appendix B.7 gives the construction.

2.4 Family Members and Co-workers

Starting from the set of first attempts, we link each focal individual to four types of relative: parents (biological and adoptive), siblings (all types), children (biological and adopted), and partners (married or cohabiting, identified in the year before the event). When a relative is linked to more than one focal individual, we retain the link to the focal individual with the earliest event date. All analyses are run on the *linked sample*: for each relative type, we keep focal individuals with at least one valid link of that type. Which relative types are observed depends on the focal individual's age: someone who attempts at 17 will typically have observed parents and siblings but no children or partner, while someone who attempts at 70 will typically have adult children but no living parents. Results are accordingly reported both pooled and by the focal individual's age at the event, with estimates reported only for relative types that are represented in the data.

Co-workers. To link focal individuals to their workplace peers, we use information in LISA that records, for each person-year, an employer (organization) identifier and an establishment identifier. The establishment identifier defines the workplace for our purposes, since one employer may operate many physically separate workplaces. We define the co-workers of a focal individual as all individuals employed at the focal individual's establishment in the calendar year *before* the suicide attempt; this corresponds to the workplace observed in November of that prior year. We exclude the focal individuals themselves, so the co-worker sample contains only individuals who did not attempt suicide. Roughly 61,000 focal individuals can be linked to an establishment in the year before their suicide attempt.

We flag three features of the co-worker sample. Appendix C.2 provides the full details. First, linkage to workplace peers requires an identifiable establishment. The establishment identifier is missing for 8,066 of these focal individuals, primarily because they are assigned the register's special "no fixed workplace" codes—including home-care workers and personal assistants, construction workers and salespeople with changing worksites, and contractors without a normal employment relationship. Our co-worker estimates therefore apply to focal individuals with an identifiable workplace. Second, the pre-event snapshot is taken in November of the calendar year before the event, so the effective lookback window varies with the month

of the suicide attempt: someone who attempts in January is matched to co-workers observed roughly 2 months earlier, while someone who attempts in December is matched to co-workers observed roughly 13 months earlier. Third, a co-worker linked to several focal individuals is included only once.

Defining the peer group. A workplace is not a peer group. The average establishment in our data has about 8 employees, but establishments in which someone attempts suicide the following year average around 181 employees. This difference is mechanical: larger establishments are more likely to contain at least one attempter. In an establishment of that size, the great majority of employees may never have met the focal individual. Pooling them with the focal individual's immediate colleagues would therefore attenuate any true peer effect. We therefore define the analysis sample in terms of *peers*: co-workers of the same sex as the focal individual and within five years of the focal individual's age at the event, at establishments with at most 43 employees (the median establishment size in our data). We drop establishments with a single employee, since the focal individual there has no co-workers by construction.

These criteria are intended to capture co-workers who are plausibly socially proximate to the focal individual: those of the same sex and within five years of age, working at relatively small establishments. We show that the results are similar using a 100-employee cutoff in Appendix Table A8.

2.5 Outcomes

We define outcomes for relatives and co-workers in four domains. Appendix C.3 gives exact definitions. *Mental health*: indicators for filling a prescription in each of three ATC categories, N05/N06 (the broad psychotropic category), N06A (antidepressants), and N05B (anxiolytics), constructed at quarterly frequency. We use a flow measure as our main specification, equal to one in quarters with at least one fill, and also a stock measure, equal to one from the first observed fill onward. *Mortality and self-harm*: quarterly indicators for all-cause death and for suicide death under both the narrow and broad definitions. Mortality outcomes are coded as absorbing, with the indicator equal to zero in all quarters before death and one thereafter. We retain individuals in the balanced panel after death, so mortality provides within-individual variation under individual fixed effects. For all non-mortality outcomes, observations are set to missing after the relative's own death, so those estimates are conditional on the relative's survival. *Labor market*: annual work-related income from LISA. We define this as the sum of gross wages from employment and positive net income from active self-employment, with no age restriction beyond LISA's own coverage of individuals aged 16 and above. *Couple stability*: three annual indicators for being divorced, ever having divorced, and having a different partner than in the year before the event. These outcomes are conditional on being married or partnered at $\tau = -1$ and are estimated for relatives aged 20 and older. For couple stability, we also address the separate competing risk of a spouse's death in Section 4.3. We do not exam-

ine couple-stability outcomes for the focal individual's *partner*, who mechanically loses their partner when the event is fatal.

2.6 Who Attempts Suicide, and Who Is Left Behind

Table 1 presents summary statistics for our main analysis samples: the focal individuals whose first attempt is the first suicide event in their family, and their linked parents, siblings, children, and partners. All characteristics are measured in the year before the focal individual's first attempt, and are shown by the focal individual's age at the event as well as pooled.

Three patterns stand out. First, the population at risk is large and spans the age distribution. Our first-in-family sample contains 103,416 focal individuals, spread across age groups: 33,387 first attempts by individuals under 25, another 30,035 at ages 25–44, 23,405 at ages 45–64, and 16,589 at 65 and older. Suicide attempts occur at all points in the lifecycle. The relatives we can study therefore vary with the focal individual's age. Parents and siblings are most relevant for younger focal individuals, while partners and adult children become more relevant at older ages. Siblings are represented throughout.

Second, focal individuals are strongly negatively selected. This motivates our focus on comparisons within families experiencing a suicide attempt, rather than comparisons with the general population. In the year before the event, 54% of focal individuals have filled at least one psychotropic prescription and 38% an antidepressant prescription, compared with 21% and 12% among their parents, and similar or lower rates among other relatives. Focal individuals also have low education (74% with no college) and low labor income, and 18% were born outside Sweden. Any comparison of these families to the general population would confound the effect of a suicide with this selection. Our design instead compares within the set of families experiencing a suicide attempt.

Third, the networks are large. The 103,416 focal individuals are linked to 167,571 parents, 197,550 siblings, 124,576 children, and 25,734 partners. Each attempt therefore places a substantial number of people in the population at risk of the spillovers we estimate. The mean age of linked parents at the event is 48 for focal individuals under 25 and 65 pooled. Linked children of focal individuals aged 45–64 average 25 years old, so many are adults whose own mental health, earnings, and mortality we can follow. Appendix Table A1 reports the same table for the sample of all first attempts.

2.7 Descriptive Patterns: Attempts, Deaths, and Age

Figure 1 presents the central descriptive facts about suicide in our data, plotting the age and sex distribution of suicide attempts (panel (a)), suicide deaths (panel (b)), and suicide deaths as a share of all deaths (panel (c)) from 2001–2019.

The three panels tell three different stories about where the burden of suicide falls, which together motivate the structure of the paper. Panel (a) shows that *attempts* are concentrated

among the young. They peak in the late teens and early twenties, especially among women. Panel (b) shows that *deaths* are spread much more evenly across the life cycle and skew male. Fatality rises with age and is higher for men at every age, reflecting differences in the methods used (Appendix Table A9). Overall, roughly 16% of first attempts in our data are fatal. Panel (c) shows that, as a *share of all deaths* at a given age, suicide peaks dramatically in young adulthood, where mortality from other causes is near zero. For young adults, suicide accounts for a larger share of deaths than any single other cause of death. Suicide accounts for the largest share of mortality among the young, while suicide deaths are most numerous in middle age. We therefore present our main results pooled across focal ages, but pay particular attention to the families of the young, where suicide is most prominent relative to other causes of death and where we find the largest spillovers.

Appendix Figure A1 repeats Figure 1 under the narrow X60–X84 definition. The patterns are unchanged. Appendix Figure A2 plots the annual counts of hospitalized attempts and suicide deaths over the sample period. Both decline modestly.

3 Empirical Strategy

3.1 Empirical Approach

Estimating the causal effect of a suicide on the people close to the person who dies is hard for two reasons. First, the population at risk of suicide is not observed in most data. A comparison of the families of people who die by suicide to the families of people who do not is a comparison across families that differ on nearly every dimension, including mental health, socio-economic status, and family history. Many of these characteristics are unobserved and are themselves determinants of the outcomes we study. Second, even if the at-risk population were observed, suicide would not be randomly assigned within it. The decision to attempt suicide, as well as when, by what means, and where to attempt, is related to characteristics of the person and their family.

Our data and setting let us make progress on both problems. To address the first, we condition on the act: we focus on the population of individuals who attempt suicide. This allows us to identify, in administrative data and without relying on survey recall, a population that has revealed information about its underlying suicide risk that would otherwise be unobserved. Every comparison we make is then *within* the set of families in which someone attempted suicide, so the families we compare share the key feature that they have experienced a suicide attempt. Table 1 shows how sharply this population differs from the general one. A majority of focal individuals have filled a psychotropic prescription in the year before the event, roughly two and a half times the rate among their own parents.

To address the second, we leverage elements of chance in fatality together with a parallel-trends assumption. Fatality is not random—we document below how the two groups differ—but whether an attempt proves fatal also depends on factors outside the control of the person

attempting. A substantial share of attempts are impulsive: in a population-based study of near-lethal attempts, roughly a quarter of attempters reported less than five minutes between the decision and the act (Simon et al. 2002). When the decision is made only minutes before the act, the method and circumstances of the attempt may depend on what is immediately available. Survival after an attempt depends in part on discovery and rescue: whether somebody comes home and how long an ambulance takes, for example. For self-poisoning, the most common method of hospitalized attempts (Runeson et al. 2010), survival can also depend on the amount absorbed, body weight, and tolerance. The difference is striking in the data: self-poisoning accounts for more than four in five hospitalized attempts (Runeson et al. 2010) but only 34% of suicide deaths in our data (Appendix Table A9). Two attempters who take the same substance can therefore end up on either side of our treatment indicator.

We therefore compare the families of individuals whose first attempt is fatal to the families of individuals whose first attempt is not, in an event study centered on the attempt. What this comparison requires is not that fatality be as good as random among attempters—it is not—but that the two groups of families would have followed parallel paths had the attempt not been fatal. We write down the specification next and then state that requirement precisely.

3.2 Specification

We compare the outcomes of relatives (and co-workers) of focal individuals whose first attempt was fatal to the outcomes of relatives of focal individuals whose first attempt was non-fatal, in an event study centered on the date of the attempt. For a relative i of a focal individual, we estimate

$$Y_{it} = \alpha_i + \sum_{\tau} \kappa_\tau D_{\tau,it} + \sum_{\tau} \sigma_\tau D_{\tau,it} \times Fatal_i + \gamma_t + \beta \mathbf{X}_{it} + \epsilon_{it}, \tag{1}$$

where Y_{it} is an individual-level outcome, t is absolute time (quarter or year, depending on the outcome), and τ is time relative to the focal individual's first attempt. $D_{\tau,it}$ are relative-time indicators, α_i are individual fixed effects for the relative, and γ_t are calendar-time fixed effects. $Fatal_i$ is an indicator equal to one if the focal individual's first attempt was fatal. $\mathbf{X}_{it}$ contains fixed effects for the relative's age in two-year bins. We restrict to relatives alive at $\tau = -1$. In the tables we report grouped versions of the σ_τ: the average differential over post-event years one through five and, for annual outcomes, over years six through ten.

The coefficients of interest are the σ_τ, which trace out how the time path of the outcome for relatives of fatal attempters differs from that for relatives of non-fatal attempters. The comparison is not between people who experience an attempt at different times. It is between people who all experience an attempt at $\tau = 0$ and whose paths then diverge according to whether the attempt proves fatal. This eliminates the usual concerns with staggered-adoption designs. There is no never-treated group, no comparison of early- and late-treated units, and no treatment-timing heterogeneity, because every unit experiences the attempt at $\tau = 0$ and the treatment contrast is cross-sectional in $Fatal_i$.

Standard errors are clustered at the level of the focal individual. This is the level at which treatment varies: $Fatal_i$ is a feature of the focal individual's attempt, and every relative of a given focal individual shares it, so clustering on the relative would treat the siblings of the same decedent as independent draws. Clustering on the focal individual allows arbitrary correlation in the errors across all relatives of the same focal individual and over time.

3.3 Identifying Assumptions and Estimand

Parallel trends. Interpreting σ_τ as the causal effect of a relative's suicide, relative to the counterfactual in which that relative survives an attempt, requires that relatives of non-fatal attempters provide an accurate picture of how relatives of fatal attempters would have fared had the attempt not been fatal.

Importantly, this design does not require relatives of fatal and non-fatal attempters to be equal in levels. Indeed, they are not: Appendix Table A2 reports characteristics of focal individuals and each relative type by whether the first attempt was fatal. Fatal first attempters are on average fifteen years older than non-fatal ones (53 versus 38) and more often male (75% versus 42%). Their relatives are correspondingly older: parents average 77 years at the event versus 63, siblings 46 versus 32, and adult children 32 versus 20. On education, labor income, marital status, and pre-event psychotropic use, however, the two groups are much more similar. In our empirical design, the individual fixed effects absorb level differences between the groups, while the age fixed effects absorb the life-cycle profile of each outcome.

The identifying assumption that the design relies on, instead, is about the evolution of the outcomes over time: the outcomes of relatives of fatal attempters would have evolved similarly to those of relatives of non-fatal attempters, had the attempt been non-fatal.

This assumption is not directly testable, but we examine pre-event trajectories for every outcome, every relative type, and every age stratum. Across mental-health, mortality, and suicide outcomes, and across all four relative types, the pre-event paths of the two groups are close to indistinguishable, and the divergence begins at the event (Figures 3, 4, and 6; Appendix Figures A4–A6). There are two exceptions. The first is parental labor income when pooling across focal ages. Its pre-event paths are not parallel, for a reason we understand: parents of older focal individuals, past peak earnings, are overrepresented among fatal attempts, so the two groups sit at different points of the earnings life cycle.[5] A related pattern appears in the couple-stability outcome for parents and siblings, which we discuss in Section 4.3. The second exception is co-workers' labor-market and marital outcomes, which exhibit pre-trends of their own; we report them but place no weight on them, and none of our conclusions rests on them (Appendix Figure A6).

[5] In contrast, we find no such pre-trends in the under-25 focal sample, where the age difference among parents in the two groups is two years rather than fourteen. There, the two groups track each other for a decade before the event (Figure 3).

Estimand. Under parallel trends, σ_τ is the effect on a family of a relative's first attempt being fatal rather than non-fatal. One could instead treat fatality as an instrument for the focal individual's death and rescale σ_τ by the first stage. We do not, mainly because the first stage is close to one. Every fatal first attempt is a death at $\tau = 0$, and few survivors die soon after (2.7% within a year and 5.6% within three; Figure 2). The five-year first stage is therefore between 0.876 and 0.934 depending on the relative type (Table 2, column (1)), so two-stage estimates would therefore inflate ours by 7 to 14%. Rescaling would also require an exclusion restriction we do not otherwise need: that a fatal attempt affects the family only through the death, rather than, for instance, through what a more determined attempt reveals about the focal individual. The accident benchmark in Section 3.4 speaks to this concern. The reduced form is in any case the policy-relevant object. The counterfactual to a fatal attempt is not a family untouched by suicide, but a family whose relative survived an attempt. This is the margin on which suicide-prevention policy operates. Moreover, because some survivors die later, our contrast, if anything, attenuates over the horizon.

3.4 Benchmarking Against Sudden Accidental Death

A natural question is whether the effects we attribute to suicide reflect suicide specifically or bereavement more generally. If a family's mental health deteriorates after a suicide, is that because a relative died by suicide, or simply because a relative died suddenly and unexpectedly?

To separate the two, we construct a parallel event sample of *traffic accidents*, applying the same fatal-versus-non-fatal logic. We identify all individuals hospitalized following, or dying from, a land traffic accident (ICD-10 V01–V19 and V30–V89, excluding motorcycle accidents and any event flagged as self-harm). We take the first such incident per individual, classify it as fatal or non-fatal using the same date-matching rule, and link the same relative types to the same outcomes (Appendix D.2). Traffic accidents provide a useful benchmark because, like suicide, they can cause sudden and unexpected death. Unlike suicide, however, they reveal nothing about the deceased's state of mind, carry none of its stigma, and provide no plausible mechanism for transmitting suicidal behavior.

The two samples differ sharply in their age-at-event composition. Fatal traffic accidents occur at both young and old ages, while fatal suicide attempts are most common among middle-aged men. Because outcomes vary substantially with age, and because the relatives we observe depend on the focal individual's age, we reweight the traffic sample so that, within each fatality stratum, its age-at-event distribution matches that of the suicide sample (Appendix D.3). Importantly, the traffic sample is not a matched control group for the suicide sample. It is a separate quasi-experiment, with its own fatal-versus-non-fatal comparison. The reweighting simply aligns the age composition of the two samples.

The two populations differ on dimensions other than age, so the benchmark has a limited interpretation. It cannot tell us how much of the effects of suicide reflects bereavement and how much is specific to suicide. We therefore use the benchmark to distinguish these mechanisms

rather than to decompose the overall effects into a 'bereavement share" and a 'suicide share."

4 Results

Table 2 is the paper's central exhibit. It reports grouped event-study estimates of Equation (1)—the average effect over post-event years one through five and, for annual outcomes, years six through ten—for each of the four relative types, pooling focal individuals of all ages. Table 3 reports the same specification for focal individuals under 25, where the identity of the loss is unambiguous and the effects are largest; Appendix Tables A5 and A6 report the intermediate age strata.

4.1 First Stage

Figure 2 shows the cumulative share of focal individuals who have died, by month relative to the first attempt, separately for fatal and non-fatal first attempts. By construction, the fatal curve jumps to one at the event. The non-fatal curve rises only gradually: surviving the first attempt strongly predicts survival over the following three years, though the slow upward drift—reflecting repeat attempts and elevated all-cause mortality among survivors—is itself a substantively important fact about this population, and it is why our estimates should be read as conservative. In the regression samples, the five-year post-event gap in the probability that the focal individual has died ranges from 0.876 for the sample linked to adult children—focal individuals old enough to have them, and therefore older and with higher background mortality—to 0.934 for the sample linked to siblings (Table 2, column (1)). Appendix Figure A3 shows the same pattern separately by sex.

4.2 The Costs Borne by the Family

Mental health. A child's suicide leaves a deep and persistent mark on parents' mental health. The first row of Figure 3 shows the two hallmarks of the design in the under-25 sample: parents of fatal and non-fatal attempters track each other closely for the full five pre-event years, and separate sharply at the event, after which the fatal group settles at a persistently elevated level. Over the five post-event years the grouped estimate is 9.6 percentage points on a pre-event base of 16.6%—a 58% increase (Table 3, Panel A, column (6)).

Pooling all focal ages, the corresponding effect is 5.0 percentage points on a base of 20.4%—a 25% increase—with an antidepressant-specific effect of 2.9 percentage points on a base of 11.2% (Table 2, Panel A, columns (6)–(7)). Partners show a similar pattern: 4.6 percentage points on a base of 20.4% (Panel D). Effects on siblings and adult children are an order of magnitude smaller—0.7 percentage points in each case (Panels B–C)—though precisely estimated; Appendix Figure A5 shows the corresponding event studies. Note the base rates themselves. One in five parents and partners of attempters is already filling a psychotropic prescription in the

quarter before the event, against 11% among siblings and 8% among adult children. This is the selection the design is built to absorb, and it is why the proportional effects are as informative as the levels.

Economic outcomes. The economic costs fall on mothers and on partners. The second row of Figure 3 shows maternal work income in the under-25 sample: the fatal and non-fatal groups rise in lockstep for a decade before the event, then split at it, with no recovery over the ten years we observe. The grouped estimate is a decline of 230 hundred SEK per year—roughly 23,000 SEK, or 10.9% of the pre-event mean of 2,115 hundred SEK (Table 3, Panel A, column (3)). This is strikingly close to the roughly 10% long-run parental income loss that van den Berg et al. (2017) estimate following the accidental death of a child in Sweden—a paper whose design our accident benchmark in effect nests, and whose magnitude we recover on the suicide margin.

Pooling all focal ages, maternal income declines by 6.6% over years one through five and 5.9% over years six through ten, while the paternal decline is smaller (1.5%) and fades (Table 2, Panel A, columns (3)–(4)). As flagged in Section 3.3, the pooled raw income series exhibit differential life-cycle trends across the two groups, so we place more weight on the under-25 estimates for this outcome. Partners' income falls by 3.6% (Panel D, column (2)) and siblings' by 0.8% (Panel B). The concentration of the earnings response on mothers and partners, alongside the mental-health results, points to impaired capacity to work rather than to a change in the household's need for labor income.

Mortality. The loss propagates into survivors' own mortality. Parents' all-cause mortality is 1.4 percentage points higher over the five years following a fatal attempt, and partners' is 2.0 percentage points higher (Table 2, column (11)); siblings' rises by 0.3 percentage points and adult children's by 0.1. These are large numbers—far too large to be accounted for by the suicide-specific mortality we turn to below—and, in proportional terms, they are comparable to the 20–35% increases in widowers' mortality estimated by Espinosa and Evans (2008). They are also, as Section 5 shows, the quantitatively dominant component of the mortality cost of a suicide.

4.3 Couple Stability: Divorce Falls

One cost we do not find is couple dissolution. Among parents married in the year before the event, the probability of having ever divorced *declines* by 2.3 percentage points over years one through five and by 3.6 percentage points over years six through ten, against a base of 12.4%—reductions of 19% and 29% (Table 2, column (5)). The pattern repeats for siblings (2.1 and 2.9 points on a base of 8.4%) and for adult children (0.7 and 1.4 points on a base of 7.6%), and it is visible in the raw data (Figure 5); we return below to the pre-event path in that figure, which is not flat.

4.4 The Transmission of Suicide Within the Family

Column (8) of Table 2 and Figure 4 show that for each relative type, after a family member's death by suicide, surviving relatives' *own* death by suicide becomes more likely. Over the five post-event years, the probability that a partner dies by suicide rises by 0.43 percentage points; a parent, by 0.07; a sibling, by 0.08; an adult child, by 0.03. The first three are statistically significant at the 1% level. These estimates use the narrow definition of a suicide death (X60–X84); under the broad definition that adds deaths of undetermined intent, the estimates are the same or larger. Against cumulative baseline suicide mortality that rounds to zero over a five-year window, these are large proportional increases, and the event-study panels in Figure 4 show no differential pre-event suicide mortality in any relative type: the divergence begins at the event.

The direction of these effects is also of interest. Vertical transmission from parents to children is what a genetic or child-rearing account of familial suicide risk might predict, and it is the direction the literature has emphasized. But we also find transmission *upward*, from children to parents; *laterally*, across siblings; and *between partners*, where there is no genetic link at all. The largest coefficient in the family, by an order of magnitude, is the partner effect. A hereditary account cannot generate these patterns; exposure to the suicide itself can.

The age anatomy. The pooled estimates in column (8) average over configurations in which the direction of transmission differs, so it is worth separating the two ends of the focal age distribution, where the direction is unambiguous. When the person who dies is under 25—so that the bereaved parents are typically in their forties and fifties—parents' psychotropic use rises by 9.6 percentage points and their own suicide mortality rises by 0.25 percentage points over five years (Table 3, Panel A, columns (6) and (8)). When the person who dies is 45–64—so that the bereaved children are typically young adults—the children's psychotropic use rises by 1.0 percentage points and their own suicide mortality by 0.04 percentage points, the latter imprecisely estimated (Appendix Table A6, Panel C, columns (4) and (6)). In the middle stratum, where partners are best populated, partners lose 14.2% of their work income and parents' own suicide mortality rises by 0.09 percentage points (Appendix Table A5, Panels D and A). Transmission is at work at both ends of the family tree, and it is strongest where the tie is closest: parents who lose a child, and partners who lose a partner. Appendix Table A7 reports the same specification without the first-in-family restriction; the patterns are unchanged.

4.5 Beyond the Family: Workplace Peers

In Progress

5 The Social Cost of a Suicide

The estimates of Section 4 can be combined into two summary numbers: the expected number of additional suicide deaths caused per suicide death—a "suicide multiplier"—and the implied understatement in an accounting that counts only the decedent. The calculation is deliberately transparent, and every input is a coefficient or a sample count that appears in a table in this paper. Appendix D.5 reports the inputs and the arithmetic cell by cell.

5.1 The Suicide Multiplier

The logic is simple. Each suicide death exposes a network of survivors; our event studies estimate the increase in each survivor type's own five-year suicide mortality; and the linked-sample counts pin down how many survivors of each type a suicide death exposes. The expected number of induced suicide deaths per suicide death is

$$m = \sum_r n_r \beta_r, \tag{2}$$

where r indexes survivor types (parents, siblings, adult children, partners, workplace peers), n_r is the average number of type-r survivors at risk per focal individual, and β_r is the estimated effect of a fatal versus non-fatal attempt on a type-r survivor's own suicide mortality.

We measure n_r in the same estimation sample that produces β_r: the number of type-r relative individuals in the suicide-mortality column, divided by the 103,416 first-in-family focal individuals of Table 1. This gives 1.34 parents, 1.83 siblings, 0.99 adult children, and 0.25 partners per focal individual—4.40 close family members in total—plus 0.49 same-demographic workplace peers, computed as 61,171 peers divided by the 123,609 focal individuals in the all-first-attempts sample (Appendix Table A1), because the co-worker sample is not first-in-family restricted. [6]

Applying Equation (2) yields a family contribution of 0.0038 and a peer contribution of 0.0002: roughly *0.4 additional suicide deaths per 100 suicide deaths* within five years. Two considerations push this figure in opposite directions, and we treat it as the conservative end of a range. On the one hand, the grouped coefficient on a cumulative outcome is the *average* differential over the five-year window, not the differential at the end of it; if the differential grows roughly linearly from zero—a good approximation to the event-study paths in Figure 4—the cumulative effect *at* five years is about 1.9 times larger, or roughly *0.8 per 100*. On the other hand, these are five-year, first-exposure effects that exclude longer horizons (the event studies show no fade-out), induced non-fatal attempts, survivor types we do not link—friends, schoolmates, extended family, co-workers at large or itinerant workplaces—and second-round

[6] Measuring n_r in the estimation sample rather than from the descriptive counts of Table 1 matters, because the estimation sample retains only relatives alive at $\tau = -1$, which is the population actually at risk of the outcome. It is also the only basis on which all five survivor types can be computed the same way; Appendix D.5 reports the alternative.

propagation. Even the upper end understates the total.

Our range of 0.4–0.8 induced suicide deaths per 100 lines up with the one external benchmark available. Pitman et al. (2022) estimate, from Danish registry data under a very different case-control design, that 0.69% of suicides are attributable to bereavement by the suicide of a first-degree relative or partner. That an attributable-fraction calculation and a forward-looking causal calculation, in different countries and under different designs, land on the same order of magnitude is reassuring for both.

Two caveats belong with the number rather than after it. First, n_r is measured across fatal and non-fatal focal individuals pooled. Fatal attempters are older on average, so they have fewer living parents and more adult children than the pooled average; the sign of the net effect on m is ambiguous, because parents and adult children enter with different β_r. Second, the peer term is the roughest, since it combines a peer count and a linked share computed on different samples from the family terms; at 6% of the total it does not move the headline.

5.2 The Mortality Cost Beyond Suicide

The suicide multiplier is not the policy-relevant total. The counterfactual of a prevented suicide is that the person survives, so the benefit of prevention includes *all* of the network costs our design measures—bereavement and contagion alike. The largest of these, in lives, is the effect on survivors' all-cause mortality (Table 2, column (11)). Repeating Equation (2) with the all-cause coefficients yields 0.030 excess survivor deaths per suicide death on the window-average basis and 0.058 on the five-year cumulative basis: *three to six additional deaths among close family members per 100 suicide deaths* within five years, an order of magnitude larger than the suicide-specific transmission alone.

The implication for accounting is immediate. A calculation that values a prevented suicide at one statistical life misses the network mortality entirely: within five years alone, preventing 100 suicides saves not 100 lives but roughly 103 to 106. Conventional accounting therefore understates the mortality benefits of prevention by *roughly 3–6%*—and this is a floor, because it counts neither the horizon beyond five years, nor induced attempts, nor the morbidity and economic costs.

Putting a price on it is a further step, and one we take only to fix orders of magnitude. At the value of a statistical life recommended for Swedish policy analysis, SEK 22 million in 2012 prices (Hultkrantz and Svensson 2012), the excess network mortality of 0.030 to 0.058 deaths per suicide adds SEK 0.7 to 1.3 million to the benefit of each prevented suicide. In US terms, the value of a statistical life used by the federal agencies is about $14 million: the Department of Transportation's guidance value for 2025 is $14.2 million (U.S. Department of Transportation 2025), and the central value in the Department of Health and Human Services' 2026 guidelines is $14.1 million in 2025 dollars (U.S. Department of Health and Human Services 2026); a recent meta-analysis of the academic literature puts the central estimate lower, at $8.0 million in 2019 dollars (Banzhaf 2022). At the $14.2 million value, the network mortality alone is worth

roughly $0.4 to $0.8 million per suicide, and the mortality cost of one suicide—the decedent's own life plus the deaths it induces among close relatives within five years—is roughly $14.6 to $15.0 million rather than $14.2 million; Appendix D.5 reports the same calculation at the other values, under which the network component is $0.2 to $0.6 million and the proportional understatement is, by construction, the same 3–6%. The earnings losses of Section 4.2 add roughly another SEK 0.1 million: the pooled maternal loss of 10,100 SEK per year across 0.70 linked mothers, plus the partner loss of 6,700 SEK per year across 0.25 linked partners, sustained over ten years and undiscounted. The mental-health morbidity—the largest effect we document, and the one that touches the most people—remains unpriced. Every element of this calculation is therefore a lower bound on the object it is standing in for.

6 Concluding Discussion

To be added

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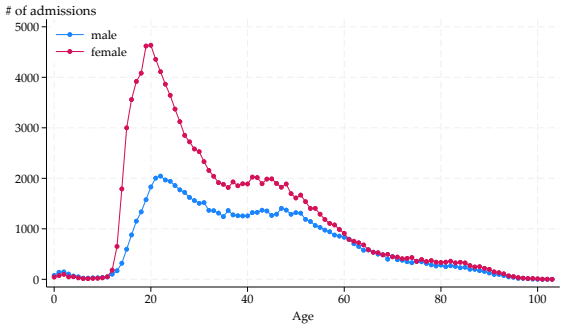
W. Kip Viscusi and Joseph E. Aldy. The value of a statistical life: A critical review of market estimates throughout the world. *Journal of Risk and Uncertainty*, 27(1):5–76, 2003. doi: 10.1023/A:1025598106257.

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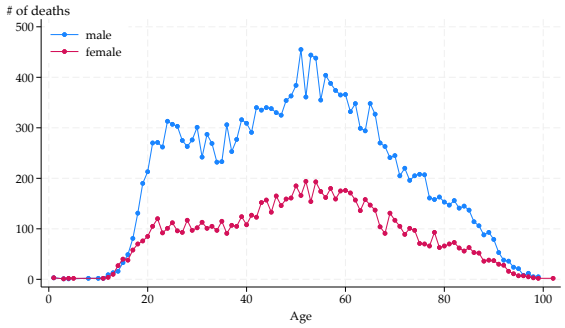
Figures and Tables

Figure 1: Suicide attempts and suicide deaths across the life cycle

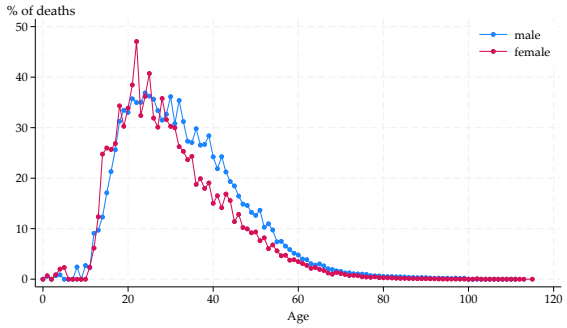
(a) Suicide attempts, by age and sex



(b) Suicide deaths, by age and sex

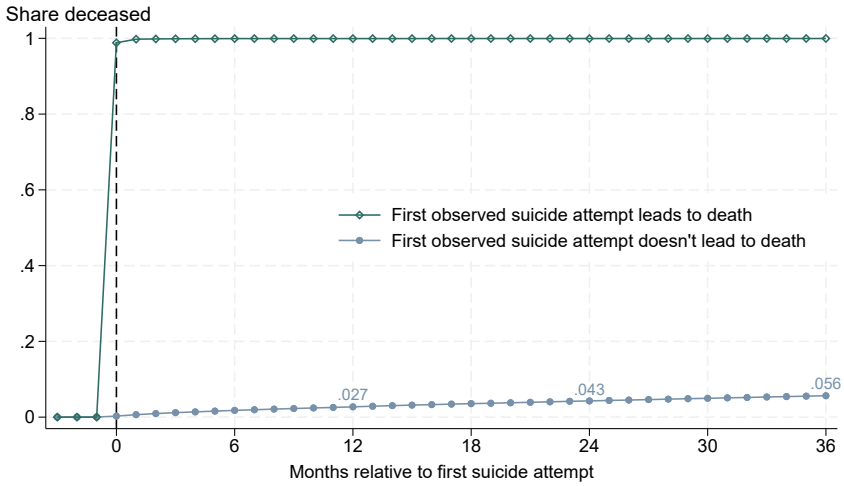


(c) Suicide deaths as a share of all deaths



Notes: Panel (a) plots counts of inpatient admissions for suicide attempts by age and sex over 2001–2019; panel (b) plots counts of suicide deaths by age and sex; panel (c) plots suicide deaths as a share of all deaths in the corresponding age-sex cell. All panels use the broad definition (ICD-10 X60–X84 and Y10–Y34). Appendix Figure A1 shows the corresponding panels under the narrow definition (X60–X84 only). Source: National Patient Register and Cause of Death Register.

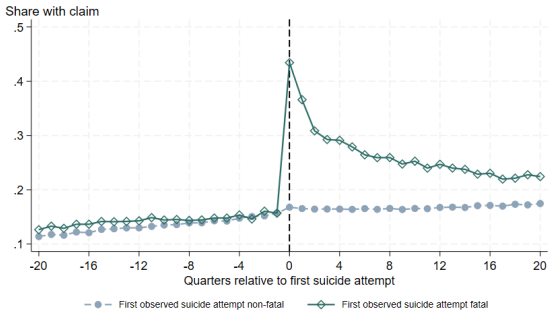
Figure 2: First stage: cumulative mortality of focal individuals after the first attempt



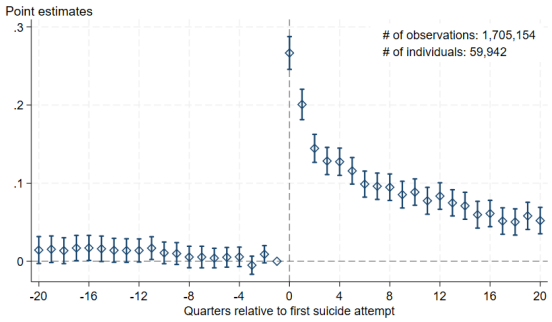
Notes: The figure plots the cumulative share of focal individuals who have died, by month relative to the first suicide attempt, separately for fatal and non-fatal first attempts. First-in-family sample, all focal ages. The corresponding regression estimates are in column (1) of Table 2. Appendix Figure A3 reports the figure separately by sex.

Figure 3: A child's suicide and parental outcomes: mental health and maternal income (focal individuals under 25)

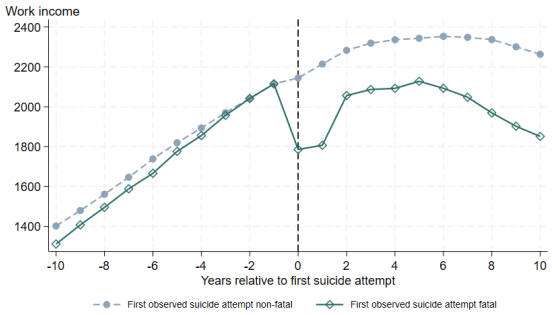
(a) Any psychotropic Rx, raw data



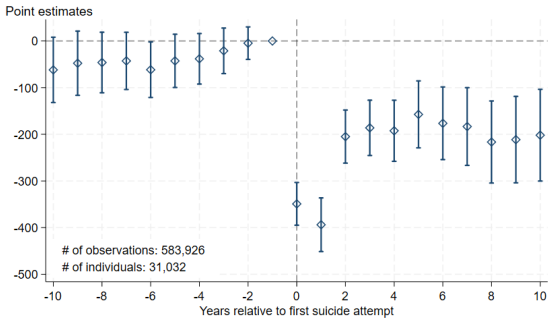
(b) Any psychotropic Rx, event study



(c) Maternal work income, raw data



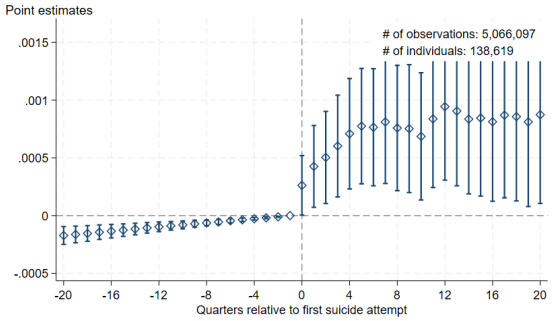
(d) Maternal work income, event study



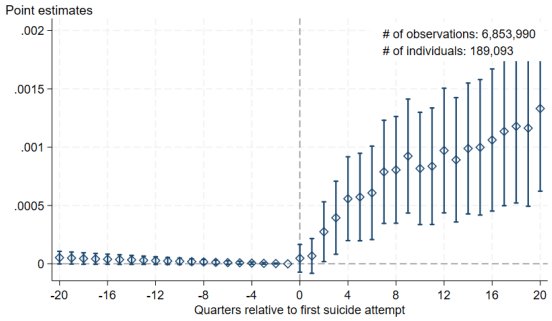
Notes: Left panels plot normalized mean outcomes for parents of focal individuals whose first attempt was fatal versus non-fatal, around the event. Right panels report event-study estimates of Equation (1), with individual fixed effects, age-bin fixed effects, and calendar-time fixed effects; standard errors clustered at the focal-individual level; reference period $\tau = -1$. First-in-family sample, focal individuals under 25 at the event. Row 1: indicator for at least one N05/N06 prescription fill (quarterly). Row 2: maternal annual work income. Corresponding grouped estimates are in Table 3; pooled estimates for all focal ages are in Table 2. Appendix Figure A4 shows the corresponding panels pooling all focal ages, where the raw income series display the differential life-cycle trends discussed in Section 3.3.

Figure 4: The transmission of suicide within the family: relatives' own death by suicide

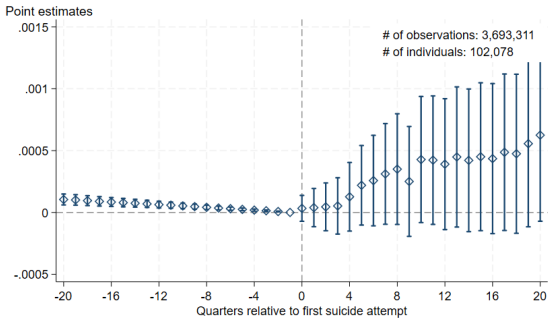
(a) Parents



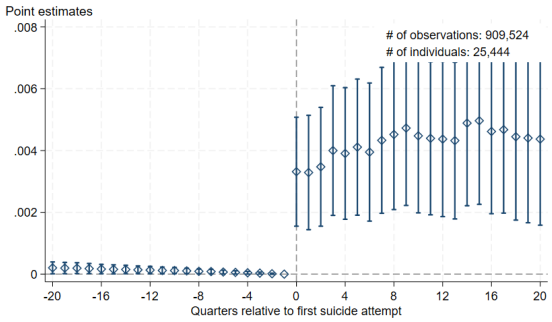
(b) Siblings



(c) Children



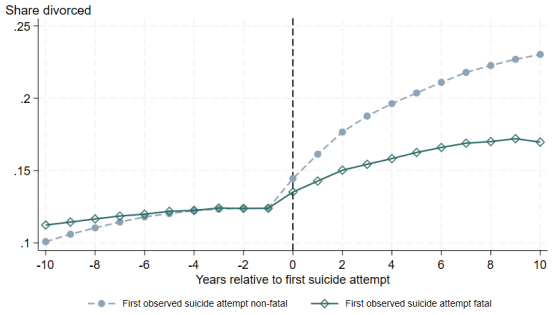
(d) Partners



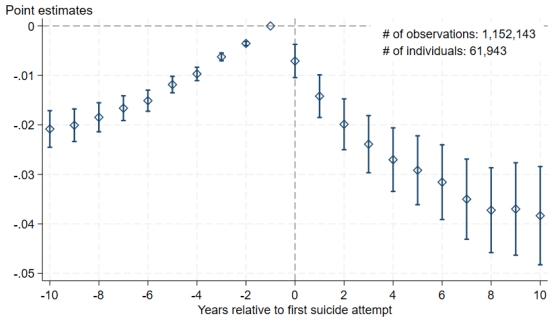
Notes: Each panel reports event-study estimates of Equation (1) where the outcome is an indicator for the relative having died by suicide (X60–X84) by quarter t (stock measure); the panel is balanced, with deceased individuals retained at the absorbing value. First-in-family sample, all focal ages. Standard errors clustered at the focal-individual level. Grouped estimates are in column (8) of Table 2; the corresponding traffic-accident estimates are in column (7) of Table 4, column (5) of Table 5, and column (6) of Appendix Tables A3 and A4. The tables also report the broad measure (X60–X84 and Y10–Y34) in the column directly following the narrow one.

Figure 5: Couple stability: parents' probability of having ever divorced

(a) Raw data



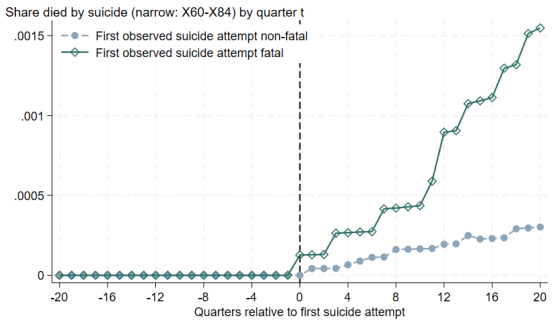
(b) Event study



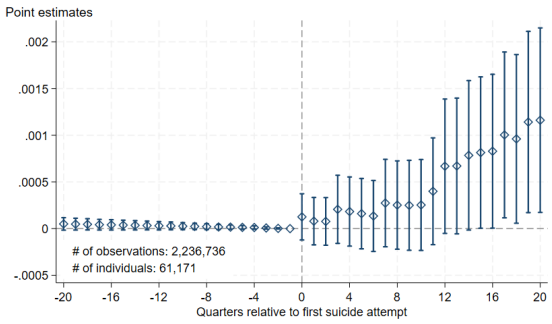
Notes: Indicator for having ever divorced, conditional on being married at $\tau = -1$; annual outcome; first-in-family sample, all focal ages. Left panel: normalized means for parents of fatal versus non-fatal first attempters. Right panel: event-study estimates of Equation (1). Grouped estimates are in column (5) of Table 2; the traffic-accident counterpart is in column (4) of Table 4. Appendix Figure A7 shows that restricting each year to couples in which both spouses are alive leaves the estimates unchanged; Appendix Figure A9 reweights the two groups to a common baseline composition, which removes the pre-event trend visible in the right panel (Sections 3.3 and 4.3).

Figure 6: The transmission of suicide in the workplace: peers' own death by suicide

(a) Raw data



(b) Event study



Notes: The figure plots the probability that a workplace peer has died by suicide (stock measure) around a co-worker's first suicide attempt, comparing fatal to non-fatal attempts. Peers are co-workers of the same sex within five years of age of the focal individual, at establishments with at most 43 employees (the median). Left panel: normalized means. Right panel: event-study estimates of Equation (1); standard errors clustered at the focal-individual level.

Table 1: Summary statistics for the main analysis samples

Focal age at attempt:	<25	25–44	45–64	65+	All
Panel A: Focal individuals					
Age	18.5	33.8	53.6	77.2	40.3
	(4.0)	(6.0)	(5.6)	(8.3)	(21.4)
No college	0.65	0.77	0.76	0.81	0.74
	(0.48)	(0.42)	(0.43)	(0.39)	(0.44)
Some college	0.03	0.11	0.11	0.07	0.08
	(0.16)	(0.31)	(0.31)	(0.25)	(0.26)
Full college (or more)	0.00	0.10	0.11	0.09	0.07
	(0.05)	(0.30)	(0.32)	(0.29)	(0.26)
Education missing	0.32	0.03	0.01	0.03	0.12
	(0.46)	(0.17)	(0.12)	(0.17)	(0.32)
Married	0.02	0.21	0.35	0.38	0.22
	(0.13)	(0.41)	(0.48)	(0.48)	(0.42)
Having ever divorced	0.00	0.15	0.37	0.23	0.18
	(0.06)	(0.36)	(0.48)	(0.42)	(0.38)
Work income (in 100 SEK)	4.3	12.1	14.0		8.5
	(7.4)	(14.2)	(19.1)		(14.2)
Disposable income (in 100 SEK)	6.0	17.2	19.8		15.0
	(6.6)	(86.8)	(30.0)		(55.6)
Born outside of Sweden	0.15	0.24	0.20	0.13	0.18
	(0.36)	(0.43)	(0.40)	(0.34)	(0.39)
At least one mental health Rx	0.37	0.58	0.64	0.67	0.54
	(0.48)	(0.49)	(0.48)	(0.47)	(0.50)
At least one antidepressant Rx	0.27	0.44	0.46	0.38	0.38
	(0.44)	(0.50)	(0.50)	(0.49)	(0.48)
No. of observations	33,387	30,035	23,405	16,589	103,416
Panel B: Parents					
Age	48.2	61.9	83.0	102.5	65.1
	(7.2)	(8.5)	(9.4)	(8.8)	(19.3)
No college	0.67	0.67	0.38	0.04	0.55
	(0.47)	(0.47)	(0.49)	(0.20)	(0.50)
Some college	0.12	0.08	0.02	0.00	0.08
	(0.33)	(0.27)	(0.15)	(0.03)	(0.27)
Full college (or more)	0.16	0.10	0.03	0.00	0.10
	(0.36)	(0.30)	(0.16)	(0.03)	(0.29)
Education missing	0.05	0.15	0.57	0.96	0.28
	(0.21)	(0.36)	(0.50)	(0.20)	(0.45)
Married	0.51	0.55	0.46	0.13	0.52
	(0.50)	(0.50)	(0.50)	(0.34)	(0.50)
Having ever divorced	0.34	0.36	0.24	0.13	0.33
	(0.47)	(0.48)	(0.42)	(0.34)	(0.47)
Work income (in 100 SEK)	25.1	16.4			18.5
	(24.4)	(19.7)			(22.5)
Disposable income (in 100 SEK)	25.1	22.2			22.8
	(51.9)	(27.1)			(40.9)
Born outside of Sweden	0.21	0.19	0.11	0.04	0.17
	(0.41)	(0.39)	(0.31)	(0.19)	(0.37)
At least one mental health Rx	0.23	0.25	0.21	0.04	0.21
	(0.42)	(0.43)	(0.41)	(0.18)	(0.41)
At least one antidepressant Rx	0.15	0.14	0.10	0.02	0.12
	(0.36)	(0.34)	(0.30)	(0.12)	(0.33)
No. of observations	63,537	50,601	39,355	14,078	167,571

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(continued)

Focal age at attempt:	<25	25–44	45–64	65+	All
Panel C: Siblings					
Age	18.3	32.9	53.3	68.3	34.2
	(9.6)	(11.1)	(9.7)	(7.9)	(19.1)
No college	0.45	0.65	0.68	0.63	0.57
	(0.50)	(0.48)	(0.47)	(0.48)	(0.49)
Some college	0.05	0.10	0.10	0.08	0.08
	(0.22)	(0.30)	(0.30)	(0.27)	(0.27)
Full college (or more)	0.04	0.11	0.12	0.10	0.08
	(0.20)	(0.32)	(0.32)	(0.30)	(0.28)
Education missing	0.46	0.14	0.10	0.18	0.26
	(0.50)	(0.35)	(0.30)	(0.39)	(0.44)
Married	0.09	0.27	0.49	0.56	0.30
	(0.29)	(0.44)	(0.50)	(0.50)	(0.46)
Having ever divorced		0.10	0.24	0.24	0.13
		(0.30)	(0.43)	(0.42)	(0.33)
Work income (in 100 SEK)		19.4	23.3		17.3
		(18.6)	(22.4)		(19.2)
Disposable income (in 100 SEK)	11.7	19.4	24.2		18.8
	(11.7)	(45.3)	(49.3)		(48.2)
Born outside of Sweden	0.11	0.09	0.05	0.02	0.08
	(0.32)	(0.29)	(0.21)	(0.13)	(0.28)
At least one mental health Rx	0.09	0.16	0.21	0.24	0.15
	(0.29)	(0.36)	(0.40)	(0.42)	(0.36)
At least one antidepressant Rx	0.05	0.10	0.12	0.11	0.09
	(0.22)	(0.30)	(0.33)	(0.31)	(0.28)
No. of observations	78,129	60,631	45,491	13,299	197,550
Panel D: Children					
Age			24.5	48.3	21.6
			(8.7)	(9.8)	(19.8)
No college			0.60	0.57	0.38
			(0.49)	(0.49)	(0.48)
Some college			0.09	0.14	0.06
			(0.28)	(0.35)	(0.24)
Full college (or more)			0.09	0.20	0.08
			(0.28)	(0.40)	(0.27)
Education missing			0.23	0.08	0.48
			(0.42)	(0.28)	(0.50)
Married			0.13	0.49	0.27
			(0.34)	(0.50)	(0.44)
Having ever divorced			0.03	0.19	0.10
			(0.18)	(0.39)	(0.30)
Work income (in 100 SEK)			15.2	29.3	20.1
			(21.2)	(48.8)	(36.0)
Disposable income (in 100 SEK)			15.3	27.9	19.7
			(27.2)	(53.5)	(40.3)
Born outside of Sweden			0.07	0.04	0.06
			(0.26)	(0.20)	(0.23)
At least one mental health Rx			0.12	0.18	0.11
			(0.33)	(0.38)	(0.31)
At least one antidepressant Rx			0.08	0.11	0.06
			(0.27)	(0.31)	(0.24)
No. of observations			41,806	30,194	124,576

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Focal age at attempt:	<25	25–44	45–64	65+	All
Panel E: Partners					
Age		36.9	53.5	74.1	52.0
		(8.0)	(8.2)	(8.8)	(17.2)
No college		0.71	0.70	0.79	0.73
		(0.45)	(0.46)	(0.41)	(0.44)
Some college		0.12	0.13	0.08	0.11
		(0.33)	(0.34)	(0.28)	(0.32)
Full college (or more)		0.15	0.16	0.12	0.14
		(0.35)	(0.37)	(0.32)	(0.35)
Education missing		0.02	0.01	0.01	0.02
		(0.15)	(0.10)	(0.12)	(0.13)
Married		0.60	0.79	0.92	0.74
		(0.49)	(0.41)	(0.27)	(0.44)
Having ever divorced					
Work income (in 100 SEK)		21.7	25.2		18.4
		(18.5)	(25.9)		(38.9)
Disposable income (in 100 SEK)		21.8	25.6		22.0
		(26.7)	(59.0)		(51.3)
Born outside of Sweden		0.27	0.19	0.13	0.21
		(0.44)	(0.39)	(0.33)	(0.41)
At least one mental health Rx		0.19	0.26	0.41	0.27
		(0.39)	(0.44)	(0.49)	(0.44)
At least one antidepressant Rx		0.13	0.16	0.18	0.15
		(0.33)	(0.37)	(0.39)	(0.36)
No. of observations		8,641	9,468	6,528	25,734

Note: Means with sample standard deviations in parentheses. Columns refer to the focal individual's age at the selected first attempt, including exactly age 65 in the 65+ group. Age is measured at the event; education, income, marital status and prescriptions are measured in the preceding calendar year. LISA covariates are generally available from age 16; unavailable values remain missing, with education missing shown explicitly. Mental-health (N05/N06) and antidepressant (N06A) prescription indicators use complete calendar years 2006–2019 only; within coverage, no recorded claim is coded zero, and outside coverage values are missing. Each characteristic uses its own nonmissing denominator. Observations count focal–relative pairs in relative panels and distinct focal individuals in Panel A; a relative linked to multiple focals may appear more than once. Missing focal ages contribute only to the All column. The blank-cell pattern of the supplied table layout is retained intentionally; blanks can also reflect unavailable statistics. Values are copied from the All columns of the September 10, 2026 descriptives exports.

Table 2: Event Study Estimates by Relative Type: Suicide Attempt—First in Family, All Focal Ages

	First stage	Labor market & marital				Health outcomes					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Time period	Focal ind. died by t	Work income	Maternal income	Paternal income	Ever divorced	At least one mental health Rx	At least one antidepressant Rx	Suicide death (narrow)	Suicide death (broad)	Traffic death	All-cause death
Panel A: Parents											
Y1–Y5	0.930***	-68.310***	-101.455***	-33.867*	-0.023***	0.050***	0.029***	0.0007***	0.0008***	-0.0001**	0.0135***
	(0.001)	(10.789)	(11.533)	(18.724)	(0.003)	(0.003)	(0.003)	(0.0003)	(0.0003)	(0.0000)	(0.0015)
Y6–Y10		-48.266***	-89.685***	-14.648	-0.036***						
		(17.710)	(16.812)	(31.956)	(0.004)						
Mean dep. var. at $\tau = -1$	0.000	1,846.739	1,529.045	2,221.165	0.124	0.204	0.112	0.0000	0.0000	0.0000	0.0000
No. of observations	3,493,077	2,293,517	1,240,574	1,052,943	1,152,090	3,437,551	3,437,551	5,065,982	5,065,982	5,065,982	5,629,328
No. of individuals	85,197	134,958	72,170	62,788	61,940	122,154	122,154	138,616	138,616	138,616	138,616
No. of focal individuals	85,197	75,110	71,925	62,447	36,593	69,776	69,776	78,721	78,721	78,721	78,721
Panel B: Siblings											
Y1–Y5	0.934***	-14.190			-0.021***	0.007***	0.004**	0.0008***	0.0009***	0.0000	0.0034***
	(0.001)	(8.638)			(0.002)	(0.002)	(0.002)	(0.0002)	(0.0002)	(0.0001)	(0.0006)
Y6–Y10		-25.386*			-0.029***						
		(13.287)			(0.004)						
Mean dep. var. at $\tau = -1$	0.000	1,733.510			0.084	0.109	0.062	0.0000	0.0000	0.0000	0.0000
No. of observations	3,232,153	2,827,646			834,789	5,242,863	5,242,863	6,857,831	6,857,831	6,857,831	7,616,101
No. of individuals	78,833	179,014			44,257	186,129	186,129	189,202	189,202	189,202	189,202
No. of focal individuals	78,833	76,355			29,353	76,748	76,748	77,050	77,050	77,050	77,050
Panel C: Children											
Y1–Y5	0.876***	-17.067			-0.007*	0.007***	0.007***	0.0003	0.0005**	0.0001	0.0010**
	(0.002)	(23.605)			(0.004)	(0.002)	(0.002)	(0.0002)	(0.0002)	(0.0001)	(0.0005)
Y6–Y10		41.035			-0.014**						
		(28.141)			(0.006)						
Mean dep. var. at $\tau = -1$	0.000	2,015.473			0.076	0.083	0.046	0.0000	0.0000	0.0000	0.0000
No. of observations	2,336,180	1,296,242			338,940	2,803,261	2,803,261	3,699,725	3,699,725	3,699,725	4,102,482
No. of individuals	56,980	90,609			17,914	101,222	101,222	102,259	102,259	102,259	102,259
No. of focal individuals	56,980	41,904			12,476	46,335	46,335	46,441	46,441	46,441	46,441
Panel D: Partners											
Y1–Y5	0.890***	-66.593***				0.046***	0.034***	0.0043***	0.0063***	-0.0004***	0.0201***
	(0.002)	(19.898)				(0.006)	(0.005)	(0.0012)	(0.0013)	(0.0001)	(0.0032)
Y6–Y10		43.931									
		(30.231)									
Mean dep. var. at $\tau = -1$	0.000	1,835.663				0.204	0.108	0.0000	0.0000	0.0000	0.0000
No. of observations	1,056,324	454,202				713,131	713,131	909,643	909,643	909,643	1,032,022
No. of individuals	25,764	25,699				25,132	25,132	25,448	25,448	25,448	25,448
No. of focal individuals	25,764	25,699				25,132	25,132	25,448	25,448	25,448	25,448

Notes: Sample pooled over all focal individual ages at the time of the event. Col. (1) reports the first stage: the coefficient on the grouped post-period indicator interacted with the fatal-event dummy, where the outcome is an indicator for whether the focal individual has died by quarter q (stock measure). Cols. (2)–(11) report reduced-form effects on family member outcomes, estimated with individual FE, family member age-bin FE, and calendar time FE. The mean dependent variable at $\tau = -1$ and the number of focal individuals are calculated within each column's estimation sample. Standard errors (in parentheses) are clustered at the focal-individual level. For the first-stage and death outcomes, the mean at $\tau = -1$ is approximately zero by construction because pre-event deaths are excluded. Where reported, suicide and traffic death outcomes cover 2001–2017; other outcomes use their available coverage periods. Col. (2): work income with no explicit age restriction; cols. (3)–(4): maternal and paternal income (Parents panel only), with no explicit age restriction. Col. (5): ever divorced, restricted to relatives aged 20+ and married at $\tau = -1$ (excluded for Panel D Partners, mechanically contaminated by focal-individual death). Cols. (6)–(7): quarterly drug claim outcomes (Y1–5 = quarters 1–20). Col. (8): quarterly suicide death, narrow measure (X60–X84, stock). Col. (9): quarterly suicide death, broad measure (X60–X84 and Y10–Y34, i.e. including events of undetermined intent, stock). Col. (10): quarterly traffic-related death of family member (V01–V89, stock). Col. (11): quarterly all-cause death of family member (stock). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 3: Event Study Estimates by Relative Type: Suicide Attempt—First in Family, Focal Individual Aged 12–24

	First stage	Labor market & marital				Health outcomes					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Time period	Focal ind. died by t	Work income	Maternal income	Paternal income	Ever divorced	At least one mental health Rx	At least one antidepressant Rx	Suicide death (narrow)	Suicide death (broad)	Traffic death	All-cause death
Panel A: Parents											
Y1–Y5	0.982***	-164.601***	-229.706***	-95.310**	-0.022***	0.096***	0.060***	0.0025**	0.0027**	-0.0002***	0.0026
	(0.001)	(22.864)	(26.958)	(38.182)	(0.008)	(0.007)	(0.005)	(0.0010)	(0.0011)	(0.0001)	(0.0016)
Y6–Y10		-125.868***	-194.954***	-58.037	-0.029**						
		(41.859)	(39.773)	(71.182)	(0.012)						
Mean dep. var. at $\tau = -1$	0.000	2,524.815	2,114.945	2,961.937	0.145	0.166	0.106	0.0000	0.0000	0.0000	0.0000
No. of observations	1,288,097	1,131,909	583,904	548,005	569,258	1,705,084	1,705,084	2,209,864	2,209,864	2,209,864	2,445,145
No. of individuals	31,417	60,641	31,031	29,610	30,024	59,940	59,940	60,199	60,199	60,199	60,199
No. of focal individuals	31,417	31,377	31,005	29,537	17,709	31,353	31,353	31,368	31,368	31,368	31,368
Panel B: Siblings											
Y1–Y5	0.982***					0.012***	0.008**	0.0003	0.0001	0.0002	-0.0006
	(0.001)					(0.004)	(0.004)	(0.0004)	(0.0004)	(0.0003)	(0.0005)
Mean dep. var. at $\tau = -1$	0.000					0.064	0.033	0.0000	0.0000	0.0000	0.0000
No. of observations	1,229,426					2,083,566	2,083,566	2,704,640	2,704,640	2,704,640	2,982,837
No. of individuals	29,986					74,598	74,598	75,023	75,023	75,023	75,023
No. of focal individuals	29,986					29,918	29,918	29,930	29,930	29,930	29,930

Notes: Sample restricted to focal individuals aged 12–24 at the time of the event. Col. (1) reports the first stage: the coefficient on the grouped post-period indicator interacted with the fatal-event dummy, where the outcome is an indicator for whether the focal individual has died by quarter q (stock measure). This regression is estimated on a focal-individual-level quarterly panel ($\tau \in [-20, 20]$), with individual FE, age-bin FE, and calendar quarter FE. Cols. (2)–(11) report reduced-form effects on family member outcomes, estimated with individual FE, family member age-bin FE, and calendar time FE. The mean dependent variable at $\tau = -1$ and the number of focal individuals are calculated within each column's estimation sample. Standard errors (in parentheses) are clustered at the focal-individual level. For the first-stage and death outcomes, the mean at $\tau = -1$ is approximately zero by construction because pre-event deaths are excluded. Where reported, suicide and traffic death outcomes cover 2001–2017; other outcomes use their available coverage periods. Cols. (2)–(4): work income with no explicit relative-age restriction. Col. (2): all parents; col. (3): mothers; col. (4): fathers. Col. (5): ever divorced, restricted to relatives aged 20+ and married at $\tau = -1$. Cols. (6)–(7): quarterly drug claim outcomes (Y1–5 = quarters 1–20). Col. (8): quarterly suicide death, narrow measure (X60–X84, stock). Col. (9): quarterly suicide death, broad measure (X60–X84 and Y10–Y34, i.e. including events of undetermined intent, stock). Col. (10): quarterly traffic-related death of family member (V01–V89, stock). Col. (11): quarterly all-cause death of family member (stock). Panel B (Siblings): only first-stage and health outcomes reported. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 4: Event Study Estimates for Parents of Focal Individuals (All Ages): First-in-Family Suicide Sample

	First stage	Economic outcomes			Health outcomes					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Time period	Focal ind. died by t	Maternal income	Paternal income	Ever divorced	At least one mental health Rx	At least one antidepressant Rx	Suicide death (narrow)	Suicide death (broad)	Traffic death	All-cause death
Panel A: Suicide attempt (X60–X84, Y10–Y34), first in family										
Y1–Y5	0.930***	-101.455***	-33.867*	-0.023***	0.050***	0.029***	0.0007***	0.0008***	-0.0001**	0.0135***
	(0.001)	(11.533)	(18.724)	(0.003)	(0.003)	(0.003)	(0.0003)	(0.0003)	(0.0000)	(0.0015)
Y6–Y10		-89.685***	-14.648	-0.036***						
		(16.812)	(31.956)	(0.004)						
Mean dep. var. at $\tau = -1$	0.000	1,529.045	2,221.165	0.124	0.204	0.112	0.0000	0.0000	0.0000	0.0000
No. of observations	3,493,077	1,240,574	1,052,943	1,152,090	3,437,551	3,437,551	5,065,982	5,065,982	5,065,982	5,629,328
No. of individuals	85,197	72,170	62,788	61,940	122,154	122,154	138,616	138,616	138,616	138,616
No. of focal individuals	85,197	71,925	62,447	36,593	69,776	69,776	78,721	78,721	78,721	78,721
Panel B: Traffic accident (V01–V89, reweighted)										
Y1–Y5	0.975***	-72.312***	-40.223	-0.012***	0.038***	0.019***	0.0004	0.0005	0.0001	0.0114***
	(0.001)	(14.588)	(39.044)	(0.003)	(0.006)	(0.005)	(0.0004)	(0.0004)	(0.0002)	(0.0029)
Y6–Y10		-37.276*	-12.139	-0.027***						
		(22.324)	(49.572)	(0.005)						
Mean dep. var. at $\tau = -1$	0.000	1,584.094	2,305.593	0.086	0.154	0.083	0.0000	0.0000	0.0000	0.0000
No. of observations	5,530,941	1,734,033	1,475,525	1,787,313	4,081,416	4,081,416	7,415,059	7,415,059	7,415,059	7,415,059
No. of individuals	134,901	98,631	85,848	92,547	163,745	163,745	191,640	191,640	191,640	191,640
No. of focal individuals	134,901	98,290	85,407	52,113	93,988	93,988	109,475	109,475	109,475	109,475

Notes: Panel A restricts the suicide-attempt sample to focal individuals who are first in their family to attempt suicide; Panel B is the reweighted traffic-accident comparison. Sample pools focal individuals of all ages at the time of the event. Col. (1) reports the first stage (focal individual died by quarter q). Cols. (2)–(10) report reduced-form effects on parental outcomes. All regressions include individual FE, age-bin FE, and calendar time FE. The mean dependent variable at $\tau = -1$ and the number of focal individuals are calculated within each column's estimation sample. Standard errors (in parentheses) are clustered at the focal-individual level. For the first-stage and death outcomes, the mean at $\tau = -1$ is approximately zero by construction because pre-event deaths are excluded. Where reported, suicide and traffic death outcomes cover 2001–2017; other outcomes use their available coverage periods. Panel B: traffic sample reweighted to match the suicide sample's focal-individual age distribution across the full age distribution. Cols. (2)–(3): work income for parents with no explicit age restriction; col. (2) mothers, col. (3) fathers. Col. (4): ever divorced, restricted to parents aged 20+ and married at $\tau = -1$. Cols. (5)–(6): quarterly drug claim outcomes (Y1–5 = quarters 1–20). Col. (7): quarterly suicide death, narrow measure (X60–X84, stock, 4 d.p.). Col. (8): quarterly suicide death, broad measure (X60–X84 and Y10–Y34, i.e. including events of undetermined intent, stock, 4 d.p.). Col. (9): quarterly traffic-related death of family member (V01–V89, stock, 4 d.p.). Col. (10): quarterly all-cause death of family member (stock, 4 d.p.). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 5: Event Study Estimates for Partners of Focal Individuals (All Ages): First-in-Family Suicide Sample

	First stage	Economic	Health outcomes					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Time period	Focal ind. died by t	Work income	At least one mental health Rx	At least one antidepressant Rx	Suicide death (narrow)	Suicide death (broad)	Traffic death	All-cause death
Panel A: Suicide attempt (X60–X84, Y10–Y34), first in family								
Y1–Y5	0.890***	-66.593***	0.046***	0.034***	0.0043***	0.0063***	-0.0004***	0.0201***
	(0.002)	(19.898)	(0.006)	(0.005)	(0.0012)	(0.0013)	(0.0001)	(0.0032)
Y6–Y10		43.931						
		(30.231)						
Mean dep. var. at $\tau = -1$	0.000	1,835.663	0.204	0.108	0.0000	0.0000	0.0000	0.0000
No. of observations	1,056,324	454,202	713,131	713,131	909,643	909,643	909,643	1,032,022
No. of individuals	25,764	25,699	25,132	25,132	25,448	25,448	25,448	25,448
No. of focal individuals	25,764	25,699	25,132	25,132	25,448	25,448	25,448	25,448
Panel B: Traffic accident (V01–V89, reweighted)								
Y1–Y5	0.952***	-168.379***	0.054***	0.034***	0.0001	0.0004	0.0569***	0.0591***
	(0.001)	(26.962)	(0.010)	(0.008)	(0.0005)	(0.0006)	(0.0055)	(0.0063)
Y6–Y10		-72.444*						
		(40.119)						
Mean dep. var. at $\tau = -1$	0.000	1,983.380	0.119	0.065	0.0000	0.0000	0.0000	0.0000
No. of observations	2,061,357	938,393	1,299,642	1,299,642	1,893,952	1,893,952	1,893,952	1,893,952
No. of individuals	50,277	50,238	49,484	49,484	49,980	49,980	49,980	49,980
No. of focal individuals	50,277	50,238	49,484	49,484	49,980	49,980	49,980	49,980

Notes: Panel A restricts the suicide-attempt sample to focal individuals who are first in their family to attempt suicide; Panel B is the reweighted traffic-accident comparison. Sample pools focal individuals of all ages at the time of the event. Col. (1) reports the first stage (focal individual died by quarter q). Cols. (2)–(8) report reduced-form effects on outcomes for partners of focal individuals. All regressions include individual FE, age-bin FE, and calendar time FE. The mean dependent variable at $\tau = -1$ and the number of focal individuals are calculated within each column's estimation sample. Standard errors (in parentheses) are clustered at the focal-individual level. For the first-stage and death outcomes, the mean at $\tau = -1$ is approximately zero by construction because pre-event deaths are excluded. Where reported, suicide and traffic death outcomes cover 2001–2017; other outcomes use their available coverage periods. Panel B: traffic sample reweighted to match the suicide sample's focal-individual age distribution across the full age distribution. Col. (2): work income for partners with no explicit age restriction. Cols. (3)–(4): quarterly drug claim outcomes (Y1–5 = quarters 1–20). Col. (5): quarterly suicide death, narrow measure (X60–X84, stock, 4 d.p.). Col. (6): quarterly suicide death, broad measure (X60–X84 and Y10–Y34, i.e. including events of undetermined intent, stock, 4 d.p.). Col. (7): quarterly traffic-related death of family member (V01–V89, stock, 4 d.p.). Col. (8): quarterly all-cause death of family member (stock, 4 d.p.). Ever-divorced outcome omitted for partners (mechanically contaminated by focal-individual death). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.