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UNIVERSITY OF CALIFORNIA, SAN DIEGO

Loss, Change, Adaptation: how people change when their lives do

A Dissertation submitted in partial satisfaction of the
requirements for the degree
Doctor of Philosophy

in

Political Science

by

William Ryan Hobbs

Committee in charge:

Professor James Fowler, Chair
Professor Scott Desposato
Professor Jesse Driscoll
Professor Seth Hill
Professor Kevin Patrick

2016

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The Dissertation of William Ryan Hobbs is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California, San Diego

2016

DEDICATION

For my father and my mother.

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William Hobbs, David Lindsey, "Presidential Effort and International Outcomes: evidence for an executive bottleneck.", *Journal of Politics*, 77(4), 2015.

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ABSTRACT OF THE DISSERTATION

Loss, Change, Adaptation: how people change when their lives do

by

William Ryan Hobbs

Doctor of Philosophy in Political Science

University of California, San Diego, 2016

Professor James Fowler, Chair

Many prominent theories in political science, and social science generally, argue that individuals are imprinted with behaviors and identities over the course of their lives. According to these theories, behaviors and identities persist due to internal, psychological consistency. Here, I argue that persistence in behavior, identity, and social interaction is often maintained by social networks, rather than individuals. Many day-to-day behaviors and identities would be constantly redefined and updated if there were reasons to do so. In other words, imprinting is often highly context specific, consistency in individuals is a property of stable social structures, and individuals can change very quickly when social networks

around them allow or encourage it. The first paper in this dissertation ("Widowhood Effects in Voter Participation") studies why people are less likely to vote after their spouse dies. I show that a rapid, permanent drop in turnout is mostly not due to the trauma of the loss or a slowly declining interest in politics. Widows and widowers vote less because their spouse motivated them to vote. The second paper ("Partisan Attachment or Life Stability?") argues that partisan affiliations are stable in the United States electorate because day-to-day lives do not change very much. When lives do change, people reconsider their partisanship and are more likely to change it. A central argument in this paper is that age is associated with increasing partisan stability only because life stability increases with age. The last paper ("Plasticity in Human Social Networks") characterizes social network adaptations after the death of a friend. I show that mutual friends become permanently closer to each other after the shared loss, recovering the same volume of interactions that was lost from the death. Younger people tend to contribute more to recovery than older, but older people contribute equally when a loss was sudden and unexpected. The observed recovery occurs in distinct, overlapping patterns and is mathematically similar to shock responses in small-scale biological networks.

Chapter 1

Widowhood Effects in Voter Participation

Past research suggests that spouses influence one another to vote, but relies almost exclusively on correlation in turnout. It is therefore difficult to establish whether spouses mobilize each other or tend to marry similar others. Here, we test the dependency hypothesis by examining voting behavior before and after the death of a spouse. We link nearly 6 million California voter records to Social Security death records, and use both coarsened exact matching and multiple cohort comparison to estimate the effects of spousal loss. The results show that after turnout rates stabilize, widowed individuals vote nine percentage points less than they would had their spouse still been living, and that this change may persist indefinitely. Variations in this "widowhood effect" on voting support a social isolation explanation for the drop in turnout. A conservative estimate of this effect nationwide accounts for 1 million 'lost votes' in non-presidential elections.

Recent experimental studies highlight the importance of social mobilization, and demonstrate that efforts to increase turnout may not only convince those directly contacted to vote, but also their political partners, friends, and family members. For example, Nickerson [1] conducted a get out the vote (GOTV) field experiment with two person households and showed that sixty percent of the increased propensity to turn out resulting from the treatment was passed from one household member to the other. Gerber, Green, and Larimer [2] also conducted a GOTV experiment in which they promised to show neighbors whether or not a person voted. This raised turnout by eight percent – one of the largest effects ever observed for a by-mail treatment. Bond et al. [3] conducted a 61 million person experiment on Facebook and showed that a GOTV message not only increased turnout among the recipients, but among the recipients' friends as well. Observational work by Cutts and Fieldhouse [4] provides evidence that the occupants in two-person households have substantially larger effects on each others' turnout propensity than other geographically proximate electors.

The basis of many of these studies is a theory of social dependency. That is, forms of political participation that are strongly motivated by inter-personal influences may in turn be strongly dependent on them. This dependency, and a lack of (community-based) social connectedness and mobilization in recent decades, may account for variation in political participation rates [5, 6, 2]. Importantly, however, these studies do not directly test whether increased social isolation will have large and long-lasting (negative) effects on political participation.

Here, we study political inter-personal dependency directly – specifically, dependency within a spousal relationship. This is a natural starting place for research on social isolation/connection and turnout. Spouses have long been identified as important source of mobilization and influence [7, 8].

To better understand spousal dependency, we examine turnout before and after a spouse's death. This is an analysis of voting behavior when a person finds him or herself alone, cut off from a relationship that for many people is the strongest they experience. Turnout before the death is important because many deaths result from chronic illness and will take their toll before the event (e.g. from caregiver burden, during which the terminally ill spouse may have limited interaction capacity and/or be unable to return social support). Turnout after the death is also important because it will help us to see whether people return to their pre-death levels of participation. In particular, we study the one year anniversary of the death to see whether changes in personal health or the grieving process itself may be contributing to turnout decline. To identify the effect of inter-spousal mobilization, we compare changes among widowed voters by spouses' past voting histories, and observe whether differences vary with age.

1.1 Existing Theories on the Marital Turnout Boost

It is well-established that married individuals vote more than never married, divorced, or widowed people [8, 9, 10, 11], and that the beneficial effects of marriage, in terms of political participation at least, appear to increase with time [9]. These observations are among the most fundamental in the political participation literature. However, it is less clear whether spouses might indeed be significant mobilizing influences. More consistent voters may be more likely to marry, and the similarly voting spouses may have been politically similar prior to their wedding [12, 13].

Rather than focusing on the absence of a spouse, the current literature focuses on the presence of a spouse, and there are a number of existing theories that attempt to explain the phenomenon of higher turnout among married people. Three of these dominate contemporary conceptualizations of marriage and voting.

First, a number of researchers have applied life-cycle explanations to the marital turnout boost. Participation in politics, and formal organizations in general, might change over time through new and lost familial attachments [10, 14, 11]. This conceptualization is supported by observed changes in turnout by age and family structure. Recent analyses support this hypothesis by showing that major marital transitions, entry into marriage, separation, and divorce, decrease political participation [10]. Past research also observes lower rates of participation among widows and widowers than among married individuals [15, 11], but has not found a statistically significant effect in the transition to widowhood

[10]. Notably, and of relevance to this research, there is little survey evidence that electors disengage from passive forms of political participation (such as voting) as they reach old age [16].

Second, a number of social scientists hypothesize that discussion of political topics among spouses induces greater political interest and political participation. This theory is supported by survey evidence which indicates that spouses and family members are the most cited political discussion partners [17] (in addition to being the most cited discussion partners more generally [18]). Further, spouses are the most likely recipients of efforts to persuade another person to vote [10, 19]. This theory not only posits that spouses influence each other, but that the primary mechanism of increased political participation, increased political interest, accumulates over time.

Third, scholars emphasize the importance of inter-personal mobilization around polling time. In *The American Voter* [8], the authors note that people with 'very low motivation who have gone to the polls' cite inter-personal influence as their primary motivator. Wolfinger [9] hypothesized that marriage would be the most important source of such inter-personal influence. This hypothesis was founded in the work of Glaser [7]. Glaser argued that marriage could increase turnout by twenty percentage points, specifically for those that, if alone, might be less personally motivated to vote. Wolfinger [11] posits that recently divorced or widowed persons might have grown accustomed to the assistance of a spouse and suffer from the lack of such assistance at voting time.

Somewhat surprisingly, given these encompassing approaches to marriage and turnout, we know relatively little about the degree of inter-personal influence (and dependency) within couples. It is difficult to disentangle the reasons for the observed differences in political behaviors of married and single voters. To pose the question succinctly: is the fundamental cause of the turnout discrepancy between married, divorced, and widowed voters that many unmarried, 'unpaired', potential voters are alone at polling time? Evidence supporting this social isolation hypothesis could have large implications for changing patterns of voting behavior in the American electorate. Recent work on social isolation in the United States suggests that Americans find themselves increasingly alone [6, 20].

Several factors could be at play within this lonely non-voter perspective. For example, are spouses influencing each other to become more politically involved or do they possess identical voting records because coordinated their voting behavior? Could attendance at political events or turning out to vote be social ('moral') support from one less political spouse to the other (analagous to an individual who very much wants to visit a certain restaurant, but is unwilling to go alone, or a less religious spouse indulging a more religious one)? At old ages, could some voters be (or, nearly equivalently, feel) entirely dependent on a healthier spouse? Regardless of the (possibly unique) mobilizing factor, the unifying, necessary condition for these influences is a voting partner. Should these partnerships exist at varying prevalence within the electorate, this will have fundamental implications for democratic representation.

For most Americans, a spouse is the most likely source for such a political pairing. This is well-supported by findings in the political discussion and inter-personal influence literature noted above [7, 18, 17]. Also, many people consider voting to be a duty [21] and political scientists have incorporated this assumption into theories of political participation [22, 23, 2]. Given this, and if such a duty might be alternatively be considered a chore to be checked off the 'to-do list,' it might be unusual to invite a friend to go vote with you unless you are both highly political. With this logic, whether spouses (or any couple living together) serve as voting partners might be less dependent on their levels of political interest.

1.2 Grieving, Health, and Behavior After the Death of a Spouse

It is well-established that the death of a spouse has profound consequences. The recently widowed are at a much greater risk of death following the loss of a spouse [24, 25, 26]. Survival after the hospitalization of a spouse and spousal death vary by disease [26, 27] and men are more adversely affected by spousal death than women [25, 24, 28], though men and women respond similarly to the hospitalization of a spouse [26]. Some works find greater mortality risk among younger widows and widowers [25, 26], while others find no significant difference by age [28]. This phenomenon is conventionally termed the widower effect, or, more colloquially, 'dying of a broken heart'.

Nonetheless, mortality risk is highest in the *first three months* following the death of a spouse and stabilizes (at a higher mortality risk than before) after *one year* [29]. Rates of depression also stabilize one year into widow(er)hood [30]. Moreover, some changes in actions related to widowhood might be expected to precede the actual death of the spouse, and to reflect the burden of caring for the spouse [26], especially since the terminal period can be significant [31]. For this reason, we expect turnout to be lower not only after the death of a spouse, but also for some period beforehand. People caring for a chronically ill spouse should be less likely to vote.

1.3 Research Design

Past research has established that widowed and divorced voters, even after suitable controls, are significantly less likely to vote than their married counterparts [15, 11]. However, these analyses are unable to determine the time frame of this behavioral change.

In contrast, our research design is intended to measure behavioral change throughout the transition from being married to widowed. We also seek to determine to what extent reduced turnout during this transition may be attributed the loss of a partner (and social isolation), rather than disengagement, emotional trauma, disability, or some other factor. We first establish a baseline turnout rate in the years preceding a spouseâĂŹs death via exact matching of widowed voters to married voters on relevant criteria, including past voting history, age, and gender. We then measure the difference in widowsâĂŹ turnout and baseline turnout before and after the death of their spouse.

A gradual decline prior to the death may be caused by coping with a chronically ill spouse or oneâĂŹs own poor health. However, if it is oneâĂŹs own poor health then we expect this decline to continue after the spouseâĂŹs death. A gradual recovery towards previous political behavior suggests an emotional explanation for behavioral changes. Meanwhile, an indefinite change would be best explained by spousal dependency.

1.3.1 Methods

Our analysis is a combination of seventy-eight case-control trials, one for each week in the six months prior to, and twelve months after, the death of a spouse. We also use three longitudinal observations for each trial to compare voting behavior between and within cohorts, and further analyze turnout immediately before and after the anniversary of the death of a spouse.

For each of the seventy-eight weeks surrounding the California Special Statewide 2009, Gubernatorial Primary 2010, and Gubernatorial General 2010 elections, we match likely widowed voters to pairs of likely married voters by exact past voting history, party identification, gender, age discrepancy between spouses, and age group. Age and age discrepancy are ‘coarsened’, as described by Iacus et al. [32] to avoid preferentially dropping older and/or less consistent voters. This exact matching is possible due to the size of the utilized dataset. In the next section, we describe the means through which we identify 60,000 cases (spousal deaths) and 5,800,000 controls. Through this, we examine the effect of the death of a spouse on the propensity to turn out in the year and a half before and after the death of a spouse.

The analysis of the overall patterns in turnout over the seventy-eight weeks surrounding the death of a spouse deploys both a between-cohort and within-cohort analysis of these individual case-control trials for each of the three elections. Week fifty-two (by time from spousal death – negative numbers denote time preceding a death) cohorts experience the anniversary of a spouse’s death on the week of an election. The week zero cohorts include voters whose spouses died the week of an election. The anniversary discontinuity, at 52 weeks, is important because it may mark a turning point in grieving and behavioral adaptation, as suggested by widowhood effect health research including Jin and Christakis [33], and by prior (mixed) work on anniversary-related and other date-related health effects [34, 35]. Also, the emotional effect of the anniversary of a death is something of a truism in the psychiatry literature (though it has been studied in one empirical work on spousal death, [36]). Most studies of emotional health conduct surveys in the thirteenth month of grieving rather than the twelfth to avoid this effect (see, for example, [37, 30]). Because this effect (to our knowledge) has never been shown in quantitative work, point displacement at this time point (fifty-two weeks) permits preliminary confirmation of the anniversary as a major psychological event. We set discontinuities at weeks zero and fifty-two to account for expected sudden and substantial behavioral shifts at these two time points. We also examine the suddenness of behavioral changes to determine whether observed turnout rates among widowed voters may be primarily due to the loss of social support rather than the onset of personal illness.

To isolate the effect of the loss of a political partner, we compare turnout rates among widowed voters who in 2004 through 2006 voted more, the same amount, or less than their spouses. Next, we consider the influence of the grieving process on voting widowhood effects by determining whether widows return to previous voting behavior of the observed year and a half period. We argue that the difference among turnout rates for these groups is a useful approximation of mobilizing influence, once we establish variation by age and that independent participation among couples five years before a death cannot be attributed to the chronic illness of one spouse. The treatment groups (widowed voters) are compared to a matched baseline turnout in control groups (electors with similar ages, voting histories, and other covariates, weighted by their representation in the widowed population).

1.3.2 Data

Because marital status is confidential information in publicly available data and the theories we wish to test posit that it is a spousal partner rather than the institution of marriage that will most influence observed differences in turnout among married and previously married voters, we use a validated algorithm to infer likely spousal relationships from voter residency data.

At the ages persons are most likely to experience a death, cohabiting couples of similar ages are likely to be spouses, partners, or in some cases, siblings.[1] Within the

[1] In 2003, respectively only two and three percent of women and men over the age of sixty-five lived with non-relatives, seventeen and seven percent lived with non-spouse relatives, and forty-one and seventy-one percent lived with their spouses [38]. Many more women than men lived alone, forty to nineteen percent, due to greater rates of widowhood [38]. We first accept these rates as given, and then we observe the incidence of same-sex couples with the same last name to infer the prevalence of non-spouses in our analysis.

theoretical framework of political partnerships (whether from a political discussion or interpersonal perspectives), all of these relationships can resemble a spousal connection, though possibly to differing degrees. We limit the analysis to couples sharing the same last name to decrease the possibility that observed pairs are unrelated voters who list the same address. While this removes many people who are common law spousal partners, it ensures that the remaining sample is less likely to contain non-spouses.

The analysis utilizes four data sources: the California Voter Records for 2009 and 2010, the Social Security Death Master File, and the 2000 Census zip code level geographic data.

The California Voter Records contain individual-level registered voter information including full name, date of birth, complete address, gender, party affiliation, and voting history from the 2004 Presidential General Election through the 2010 Gubernatorial General Election.[2] The Social Security Death Master File (SSDMF) is a record of all deaths reported to the Social Security Administration (SSA), close to 90 million total deaths in the file used for this research. The individual-level records include full name, date of birth, date of death, social security number, zip code of last residence, and the source of the death notice received by the SSA. The file used for the research is the version last updated in March 2011. As of 1997, following the introduction of policies to enforce death reporting, the SSDMF included over 95 percent of deaths occurring after the age of sixty-five, around 80 percent for ages fifty-five to sixty-four, and around 75 percent for ages twenty-five to fifty-four [40]. It is less likely to include death records for women and foreign-born naturalized citizens than for

[2] Not all of this data is complete, much like the Los Angeles County records used by Brady and McNulty [39]. For example, out of the over 17 million voters in the file, over 100,000 do not list a full date of birth (the majority of those who do, omit the year), around one-third do not include gender (partly because this is no longer included on voter registrations), and around 1.46 million voters were expunged between 2009 and 2010, in compliance with California voter record policy. The implications of the removal of voters from the registry are explained in the discussion section. Voters who experience long, debilitating illness are more likely to be absent from our analysis.

men born in the United States [41].[3]

1.3.3 Algorithm to Infer Spousal Relationships

To implement the algorithm, we first create a dataset of all households in California by grouping voters with the same listed address, excluding addresses with more than six household members (to exclude group homes). Next, we link household members whose ages are within fifteen years and who share the same last name. The dyadic linking duplicates the voter records, with each individual in a spouse pair classified in one observation as a ‘subject’ (the person whose turnout we will measure) and in another as a ‘spouse’ (the person whose death may affect the subject’s behavior). Lastly, we remove observations where the deceased voter is designated the subject.

We link registered voters in the two California Voter Records by exact full name and date of birth and deceased voters to the Social Security Death Master File in the same way. To ensure that deceased voters are identified accurately, this dataset excludes households with at least one occupant whose full name and date of birth are duplicated in the California Voter Records or in the Social Security Death Master File. This also avoids over-counting of the many duplicate records present in the file (up to 100,000 if we compare the incidence of persons sharing the same name and birth date in the Death Master File to that in the California Voter Record).

We remove records in which there are more than three household members of age within fifteen years who share the same last name. We also remove subject-spouse pairs

[3]The Social Security Death Master File cannot be fully relied upon to identify whether a voter is living. However, the California Registrar of Voters uses the Department of Public Health records to remove deceased voters from the voter registry. Comparing the rates of removal for voters identified by the Social Security Death Master File as deceased, we find that California counties remove eighty to ninety percent of deceased voters from the Voter Record prior to an election. Only Los Angeles County varies significantly from this. It has a thirty percent removal rate.

who experience a within-household death to another generation (another subject-spouse pair in the same household). We exclude pairs included in a three person generation because we are unable to infer probable spousal relationships. Pairs which experience a death in household, but not within the subject-spouse pair, are excluded to prevent these voters from being included in the controls. Income and population density variables from the 2000 Census are by zip code.

This process leaves us with around 5.8 million controls (half as many households) with a living spouse and 60,000 cases with a deceased spouse. These remaining dyads are the examined ‘spouses’. We impute gender when voter records include a gender-specific title (like “Mr.”). A total of 1.5 percent of the female deceased “spouses” are the same sex as the cohabiting voter corresponding to the profile of a spouse, while 4.8 percent of the male deceased spouses are of the same gender as the identified subject. We assume that these numbers are upper bounds (that siblings are more likely to live together if they are the same sex).

1.3.4 Matching

Once we have identified probable spouses in the California Voter Record, we match voters who have lost a spouse in the past year to voters who have not. The purpose of this process is to balance treatment and control groups on covariates that predict spousal deaths, and restrict our analysis to only the voting population likely to experience spousal deaths. This matching is partitioned by weeks since the death of a spouse at election time to enable multiple between- and within-cohort comparisons. Because the counts of days between California elections are multiples of seven, cases belong in the same weekly cohorts for each analyzed election, Gubernatorial General 2010, Gubernatorial Primary 2010, and

Special Statewide 2009. The criteria for this matching are shown in Table 1.1. The couples are matched exactly, in some cases within groupings, on a many-to-many basis. This implementation utilizes the methods described in Iacus et al. [32]. Matched cases (m_T)and controls (m_C) used in the analysis receive weights described in Equation 1.1. The control weights are the ratio of cases to controls in the matched stratum S ($\frac{m_T^S}{m_C^S}$), multiplied by the ratio of matched controls to matched cases in the trial ($\frac{m_c}{m_t}$) (which is constant among in-trial strata). Unmatched cases and controls receive a weight of zero.

$$w_i = \begin{cases} 1, & i \in T^S \\ \frac{m_T^S}{m_C^S}, & i \in C^S, (\times constant = \frac{m_c}{m_t}) \end{cases} \tag{1.1}$$

The matching criteria directly and indirectly account for a number of important factors which affect the comparability of these two populations (widowed and married voters). The first and most obvious of these is the age of a spouse. Also, women are much more likely to survive their spouses than men. Fifty-one percent of ever-married women over seventy have been widowed while twenty-three percent of men over the age of seventy have been widowed [42]. Party affiliation helps account for variation by political cycle.

The past voting history boolean variables are catch-all matching terms. Because voting behavior is determined by a number of factors, including habit [43], persistence [44], socioeconomic status, and natural predisposition [45], matching on past voting history partially controls for these variables (between cases and controls).

1.3.5 Calculation of Treatment Effect

Given the exact matching process and the size of the dataset (60,000 cases and 5,800,000 controls), calculation of treatment effects is non-parametric and makes few

Table 1.1: Match Criteria

	Subject Variables	Spouse Variables	Subject-Spouse Var.
Age	-	Age [1]	Age Discrepancy [1]
Gender	Male/Female/Unknown	Male/Female/Unknown	-
Household Occ.	-	-	Nmbr. Reg. Voters
Party Affiliation	Democrat/Republican/Other	Democrat/Republican/Other	-
Voting History	GG06, GP06, SS05, PG04 [2]	GG06, GP06, SS05, PG04 [2]	More/Same/Fewer

[1] Coarsened - groups:
Age: (18:24), (25:29), (30:34), (35:39), (40:44) ... (75:79), (80:84), (85:89), (90:94), (95:115)
Age discrepancy: (-15:-6), (-5:-2), (-1:1), (2:5), (6:15)
Household occupancy: (2), (3:6)

[2] California statewide elections 2004-2006:
GG06 - Gubernatorial General 2006 GP06 - Gubernatorial Primary 2006
SS05 - Special Statewide 2005 PG04 - Presidential General 2004

assumptions. We use average treatment effect on the treated (ATET) to calculate the widowhood effect for voting. The calculation is the turnout rate of married voters minus the turnout rate of widowed voters, weighted according to their proportional representation in the widowed population (matched cases receive a weight of one, matched controls receive a weight of the number of cases within the matched strata over the number of controls within the matched strata as described in Equation 1.1, unmatched cases and controls receive a weight of zero). Point estimates, standard errors and 95% confidence intervals are calculated using the standard bivariate regression formulas. In Equation 1.2, V_{1e} is the turnout of cases and V_{0e} is the turnout of controls. D_a represents the distribution of treatment covariates ($D_a = 1$ meaning that observed treatment covariates of controls are distributed identically, within the coarsened matching bounds, to covariates in the treatment group. This controls for the probability of experiencing the death of a spouse.) and w_i is the weights assigned to cases and controls.

$$ATET = E[V_{1e} - V_{0e}|D_a = 1] = E[V_{1e}] - E[V_{0e}|w_i] \tag{1.2}$$

Next, we divide this estimate by the weighted mean of control turnout rates to obtain the proportional treatment effect. See Equation 1.3. This estimates the proportion of widowed voters who did not vote, but would have voted had their spouse been alive at election time.

$$PATET = E[V_{1e} - V_{0e}|D_a = 1]/E[V_{0e}|w_i] \tag{1.3}$$

We calculate these estimates for each cohort and their three longitudinal observations. This partitioning allows us to view progressive behavioral changes in the year and half surrounding spousal death. We add loess smooth (discontinuity at week zero) and least-squares (discontinuity at week fifty-two) regressions to better visualize behavioral changes over the course of the observed period. The least-squares regressions begin at week fifteen. This excludes both the acute mourning period (this is a separate, highly non-linear recovery process) and early mail-in ballots (this removes subject votes cast while a spouse was still living, but who died before the election).

1.4 Results and Analysis

After we have matched voters and calculated the average treatment effect for voters who experience a spouse's death in a given week (and treatment effects for age, gender, and voting subgroups), we first align the results by the weeks since spousal death at election time. The overall results appear in Figure 1.1.

In Figure 1.1, each point represents the estimated widowhood effect (average treatment effect on the treated) for those voters whose lost a spouse in a specific week (week zero is the week of an election). For illustrative purposes only, we highlight four cohorts through the one and a half year observation period (two circled on the left-hand side and two outlined with squares on the right). The average number of cases per week was 644

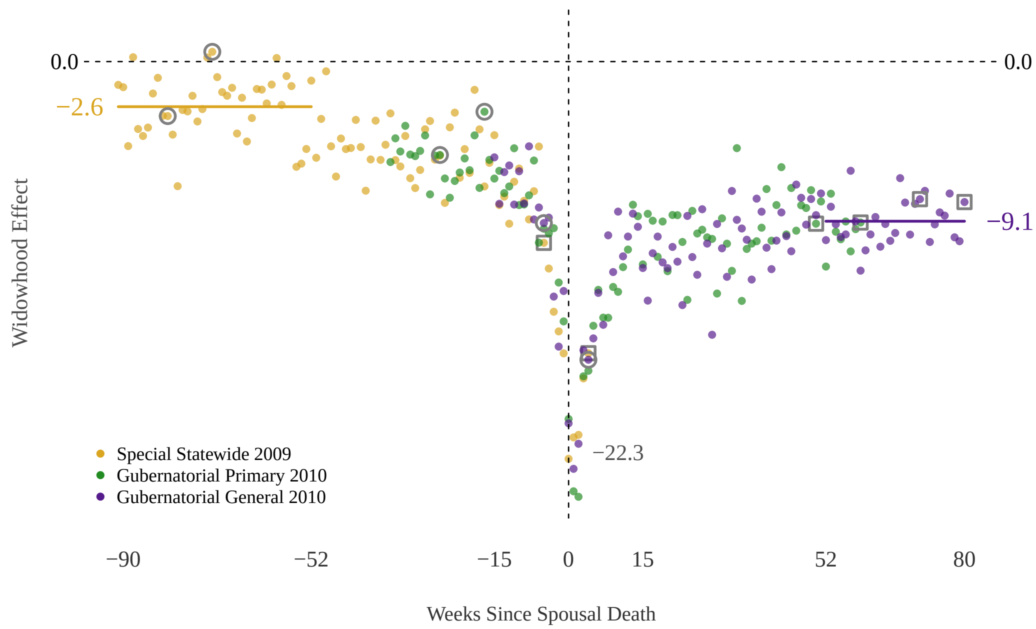

Figure 1.1: *Overall widowhood effects, aligned by weeks since spousal death at election time*. This plot shows the turnout changes for 60,856 widowed voters. The line at 0.0 is the base turnout of matched and weighted controls and each point displacement is the average difference in turnout between matched subjects and weighted controls for a weekly cohort (the average treatment effect on the treated). Groups of widows and widowers who would lose a spouse in the weeks following an election are the -90 through -1 week cohorts on the left of the vertical line at 0 (election week) and groups who lost a spouse prior to an election are the 1 through 80 week cohorts on the right. Each subject, and his or her weekly cohort, appears three times in the figure–once for each election. To illustrate the three observations, we circle two cohorts of caregiving/soon-to-be-widowed voters and draw squares around two cohorts of recently widowed voters.

and the rate of successful matches for cases was 98%. Unmatched cases receive a weight of zero and are not included in the estimate (according to the procedure described above from Iacus et al. [32]). The x-axis is the weeks since the death of a spouse, and the y-axis is the widowhood effect on voting, i.e. the difference in turnout rates between cases and matched controls. The observed time period – the time between the first available California Voter Record (voters deceased before this time had already been removed from the record) and the version of the available Social Security Death Master File – spans three elections.

There are three main results. 1) Turnout rates of cases decrease precipitously in the weeks immediately preceding a spouseâĂŹs death and reach a nadir when the death occurs near Election Day. 2) After turnout rates increase and then stabilize around three months following spousal death, about eleven percent of widowed voters no longer turn out to vote (ATET percentage point estimate: -.091, OLS standard error: .003; control turnout estimate: .830, OLS standard error: .00026). 3) The turnout of widowed voters increases greatly only in the acute mourning period, and increases are statistically significant only in the first year (weeks fifteen through fifty-two OLS slope: .00092, standard error: .00025, p-value: .00068 – weeks fifty-two through eighty OLS slope: .00032, standard error: .00029, p-value: .283). Given that most behaviors and rates of depression following the death of a spouse stabilize after one year [33, 30], this suggests that many widowed voters may discontinue voting indefinitely. This is further analyzed in the final results section on recovery to previous voting behavior where we show variation by household occupancy. Table 1.2 in the online supporting information (SI) shows aggregated widowhood effect estimates by partition and time period before and after spousal death.

Figure 1.5 in the supporting information aligns the results by cohort, meaning that the points vertically aligned in this figure are identical groups of voters. This graphic shows turnout of both cases and controls, and displays a proportional widowhood effect (the proportion of widowed individuals who did not vote but would have voted had their spouse been living), as opposed to the mean difference in case and control turnout shown in Figure 1.1.[4]

1.4.1 Changes in Turnout by Age and Gender

A gender difference in widowhood effects is one of the most established findings in widowhood effect health research. Men are more adversely affected than women, especially in the period surrounding spousal death [25, 24, 28]. This is commonly attributed to differences in the salutatory effects of marriage for men and women, and differing emotional and practical responses.

Figure 1.2 compares widowhood effects among men and women. The left-hand side of this graph shows differences in turnout between cases and matched controls for the Special Statewide 2009 and the right-hand side shows the Gubernatorial General 2010. We observe spousal deaths for only one month prior to the Special Statewide 2009 and for three months following the Gubernatorial General 2010. We are able to compare the different election turnout rates because the absolute difference between cases and controls over weeks since the death of a spouse is constant between low and high salience elections (as shown in the previous figure). The Gubernatorial Primary 2010 is excluded because the additional points reduce graphical clarity and are substantively equivalent to those from the Special Statewide and Gubernatorial 2010. The x-axis is the weeks since the death of a

[4]It is relevant to note that the voters appearing on the more recent end (the left side of the figures) of the observation period are, on average, one year older than the voters on the earlier end (the right side). This is evident in the slightly lower base turnout rates. However, the turnout rates of the matched controls differ by only .25 percentage points. Also, there are around twelve percent more cases in the winter season (noticeable in the smaller variance for the winter season), and there are eight percent fewer cases in summer 2010 than in summer 2009. This discrepancy is likely due to both out-of-state changes of address among widow(er)s and the deaths of surviving spouses.

spouse, and the y-axis is the time since spousal death. The numbers on the left-hand side note mean widowhood effects for weeks negative ninety-one through negative fifty-two and the numbers on the right-hand side are mean widowhood effects for weeks fifty-two through eighty.

Next, we calculate aggregated effects (taking the mean across time would assign different weights because there are more cases in winter and more cases on the left side of the graph). These numbers are: weeks -91:-52 men -.020 (95% ci: -.014, -.026), women -.030 (95% ci: -.026, -.035); weeks 52:80 men -.087 (95% ci: -.081, -.093), women -.088 (95% ci: -.084, -.092). The complete aggregated results are shown in Table 1.2 in the supporting information.

In the year surrounding spousal death, men are slightly less adversely affected than women. Using a simple differences in means test for widowhood effects at weeks fifty-two before spousal death through fifty-two after spousal death, this difference is statistically significant at $\alpha = .05$. However, the aggregate widowhood effects in the Gubernatorial General 2010 in weeks fifty-two through eighty after spousal death, the period for which turnout rates are stable, are not substantively different between genders. As noted above, they are -.087 ATET for men and -.088 ATET for women. This difference is not statistically significant.

Figure 1.8 in the supporting information compares progressive differences in widowhood effects by age. Voters under the age of sixty-five are more adversely affected than older voters, and the effect decreases up to age seventy-five (a finding that parallels prior work showing a larger widowhood effect with respect to health when the decedent or surviving spouses are younger). The increase in the adverse effect at old ages is conceivably due to a decreased ability to travel to a polling station or obtain and complete a mail-in ballot. We

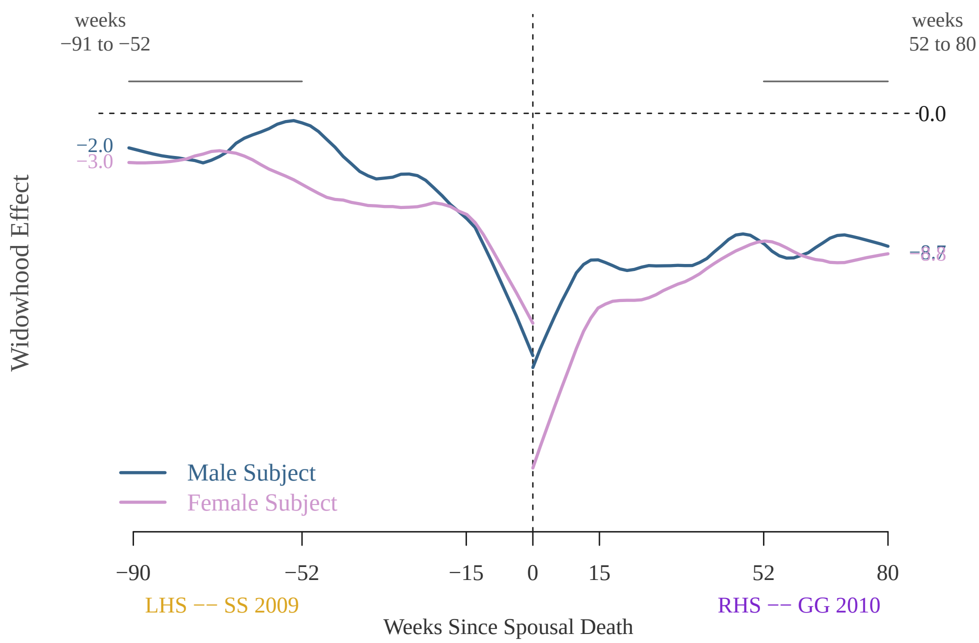

Figure 1.2: *Gender comparison.* This figure shows the separate changes in turnout levels for widows and widowers. The line at 0.0 is the base turnout of matched and weighted controls and the loess curve (span = .3) shows the smoothed average difference in turnout levels between matched subjects and weighed controls for weekly cohorts (the smoothed average treatment effect on the treated by week). As in the figure above, the widows and widowers on the left of the vertical line at 0 (election week) would lose a spouse in the weeks following an election and those on the right lost a spouse prior to an election. The left-hand side of this graph shows differences for the Special Statewide 2009 and the right-hand side shows the Gubernatorial General 2010. Figure 1.1 and Table 1.2 in the supporting information show that absolute differences between cases and controls over weeks since the death of a spouse are constant between low and high salience elections.

exclude pairs that possess different voting histories because matching drops many cases in the ‘more votes than spouse’ and ‘fewer votes than spouse’ partitions at advanced ages. This aids the analysis of inter-personal mobilization, since it likely removes pairs with unusual voting records that may be attributable to illness, but misrepresents an analysis of widowhood effects by age.

The greater effects at young ages may be attributed to greater emotional trauma for deaths at young ages (when deaths are more sudden and unexpected), a greater dependency on a political partner, or a combination of both of these two factors. Subramanian et al. [46] find that a high concentration of widowed individuals moderates the adverse health effects of widowhood. In the context of voting, this suggests the possibility that greater social isolation resulting from widowhood at relatively young ages is further plausible explanation.

The supporting information also contains Figures 1.6 and 1.7, showing turnout by age for the test elections and baseline elections (note that baseline elections are matched and not directly calculated in the widowhood effect estimates) for both widowed individuals and matched controls. Of most relevance to the current analysis, they show 1) that turnout rates in the age groups most likely to experience a spousal death are comparable between low and high salience elections, and 2) the pruning effects of the matching process restrict most of our control sample to the forty-five to ninety age range.

1.4.2 The Effect of the Loss of a Political Partner

While the analysis so far suggests that people are less likely to vote once their spouse dies, it does not investigate the potential causal mechanism. Next, we test whether or not the political behavior of the deceased spouse influences the size of the widowhood effect. If it does and if the variation in the effect does not substantively change with age in the matched sample, then the result suggests that the loss of a mobilizing partner is driving the effect. If variation in past behavior does not influence the widowhood effect or if the results change substantively with age, then the loss of social support, physical disability, and/or depression may be the most important causal mechanisms.

Figure 1.3 compares widowed individuals partitioned by whether, in 2004 through 2006, they voted while their spouse abstained, possessed the same voting history, or abstained while their spouse voted. This figure is identical to the gender comparison graphic. The left-hand side shows differences in turnout between cases and matched controls for the 2009 Special Statewide Election and the right-hand side shows the 2010 Gubernatorial General Election. As in Figure 1.2, Gubernatorial Primary 2010 is excluded because the additional points reduce graphical clarity and are substantively equivalent to those from the Special Statewide and Gubernatorial 2010.

Notice that the discrepancies between cases and controls and their progression leading up to spousal death are approximately equivalent in all three partitions. In contrast, the differences among these relative voting history subgroups after spousal death are substantial, and greater than any others we observe in this research. These findings strongly support a social mobilization explanation for widowhood effects in voter participation. Spouses motivate each other to vote, even up to the weeks just before death.

Since past voting histories may be a proxy for health, we analyze how the widowhood effect for pairs with different voting histories varies with age. The results of this analysis are shown in Figure 1.9 the supporting information. The differences in widowhood effects between subjects whose ‘spouse votes more’ and subjects whose ‘spouse votes less’ are constant over the age range. In contrast, if poor health among people who vote less than their spouse were driving the result, then we would expect this difference to be smaller in

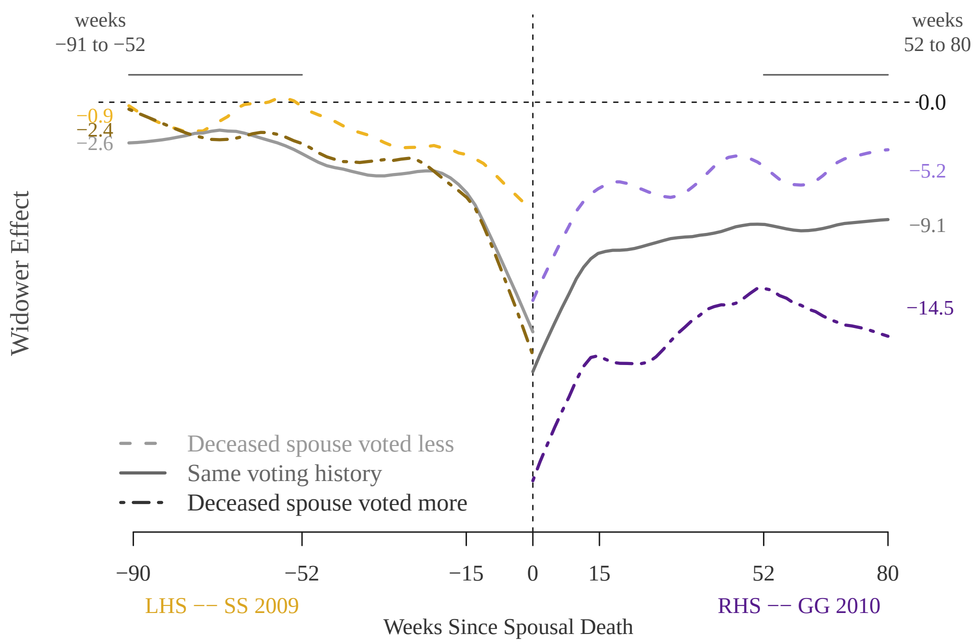

Figure 1.3: *Voting history comparison.* This figure shows the separate changes in turnout levels for widows and widowers who in the past vote more, the same, or less than their spouses. The voting relationships proxy for inter-spousal mobilization. Figure 1.9 in the supporting information shows that these relationships are not proxies for health. As in the previous figures, the line at 0.0 is the base turnout of matched and weighted controls and the loess curve (span = .3) shows the smoothed average differences in turnout levels between matched subjects and weighed controls for weekly cohorts (the smoothed average treatment effect on the treated by week). The widows and widowers on the left of the vertical line at 0 (election week) would lose a spouse in the weeks following an election and those on the right lost a spouse prior to an election.

younger people for whom the incidence of debilitating illness is much lower.

1.4.3 Recovery Past Fifty-Two Weeks

In most of our results, we see the turnout of widows and widowers increase up to fifty-two weeks before it flattens or again declines. This is similar to results in most widowhood effect research that finds health behaviors and outcomes *stabilize* after one year [29, 30]. Accordingly, the characteristics of any groups that are able to return to pre-death levels of turnout may suggest factors which, over the long-term, ameliorate the adverse effect of widowhood and personal loss on voting.

In Figure 1.4 (and Figure 1.10 in the supporting information), the x-axis spans weeks fifteen through eighty after the death of a spouse. The y-axis is the widowhood effect. We draw a discontinuity at week fifty-two, the anniversary of spousal death where we see most turnout rates stabilize, and highlight turnout during this week (as an indicator of whether the anniversary is a significant displacement in turnout rates and a verified anniversary effect). The means on the left and right hand side of the figure are for weeks fifteen through twenty-five and seventy through eighty respectively.

Figure 1.4 shows recovery past the anniversary of spousal death by the number of registered voters in the household. Voters in households of three or more voters are less likely to be living alone after a spouse's death. In this dataset, "living with others", or "not alone", means that the widowed individual was living with another registered voter, in addition to the spouse, in 2009 (prior to the spouse's death). Because the spouse identification algorithm excludes voters with two possible spouses, meaning two cohabitants with an age within fifteen years of the voter and a shared last name, along with address listed by more than six individuals (regardless of last name or age), the additional household members who are registered to vote are likely to be adult children or family members from another generation, such as cousins, nieces or uncles. Widowed individuals who may have moved to be closer to family are included in the "alone" category because we observe their movement only when they re-register to vote. The solid line is the observed recovery, while the dotted line is an estimate of the recovery that is unobserved due to widowed individuals moving out-of-state.[5]

This comparison of widowed individuals who are alone against those who are not alone shows that close to half of widowed individuals return to previous voting behavior in the year and a half after the loss of a spouse. Recovery in widowed individuals living alone is limited. In the supporting information, we show that voters over eighty recover substantially in the first year before their turnout rates plateau, and that voters under sixty-five also experience continual recovery. However, the continual recovery among voters under sixty-five is less substantial than that for those living wither others (and not statistically significant), suggesting that social support is a greater ameliorating factor than young age.

This result suggests concrete means through which widowed individuals may maintain civic engagement. Widowed voters living with others are less socially isolated and may able to receive assistance when needed (including a ride to a polling place). Living with

[5]We utilize two estimates of this unobserved recovery. The 2010-2011 Current Population Survey (CPS) [47] estimates that 16% of American widow(er)s who move, move between states. This is higher than out-of-state movements from California, but we use it as a conservative estimate. We multiply 16% by the proportion of widows who re-register at new addresses before the anniversary of spousal death in our sample and multiply by 20% for re-registrations at new addresses after the anniversary (because out-of-state relocation might occur slightly later). We then multiply this by the turnout of controls (a widowhood effect of zero because re-registration is correlated with voting) and add this "unobserved recovery" estimate to the fitted values of the observed data. This first estimate is the dotted line in Figure 1.4. The second estimate uses 16% before and after the anniversary and the case turnout (which takes an unobserved widowhood effect equal to the observed effect). The second estimate is the lower bound in our estimate of the recovery rate and the first estimate is the upper bound. We do not include this unobserved recovery in the overall estimates of the widowhood effect because of the likelihood of an unobserved effect from illnesses sufficiently debilitating and chronic to result in the de-registration of the dying and their caregivers before the observation period. This exclusion assumes the effect of long-term, debilitating illness is at least the size of the unobserved recovery (greater than one to two percent of the sample size).

others may increase the motivation to vote (and it is noteworthy, if unsurprising, that living alone may lead to depression [48]).

1.5 Discussion

Here, we estimate that approximately 70 percent of the change from married to widowed turnout may occur during the year immediately surrounding the death of a spouse. Moreover, spouses who voted more than the decedent experienced a substantially smaller widowhood effect with respect to voting than spouses who voted less than their deceased partner. This difference is not explained by the possibility of differences in personal health and disability between these two groups. Overall, we see that around 11 percent of voters who otherwise would have voted no longer vote even a year a half after the death of a spouse.

Social connection, either by increasing the motivation to vote or decreasing the obstacles to voting, may greatly attenuate the adverse effects of spousal loss on voter participation. We observe substantially less recovery past the acute grieving period among widowed individuals who likely live alone than among widowed individuals living with another registered voter (who in our sample are very likely to be children or other different-generation family members). After stabilizing around four months after spousal death, observed turnout rates among solitary widowed individuals increase up to the anniversary of spousal death before flattening or again declining. Turnout rates among those living with others rise continually. We estimate that around 20 to 30 percent of voters living alone after a spouseâĂŹs death return to previous voting behavior within a year and a half of the death, while 50 percent of voters still living with others return to previous rates of voting in the same time period. This variation is not explained by differences in age or residential mobility between the two groups.

The findings that the loss of a mobilizing partner and relative social isolation likely drive the sudden, varied, and persistent changes in voter participation beyond that attributable to emotional trauma from spousal death also have significant implications for observations on the turnout discrepancies between married and divorced voters. Somewhat counterintuitively, works that compare the effects of widowhood and divorce suggest that divorce is at least as, and perhaps more, detrimental than widowhood in the long-term. The similarity of divorce and widowhood is a consistent finding for turnout discrepancies [10, 11], loneliness [49], levels of subjective well-being [50], and morality risk [24].

A conservative estimate of the net electoral losses from spousal death and divorce, assuming persistent changes with some recovery, accounts for 1.1 million lost votes in non-presidential elections (6% of 9.8 million widowed and 3% of 17.6 million divorced/separated registered voters [42]). Strong effects for the divorced and never-married would increase this estimate by up to 3 million votes.

To place this sample and the findings on spousal mobilization in perspective, 61% of spouses in our study possess identical turnout histories for all four observed low salience elections (gubernatorial primary and special statewide elections between 2005 and 2010). The estimated 'contagion' effect for two person households in Nickerson [1], using experiments conducted during low salience elections, was 60%. Gerber et al. [2] calculated a treatment effect of 8 percentage points in a low salience election for its GOTV experiment on social pressure. Here, the estimated overall widowhood effect for the low salience Gubernatorial Primary 2010 was -9.6 percentage points (weeks fifteen through fifty-two after spousal death–the estimated effect was -10.3 percentage points in the Gubernatorial General 2010 for the same time period and one percentage point smaller after week fifty-two). We observe around 10 percentage points difference between the "more political" and "less political" spouse partitions for the Gubernatorial General 2010 and 8 percentage points difference in the primary.

There are several limitations of this study. First, we do not know the specific reasons that widows and widowers stop voting. Though spousal mobilization and social isolation explain aggregate variation and social support seems to either increase the motivation to vote or decrease obstacles to voting, these factors tell us little about the individual experiences of widowed individuals. Also, those couples who discontinue voting long before the death of one spouse may be excluded from our sample. This selection may lead to underestimation of the effects of social isolation, health, and emotional trauma.

Second, our study is limited to non-presidential elections. Levels of (potentially compensatory) social mobilization are presumably higher during very high salience presidential elections, and whether or not the widowhood effect observed here extends to these contests remains open to speculation and further empirical work. We note, however, that turnout rates and effects of spousal death in our study population (many of whom are senior citizens) are comparable between special, midterm primary, and midterm general elections.

Next, while our results vary less substantially by geography than by differences in spousal voting histories and household occupancies (consistent with predominantly 'small spatial scale', or household, effects on turnout observed by Cutts and Fieldhouse [4]), we note that California-specific contextual effects may limit the exact external validity of our findings. For example, California permits mail-in ballots and some areas have vote-by-mail only precincts. Assignment to vote-by-mail decreases participation in high-salience elections and may increase participation in low salience elections [51]. In preliminary work, we have observed no statistically significant differences in areas with very low population density (areas more likely to have vote-by-mail only precincts) in the 2010 general election.

However, we cannot yet rule out state-by-state variation or variation contingent on specific voting rules.

With respect to the possible return of individuals to previous voting behavior, though our results suggest somewhat limited recovery past one year, there is still likely to be unobserved recovery in the long-term, especially as widowed individuals re-establish close relationships. It is also important to note that the possibly indefinite drop in turnout does not imply a lack of recovery in a broader sense (emotional or physical health, for example).

Finally, while we find that voters living with others return to previous voting behavior, we note that family social support (and perhaps income) may determine the probability that elderly voters live with their children. We cannot distinguish between the presumed support (either emotional or practical) which determined living arrangement and that support that might be provided by default in a shared living environment. Conversely, the poor health of the elderly parents may determine living arrangements. We do not see evidence of this in our sample, but, if the case, it could lead to an underestimation of the positive effect of living with others.

1.6 Conclusion

The death of a spouse greatly decreases one's propensity to vote. Though the large effect of emotional trauma on voter participation (both around spousal death and for the week of the anniversary of the death) is an important finding, our main contribution is perhaps our exploration of substantial variation and persistency in voter participation changes after the death of a spouse. Moreover, the proposed explanatory variables – the loss of a mobilizing partner and an increase in social isolation – are not exclusive to widowhood, and hence more broadly relevant.

Our work departs from previous research on social mobilization by directly measuring the effects of induced social isolation on turnout. The approach improves on previous aggregate and/or cross-sectional studies of social isolation and civic participation by using longitudinal and matched between cohort analyses to provide plausibly causal evidence for isolation effects. Of theoretical interest, our analysis shows that there exist both highly independent and dependent voters in spousal relationships, and that the magnitude of interpersonal dependency effects on turnout can be substantial. We further show that the presence of other registered voters in a household may, over time, compensate for the loss of a voting partner, suggesting that turnout lost with the absence of a social connection can be, but is not always, recovered through others.

We hope that the findings here, and our proposed explanations, serve as a foundation for further direct assessments of social isolation and civic engagement. Identifying forms of social isolation that are involuntary and reversible, along with additional means to compensate for lost social mobilization and/or connection to society (including whether existing social mobilization techniques do exactly this), may be especially fruitful research.

It is relevant to note three additional areas that merit future study: the prevalence and impact of social isolation by socioeconomic status, the influence of divorce and widowhood on the estimated effect of mobility on turnout, and advocacy for the seriously ill.

Poor health and vulnerability to stressful events, including divorce and widowhood, are correlated with low socioeconomic status. Though we are unable to study effects by socioeconomic status at an individual level, an SES gradient appears probable. In our sample, there is a greater effect in zip codes with per capita income below $35,000.

We further note that our findings bear on studies which find that residential mobility appears to decrease turnout, such as Squire et al. [52], but that do not control for changes in marital status. Newly widowed and divorced individuals often move in the year immediately following spousal death or separation. For example, Speare and Goldscheider [52] found that around 50 percent of divorced individuals moved during the year of their divorce, compared with 7 percent of the currently married and 14 percent of those not divorced during the year (the highest rates of mobility, close to 70 percent, were for newlyweds who have also been shown to exhibit low turnout rates during marital transition [10]). In the same study, 11 percent of widowed individuals moved during the year their spouse died, compared with 7 percent of widowed generally individuals. These numbers are not especially surprising. For example, separated individuals who exhibit the lowest turnout rates by family structure [11] do not live with each other by definition. This should be taken into consideration when calculating the effect of residential mobility on turnout. In our results, the period of greatest recovery (weeks fifteen through the one year) is also the period of greatest residential mobility after spousal loss or separation.

The last important area of future study is the ability of the seriously ill, their caregivers, and their loved ones (along with others going through similar crisis) to advocate for their political concerns. For example, disability alone may decrease turnout by up to 20 percentage points [53]. Induced social isolation from certain disabilities or illnesses may negatively affect political participation well beyond the impact of physical health on voting. More broadly, social isolation may be a major form of disenfranchisement. Isolated individuals and families be most in need of government assistance during crisis, and the least likely to request it.

1.7 Additional information

1.7.1 Overview

This paper measures turnout rates before and after a spousal death to estimate the effects of spousal loss on turnout, and analyzes variations in turnout changes to evaluate social explanations for the observed widowhood effects.

The social explanation analysis is based on three assumptions: 1) persistent changes in turnout (changes not accompanied by a substantial gradual recovery for the year half of a spouse's death) are directly attributable to the absence of a spouse, and not the loss and grieving process; 2) relative voting histories of spouses (whether one spouse votes more, the same amount, or less than the other) are indicators of political engagement, and observed variations in turnout changes by relative spousal voting histories are attributable to differences in political engagement; and 3) changes in turnout caused by the absence of a mobilizing spouse can be moderated (over time) by the presence of other electors in the same household (e.g. as they assume new household and social support roles).

The tests of social explanations for drops in turnout following the death of a spouse are presented in the body of the article. We observe limited aggregate return to previous voting behavior in the year and a half following the death of a spouse and no aggregate return to previous voting behavior past the one year anniversary of spousal death, electors who voted less than the decedent spouse are substantially more affected by the spouse's absence than electors who voted more, and widowed individuals who lived with other electors (likely family members) at the time of their spouse's death gradually return to previous voting behavior past the one year mark.

In this supporting information, we detail the structure of the dataset used in the above analysis, and test alternative explanations for observed turnout patterns.

1.7.2 Data Structure

Our longitudinal and between cohort analyses are constructed from three elections – the Special Statewide 2009, the Gubernatorial Primary 2010, and the Gubernatorial General 2010. In the main article (Figure 1.1), we arrange cohorts by weeks since spousal death and show that the average treatment effect on the treated (ATET) estimates for each of the three elections are comparable when arranged this way. Given this and that we do not observe turnout rates for one and half years before and after spousal deaths for each of the three elections, we treat turnout rates in each election as representative.

Specifically, for graphical clarity and because the results are substantively equivalent, we do not display the Gubernatorial Primary 2010 results in the gender and voting history comparisons (Figures 1.2 and 1.3). Also, the analysis of recovery over the study period (in Figure 1.10) is restricted to the Gubernatorial General 2010 because we do not observe widowed individuals for a sufficiently long period following the special and primary elections. This approach is further supported in Figures 1.6 and 1.7 where we show that turnout patterns in the study population (most of which is over the age of 45) are similar for each election.

In Figure 1.5, we show the original data structure for our analysis. The x-axis in each plot is still the weeks since spousal death, but each plot shows cohorts/calendar weeks vertically aligned – each widowed cohort at the same chronological/horizontal position on across rows is the same group of individuals. We also note calendar date by season and year at the top of each plot in the middle column of this figure – while weeks since spousal death increases from left to right, calendar time is more recent on the left side of the plots. The rows plots are also arranged vertically (top to bottom) by chronological date of the election, and the election dates (at x-axis 0) move from right to left across rows.

The plots are otherwise very similar to those in the main text. The left column of plots shows the weighted (using match weights), average turnout rates (y-axis) for both cases (crosses) and controls (circles). In each remaining plots for the paper, the control turnout rate is the baseline ‘0.0’. In the middle and right columns of plots, the y-axis is the proportional treatment effect on the treated. The middle column of plots is the data setup used in the overall, gender, and voting history analyses in the main text. The right column of plots shows the data setup for the recovery analyses (focused on weeks 15 through 52, and the post one year anniversary period).

1.7.3 Age Analysis and Tests of Alternate Explanations

Study Population and Comparability of Elections

We match electors by age and other covariates because not all individuals are equally likely to experience the death of a spouse. Individuals who are nearly equivalent in these covariates are weighted according to their reperesentation in the widowed population, and those individuals for whom we observe either no corresponding cases or controls are dropped from the analysis entirely. Figures 1.6 and 1.7 shows the effects of this matching and pruning process on the representativeness of our sample. Our analysis is most representative for the forty-five to ninety age range. Figures 1.6 and 1.7 further show that turnout patterns in this age group are comparable in low and high salience elections.

Age-related disability and magnitude of widowhood effects

An alternative explanation for turnout discrepancies between spouses with different voting histories is the relative health of each spouse. If this is the case, that differences in past voting history are determined by disability, then we should observe smaller discrepancies in turnout among surviving, younger spouses for whom rates of long-term, debilitating illness are very low. That is, though it is perhaps likely that the deceased spouses, both old and young, were chronically ill, it is much less likely that two young spouses have terminal or prohibitively debilitating illnesses than two older spouses.

Given this, we analyze changes in turnout by age. Consistent with disability-related dependency affecting the ability to vote, figure 1.8 shows that widowhood effects increase past age seventy-five. However, disability does not appear to be a determinant of turnout discrepancies between relative voting history subgroups (those who voted more or less than the deceased spouse). Figure 1.9 shows that the differences in widowhood effects between subjects whose spouse votes more and subjects whose spouse votes less are the same across age groups.

1.7.4 Recovery Analysis and Tests of Alternate Explanations

We show in the main text that returns to previous turnout rates past the one year anniversary of a spouse's death are significant only among those living with others at the time of the death (and presumably living with others in the time after the death). However, living with others or the ability to return to previous voting behavior is perhaps determined by other factors. For example, younger widows may be less likely to have functional limitations and therefore more able to return to previous behavior. Further, the ability to vote may be determined by community support and the accessibility of polling places. In Figures 1.10, 1.11, and 1.12, we show that the age, population density, and area per capita income are less strong predictors of recovery rates in the post-anniversary period than household occupancy.

1.7.5 Further Tests of Alternative Explanations

To allow readers to easily assess significance levels, proportional treatment effects, and possible alternative explanations for observed patterns, we include below a number of aggregate results not displayed in the main article. In particular, we highlight that low past turnout rates are less strong predictors of widowhood effects than social, relative voting history comparisons.

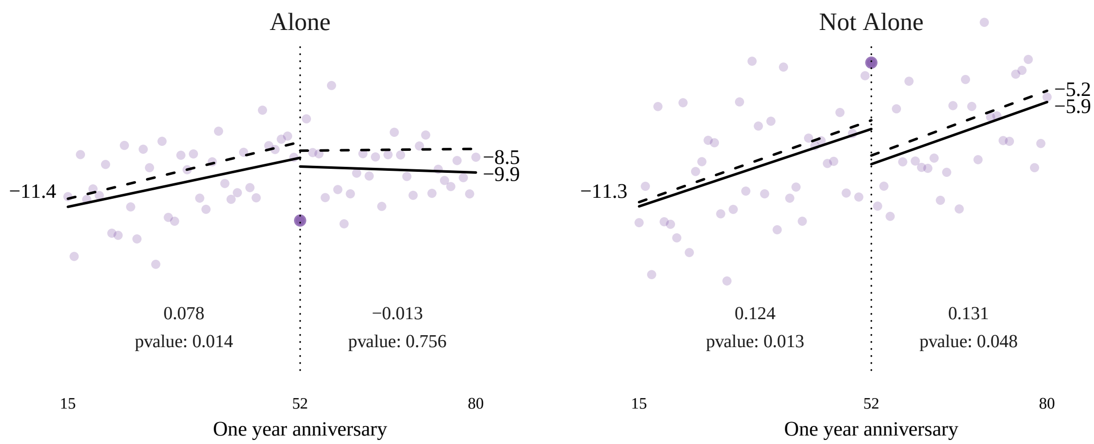

Figure 1.4: *"Recovery" by number of registered voters in household.* This figure shows widows' and widowers' return to previous turnout levels in weeks 15 through 80 after the death of a spouse. We draw a discontinuity at week 52 to account for anniversary effects and highlight turnout during the week of the anniversary. The solid line is a linear regression on observed changes in turnout rates and the dotted lines are the changes in turnout rates possibly unobserved due to movement out of California.

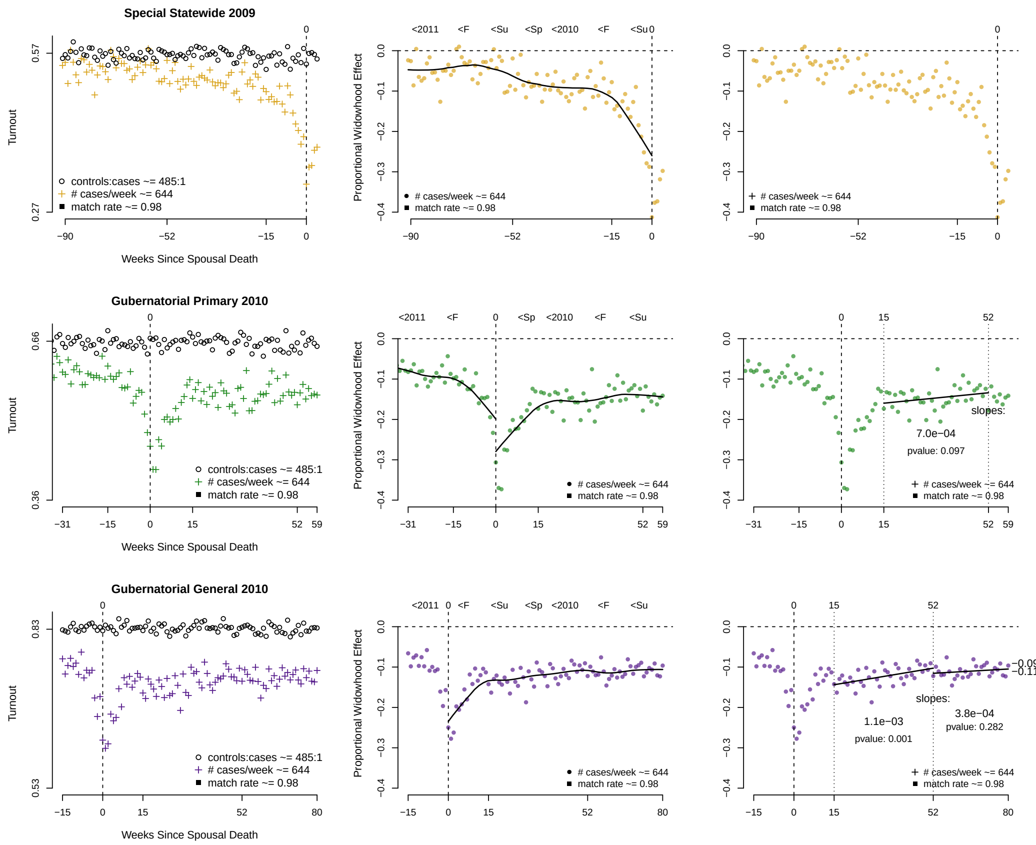

Figure 1.5: *Overall widowhood effects, aligned by cohort.* This figure shows the turnout rates for 60,856 widows and widowers and over 5 million matched controls, aligned by the calendar week of spousal death. The weeks since spousal death are noted on the bottom axis and the chronological season on the top axis. The top row shows turnout for the Special Statewide 2009, the middle the Gubernatorial Primary 2010, and the bottom the Gubernatorial General 2010. The left column of plots shows the unadjusted turnout rates for cases and controls, while the middle and right columns show the average treatment effects divided by the turnout rates of the controls (for a proportional "widowhood effect").

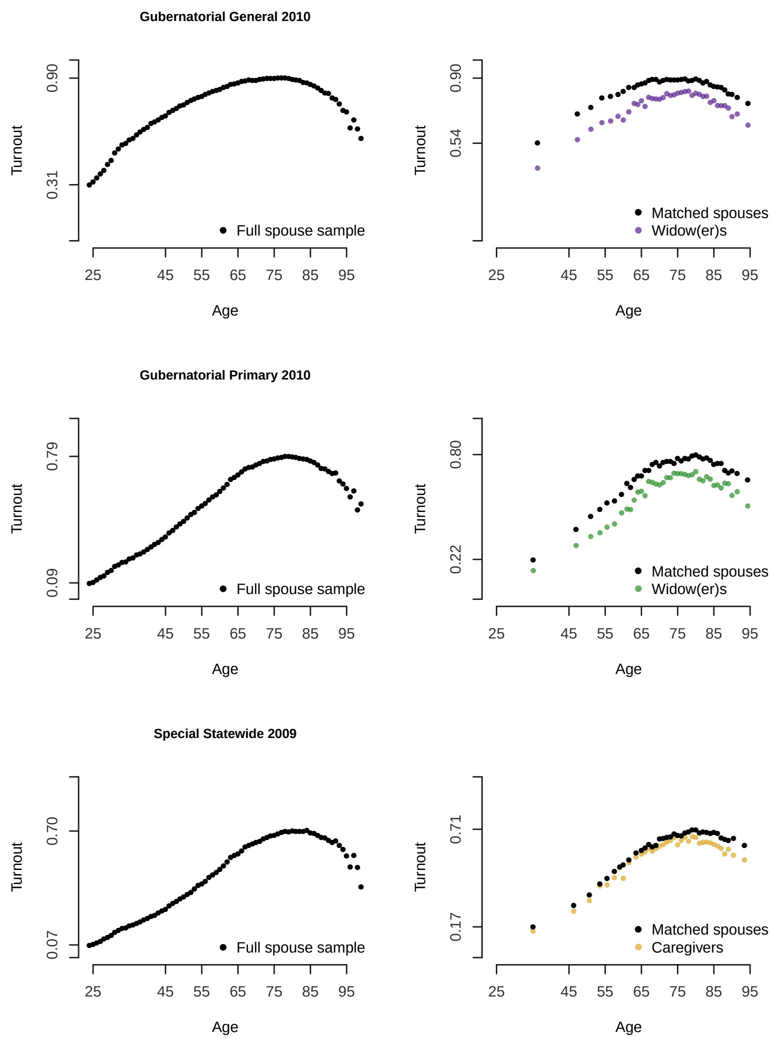

Figure 1.6: In-sample turnout, by age (identical voting histories only)

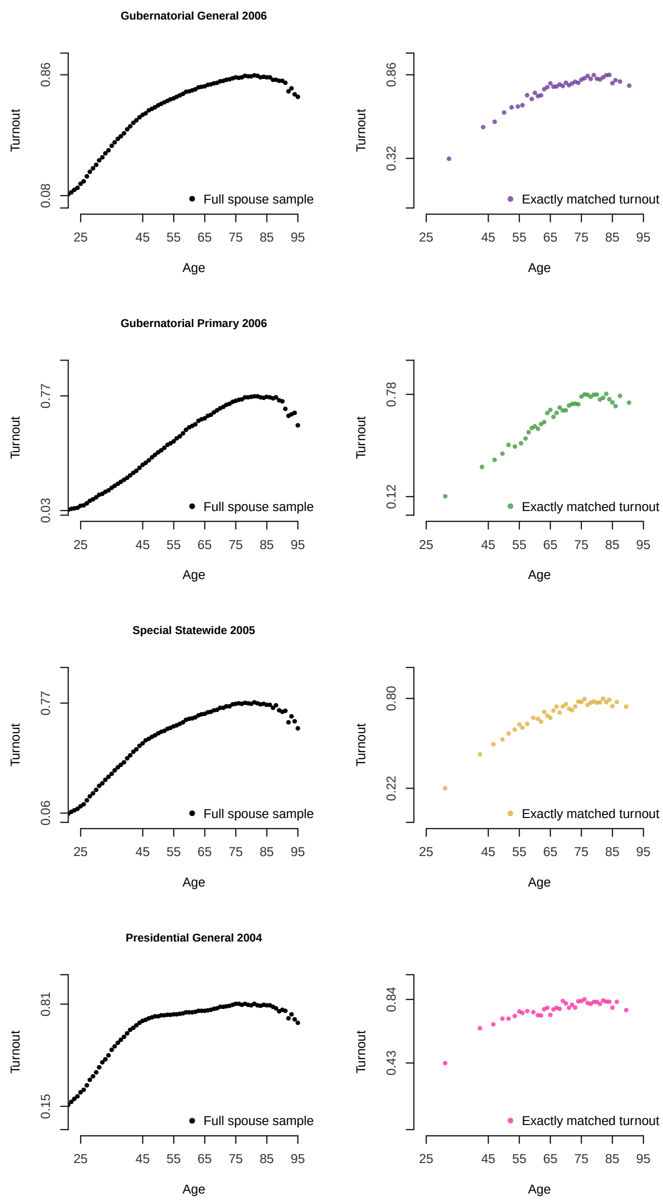

Figure 1.7: In-sample turnout in matched elections, by age (identical voting histories only)

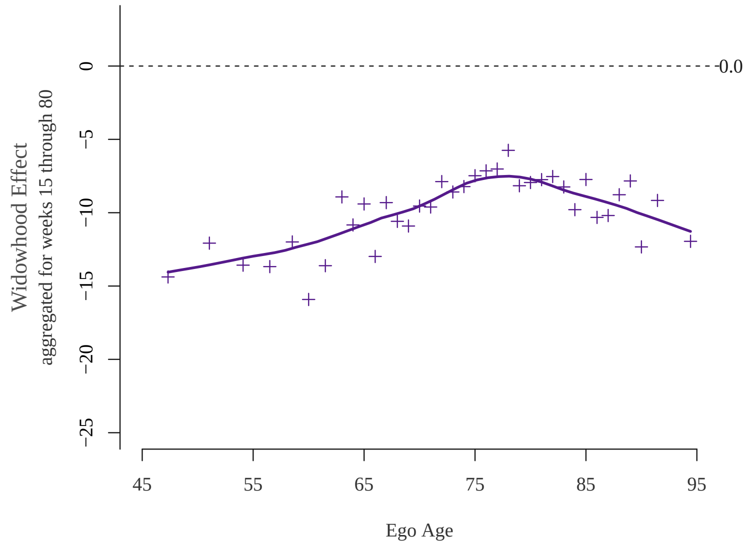

Figure 1.8: *Widowhood effects, by age.* This figure shows the loess smooth (span = .4) of the average treatment effect on the treated by age in the 2010 California Gubernatorial General election. It shows subjects whose spouses possessed identical voting histories. Cohorts include widows and widowers who lost a spouse 15 to 80 weeks prior to the election.

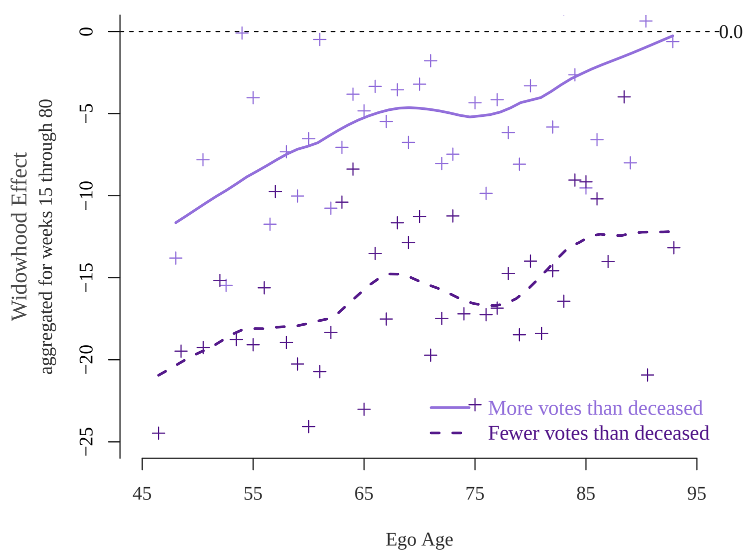

Figure 1.9: *Voting history comparison, by age.* This figure shows the loess smooth (span = .4) of the average treatment effect on the treated by age in the 2010 California Gubernatorial General election. It separates subjects whose deceased spouses vote more and those whose spouses voted less to show that past voting history is not a proxy for health. Cohorts include widows and widowers who lost a spouse 15 to 80 weeks prior to the election.

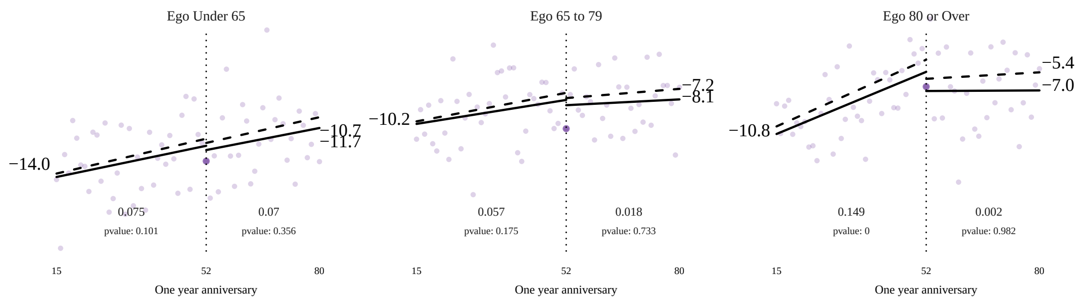

Figure 1.10: Recovery, by age

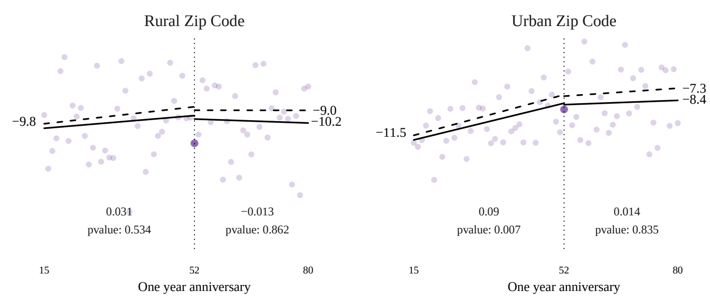

Figure 1.11: Recovery, by population density

Figure 1.12: Recovery, by per capita income in zip code

Table 1.2: Aggregated widowhood effects with 95% confidence intervals, sample sizes, and match rates

All Registered Voters						
Special Statewide 2009	-0.026 (-4.5%)	-0.049 (-8.7%)	-0.098 (-17.6%)			
	(-0.023, -0.029)	(-0.046, -0.052)	(-0.093, -0.104)			
	n = 26150, m = 0.98	n = 23484, m = 0.98	n = 8530, m = 0.98			
Gubernatorial Primary 2010			-0.089 (-13.6%)	-0.149 (-22.6%)	-0.096 (-14.6%)	
			(-0.084, -0.094)	(-0.144, -0.154)	(-0.092, -0.099)	
			n = 9402, m = 0.99	n = 9763, m = 0.98	n = 22459, m = 0.98	
Gubernatorial General 2010			-0.091 (-11%)	-0.14 (-16.7%)	-0.103 (-12.3%)	-0.091 (-11%)
			(-0.088, -0.095)	(-0.136, -0.143)	(-0.1, -0.105)	(-0.088, -0.094)
			n = 10779, m = 0.98	n = 9513, m = 0.99	n = 23820, m = 0.98	n = 16223, m = 0.98
Female Subject						
Special Statewide 2009	-0.03 (-5.3%)	-0.055 (-9.7%)	-0.098 (-17.3%)			
	(-0.026, -0.035)	(-0.05, -0.059)	(-0.09, -0.105)			
	n = 12468, m = 0.99	n = 11174, m = 0.99	n = 4036, m = 0.99			
Gubernatorial Primary 2010			-0.101 (-15.5%)	-0.153 (-23.3%)	-0.099 (-15.2%)	
			(-0.094, -0.108)	(-0.146, -0.16)	(-0.095, -0.104)	
			n = 4603, m = 0.99	n = 4615, m = 0.99	n = 10660, m = 0.99	
Gubernatorial General 2010			-0.101 (-12.1%)	-0.151 (-18.1%)	-0.103 (-12.4%)	-0.088 (-10.6%)
			(-0.096, -0.106)	(-0.146, -0.157)	(-0.1, -0.107)	(-0.084, -0.092)
			n = 5109, m = 0.99	n = 4522, m = 0.99	n = 11425, m = 0.99	n = 7660, m = 0.99
Male Subject						
Special Statewide 2009	-0.02 (-3.5%)	-0.036 (-6.3%)	-0.088 (-15.7%)			
	(-0.014, -0.026)	(-0.029, -0.043)	(-0.077, -0.1)			
	n = 6172, m = 0.98	n = 5561, m = 0.98	n = 1924, m = 0.98			

Table 1.2: Aggregated widowhood effects with 95% confidence intervals, sample sizes, and match rates, continued

	weeks -91 to 52	weeks -52 to -15	weeks -15 to 0	weeks 0 to 15	weeks 15 to 52	weeks 52 to 80
Gubernatorial Primary 2010			-0.079 (-11.8%)	-0.127 (-19.1%)	-0.09 (-13.5%)	
			(-0.068, -0.089)	(-0.117, -0.137)	(-0.083, -0.096)	
			n = 2161, m = 0.98	n = 2275, m = 0.98	n = 5308, m = 0.98	
Gubernatorial General 2010			-0.079 (-9.5%)	-0.106 (-12.6%)	-0.091 (-10.9%)	-0.087 (-10.5%)
			(-0.072, -0.087)	(-0.098, -0.114)	(-0.087, -0.096)	(-0.081, -0.093)
			n = 2552, m = 0.98	n = 2256, m = 0.99	n = 5622, m = 0.98	n = 3731, m = 0.98
Subject Under 65						
Special Statewide 2009	-0.016 (-4.2%)	-0.034 (-8.9%)	-0.074 (-19.3%)			
	(-0.01, -0.022)	(-0.028, -0.04)	(-0.064, -0.084)			
	n = 6766, m = 1	n = 6097, m = 0.99	n = 2394, m = 1			
Gubernatorial Primary 2010			-0.071 (-14.8%)	-0.135 (-28.2%)	-0.104 (-21.9%)	
			(-0.061, -0.081)	(-0.125, -0.145)	(-0.098, -0.111)	
			n = 2506, m = 0.99	n = 2493, m = 1	n = 5997, m = 0.99	
Gubernatorial General 2010			-0.087 (-11.5%)	-0.183 (-23.6%)	-0.137 (-17.9%)	-0.117 (-15.2%)
			(-0.079, -0.096)	(-0.174, -0.191)	(-0.132, -0.143)	(-0.11, -0.123)
			n = 2704, m = 1	n = 2528, m = 0.99	n = 6156, m = 0.99	n = 4422, m = 1
Subject 65 to 79						
Special Statewide 2009	-0.021 (-3.4%)	-0.041 (-6.6%)	-0.095 (-15.5%)			
	(-0.016, -0.026)	(-0.035, -0.046)	(-0.087, -0.104)			
	n = 10481, m = 0.99	n = 9321, m = 0.99	n = 3369, m = 0.99			
Gubernatorial Primary 2010			-0.092 (-12.8%)	-0.155 (-21.5%)	-0.089 (-12.3%)	
			(-0.085, -0.1)	(-0.148, -0.162)	(-0.084, -0.094)	
			n = 3819, m = 0.99	n = 3853, m = 0.99	n = 8943, m = 0.99	
Gubernatorial General 2010			-0.083 (-9.5%)	-0.116 (-13.2%)	-0.094 (-10.7%)	-0.085 (-9.7%)
			(-0.078, -0.088)	(-0.11, -0.121)	(-0.09, -0.097)	(-0.08, -0.089)

Table 1.2: Aggregated widowhood effects with 95% confidence intervals, sample sizes, and match rates, continued

	weeks -91 to 52	weeks -52 to -15	weeks -15 to 0	weeks 0 to 15	weeks 15 to 52	weeks 52 to 80
			n = 4323, m = 0.99	n = 3795, m = 0.99	n = 9475, m = 0.99	n = 6445, m = 0.99
Subject 80 or Over						
Special Statewide 2009	-0.038 (-5.8%)	-0.07 (-10.7%)	-0.125 (-19.3%)			
	(-0.033, -0.043)	(-0.065, -0.075)	(-0.116, -0.134)			
	n = 8803, m = 0.96	n = 7958, m = 0.96	n = 2738, m = 0.95			
Gubernatorial Primary 2010			-0.1 (-13.9%)	-0.154 (-21.4%)	-0.097 (-13.4%)	
			(-0.092, -0.108)	(-0.146, -0.162)	(-0.092, -0.102)	
			n = 3043, m = 0.96	n = 3368, m = 0.96	n = 7434, m = 0.96	
Gubernatorial General 2010			-0.105 (-12.5%)	-0.133 (-15.9%)	-0.087 (-10.4%)	-0.076 (-9.2%)
			(-0.098, -0.111)	(-0.126, -0.14)	(-0.082, -0.091)	(-0.071, -0.082)
			n = 3705, m = 0.96	n = 3159, m = 0.97	n = 8080, m = 0.96	n = 5299, m = 0.96
Female Subject Under 65						
Special Statewide 2009	-0.019 (-5%)	-0.032 (-8.4%)	-0.075 (-19.3%)			
	(-0.011, -0.028)	(-0.023, -0.041)	(-0.061, -0.09)			
	n = 3284, m = 1	n = 2940, m = 1	n = 1119, m = 1			
Gubernatorial Primary 2010			-0.074 (-15.2%)	-0.143 (-29.8%)	-0.112 (-23.5%)	
			(-0.06, -0.088)	(-0.129, -0.157)	(-0.103, -0.121)	
			n = 1247, m = 1	n = 1233, m = 1	n = 2837, m = 1	
Gubernatorial General 2010			-0.091 (-11.7%)	-0.195 (-24.8%)	-0.141 (-18.2%)	-0.117 (-15%)
			(-0.079, -0.103)	(-0.183, -0.206)	(-0.134, -0.149)	(-0.108, -0.126)
			n = 1264, m = 1	n = 1243, m = 1	n = 3022, m = 1	n = 2087, m = 1
Male Subject Under 65						
Special Statewide 2009	-0.002 (-0.6%)	-0.02 (-5.2%)	-0.06 (-16.5%)			
	(0.01, -0.014)	(-0.007, -0.032)	(-0.04, -0.081)			
	n = 1608, m = 0.99	n = 1485, m = 0.99	n = 549, m = 1			

Table 1.2: Aggregated widowhood effects with 95% confidence intervals, sample sizes, and match rates, continued

	weeks -91 to 52	weeks -52 to -15	weeks -15 to 0	weeks 0 to 15	weeks 15 to 52	weeks 52 to 80
Gubernatorial Primary 2010			-0.078 (-16.6%)	-0.103 (-21.4%)	-0.084 (-17.9%)	
			(-0.057, -0.099)	(-0.083, -0.124)	(-0.071, -0.097)	
			n = 572, m = 0.99	n = 597, m = 1	n = 1437, m = 0.99	
Gubernatorial General 2010			-0.062 (-8.3%)	-0.136 (-17.9%)	-0.115 (-15%)	-0.094 (-12.5%)
			(-0.045, -0.079)	(-0.119, -0.154)	(-0.104, -0.126)	(-0.08, -0.107)
			n = 658, m = 0.99	n = 598, m = 0.99	n = 1483, m = 0.99	n = 1033, m = 1
Female Subject 65 to 79						
Special Statewide 2009	-0.024 (-3.9%)	-0.05 (-8%)	-0.088 (-14.2%)			
	(-0.017, -0.031)	(-0.042, -0.057)	(-0.076, -0.1)			
	n = 5211, m = 0.99	n = 4476, m = 0.99	n = 1648, m = 0.99			
Gubernatorial Primary 2010			-0.11 (-15.1%)	-0.161 (-22%)	-0.09 (-12.4%)	
			(-0.099, -0.12)	(-0.15, -0.171)	(-0.083, -0.097)	
			n = 1945, m = 0.99	n = 1804, m = 0.99	n = 4337, m = 0.99	
Gubernatorial General 2010			-0.086 (-9.8%)	-0.132 (-15.1%)	-0.093 (-10.6%)	-0.085 (-9.6%)
			(-0.079, -0.093)	(-0.124, -0.139)	(-0.089, -0.098)	(-0.079, -0.091)
			n = 2148, m = 0.99	n = 1892, m = 0.99	n = 4596, m = 0.99	n = 3113, m = 0.99
Male Subject 65 to 79						
Special Statewide 2009	-0.022 (-3.6%)	-0.034 (-5.3%)	-0.085 (-13.7%)			
	(-0.012, -0.033)	(-0.023, -0.044)	(-0.066, -0.103)			
	n = 2206, m = 0.98	n = 2034, m = 0.99	n = 697, m = 0.98			
Gubernatorial Primary 2010			-0.072 (-10.1%)	-0.147 (-20.4%)	-0.092 (-12.6%)	
			(-0.056, -0.088)	(-0.132, -0.163)	(-0.082, -0.102)	
			n = 787, m = 0.99	n = 845, m = 0.99	n = 1942, m = 0.98	
Gubernatorial General 2010			-0.092 (-10.4%)	-0.091 (-10.3%)	-0.095 (-10.7%)	-0.082 (-9.3%)
			(-0.081, -0.103)	(-0.079, -0.102)	(-0.087, -0.102)	(-0.073, -0.09)

Table 1.2: Aggregated widowhood effects with 95% confidence intervals, sample sizes, and match rates, continued

	weeks -91 to 52	weeks -52 to -15	weeks -15 to 0	weeks 0 to 15	weeks 15 to 52	weeks 52 to 80
			n = 918, m = 0.98	n = 788, m = 0.99	n = 2065, m = 0.99	n = 1362, m = 0.98
Female Subject 80 or Over						
Special Statewide 2009	-0.049 (-7.4%)	-0.079 (-12.2%)	-0.134 (-20.7%)			
	(-0.041, -0.056)	(-0.071, -0.087)	(-0.121, -0.148)			
	n = 3932, m = 0.97	n = 3706, m = 0.96	n = 1255, m = 0.96			
Gubernatorial Primary 2010			-0.113 (-16%)	-0.153 (-21.6%)	-0.103 (-14.4%)	
			(-0.1, -0.125)	(-0.141, -0.165)	(-0.095, -0.111)	
			n = 1399, m = 0.97	n = 1551, m = 0.96	n = 3447, m = 0.96	
Gubernatorial General 2010			-0.129 (-15.4%)	-0.138 (-16.7%)	-0.084 (-10.2%)	-0.067 (-8.2%)
			(-0.12, -0.139)	(-0.128, -0.149)	(-0.077, -0.09)	(-0.059, -0.075)
			n = 1677, m = 0.96	n = 1373, m = 0.97	n = 3757, m = 0.96	n = 2432, m = 0.96
Male Subject 80 or Over						
Special Statewide 2009	-0.029 (-4.4%)	-0.049 (-7.4%)	-0.113 (-16.8%)			
	(-0.019, -0.039)	(-0.039, -0.06)	(-0.095, -0.131)			
	n = 2334, m = 0.96	n = 2019, m = 0.96	n = 669, m = 0.95			
Gubernatorial Primary 2010			-0.084 (-11.1%)	-0.127 (-17.1%)	-0.09 (-12.2%)	
			(-0.068, -0.099)	(-0.111, -0.142)	(-0.08, -0.101)	
			n = 795, m = 0.97	n = 825, m = 0.96	n = 1908, m = 0.96	
Gubernatorial General 2010			-0.082 (-9.7%)	-0.097 (-11.4%)	-0.074 (-8.7%)	-0.087 (-10.2%)
			(-0.07, -0.093)	(-0.085, -0.109)	(-0.066, -0.082)	(-0.077, -0.097)
			n = 964, m = 0.95	n = 863, m = 0.98	n = 2051, m = 0.96	n = 1323, m = 0.96
Female Subject - Opposite Sex Only						
Special Statewide 2009	-0.031 (-5.5%)	-0.051 (-9.1%)	-0.103 (-18%)			
	(-0.026, -0.036)	(-0.046, -0.056)	(-0.094, -0.111)			
	n = 10444, m = 0.99	n = 9382, m = 0.99	n = 3374, m = 0.99			

Table 1.2: Aggregated widowhood effects with 95% confidence intervals, sample sizes, and match rates, continued

	weeks -91 to 52	weeks -52 to -15	weeks -15 to 0	weeks 0 to 15	weeks 15 to 52	weeks 52 to 80
Gubernatorial Primary 2010			-0.101 (-15.5%)	-0.156 (-23.7%)	-0.104 (-15.8%)	
			(-0.094, -0.109)	(-0.148, -0.163)	(-0.098, -0.109)	
			n = 3852, m = 0.99	n = 3820, m = 0.99	n = 8979, m = 0.99	
Gubernatorial General 2010			-0.103 (-12.3%)	-0.153 (-18.2%)	-0.105 (-12.6%)	-0.088 (-10.5%)
			(-0.097, -0.109)	(-0.146, -0.159)	(-0.101, -0.109)	(-0.083, -0.093)
			n = 4289, m = 0.99	n = 3780, m = 0.99	n = 9543, m = 0.99	n = 6451, m = 0.99
Male Subject - Opposite Sex Only						
Special Statewide 2009	-0.021 (-3.7%)	-0.032 (-5.5%)	-0.095 (-16.2%)			
	(-0.015, -0.028)	(-0.025, -0.04)	(-0.083, -0.107)			
	n = 5245, m = 0.99	n = 4741, m = 0.99	n = 1622, m = 0.99			
Gubernatorial Primary 2010			-0.079 (-11.6%)	-0.137 (-20.2%)	-0.091 (-13.4%)	
			(-0.068, -0.09)	(-0.126, -0.148)	(-0.084, -0.098)	
			n = 1832, m = 0.99	n = 1944, m = 0.99	n = 4494, m = 0.99	
Gubernatorial General 2010			-0.081 (-9.6%)	-0.102 (-12%)	-0.096 (-11.2%)	-0.09 (-10.7%)
			(-0.073, -0.088)	(-0.094, -0.11)	(-0.09, -0.101)	(-0.084, -0.097)
			n = 2176, m = 0.99	n = 1917, m = 1	n = 4795, m = 0.99	n = 3140, m = 0.99
More Votes than Spouse						
Special Statewide 2009	-0.009 (-1.8%)	-0.026 (-5.3%)	-0.053 (-10.8%)			
	(-0.001, -0.017)	(-0.018, -0.035)	(-0.039, -0.067)			
	n = 3735, m = 0.92	n = 3472, m = 0.92	n = 1269, m = 0.93			
Gubernatorial Primary 2010			-0.076 (-12.3%)	-0.091 (-15%)	-0.04 (-6.6%)	
			(-0.063, -0.089)	(-0.078, -0.104)	(-0.032, -0.049)	
			n = 1334, m = 0.93	n = 1397, m = 0.92	n = 3376, m = 0.93	
Gubernatorial General 2010			-0.07 (-8.3%)	-0.09 (-10.8%)	-0.057 (-6.8%)	-0.052 (-6.2%)
			(-0.06, -0.079)	(-0.08, -0.101)	(-0.05, -0.063)	(-0.044, -0.059)

Table 1.2: Aggregated widowhood effects with 95% confidence intervals, sample sizes, and match rates, continued

	weeks -91 to 52	weeks -52 to -15	weeks -15 to 0	weeks 0 to 15	weeks 15 to 52	weeks 52 to 80
			n = 1539, m = 0.93	n = 1309, m = 0.93	n = 3517, m = 0.92	n = 2406, m = 0.92
Fewer Votes than Spouse						
Special Statewide 2009	-0.024 (-6.3%)	-0.042 (-11%)	-0.102 (-27.2%)			
	(-0.015, -0.033)	(-0.032, -0.051)	(-0.087, -0.118)			
	n = 3088, m = 0.92	n = 2726, m = 0.93	n = 998, m = 0.91			
Gubernatorial Primary 2010			-0.091 (-19.8%)	-0.171 (-36.1%)	-0.122 (-26.6%)	
			(-0.075, -0.106)	(-0.156, -0.186)	(-0.112, -0.131)	
			n = 1091, m = 0.91	n = 1113, m = 0.92	n = 2657, m = 0.92	
Gubernatorial General 2010			-0.123 (-17.2%)	-0.209 (-28.8%)	-0.167 (-23.2%)	-0.145 (-20.3%)
			(-0.11, -0.136)	(-0.196, -0.223)	(-0.158, -0.175)	(-0.135, -0.156)
			n = 1283, m = 0.92	n = 1105, m = 0.92	n = 2764, m = 0.93	n = 1870, m = 0.92
Same Voting History						
Special Statewide 2009	-0.026 (-4.2%)	-0.051 (-8.2%)	-0.106 (-17.3%)			
	(-0.022, -0.03)	(-0.047, -0.054)	(-0.099, -0.112)			
	n = 18595, m = 0.98	n = 16650, m = 0.98	n = 6009, m = 0.98			
Gubernatorial Primary 2010			-0.089 (-12.8%)	-0.156 (-22.2%)	-0.101 (-14.4%)	
			(-0.084, -0.095)	(-0.151, -0.162)	(-0.098, -0.105)	
			n = 6700, m = 0.98	n = 6978, m = 0.98	n = 15815, m = 0.98	
Gubernatorial General 2010			-0.087 (-10.3%)	-0.138 (-16.1%)	-0.099 (-11.6%)	-0.091 (-10.7%)
			(-0.083, -0.091)	(-0.133, -0.142)	(-0.096, -0.101)	(-0.088, -0.094)
			n = 7677, m = 0.98	n = 6820, m = 0.98	n = 16880, m = 0.98	n = 11473, m = 0.98
Same Voting History - No Abstains						
Special Statewide 2009	-0.03 (-3.8%)	-0.055 (-7.1%)	-0.117 (-15.1%)			
	(-0.025, -0.034)	(-0.051, -0.06)	(-0.109, -0.124)			
	n = 9436, m = 1	n = 8562, m = 1	n = 3106, m = 1			

Table 1.2: Aggregated widowhood effects with 95% confidence intervals, sample sizes, and match rates, continued

	weeks -91 to 52	weeks -52 to -15	weeks -15 to 0	weeks 0 to 15	weeks 15 to 52	weeks 52 to 80
Gubernatorial Primary 2010			-0.091 (-10.4%)	-0.166 (-19%)	-0.115 (-13.1%)	
			(-0.085, -0.097)	(-0.16, -0.171)	(-0.111, -0.118)	
			n = 3356, m = 1	n = 3559, m = 1	n = 8194, m = 1	
Gubernatorial General 2010			-0.069 (-7.3%)	-0.119 (-12.5%)	-0.079 (-8.4%)	-0.074 (-7.8%)
			(-0.065, -0.072)	(-0.115, -0.122)	(-0.077, -0.082)	(-0.071, -0.077)
			n = 3945, m = 1	n = 3440, m = 1	n = 8610, m = 1	n = 5935, m = 1
Same Voting History - One Abstain						
Special Statewide 2009	-0.022 (-3.8%)	-0.049 (-8.5%)	-0.116 (-20.2%)			
	(-0.013, -0.03)	(-0.04, -0.058)	(-0.101, -0.131)			
	n = 3431, m = 0.97	n = 2953, m = 0.97	n = 1083, m = 0.97			
Gubernatorial Primary 2010			-0.116 (-17.1%)	-0.177 (-26.2%)	-0.112 (-16.7%)	
			(-0.102, -0.129)	(-0.163, -0.19)	(-0.103, -0.121)	
			n = 1250, m = 0.98	n = 1251, m = 0.97	n = 2812, m = 0.97	
Gubernatorial General 2010			-0.103 (-11.7%)	-0.169 (-19.1%)	-0.117 (-13.3%)	-0.108 (-12.3%)
			(-0.094, -0.112)	(-0.161, -0.178)	(-0.111, -0.123)	(-0.101, -0.116)
			n = 1366, m = 0.97	n = 1284, m = 0.98	n = 3083, m = 0.97	n = 2037, m = 0.97
Same Voting History - Two Abstains						
Special Statewide 2009	-0.028 (-5.5%)	-0.043 (-8.7%)	-0.11 (-23%)			
	(-0.017, -0.038)	(-0.032, -0.054)	(-0.091, -0.129)			
	n = 2244, m = 0.95	n = 2047, m = 0.95	n = 714, m = 0.95			
Gubernatorial Primary 2010			-0.093 (-16.2%)	-0.161 (-28.7%)	-0.077 (-13.7%)	
			(-0.076, -0.11)	(-0.144, -0.178)	(-0.066, -0.089)	
			n = 834, m = 0.95	n = 854, m = 0.95	n = 1895, m = 0.95	
Gubernatorial General 2010			-0.113 (-13.8%)	-0.166 (-20.2%)	-0.105 (-13%)	-0.128 (-15.6%)
			(-0.1, -0.125)	(-0.153, -0.179)	(-0.097, -0.114)	(-0.118, -0.139)

Table 1.2: Aggregated widowhood effects with 95% confidence intervals, sample sizes, and match rates, continued

	weeks -91 to 52	weeks -52 to -15	weeks -15 to 0	weeks 0 to 15	weeks 15 to 52	weeks 52 to 80
			n = 925, m = 0.95	n = 833, m = 0.95	n = 2074, m = 0.95	n = 1355, m = 0.95
Same Voting History - Three Abstains						
Special Statewide 2009	0.002 (0.7%)	-0.021 (-9.1%)	-0.036 (-16.5%)			
	(0.013, -0.01)	(-0.009, -0.034)	(-0.016, -0.056)			
	n = 1383, m = 0.95	n = 1215, m = 0.94	n = 424, m = 0.95			
Gubernatorial Primary 2010			-0.03 (-10.7%)	-0.075 (-25.2%)	-0.055 (-19.1%)	
			(-0.01, -0.051)	(-0.054, -0.095)	(-0.042, -0.069)	
			n = 486, m = 0.94	n = 504, m = 0.94	n = 1149, m = 0.95	
Gubernatorial General 2010			-0.108 (-18%)	-0.15 (-24.5%)	-0.113 (-19.1%)	-0.072 (-12.3%)
			(-0.088, -0.128)	(-0.128, -0.171)	(-0.098, -0.127)	(-0.055, -0.089)
			n = 595, m = 0.95	n = 504, m = 0.95	n = 1207, m = 0.94	n = 836, m = 0.94
Alone						
Special Statewide 2009	-0.03 (-4.8%)	-0.053 (-8.5%)	-0.11 (-18.2%)			
	(-0.026, -0.033)	(-0.049, -0.057)	(-0.104, -0.117)			
	n = 18656, m = 0.99	n = 16781, m = 0.99	n = 6094, m = 0.99			
Gubernatorial Primary 2010			-0.094 (-13.5%)	-0.16 (-22.8%)	-0.1 (-14.3%)	
			(-0.089, -0.1)	(-0.155, -0.166)	(-0.097, -0.104)	
			n = 6687, m = 0.99	n = 7003, m = 0.98	n = 16017, m = 0.99	
Gubernatorial General 2010			-0.098 (-11.4%)	-0.141 (-16.5%)	-0.105 (-12.3%)	-0.097 (-11.3%)
			(-0.094, -0.102)	(-0.137, -0.145)	(-0.102, -0.108)	(-0.093, -0.1)
			n = 7621, m = 0.99	n = 6839, m = 0.99	n = 17055, m = 0.99	n = 11538, m = 0.99
Not Alone						
Special Statewide 2009	-0.016 (-3.5%)	-0.04 (-9.1%)	-0.068 (-15.5%)			
	(-0.01, -0.022)	(-0.034, -0.047)	(-0.058, -0.078)			
	n = 7494, m = 0.98	n = 6703, m = 0.98	n = 2436, m = 0.98			

Table 1.2: Aggregated widowhood effects with 95% confidence intervals, sample sizes, and match rates, continued

	weeks -91 to 52	weeks -52 to -15	weeks -15 to 0	weeks 0 to 15	weeks 15 to 52	weeks 52 to 80
Gubernatorial Primary 2010			-0.077 (-14.2%)	-0.12 (-22%)	-0.084 (-15.5%)	
			(-0.068, -0.087)	(-0.111, -0.13)	(-0.077, -0.09)	
			n = 2715, m = 0.98	n = 2760, m = 0.98	n = 6442, m = 0.98	
Gubernatorial General 2010			-0.076 (-9.8%)	-0.136 (-17.5%)	-0.096 (-12.4%)	-0.078 (-10.1%)
			(-0.069, -0.084)	(-0.128, -0.144)	(-0.091, -0.101)	(-0.072, -0.084)
			n = 3158, m = 0.97	n = 2674, m = 0.98	n = 6765, m = 0.98	n = 4685, m = 0.98
Rural Zip Code						
Special Statewide 2009	-0.033 (-5.3%)	-0.06 (-9.6%)	-0.115 (-18.7%)			
	(-0.028, -0.038)	(-0.054, -0.065)	(-0.106, -0.123)			
	n = 9159, m = 0.96	n = 8296, m = 0.96	n = 3003, m = 0.97			
Gubernatorial Primary 2010			-0.084 (-11.9%)	-0.16 (-22.7%)	-0.107 (-15.2%)	
			(-0.076, -0.092)	(-0.152, -0.167)	(-0.102, -0.112)	
			n = 3316, m = 0.97	n = 3514, m = 0.97	n = 7890, m = 0.96	
Gubernatorial General 2010			-0.091 (-10.6%)	-0.139 (-16.1%)	-0.111 (-12.9%)	-0.094 (-11%)
			(-0.085, -0.097)	(-0.134, -0.145)	(-0.107, -0.115)	(-0.089, -0.099)
			n = 3743, m = 0.96	n = 3339, m = 0.97	n = 8457, m = 0.96	n = 5720, m = 0.97
Urban Zip Code						
Special Statewide 2009	-0.025 (-4.6%)	-0.043 (-7.9%)	-0.089 (-16.8%)			
	(-0.021, -0.029)	(-0.039, -0.047)	(-0.082, -0.096)			
	n = 16085, m = 0.98	n = 14352, m = 0.98	n = 5254, m = 0.98			
Gubernatorial Primary 2010			-0.094 (-14.9%)	-0.144 (-22.6%)	-0.089 (-14.1%)	
			(-0.088, -0.1)	(-0.138, -0.15)	(-0.085, -0.093)	
			n = 5781, m = 0.98	n = 5933, m = 0.98	n = 13775, m = 0.98	
Gubernatorial General 2010			-0.093 (-11.4%)	-0.143 (-17.4%)	-0.098 (-12%)	-0.088 (-10.8%)
			(-0.088, -0.098)	(-0.138, -0.148)	(-0.095, -0.102)	(-0.084, -0.092)

Table 1.2: Aggregated widowhood effects with 95% confidence intervals, sample sizes, and match rates, continued

	weeks -91 to 52	weeks -52 to -15	weeks -15 to 0	weeks 0 to 15	weeks 15 to 52	weeks 52 to 80
			n = 6644, m = 0.97	n = 5856, m = 0.98	n = 14541, m = 0.98	n = 9960, m = 0.98
Per Capita Income Under 35000						
Special Statewide 2009	-0.025 (-4.5%)	-0.05 (-8.9%)	-0.103 (-18.6%)			
	(-0.021, -0.029)	(-0.046, -0.054)	(-0.096, -0.11)			
	n = 16945, m = 0.98	n = 15242, m = 0.98	n = 5554, m = 0.98			
Gubernatorial Primary 2010			-0.084 (-13.2%)	-0.153 (-24.1%)	-0.097 (-15.4%)	
			(-0.078, -0.09)	(-0.147, -0.159)	(-0.093, -0.101)	
			n = 6117, m = 0.98	n = 6296, m = 0.98	n = 14642, m = 0.98	
Gubernatorial General 2010			-0.09 (-11.1%)	-0.143 (-17.4%)	-0.106 (-13%)	-0.097 (-11.9%)
			(-0.086, -0.095)	(-0.138, -0.148)	(-0.103, -0.109)	(-0.093, -0.1)
			n = 6968, m = 0.97	n = 6171, m = 0.98	n = 15467, m = 0.98	n = 10511, m = 0.98
Per Capita Income 35000 or Over						
Special Statewide 2009	-0.032 (-5.4%)	-0.05 (-8.4%)	-0.093 (-16.1%)			
	(-0.026, -0.037)	(-0.044, -0.056)	(-0.084, -0.103)			
	n = 8346, m = 0.97	n = 7390, m = 0.96	n = 2706, m = 0.96			
Gubernatorial Primary 2010			-0.097 (-13.8%)	-0.14 (-19.7%)	-0.092 (-13%)	
			(-0.089, -0.106)	(-0.132, -0.149)	(-0.087, -0.097)	
			n = 2988, m = 0.97	n = 3130, m = 0.96	n = 7028, m = 0.96	
Gubernatorial General 2010			-0.093 (-10.7%)	-0.135 (-15.6%)	-0.095 (-11%)	-0.077 (-8.9%)
			(-0.087, -0.099)	(-0.129, -0.141)	(-0.092, -0.099)	(-0.073, -0.082)
			n = 3452, m = 0.97	n = 3024, m = 0.96	n = 7516, m = 0.96	n = 5169, m = 0.96

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Chapter 2

Partisan Attachment or Life Stability?

Stable partisanship among American voters is conventionally attributed to attachments to political parties that become stronger with age. Here, I argue that American partisanship is substantially driven by affiliations that can quickly be displaced in new social circumstances, and that partisanship is stable because lives do not change very much. Using voter registration information over 8 years for more than 20 million voters in California, in combination with ANES panel surveys, I show that individuals switch party affiliation when they move to new homes, switch when separating from a partner, and become gradually less likely to switch after retirement. Accounting for residential mobility alone reduces the association between age and party-switching by 50 to 70 percent, suggesting that life stability explains much of the age-partisan stability relationship. The combined findings suggest that stable lives, and not necessarily internalized attachments, lead to steady partisanship.

Americans rarely change their partisan identification, and this partisan stability is thought to reflect the nature of partisanship. Perhaps most prominently and productively, researchers characterize partisanship using observations that younger people are more likely to change partisan identification than older people [54, 55, 56, 57, 58, 59, 60, 61, 62, 63].

These associations between age, or length of time holding an affiliation, and partisan stability[1] are interpreted as support for theories of restrained [56], impressionable [59], and crystallized [60] partisanship. Proponents of psychological attachment explanations for partisan stability argue that social learning at young ages, especially absorption of social-psychological associations, develops into strong and unchanging partisanship in adulthood [8, 64, 65]. Others argue that voters gradually develop strong priors about which party will best serve their interests and become resistant to change through rational loyalty [54, 66]. By

[1] Or, in cross-sectional studies, greater partisan strength as a proxy for partisan identification over time.

and large, interpretations of the age-partisan stability relationship share a basis in partisan attachment: accumulation of either social-psychological or rational associations with age leads to a decreased willingness to change partisanship.

I will argue here that associations between age and party-switching are mostly not driven by partisan attachment. Individual-level partisan identification is undoubtedly very stable; however, decreased partisan change with age and long-lasting partisan affiliations should not imply attachments to the affiliations. The older a person is the less likely they are to move (until around age 70), meet new people and be in unfamiliar social circles, marry and separate, and experience sudden changes in their careers. In the aggregate and over long time-spans, the hypothesized relationship between aging and partisan attachment is very similar to the relationship between aging and life changes. In most, if not all, surveys on partisan identification, an age-based partisan attachment explanation is indistinguishable from a social and contextual explanation for partisan stability.

Given this observational equivalence, most of the increase in partisan stability with age that has been attributed to partisan attachment might actually be caused by increased continuity in social and personal circumstances. Individuals might not change party because they have no salient reason to rethink their partisanship, since the American political terrain is stable,[2] and might have few emergent social influences to introduce, inform, and affirm unfamiliar choices. They also might support a stance, not because they themselves support it, but because they respect and follow their close friends who do (and their friendships rarely change). In other words, partisan attachments might be bound to the social relationships that helped create them and new relationships might readily displace them – and we might observe increased partisan stability with age only because older people are less likely to make new friends or be in new circumstances.

I will use recent, longitudinal, and large-N data from statewide voter files in Cali-

[2] Its social group basis if not voters' perception of party performance [67, 64].

fornia, in combination with American National Election Study panel surveys, to evaluate this life stability explanation for partisan stability. Using new computational tools and the statistical power provided by massive data sets, I will test whether stable political affiliations can be explained by stable social relationships and circumstances, rather than internalized attachment among partisans. Before and after life change tests will allow me to assess whether life stability or partisan attachment better explains partisan stability, since a partisan attachment should stabilize partisan identification with or without a new environment. If life changes predict party changes, do so comparably at all ages, and greatly reduce the age association, then life stability accounts for the association between age and partisan stability, and greatly reduces the evidence in favor of partisan attachment.

Many friendships, social relationships, and socioeconomic contexts are life-long or change only gradually, so I focus on estimating the effects of residential mobility, marital separation, and retirement on partisanship. These are important topics in their own rights, but I focus on them here because they are relatively discontinuous, easy to observe, and potentially correspond to changes in both political interests and social contexts. Sudden changes should lead to party changes if life stability sustains party affiliation, and we should observe no change if instead the affiliation is driven by internal, abstract attachment to a party, since partisan symbols and other sources of abstract attachment will not change over a short period.

My results show that individuals change partisan affiliations after changing residence, are more likely to change party the farther they move, and change party after separation from a spouse or roommate. The results further show that individuals are *less* likely to change party after retiring, when life becomes more stable. Consistent with a life stability explanation for age and partisan stability associations, and at the expense of partisan attachment models, partisan switching effects are similar across voters' age range after controlling for sources of life stability.

2.1 Partisan Attachment Explanations for Partisan Stability

The difficulty in identifying large, sudden, and individual-level changes in American partisan affiliation is generally thought to be a symptom of surprisingly prevalent political hardheadedness. How is it that so few Americans claim strong interest in politics or hold opinions consistently on one side of the aisle or the other, yet so many hold so strongly to their chosen political party?

Perhaps the simplest response to this question is the argument that partisans associate political parties with a set of long-standing symbols and social cleavages [8, 64]. The average citizen does not follow politics or hold an informed set of ideologically consistent opinions, but does care very much about their own social group and perceived interests. Socialization into either party typically occurs in childhood and endures through the life-course, since individuals are able to identify which political party is the party of their social group. A partisan change or vote for the other side is a vote against their social group or class, and a voter who does not feel strongly for their side may just as well not cast a vote as consider other options.

Within this framework, partisanship not only heavily drives voter choices (which I will not question here), but also conceivably becomes increasingly self-reinforced with age. Psychologically attached partisans will interpret political information to confirm their affiliations [68], and weakly attached partisans will more strongly hold affiliations if they are led to affirm them [69, 63].[3]

This argument is similar to the claim that partisanship tends toward stability because individuals make up their mind early in life, and develop loyalties to one party or the other with political experience [54, 70, 66]. As individuals grow older, they accumulate

[3] Although, importantly, Dinas [63] finds that votes (as affirmations) do not appear to lead to a lower likelihood of changing party. Votes only affect respondents willingness to state "strong" partisanship.

knowledge about the parties. With this accumulated experience, identification becomes more grounded and, therefore, more stable.

The primary distinction between these theories is their basis in either social psychological and team-based evaluations of parties or rational evaluations of party performances. This is an important distinction because it explains partisanship's relationship to vote choice. However, partisan attachment is shared by both the social-psychological and rational theories. Both of these theories of partisanship suggest that people do not change party because they maintain an abstract set of information about the parties and their relationships to them. This information immunizes partisans to environmental pressures and new political experiences as they age.

2.2 Life Stability Theory

In contrast with the partisan attachment explanation for partisan stability in prevailing theories of partisanship, I will argue that people tend not to change party because stable circumstances in their social surroundings give them little impetus to change party. As an example of this stability, 37% of Americans have never lived outside of their hometowns [71]. This residential stability has increased in the United States in recent years [72].

When social and personal circumstances do change, however, many people might reconsider their partisan identification. Specifically, I argue, we might expect individuals whose lives have changed to 1) have altered political interests, 2) have decreased continuity in the social and personal contexts that informed prior choices and identities, and 3) be exposed to new social influences to introduce, inform, and affirm political choices and identities. This life change argument is similar to the argument that individuals will change their partisanship when the parties themselves change [64, 62]. That is, individuals will change partisanship when the parties change or when their lives change. This change in partisanship might be driven by self-interest or new social influences, but, in contrast with partisan attachment, change is precipitated by discontinuities in personal lives rather than primarily macro-level influences such as party performance or societal change.

Change in politicization might occur even if the partisanship of discussants in a new life stage are the same as the previous one's and even if an individual's ideological disposition does not change. New friends might, for example, more heavily weight a party's stance on government spending than stances abortion or immigration, and, therefore, partisanship in that social context could connote a different social identity. Mason [73] evaluates such a partisanship-issue opinion distinction and argues that partisanship is more susceptible to social influences than issue ideology. We might also expect individuals to reconsider their choices if they are more willing to construct a new identity following a major life change. In experimental settings, temporal landmarks and an induced sense of "new beginnings" lead to behavior change [74]. Similar dynamics might anchor partisan identities, especially if friendships and shared histories restrict individuals' willingness to redefine themselves.

This life stability framework is consistent with several studies of life changes and social contexts on partisanship. Most prominently, researchers have studied social conformity after residential moves [75, 76, 77] and gradual partisan convergence over a marriage [12, 78, 79].[4] These studies differ from the current approach since I also emphasize an *unfamiliar* environment, in addition to a socially divergent environment. Nonetheless, of particular note in many works in this area is the separation between partisan or specific policy opinion change and change in ideological orientation generally.[5] For example, Glaser and Gilens [77] find that Southerners who move to the North change specific, politicized policy positions on race but do not appear to become more racially tolerant. Doherty et al. [85] find that lottery winners tend to change political opinions only on issues that affect them

[4]Other researchers have identified contextual effects not necessarily related to a life change, such as social network induced ambivalence [80], conflict avoidance [81], and gradual conformity in a neighborhood [82].

[5]Recent priming research [83, 84] is a notable exception.

directly and materially (like the estate tax).[6] Also, spousal studies find that longer-married spouses are more likely to share partisan affiliations [78] and that spouses tend to converge in partisan affiliation over time [12]. However, they also find that spouses do not appear to strongly converge across issue opinions, that general ideological correspondence may be more based on similarity prior to the relationship, and that specific issue opinions that do converge tend to be more politicized.

Within this framework, then, partisanship might be relatively susceptible to new influences, compared to latent ideological orientations. Individuals might still think of the political landscape in terms of unchanging social symbols and cleavages; however, given a new social environment and the impetus to change, they might nonetheless adapt their identity and re-consider their relationships to political choices.

A number of scholars have posited a life stability effect on increasing partisan stability over the life-course (see, for example, Stoker and Bass [86])[7], and there is some, limited empirical support for this alternate life stability explanation for the age-partisan stability relationship. Miller and Sears [87] tested a very similar hypothesis for attitudinal persistence – focusing on conflicting demographic environments as proxies for encountering conflicting norms in adulthood rather than, as I will suggest here, *any* environment that is both unfamiliar and that introduces new social influences – and argued that persistence in attitudes appears to be at least partially attributable to environmental continuity. Other scholars have argued that stability might be driven by the continuity of political experience, especially in studies of generational change [88] and major political events [54, 89]. Systematic testing of the life stability explanation for partisan stability has perhaps been limited by difficulties in identifying and studying environmental discontinuity in small panel studies, resulting in a very limited ability to test the mediating effect of life stability on the age-partisan stability

[6] Although, notably, lottery wins might affect individuals in much more specific ways than major life changes generally (i.e. changing wealth, but not necessarily introducing new friends or a new workplace).

[7] And, somewhat counter to their broader theoretical framework, the authors of *The American Voter* did not rule out "social milieu" effects later in life.

association.

2.3 Research Design

The research design of this paper will distinguish life stability from partisan attachment in the aging-stability relationship by testing two major empirical implications of the life stability theory that are inconsistent with partisan attachment. First, if the life stability model is correct, people will be more likely to change party after a life change, and will be unlikely to change party otherwise. Second, controlling for life changes will reduce or eliminate the relationship between age and partisan stability. If partisan attachment entirely accounts for aging-stability, older people will be less likely to change party than younger people, independent of their circumstances.

Figure 2.1 displays the logic of the research design. Partisan attachment theories suggest an underlying willingness to change that continuously decreases with age (green line in the top-left panel). This individual-level underlying willingness to change manifests as a continuously declining party-switching rate at an aggregated level (black line in the top-left panel). Partisans are not more likely to change party when their lives have changed because partisan attachment is abstract and buffered from current personal experience (middle-left panel). Because there is no difference in partisan change following life changes, controlling for life changes has no effect on the aging-stability relationship (orange line in the bottom-left panel).

In the life stability theory, individuals are more willing to change party when their lives have changed, and perceive little reason to change party when their lives are stable (green line in the top-right panel). They are then more likely to change party following life changes, and unlikely to do so during more stable periods (middle-left panel). This frequency of *impetus* to change (life changes) declines with age, but the willingness to change following life changes does not decline.

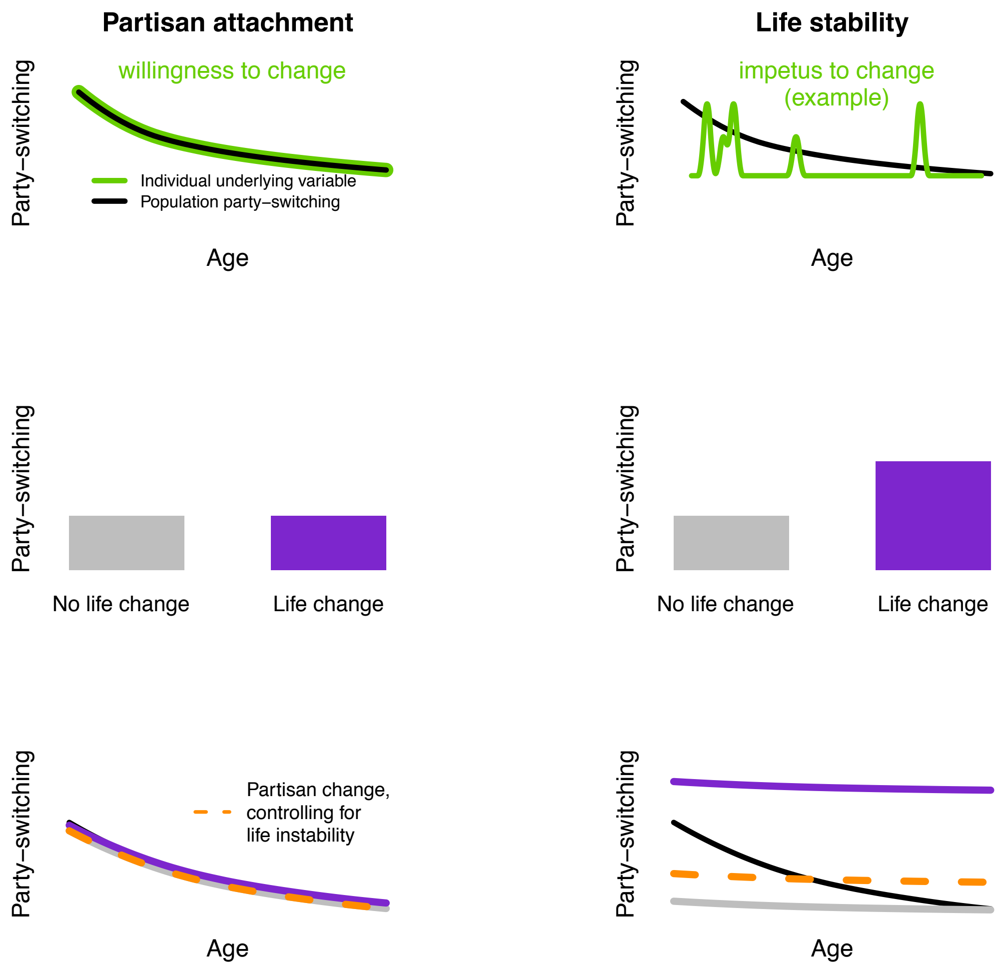

Figure 2.1: *Partisan attachment theory vs. life stability theory.* Partisan attachment explanations suggest an underlying willingness to change that decreases with age (in some, an underyling willingness that decreases much more greatly at young ages – e.g. the 20s – than at older ages). Here, I argue that periodic life instability, that is more common at young ages, drives the observed relationships (in the aggregate and over long time-spans) between age and party-switching. In the analysis here, I will separate the periods of "low" stability and "high" stability and then show that aging-stability, though perhaps driven in part by "attachments", fixed social identities, and loyalty, is *more* attributable to frequent life changes at young ages. This finding suggests that partisan stability overall would be higher if Americans' lives were less stable – for example, if American society and the structure of the economy created more dynamic lives and correspondingly dynamic social and political interests.

Despite this same willingness to change at all ages, the life stability theory *also* manifests as a continuously declining party-switching rate at the aggregate level (black line in the top-right panel). However, if party-switching is driven by the frequency of periodic life changes rather than continuously increasing partisan attachment, I will be able to identify different levels of partisan change during periods of life stability and instability (middle-right panel). Once I separate these punctuated phases from each other (i.e. impetus to change vs. no impetus to change phases), individuals experiencing life changes (or not) will switch parties at similar rates at all ages (gray and purple lines in the bottom-right panel). The relationship between age and partisan stability will then be reduced or disappear when I control for life changes (orange line in the bottom-right panel).

2.3.1 Life stability vs. partisan attachment: specific tests

The purpose of this analysis is to test the possibility that life stability might explain partisan stability, especially increasing stability with age. Because of this, I will focus on tests that distinguish the life stability explanation from partisan attachment theories. I will further test the effects of life changes that are likely to correspond to simultaneous changes in political interests, social discontinuities, and changes in social influences, so that this combined influence will provide a more comprehensive estimate for the total effect of increasing life stability on increasing partisan stability with age. I will not, in this paper, attempt to distinguish possible subsets of the life stability explanation from each other (e.g. the effects of economic/job stability vs. continuity in social contexts). This effort is worthwhile once we establish that life stability might in fact better explain partisan stability than partisan attachment.

Table 2.1 summarizes the analyses I will use here to test whether party affiliation changes might be related to life stability. The first set of tests (top left cell of Table 2.1) will evaluate whether individuals who change residence are more likely to change party. While friends have similar partisan identities and ideologies, an individual in a new place will need to establish new relationships, and will have somewhat limited influence over those choices [90, 91], with the likely exception of romantic partnerships. Further, many moves are driven by new jobs [92][8] and children [93]. These interests might inform political choices (e.g. a child's schooling can increase the importance of preferences on local education politics), and a residential move on their basis might be a strong indicator of their emergent and contemporaneous importance in a person's life.

I will first evaluate this in the American National Election Study, then replicate the analysis using the California voter record (bottom left cell). For both of these analyses, I will test whether controlling for residential mobility greatly reduces the relationship between age and partisan stability. I will then test whether people who move farther from their prior residence are more likely to change party than those who move shorter distances (bottom left cell of Table 2.1). Individuals should not only change party when their lives changes, but should be more likely to change party the more their lives change. This is a test meant to evaluate party change independently of where an individual moves, rather than the partisan makeup of a destination. Although there are small zip code and district partisanship effects here, controlling for neighborhood partisanship does not alter the results. In other words, this will be a test of a social life and circumstance shuffling (and corresponding political reorientation) or even "re-socialization".

As a robustness check, I will analyze whether individuals who moved in immediately prior years (2006-2012) change party much less than all movers (who move 2012 to 2014), controlling for frequency and distance of past moves. This test allows me to evaluate whether long-term drift in partisanship that only manifests after a cue to update party affiliation in the voter record affects the estimates.[9] Movers receive the same cues to update their party

[8]Being laid off might have no effect because many individuals will seek out a comparable job. In this case, the affected individual might not perceive unemployment as a long-term change or a political, societal-level problem. Also, like retirement, losing a job might be socially isolating.

[9]Voters who transfer their voter registration to a new residence are more likely to change party, but they

registration, so this confounder does not affect variation by distance, and repeat movers will have previously received the cues to change party. I note that the repeat mover analyses help rule out the explanation that these tests identify "movers" vs. "stayer" types of people in these analyses, since repeat movers should have higher rates of party-switching.

After this set of residential mobility results, I will then test whether changing political discussion partners alters political affiliations (center-bottom cell of Table 2.1). Because it is difficult to observe changes in friendships, I will focus on the effects of changes in relationships I can observe: spouses. Changes in martial relationships are not only easy to observe, but relatively likely to affect political decision-making. Spouses and family members are the most cited discussion partners in social surveys for both political discussion [17] and discussion generally [18]. In addition, changes in martial status tend to lead to residence changes [94], and separated spouses who move might establish new relationships to displace the influence of their ex-spouse.

In these marital separation tests, I will test whether probable spouses who separate are also likely to quickly diverge in partisanship. I will show estimates for individuals who separated in 2012-2014 to show that there is little change in partisanship prior to separation and a large sudden change at the separation. I will then show estimates for individuals who separated in 2008-2010 to show that there is a jump in partisan divergence immediately after the separation and that this divergence continues at a lower rate thereafter.

Finally, I will show that rates of party switching *suddenly* change slope at retirement (right-bottom cell of Table 2.1). A sudden change in rate is consistent with a life stability

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receive cues to update registration information, including party affiliation, after doing this. *While partisan identification measurement in the American National Election Study is not affected by this consideration*, in the official voter record, movers might update their party affiliation when they move, even though their partisan identification might have changed prior to the move. To ensure that the results here are not strongly driven by inconsistency between *stated* party affiliation in the voter record and partisan identification, I first test whether residence changes predict partisan identification changes in the American National Election panel studies. In these surveys, respondents were simply asked to state their party identification and did so at regular intervals. I then replicate these results, testing in the California voter record whether adjusting for residential mobility reduces the relationship between age and party-switching in official records and also, *given* a move, whether partisans who move farther are more likely to change party.

explanation, while smooth declines (i.e. roughly the same slope) through retirement are consistent with a psychological attachment explanation. This is not a residential mobility test and I show in the appendix that changes in slope occur for movers and non-movers alike. As individuals leave the workforce, we might expect them to have more stable day-to-day lives, and to be exposed to fewer social influences on partisanship, since the workplace is a very important source of cross-cutting discourse [95]. We might also think that retirees could start voting on focused issues, as fixed incomes and health care concerns (and the government's involvement in them) become more important [96, 97]. In this analysis, I will show (in the appendix) that the change in slope at retirement precisely corresponds to age rather than year of birth (e.g. comparing age 65 in 2006 to age 65 in 2012 to a 1941/1947 birth year in 2006 and 2012), strongly suggesting that this an age effect and not a cohort effect.

For the age and time from retirement as well as residential mobility tests, the improvement over prior works is primarily the precise test of fit between the expected and observed functional forms of party-switching enabled by a dataset many times larger than in prior analyses. For the spousal influence tests, the improvement is to study sudden change after separation, since gradually increasing similarity over time might be attributable to difficult-to-measure prior similarity between spouses (and these base similarities, by definition, cannot not quickly disappear after separation). Competing (socialization, discussion, self-reinforcement) partisanship theories would suggest that ex-spouses and roommates should diverge after separation slowly or not at all. Like the retirement and residential mobility analyses, this separation analysis was not possible in smaller, prior studies.

I note that none of these tests guarantee a change in circumstance or social environment. In this sense, all estimates here are *intent-to-treat* estimates. While the analyses could leverage the partisan composition of a destination, homophily in social ties complicate the interpretation of partisan composition estimates. Individuals might, for example, maintain

Table 2.1: *Life change effect tests.* This table displays the series of tests for whether life changes lead to new party affiliations, and whether controlling for life changes reduces associations between age and party-switching.

	Residential mobility	**Separation**	**Retirement**
ANES DV: 4 pts on 7 pt scale	Do movers change more than non-movers? Does controlling for life stability reduce the age relationship?	Not enough divorces	Few retirees (imprecise)
Voter record DV: Dem-Rep, Rep-Dem	Do movers change more the farther they move? Does controlling for life stability reduce the aging-stability relationship?	Do separating spouses diverge more than moving spouses?	Do retirees become *less* likely to change party?

more like-minded friendships when surrounded by ideologically distant people. The focus here, instead, is to estimate the proportion of the age-partisan stability association that can explained by sudden partisan shifts around common major life changes, rather than a gradually increasing psychological attachment to a political party. The estimates will combine the probability of new, politically relevant circumstances or social environments (given life changes) and the effects of new influences. Both of these variables, exposure and influence, are central to understanding the meaning of stable partisanship in the American electorate.

California voter record

I use the California statewide voter record to analyze party affiliation changes by age, residence change, and whether probable spouses have separated. The California voter record is the full list of registered voters in the state, and includes name, date of birth, and residence, as well as party affiliation and voting history. Voter registration information, including unique identification, is maintained at the county level. Because California is a large state and counties are large, moves are relatively likely to be both within state and within county.

To determine whether a registered voter has moved, I link voters between biennial releases of the voter record (2008, 2010, 2012, and 2014) by county voter ID[10] and check whether their residence changed between releases. Voters can easily update their voting address when updating their vehicle address in California. This ensures that these residence changes are relatively up-to-date. If a move is within county, voters only need to check a box on the California Department of Motor Vehicles change of address web page (or the paper

[10]Using county voter identification numbers instead of first name, last name, and date of birth prevents me from considering between county moves in the main analyses, but allows me to more easily link individuals who change their last names and to more confidently identify the effect of separation. Using names instead of country voter identification numbers gives larger estimates, but I cannot clearly distinguish artifacts and county-to-county re-registration effects from life change and other social effects.

form) to transfer their voter registration. In contrast, voters cannot transfer their registration after moving to a new county and must re-register to vote (i.e. the California voter record, across county lines, does not record moves of individuals who do not fill out a re-registration form). I will limit my analysis to within county moves because of this.

Partisan affiliation (voter records) vs. partisan identification (ANES)

To further test the validity of party affiliation listed on an official voter record as an indicator of an individual's current partisan preference, I considered whether respondents in the Cooperative Congressional Elections Survey were likely to state party affiliations different from their official registration. Ansolabehere and Hersh [98] conducted a very similar, descriptive analysis, and I replicated this analysis in California to characterize the prevalence of conflicting identification and registration, specifically testing whether married individuals maintain a partisan affiliation different from their partisan identification. I found that only 2-3% of people reported partisan identification inconsistent with their partisan affiliation, and married people were perhaps slightly more likely to misreport (1-2 percentage points more likely). Ansolabehere and Hersh found a similar number of misidentifiers plus around 3% who reported other party affiliation, but, in nationwide estimates and with extensive controls, did not find a marriage effect.

Spouse/roommate-formatted voter records

My main dependent variable in the spouse/roommate analysis is an individual's change in party affiliation to an affiliation that is different from their spouse. This analysis could theoretically be completed using a panel survey of a large number of individuals with a reasonable probability of divorcing and moving to different residences, given that the survey would be completed at short, one to two year intervals. However, the sample size of that survey, and the difficulty of implementing it at sufficient quality, is prohibitively costly.

The California voter record is a strong replacement for such a panel survey, and substantially stronger than existing surveys in its ability to detect fast-acting and long-term changes. The current residences in the California voter record can be easily used to identify cohabitants and probable spouses. I identify cohabitants and spouses using a formatting algorithm that closely follows the spousal identification rules used in Hobbs et al. [99].[11] For the longitudinal analysis that covers changes from 2006 through 2014 – intended to confirm that partisan changes are both sudden and long-term – I require that both spouses were present in each voter record.

Though separation and divorce are not rare, they are also not common in any given year. Because of my large sample size, I identified 1.62 million pairs for which at least one individual moved between 2008 and 2012 (22% of partners and roommates identified in the voter record in that period) and 430 thousand in which the pairs might have separated (6% of partners and roommates identified in the voter record in that period). 830 thousand pairs with the same last name in 2008 (17% of the same last name subset) moved and 190 thousand (4% of the same last name subset) separated. As a reference, the United States yearly divorce rate (percent of married people) is around 0.7% (an approximately 3% four-year rate).[12]

[11]To identify partners and roommates, I first create a dataset of all households in California by grouping voters with the same listed address, excluding addresses with more than six household members (to exclude group homes). I then link household members whose ages are within fifteen years and who are the only two individuals within their generation in a household. Because I am interested in changes in partisanship, especially between individuals who separate, I require that subjects in the spousal analysis file change residence in a given analysis year. As an example, to be included in an analysis file for the January 2007 (2006 registration) to January 2009 (2008 registration) period, individuals must have either moved from a shared residence in 2007 to a shared residence in 2009 or from a shared residence to separate residence (this includes cases where one of the spouses remains in the previous residence or both spouses move to new residences). I do not require that all spouses move because many individuals keep their marital home after a divorce, and requiring movement limits my sample to a somewhat unusual sample of marriages (where the couples will be younger, less likely to vote, and, arguably, have less investment or social attachments in a community). Requiring that all subjects and spouses either change residence or vote to does not meaningfully alter the overall results, however.

[12]The US Census official California divorce rate was 0.43% in 1990 and the United States official divorce rate was 0.48%. These rates are based on the full population, rather than the married population. They do not take into account all partnerships.

In the American National Election Study panel surveys, only around 2% of respondents who listed "married" in the first series of interviews in 2000 later stated any other marital status. This proportion of newly divorced respondents was too small for statistically well-powered quantitative analysis. Although, the relative effects of life changes on partisan stability are large, the absolute effects are not.

American National Election Study panel survey codings

In the American National Election Study panel survey data, I code respondents as having moved if they stated that they lived in a current residence or community for fewer than 2 years (for the middle panel survey, if asked to continuing panel respondents) or 4 years (for the final survey). I code respondents as having changed party if their 7-point partisan identification changed four points or more (e.g. lean Democrat to strong Republican or weak Republican to weak Democrat). A four point change is the smallest shift on the 7-point scale that is certain to correspond to a switch from one major party to the other.[13]

2.4 Results

Associations between age and length of time holding an affiliation and partisan stability support partisan attachment models in current interpretations. The events that I will consider here are related to both of those. To the extent that I find meaningful effects, the results cast doubt on partisan attachment theories, especially those informed by socialization and increasing attachment models. To highlight this, I will show that

[13]The results are nearly identical when coding "changed party" as a four point shift for "strong" partisans and three point shifts for "weak" partisans and leaning independents, when also controlling for stated partisan strength in the first panel (i.e. controlling for whether a four point shift or a three point shift will count as party-switching). The effect sizes are smaller, but in the mediation tests more statistically significant, when counting moves from leaning one party to leaning another party as party-switching. The lean to lean specification is not desirable because respondents do not need to identify with either party to count as party "switchers". In the ANES, leaning partisan respondents describe themselves as independents, and are pressed to affiliate with one party or another.

controlling for residential mobility greatly weakens relationship between age and party-switching, illustrating the extent of confounded evidence supporting partisan attachment in partisanship. Other tests on marital separation and retirement will add evidence to support the life stability explanation over partisan attachment more generally.

2.4.1 Residential mobility

As shown in Table 2.1, there are two control groups in the residential mobility analysis. The first is non-movers in the American National Election Study panel surveys. I compare these non-movers to people who move to a new home or community. The second is people who move to new homes close to their prior residence in the California voter record. I compare short movers to people who move farther from their prior residence – in other words, a heterogenous effects test within the treated group rather than a treated vs. non-treated comparison.

Figure 2.2 shows estimates of the effect of residential mobility on party identification change (corresponding to at least a four point change on the 7 point party identification scale) from a logistic regression combining all panel years in the American National Election Study (1956-1960, 1972-1976, 1992-1997, and 2000-2004), with controls for age and highest education. A residence change roughly doubles (1.7 times) the rate at which respondents change party in a four-year period. The left side of this panel shows that this effect is specific to party-switching (a change of 4 points or more on the 7 point scale, or the smallest point change guaranteed to result in a party change).[14] Adding controls for partisan strength and interest in campaign does not meaningfully alter the residential mobility estimate. Table 2.2 in the appendix shows the same result with these additional controls. In all of the ordinary least squares models shown there, I scale all variables and center them at their median value.

[14] I estimate the effect of moving on partisanship for different partisan shifts using log-link generalized models. Coefficients from log-link generalized linear are risk ratios, while coefficients from logistic regressions are odds ratios and so affected by baseline rates. This is a meaningful distinction here because baseline rates for 2 point shifts are substantially larger than for 4 point shifts.

This centering allows us to interpret the intercept as the baseline partisan identification change. I display ordinary least squares so that the coefficients are more readily interpretable, and also show logistic regression estimates in the appendix.

The table in Figure 2.3 shows the effect of adding whether an individual has changed residence to a simple model of partisan stability in the American National Election Study panel surveys. Adding the residential mobility roughly halves the coefficient for age effects in partisan stability. Adding additional controls, shown in Table 2.2 in the appendix, slightly increases the age effect reduction. The reduction in slope on age when adding residential mobility to the model corresponds to a 48% mediated effect (p-value: 0.03 using logistic regression and a bias-corrected bootstrapped standard error as described in Tingley et al. [100]).

The figure to the right of Figure 2.3 shows the replicated comparison for the California voter record. This analysis is limited to ages 30 through 65 because I observe many what appear to be voter registrations at parents' houses before age 30 (when comparing move rates to rates in the American Community Survey) and rates of party-switching after 65, as I will show, appear to be related to retirement. While there is a relationship between age and partisan stability in the overall data, controlling for residential mobility (re-weighting the data so that movers and non-movers are equally weighted in the averages at all ages) greatly reduces the association. The relationship between logged age and party-switching here is reduced by an average of 73.9%.[15]

The life stability argument suggests that individuals not only should change party when their lives change, but should be more likely to change party the *more* their lives change. To test this, I consider whether registered electors are more likely to change party

[15] This mean of four bi-yearly estimates excludes party-switching mediation effects for individuals over 50 in years 2008-2012 (where older voters were more likely to change party than younger voters) and over 65 in years 2006-2008 and 2012-2014 (where retirement effects drive party-switching). Including over 50 voters in 2008-2012 increases the average mediation effect to 81.4%. I note that the mediation effects tests are insensitive to the magnitude of the relationship between changing residence and party-switching.

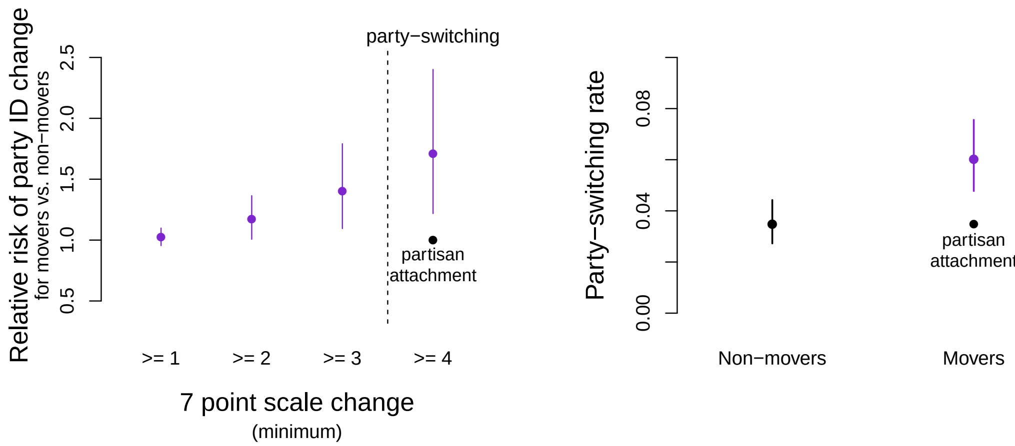

Figure 2.2: *Effect of residential mobility on party identification change in the American National Election Study panel studies.* The left panel in this figure shows the relative risk of moving on party identification change in pooled American National Election Study panel surveys, by minimum point change on the 7 point partisan identification scale. 4 points on the scale is the smallest point change certain to result in a party affiliation change (e.g. corresponding to a change from “weak Democrat” to “weak Republican” or “strong Republican” to “lean Democrat”). The right panel shows predicted probabilities for movers vs. non-movers from a logistic regression, controlling for age and highest education. Adding further controls does not meaningfully alter the estimates. The 4 point, party-switching models exclude respondents who stated “independent-independent” in the first panel.

ANES PANEL SURVEYS		
		Changed party
Moved		**0.023**
		(0.008)
		0.003
Age	**−0.008**	**−0.004**
(logged, scaled)	(0.003)	(0.004)
	0.021	*0.269*
Highest education	−0.017	−0.018
(scaled)	(0.004)	(0.004)
	< 0.001	< 0.001
Constant	0.046	0.038
(baseline change)	(0.004)	(0.004)
	< 0.001	< 0.001
Observations	3,424	3,424

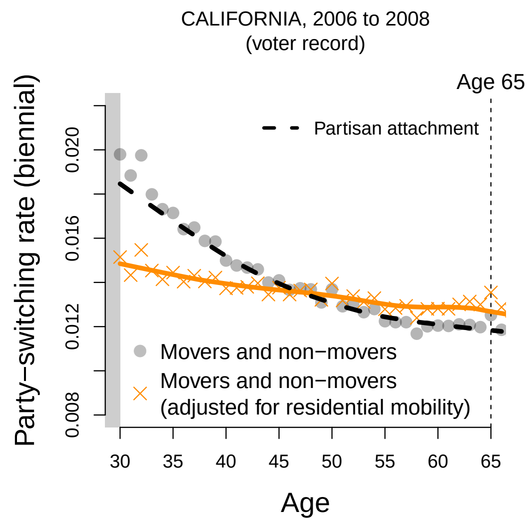

Figure 2.3: *Effects of residential mobility and age on probability of changing partisanship (left: ANES, right: California voter record).* The table (left) displays estimates from a linear regression using pooled ANES panel survey data. It shows that residential mobility substantially increases the probability of changing party (a change of 4 or more points on the 7 point partisan identification scale), and that controlling for residential mobility reduces the relationship between age and the probability of changing party. The reduction in slope on age when adding residential mobility to the model corresponds to a 48% "mediated" effect (p-value: 0.03 using logistic regression and bias-corrected bootstrapped standard errors described in Tingley et al. [100]). The figure (right) shows that controlling for residential mobility in the California voter record also greatly reduces the relationship between age and party-switching. The relationship between age (logged) and party-switching is on average reduced by 73.9%.

the farther they move from their prior residence.

The right panel of Figure 2.4 shows rates of party-switching by distance from prior residence. The party-switching rate is dose-dependent: people who move (and who receive the same official cues to change party) are more likely to change party the farther they move. The estimates are for distance quantiles (30 bins with equal numbers of observations), and distances were computed from the residence zip code centroids. These estimates are from linear regressions with dummy variables for each quantile. The x-axis is the logged distance moved and the y-axis is biennial party-switching rate for 2012 through 2014. While I display the 2012-2014 period (which took place after the California switched to an open, top-two primary), the only difference between the open, top-two years and other years in the analysis here is that the earlier years were log-linear (without the slight uptick for moves over 30 miles). An advantage of using the 2012-2014 data is that I am able to compare recent, repeat movers to all movers.

2.4.2 Spouses and roommates

To examine partisan change attributable to social influence, I test for divergence of partisan affiliation between separating spouses. I distinguish party switching (e.g. Democrat to Republican, Republican to Democrat) from party adoption (e.g. no party to Democrat, no party to Republican) and abandonment (e.g. Democrat to no party, Republican to no party). For the main results shown here, I run models on a partisan subset of the data, and measure whether a spouse changes major party while the other spouse maintains an opposing party affiliation or changes party while the other spouse switches to independence.[16] In addition, I measure whether the affiliation change effects I observe are long-lasting – that is, whether the spouses maintain party divergence in the three elections after the separation period.

[16] I show corresponding results measuring whether spouses change to independence or adopt an affiliation in the appendix.

	VOTER RECORD
	Changed party
	Non-movers
Never moved	0.0111
	(0.0001)
	All movers
Future movers	0.0142
	(0.0002)
Movers in period	0.0579
	(0.0003)
Already moved	0.0123
	(0.0002)
	Repeat Movers
First move (mover in period, move 1 of 2)	0.0557
	(0.0007)
Second move (mover in period, move 2 of 2)	0.0626
	(0.0007)

Standard deviations in parentheses.

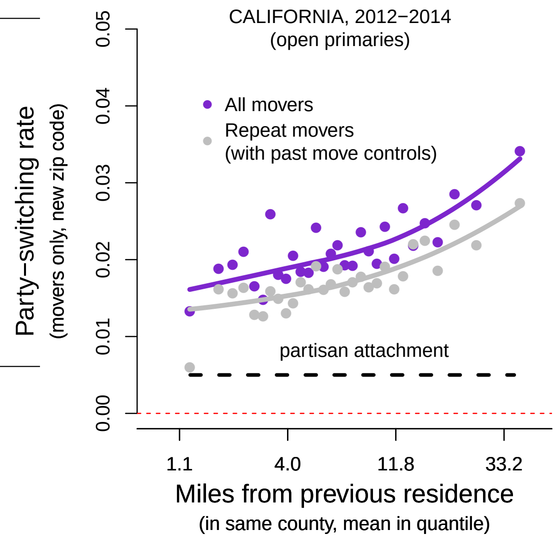

Figure 2.4: Movers change party in the voter record at rates higher than in the ANES panel surveys (ANES result shown previously, Figure 2.2), while non-movers change at slightly lower rates than in the ANES (left panel here – showing proportions weighted to match mover age distribution, and for years 2008-2012 so that all movers were observed before and after their moving periods). Mailings sent only to movers (e.g. postcards with registered party affiliation) might drive some of the replicated voter record result. However, I also show that individuals are more likely to change party the farther they move (right panel). Additionally, controlling for distance of past moves, repeat movers were slightly less likely to change party by move distance. The x-axis is the log distance moved from the previous residence (in miles). Distances were computed from zip code centroids, and the distance analysis is limited to individuals who changed zip codes during within county moves. Controls in the repeat mover analysis were centered at their (logged) means.

In contrast with other tests, I will focus on the relative estimates over absolute estimates (but will show both). The effects are multiplicative and the control groups' baseline rates (divergence between spouses who do not separate) vary from year to year.

I first describe spousal similarity among registered electors. Past works have found that spouses tend to converge in party affiliation, and that this convergence can be substantial [12]. This correspondence might not directly apply here, however, because this analysis separates party-switching from switches to and from independence. Figure 2.5 shows the partisan affiliation correspondence between spouses in the California voter record. At all ages, about 80 percent of spouses who both state a party affiliation shared the same affiliation. Younger spouses, however, have less correspondence when independent voters are included in the analysis. Many young individuals decline to state a party affiliation even when their spouse chooses to. This lower correspondence perhaps indicates low political interest or hesitancy in affiliating rather than partisan dissimilarity. Given this relatively constant partisan similarity over time, we should expect the absolute effect sizes of changes from one party affiliation and another to be small, and changes to and from independence to be large.

Given the loss of a spouse's opinion leadership and an impetus to create a life different from the married life a couple shared (here, a new home and neighborhood), we should see individuals shift party affiliation away from a spouse after residential separation. Figure 2.6 shows, in the left panel, the estimated divergence between separating spouses and roommates from a linear regression model. In the right panel, I report marginal relative estimates here because the 2008-2010 estimates are for movers only while the other years are not restricted to movers. As a reference from the raw data, 4.6% of couples who stated identical major party affiliations in 2006 then separated and still stated affiliations in 2008 diverge in party affiliation, compared to 1.6% of the still cohabiting couples. Figure 2.10 in the appendix shows the absolute estimates for the party abandonment and party switching

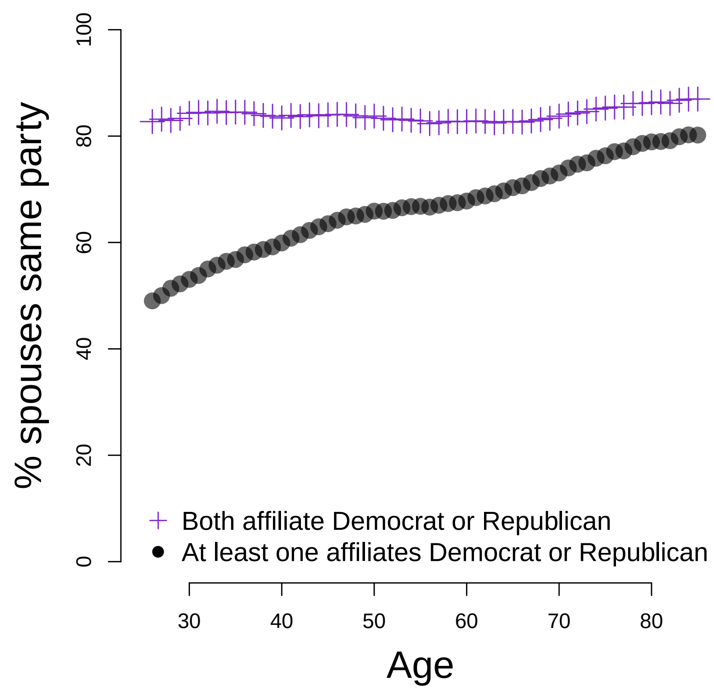

Figure 2.5: *Proportion of spouses who list the same party affiliation, by age.* This figure shows that slightly over 80 percent of spouses state identical party affiliations when they both state some major party affiliation and that this concordance is constant by age. It further shows that younger couples are more likely to have at least one spouse decline to state a party affiliation or state a third-party affiliation.

results, as well as overall and party adoption estimates. While the party adoption estimates are substantially larger than party-switching, the relative estimates are not meaningfully different.

It is likely that most of these pairs are spouses; however, some are likely to be roommates. I analyze all pairs in the main results because many of the same underlying processes may drive roommate similarity as well as spousal. The effects are similar to identical for couples/roommates who do not share the same last name.

2.4.3 Age and time from retirement

The previous results show slight increases in life instability lead to increased party-switching. I next consider whether increased stability reduces party-switching rates. To do this, I test whether the rate of party-switching changes at retirement, especially whether the slope by age is suddenly altered once an individual reaches approximately age 65.[17]

Figure 2.7 shows biennial party-switching rates in California for 2006-2008. I highlight the change in slope, where rates of party-switching are relatively flat until age 65 and then begin to more sharply decline – consistent with an effect of *increased* life stability on partisan stability. This figure is raw data and not the output of a statistical model. However, an F-test for a linear regression with a younger than 65 vs. 65 or older linear trend interaction compared to a linear regression with a linear trend from 30 through 85 is highly statistically significant (p-value: < 0.01).

Whether voters should adjust before retirement or after being in retirement is not entirely clear. I highlight age 65 as only a rough estimate of when retirement-healthcare priorities might change, and when we might expect exposures to new, competing political discussions to begin to decline.

[17] I note that there is also an increase in party-switching around retirement (bottom-right of Figure 2.8 in the appendix), but that this increase varies from year-to-year. I will leave the source of this variation to future research. For now, I note that we might expect many retirees to not be exposed to new social influences, instead spending more time with family members who are not retiring.

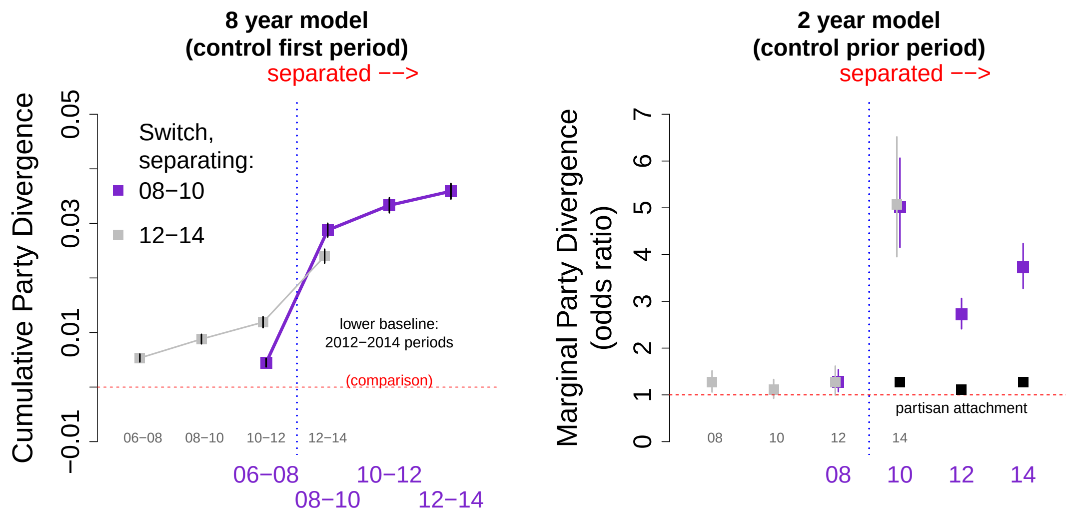

Figure 2.6: *Change in party affiliation before and after separation (absolute, cumulative and marginal, relative effects).* The absolute, cumulative effects of separation are shown in the left panel (first period controls, confidence intervals shown but very small) and the marginal, relative effects are shown in the right panel (with prior period controls). The x-axis is the time from separation (with a vertical dotted line marking the separation period), and y-axis is the party divergence estimate. Controls are age, identifying with a different party than spouse and fixed effects for party identification in the last voter record. The jump in the separating period corresponds to an absolute effect of 1-3 points for party switching (varying by election year, and greater for electors who vote less). The odds ratios in the right panel are only very slightly larger than risk ratios because the outcome variable, party-switching, is rare. I show the relative, cumulative effects in the top-right panel of Figure 2.10.

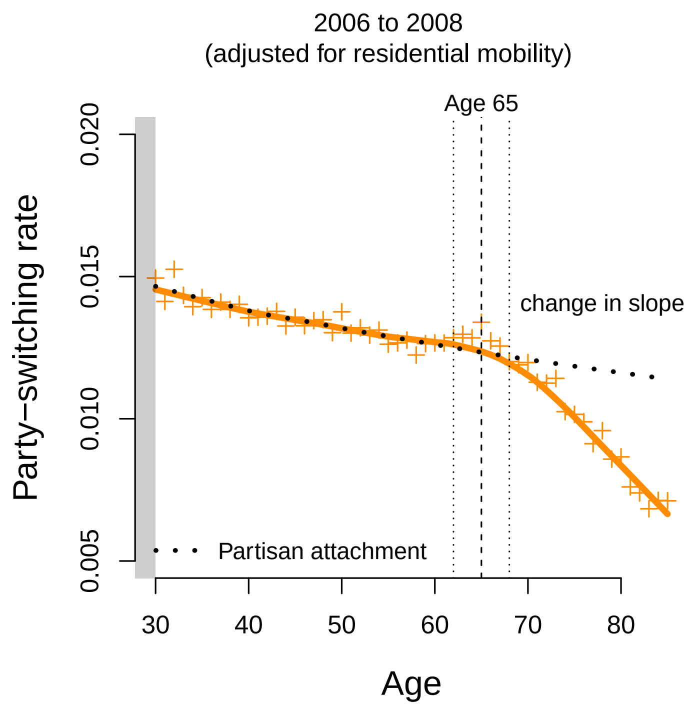

Figure 2.7: *Proportion of registered voters who switch party before and after age 65, controlling for residential mobility.* This figure shows that the relationship between age and party-switching changes slope immediately around age 65 (roughly retirement, entitlement eligibility age).

2.5 Discussion

This analysis suggests a social and personal environment stability explanation for why partisanship is so stable in the United States. The results support an alternative to important components of internalized and abstract partisan attachment theories, as the age-based partisan attachment and life stability explanations were observationally equivalent in previously analyzed data sets. While findings here cannot rule out all age-related evidence in favor of partisan attachment theories, they suggest that 50% (ANES estimate), and up to 70% (voter record estimate), of the relationship between age and party-switching is attributable to age differences in residential mobility alone. Other sources of life stability that co-vary with age, including marriage and retirement, further undermine evidence favoring internalized partisan attachment models. Increased partisan stability with age, and perhaps partisan stability generally, appears to be more driven more by limited changes in an individual's social context than by internalization, abstract attachment, or increasingly reinforced party associations.

Catch-all life change variables are appropriate here because the primary endeavor is to highlight evidence against important aspects of partisan attachment models. However, life changes do not guarantee politically relevant changes in circumstances and social influences, and I do not estimate the effects that extremely fluid lives or extreme changes in environments have on partisanship. Similar to intent-to-treat estimates, the estimates here might reflect the chances of new, politically relevant influences after ordinary life changes.

There are few experiments that involve major social reassignments. One possible exception is the *Moving to Opportunity* study (see Katz et al [101] for a description), and the results from that experiment support this emphasis. Like school voucher programs, participants in this study received housing vouchers, not necessarily changing jobs, income, or general opportunity and social status. Perhaps related, the reassignment appears to have caused social isolation among participants assigned to better neighborhoods and, because of social isolation, lower voter turnout [102]. It is possible that neighborhood changes considered here were limited to approximately within socioeconomic group moves, but that they still happened to correspond to transitions in social circles, personal circumstances, and political environments – and would not have resulted in change were political parties aligned across a uni-dimensional social divide.

It is important to re-emphasize that these results do not raise questions about the stability of partisanship. Partisanship in the United States is undoubtedly very stable, and the rates of change here, though approximately double the baseline rate of change, are not large. The take-away point, instead, is that conventional explanations for that stability might be incorrect. If lives were less stable, rates of partisan change would be correspondingly larger.

These results also do not necessarily question the stability of latent ideological dispositions. Most works in this area have found that political choices and affiliations can change without changing underlying opinions (on relatively non-politicized issues, especially). Ideological dispositions might even underly new party choices in unfamiliar social environments, with social relationships and political discussion helping to inform and organize them, connect them to political parties, and affirm the partisan associations. As an example of research that supports this hypothesis (i.e. the inherent stability of ideological disposition), Hatemi et al. [103], in a twin study of genetic influences on ideology, find that parents socialize ideological constraint in childhood, but that genetics, along with emergent environmental influences, drive ideological orientations from young adulthood on through later life. Gerber et al. [104] find that personality, another measure of long-term disposition, predicts ideology. In follow-up work, Gerber et al. [105] argue that ideology mediates an association between personality and the direction of partisan identification, but that personality affects partisan strength (i.e. willingness to identify with a party) directly.

The distinction between a life stability explanation partisan and attachment explanation for partisan stability should inform how we think about partisanship, evolving political choices, and a periodically and partially informed electorate. The life stability explanation provides potential revisions to both identity [64] and rational, running tally [70, 66] theories of partisanship. To the running tally models, it adds a periodic reconsideration component, where the accomplishments of each party are only considered by individuals when their lives have changed, forcing them to reconsider or reestablish their social relationships and their relationship to society and politics generally. To the identity model, it suggests that symbols and political associations are not especially abstract – that is, they are connected to the immediate environment, and only as stable as the relationships that formed them are.

2.6 Additional tests

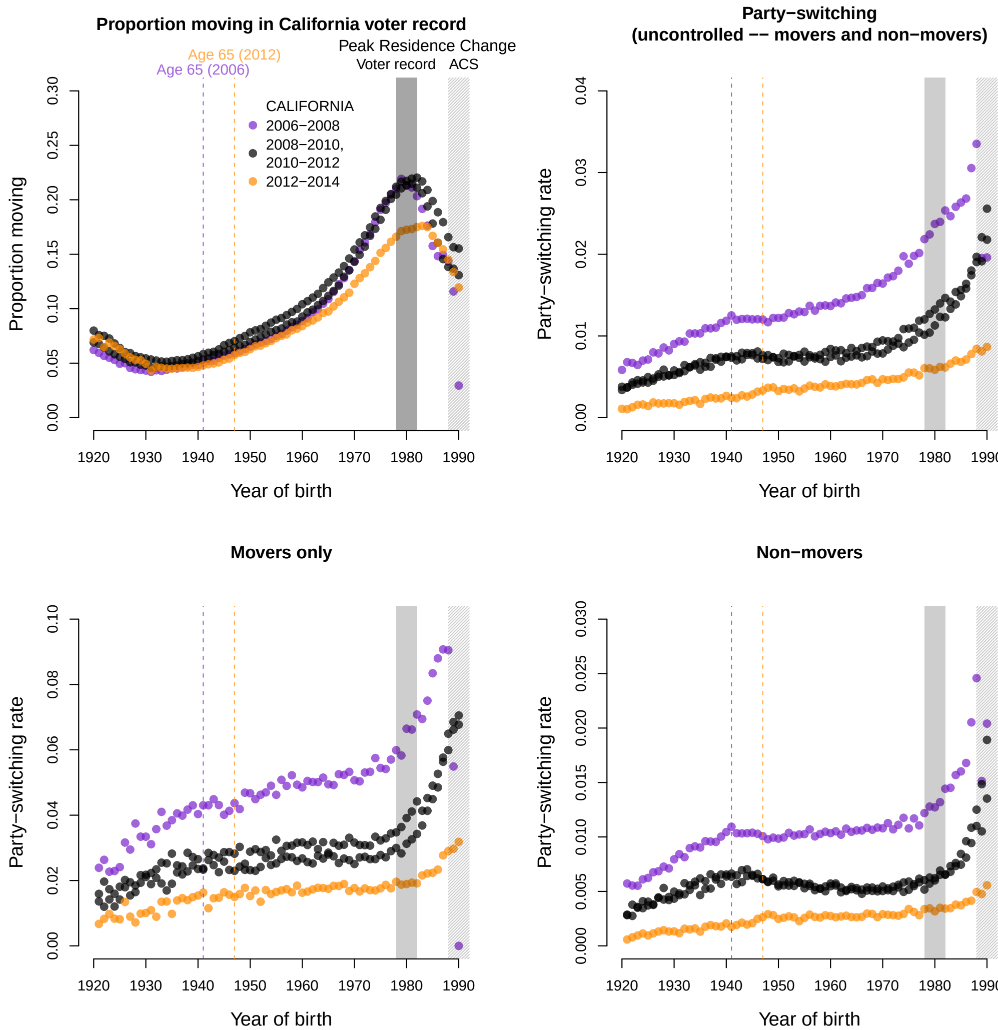

Figure 2.8: *Residential mobility and party-switching.* This figure shows relationships between residential mobility and party-switching by age. Note that all y-axes are different. This is raw data and there are no controls for covariates such as separation from partner or roommate. Mobility rates in the United States, according to the Current Population Survey, increase up to the 20-24 age group and decline thereafter. The peak here is older than that, and the increase up to around age 30 is likely to simply represent the greater observability of residential mobility in this data. Because of this, the sudden flattening of party-switching is very likely related to an abiilty to observe an individual's residential moves (rather than their parents'), and is not evidence of an impressionable years effect ending precisely after age 30.

Table 2.2: *Pooled ANES panel survey estimates, additional controls.* 'Changed party' is a partisan identification shift of four or more points on the 7 point partisan identification scale. In these models, no significant association between partisan strength and changing party means that respondents who in the first panel report strong partisan identification are not less likely to identify with or lean toward the other party four years later than respondents who lean toward a party are to strongly identify with the other party after four years. Partisan strength does predict a lower likelihood of a four point or more shift when excluding leaners in the first panel (scaled OLS coefficient: -0.01, p-value: 0.04); however, controlling for partisan strength does not affect age estimates (i.e. estimates with and without the residential mobility control). Partisan strength is a substantially better predictor of partisan abandonment and adoption than party-switching. The association between 'interested in campaign' and party-switching varies from panel to panel, and is driven by a strong negative association in the 1956-1960 and 1972-1976 panels.

	Changed party			
	OLS	*logistic*	*OLS*	*logistic*
Moved			0.024	0.571
			(0.008)	(0.186)
			0.002	*0.003*
Age	−0.007	−0.209	−0.003	−0.115
(logged, scaled)	(0.004)	(0.088)	(0.004)	(0.093)
(median in age group for '56)	*0.037*	*0.018*	*0.397*	*0.221*
Interested in campaign	−0.009	−0.219	−0.010	−0.222
(scaled)	(0.004)	(0.091)	(0.004)	(0.092)
	0.011	*0.017*	*0.010*	*0.016*
Highest education	−0.015	−0.457	−0.015	−0.454
(scaled)	(0.004)	(0.111)	(0.004)	(0.111)
	< 0.001	< 0.001	< 0.001	< 0.001
Not Married	−0.003	−0.082	−0.006	−0.153
	(0.007)	(0.193)	(0.008)	(0.195)
	0.700	*0.672*	*0.420*	*0.433*
Partisan 'strength'	0.001	0.031	0.001	0.036
(scaled)	(0.004)	(0.101)	(0.004)	(0.102)
	0.731	*0.761*	*0.706*	*0.722*
Constant	0.049	−3.064	0.041	−3.281
(baseline change)	(0.004)	(0.104)	(0.005)	(0.132)
	< 0.001	< 0.001	< 0.001	< 0.001
Observations	3,414	3,414	3,414	3,414

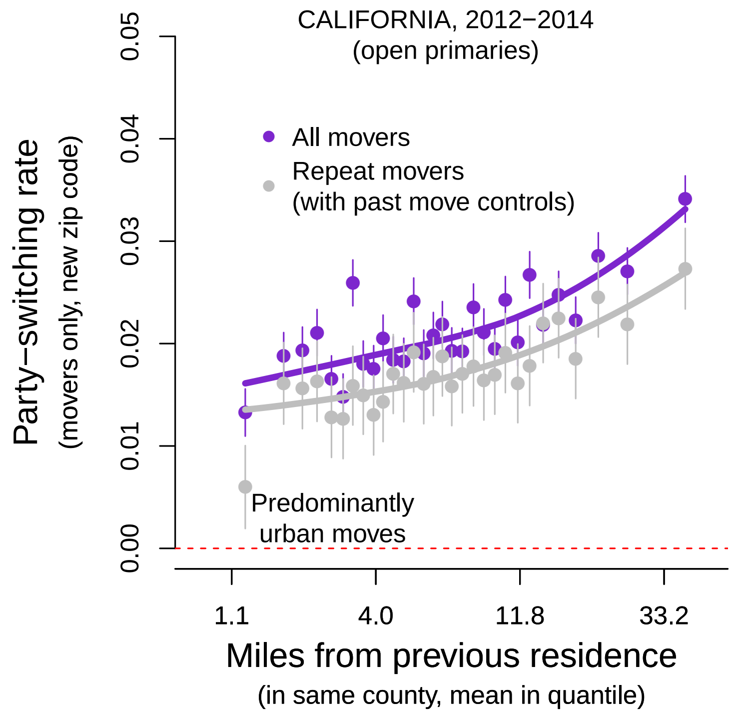

Figure 2.9: *Figure 2.4 with point confidence intervals.*

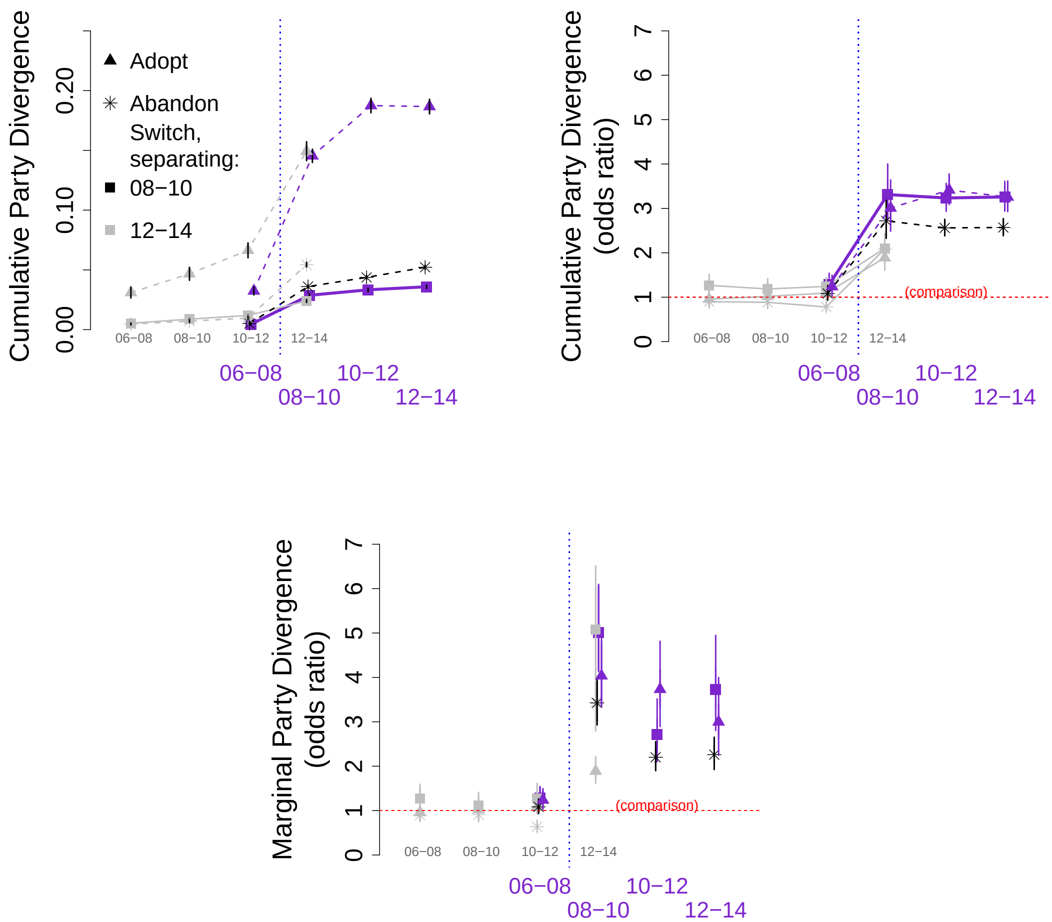

Figure 2.10: *Absolute party change effects following partner/roommate separation.* The top-left figure shows the cumulative, absolute effects of separation on dropping or adopting a party, in addition to the party-switching estimates shown in Figure 2.6 in the main text. The absolute effects of separation on movements to and from independence are substantially larger than for party-switching. The top-right figure shows the same models for the cumulative, relative effects. While the absolute effects of movements to and from independence are larger than party-switching, the relative effects (for separators vs. non-separators) are comparable in size. All controls in these two sets of models (in the top row of figures) are at the first period (2006 for the purple estimates and 2012 for the gray estimates). The bottom figure shows the marginal, relative estimates for the effect of separation on party-switching and changes to and from independence. The controls in this model are biennial (e.g. spouses' party concordance in the period before the move).

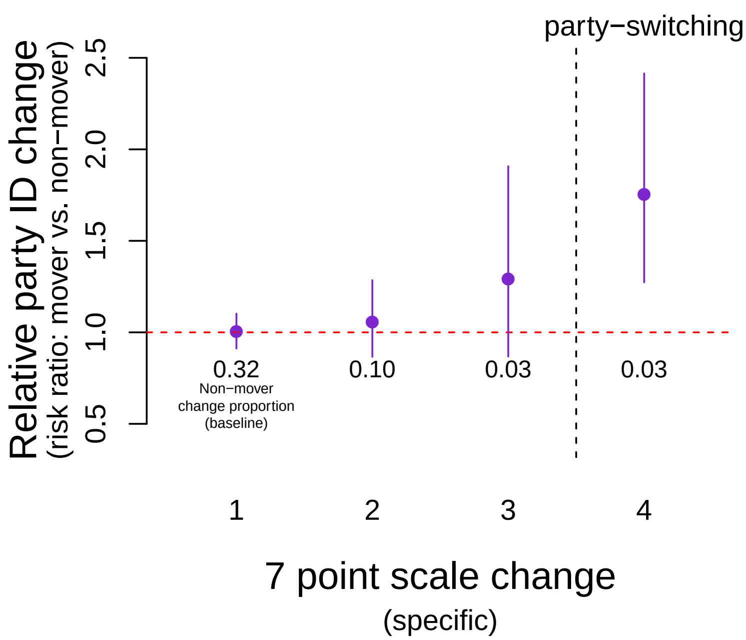

Figure 2.11: This figure shows the relative risk relationships between moving (compared to non-moving) and specific (instead of minimum) 7 point party identification scores changes in the ANES panel surveys. The effect of residential mobility on partisan identification change is limited to party-switching (and, in the ANES, does not appear to extend to changes to and from independence).

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Chapter 3

Plasticity in Human Social Networks

The capacity for self-healing in human social networks is not known. When an individual dies, his or her friends might become isolated in their grief. However, just as plasticity in neural networks boosts resilience after injury, the formation of new friendships could aid social network recovery after loss, especially if friendships form quickly. To evaluate this recovery potential, we analyzed 15,129 de-identified personal networks, composed of 771,034 close friends and acquaintances, in which the central person died, as determined from public records. We found that social networks were highly resilient: close friends of the decedent began interacting more with other friends of the decedent, both in the immediate grieving period and in new bonds that persisted even two years later. The social networks immediately recovered the same volume of interaction that was lost from the central person's death. Adults aged 18-24 were more adaptive than older adults, as were friends of people who died unexpectedly. Recovery appeared to occur in three overlapping patterns and was mathematically similar to shock responses in biological networks. These findings have far-reaching implications for research on trauma response, resilience, and the nature of friendship in human social networks.

Global robustness to random losses in physical networks is well-established [106, 107], however, the bonds in social networks after the loss of a central member may not be as repairable as power lines. Friendships are unique, with each friend providing a different kind of support. Because of this differentiation, close friend networks could experience long-term impairment after a loss. To our knowledge, no large-scale study has examined how and to what extent human social networks heal after the death of a member.

One possibility is that, like synaptic network plasticity in response to brain injury [108], friendship networks could make up for a loss by building new connections within-network. This potential could be very important in human social networks, in which most individuals have few close friends [18, 6, 109] and close friends are major sources of support [110]. There are major constraints to introducing new individuals to a friend group [111, 112], social support and discussion roles in groups can be highly differentiated [113], and suffering can facilitate group attachment [114, 115], so bereaved individuals might most easily make up for lost social connections within the existing friend group. Better understanding of the nature of recovery in social networks and the timeframe in which it takes place could improve trauma response interventions, as well as deepen our understanding of social networks' adaptability.

Existing research on social networks suggests that, after a crisis or the loss of a member, friend groups might react in three ways: 1) forming temporary bonds that dissolve, (2) forming longer-term, in-group bonds, or (3) never healing. Crisis response literature supports the notion of temporary bonds: During crises, strong connections develop for information sharing and support, but the connections are not long lasting [116]. Alternately, college students respond to natural disasters by gradually increasing local network resilience – that is, maintaining close-knit groups by forming relationships with friends-of-friends instead of outsiders [117]. Or, the networks may never recover from the loss of a central individual, permanently reducing connectivity. Research on academic collaboration networks suggests that the core functions of social networks might be greatly adversely affected after the death of an important member [118].

In this paper, we hypothesize that new social interactions will form among people who were friends with someone who died to boost social network resilience after a loss, a phenomenon we refer to as plasticity effects. Figure 3.1 illustrates these effects.

To characterize social network responses to loss, we studied the adaptations of 15,129 de-identified social networks over four years in which the central individual died, compared to 30,258 similar networks that did not experience a death. We focus on the close friends

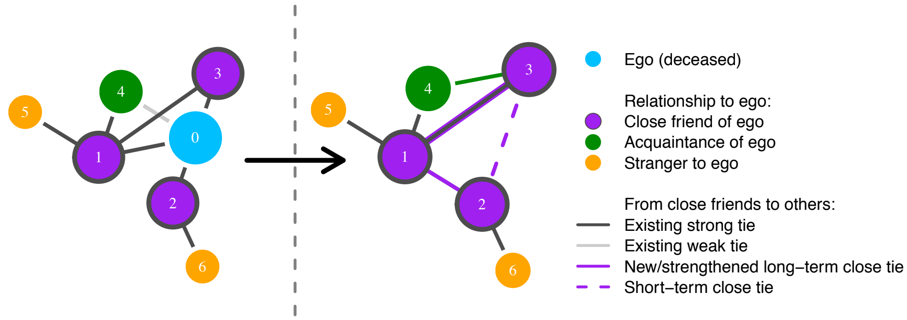

Figure 3.1: *Plasticity effects.* According to the network plasticity hypothesis, after a person dies (node 0), his or her close friends (nodes 1, 2, and 3) will begin to interact more with each other and with acquaintances of the decedent (node 4), even if they were not previously friends. These new social interactions may be temporary or long-lasting. They will not interact more with individuals who were not friends of the decedent (nodes 5 and 6).

of the decedent, examining how many new friendships, as reflected in online interactions, were created and lost over a four-year window, as well as how their communication patterns changed with other close friends of the decedent, with acquaintances, and with strangers.

3.1 Data

To conduct this study, we used Facebook data, as well as public vital records from the State of California. Our study protocol was approved by three governing bodies: the institutional review board at the University of California, San Diego; the State of California Committee for the Protection of Human Subjects; and the Vital Statistics Advisory Committee at the California Department of Public Health. Additionally, Facebook approved the project in order to better understand the use of its website for social support during periods of illness and crisis.

The analysis is restricted to Facebook users in California who met basic criteria: they had a 'real' first and last name, birthdate between 1945 and 1989 (see SOM), and at least two 'close friends' (defined below). 12,689,047 profiles fit the eligibility criteria.

Once we identified the eligible population, we matched profile information (first name or nickname, last name, and date of birth) to California Department of Public Health vital records for 2012 and 2013 to ascertain whether the individual was still living, and if not, his or her cause of death. To preserve privacy, after automatically matching to public records, all analyses were performed on de-identified, aggregate data. All data were observational; no one's experience on the site was different from usual.

In 15,129 cases, the vital records indicated that the person died between January 2012 and December 2013. The focus of the study is on the close friends of these individual-sâĂŤhow their friendship connections and communication patterns changed after the death of a friend (referred to as the subject or the decedent throughout). The study also includes a matched set of networks in which the focal person did not die for comparison (see Methods).

We characterized types of friends of the subject based on their communication during an impanel period, January through June 2011. Close friends were defined as people who communicated with the subject using Facebook comments, posts, or photo tags, or if they appeared in a photo with the subject during this six-month window. We use the term "close friends" loosely to represent individuals who interacted with the subject; this likely includes both the subject's closest confidants [119, 120] as well as other less important communication partners. We contrast these close friends with acquaintances, Facebook friends who did not communicate with the subject during the impanel period, and strangers, individuals who were not Facebook friends with the subject and did not communicate with or appear in any photos with the subject. Within the analysis sample (see Methods), the median number of close friends was 27 (25-75th percentiles: 10-69) and acquaintances (Facebook friends excluding close friends) was 64 (30 - 138). These numbers are lower than those for all Facebook users, but note that social connections and social media activity are typically lower in older populations. All Facebook users not in the close friend and acquaintance groups were counted as strangers.

We then counted how many different people the subject's close friends communicated with each month. In each case, we counted how many different people each of the close friends communicated with who were (a) other close friends of the decedent, (b) acquaintances of the decedent, and (c) strangers to the decedent. We separately counted text wall posts, comments, and photo tags (an indicator of offline interaction), counting the number of people each of the subject's close friends directed each of those actions toward during the month. We refer to the connections as social interaction edges (not to be confused with Facebook friendship edges; two people can communicate and appear together in photos without formally being friends on the site).

For each action type (wall post, comment, photo tag) and recipient type (close friend, acquaintance, stranger), we summed the monthly social interaction edges for all close friends in the networks. To combine the three action types of differing scales without making assumptions about their importance, we then used the geometric average with an adjustment to account for zeroes. Per network in the analysis sample (see Methods), there was a cumulative median of 113 monthly interaction edges between close friends and other close friends of the decedent (25-75th percentiles: 16-463), 87 (22-261) between close friends and acquaintances of the decedent, and 4,049 (1,117-12,041) between close friends and strangers of the decedent. Figure 3.5 in the SOM displays the distributions of counts of close friends and acquaintances, as well as interaction edges between close friends and, relative to the decedent, other close friends, acquaintances, and strangers.

3.2 Methods

To ensure age and gender covariate balance in our analyses, we compared the deceased individuals to a stratified random sample of non-deceased individuals. This comparison sample contained two networks matched on age, gender, and name validation (see SOM) for each network that had experienced a death. These comparison networks were randomly paired, given same age, gender, and name validation, to networks in which the central individual died. The comparison networks were assigned counterfactual dates of "death" from the paired networks. There were 45,387 social networks in this comparison sample, referred to as the "control" group to be consistent with other studies, and 15,129 networks in which the central individual died, referred to here as the "treatment" group. In total, there were 2,020,493 close friends and acquaintances in this sample, and 771,034 who experienced the death of a friend.

To further reduce confounding and ensure parallel trends in the treatment and control groups, especially unmeasured confounders related to social values, culture, and socioeconomic status, we used stabilized inverse probability regression weights. The propensity scores were estimated using a penalized regression on subject and friend characteristics (counts of subject Facebook activity, counts of close friend Facebook activity, Facebook friend self-reported education, self-reported marital status, whether they used a smartphone, and a set of 'like' space derived latent social characteristics, which we describe in the SOM). This propensity score method was previously validated using an experimental baseline [121].

We used quasi-Poisson generalized estimating equations with independent working correlation to measure changes in the number of interaction edges between the decedent's close friends and other members of the decedent's local social network. In these models, the treatment estimate was the difference-in-difference interaction between (1) whether the network includes a deceased individual, and (2) whether the time period is before or after the death. The standard errors were clustered at the ego network level. We included controls for interaction edges among close friends during the six-month impanel period to account for differences in network clustering at baseline, along with a control for Facebook activity outside of the local network (i.e. interactions with strangers) in models that measured interactions within the local social network (close friend interactions with other close friends and acquaintances). This online sociality control slightly attenuated the effect sizes, but helped account for changes in overall Facebook activity over time. To estimate the number of communication interactions "lost" by the death of the central subjects, we added close friends' communications sent to the central subjects' for the control group only. This allowed us to estimate the potential interaction edges lost in the treated compared to the control networks.

For each of the month-by-month figures, we ran the same models, substituting a continuous variable (months from death – included as fixed effects) for the binary (pre/post death) variable. This paired sampling and model setup is very similar to the coarsened exact matching approach used by Azoulay et al. [118].

3.3 Results

Our focus in all models is the number of interaction edges between the decedent's close friends and others in the decedent's social network (i.e., the number of people whom each of the close friends directed wall posts, comments, or photo tags toward). We first estimate changes in numbers of interaction edges within mutual, close friend networks of individuals who died. The purple line in Figure 3.2 displays the monthly changes in our measure of close friend interaction edges before and after the death of a friend. On average, there were 4.5% (95% CI 3.4-5.7%) more interaction edges in close friend networks nine months after losing a mutual friend than otherwise. Consistent with a temporary crisis response effect, some of these edges fade as time goes on (slope -0.026, CI -0.032 to -0.020).

The green line in Figure 3.2 displays the monthly changes in interaction edges between the decedent's close friends and acquaintances before and after the death. There were 2.6% (95% CI 1.5%-3.6%) more interaction edges with acquaintances two years after the death than before (in treatment networks, there were about 1% more close friend-to-acquaintance interaction edges before the death – perhaps related to increased communi-

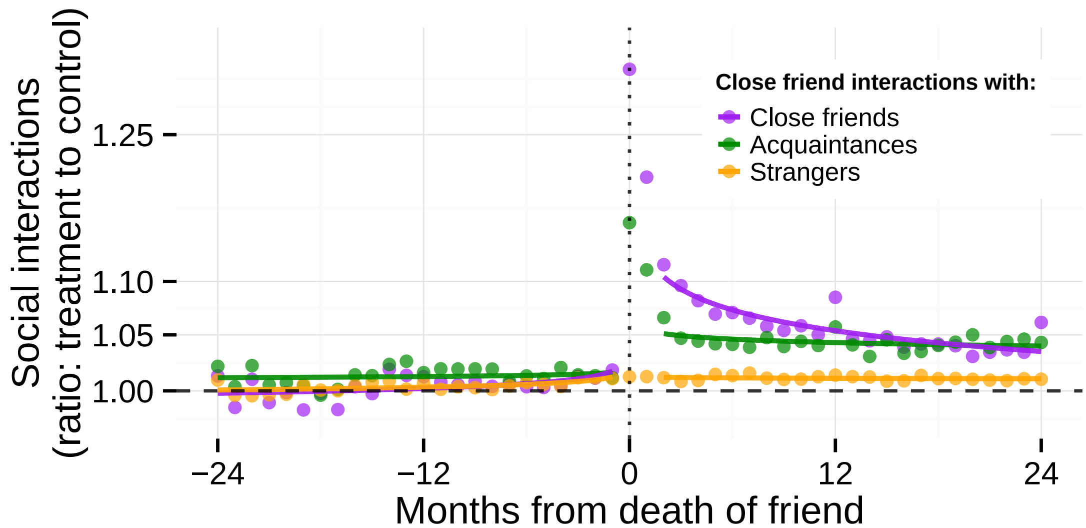

Figure 3.2: *Short and long-term interactions changes after the death of a friend.* Communication increases between friends of the deceased after the death, especially among those who were closest to the deceased. Interactions peak in the first few months, but continue to be higher two years later. Each point represents one month. The y-axis is the rate ratio from a quasi-Poisson model of social interactions in the bereaved networks relative to social interactions in control networks.

cation during illness). These interactions were significantly less likely to fade over time than the close-friend-to-close-friend interactions (slope -0.008, 95% CI -0.015 to -0.001). These networks displayed long-lasting effects, with friends developing new connections that persist for multiple years.

Finally, the orange line in Figure 3.2 shows that there was no overall change in social interactions directed toward individuals who did not know the person who died (p = 0.37), suggesting that interaction edges formed in the short and longer term were highly localized.

Because the increases in close friends' interaction edges correspond to a major loss of potential interactions (from the death of a friend), we estimate how many interaction edges were "recovered" through compensation. Panel A of Figure 3.3 shows that in networks of subjects who were aged 25 or over, the increase in interaction edges nearly to fully compensated for the loss of the interaction edges that the deceased individuals had contributed.

The gray dots in the figure estimate the absence of the ego without compensation (e.g., the interaction edges lost after the subject's death) and the turquoise dots with compensation (e.g., the lost edges plus the new edges to other close friends and acquaintances of the decedent). In networks where the subject was under 25, the surrounding friends actually increased the number of interaction edges in the local network. Simulating the percent recovered, we estimate that recovery across all age groups was 99% (simulated 95% CI 77% to 126%). Considering only compensation from close friends to other close friends, compensation was 78% (simulated 95% CI 63% to 96%). We show estimates for within close friend group recovery by age in the SOM.

To test whether younger or older individuals compensated for the loss of a friend differently (previously we considered the effect of the age of the deceased friend), we stratified our estimates based on the ages of close friends. We also distinguished text-based interactions (wall posts and comments) from photo-based interactions (photo tags) to evaluate to what extent recovery might be limited to online interactions and not extend to offline ones. Past research found that photo tags were more likely to reflect offline interactions [119] (see SOM for a principal component analysis supporting this text vs. photo distinction).

In Panel B of Figure 3.3 we show that this effect varies by the age of their friends. We observe smaller compensation effects among older friends: older adults engage in fewer new social interactions with other friends of the decedent. However, we show in Panels D and E of Figure 3.3 that this decline can be explained by both the ego and close friend ages, as well as cause of mortality. When a young person dies unexpectedly (i.e. from an unintentional injury), new interactions are high regardless of close friends' ages. After the unexpected death of a young person, friends aged 18 to 24 increased mutual friend interactions 8.7% (95% CI 2.8% to 14.7%) while friends aged 25 to 64 increased mutual friend interactions 12.9% (95% CI 8.2% to 17.6%). In Panel B, we see no difference in

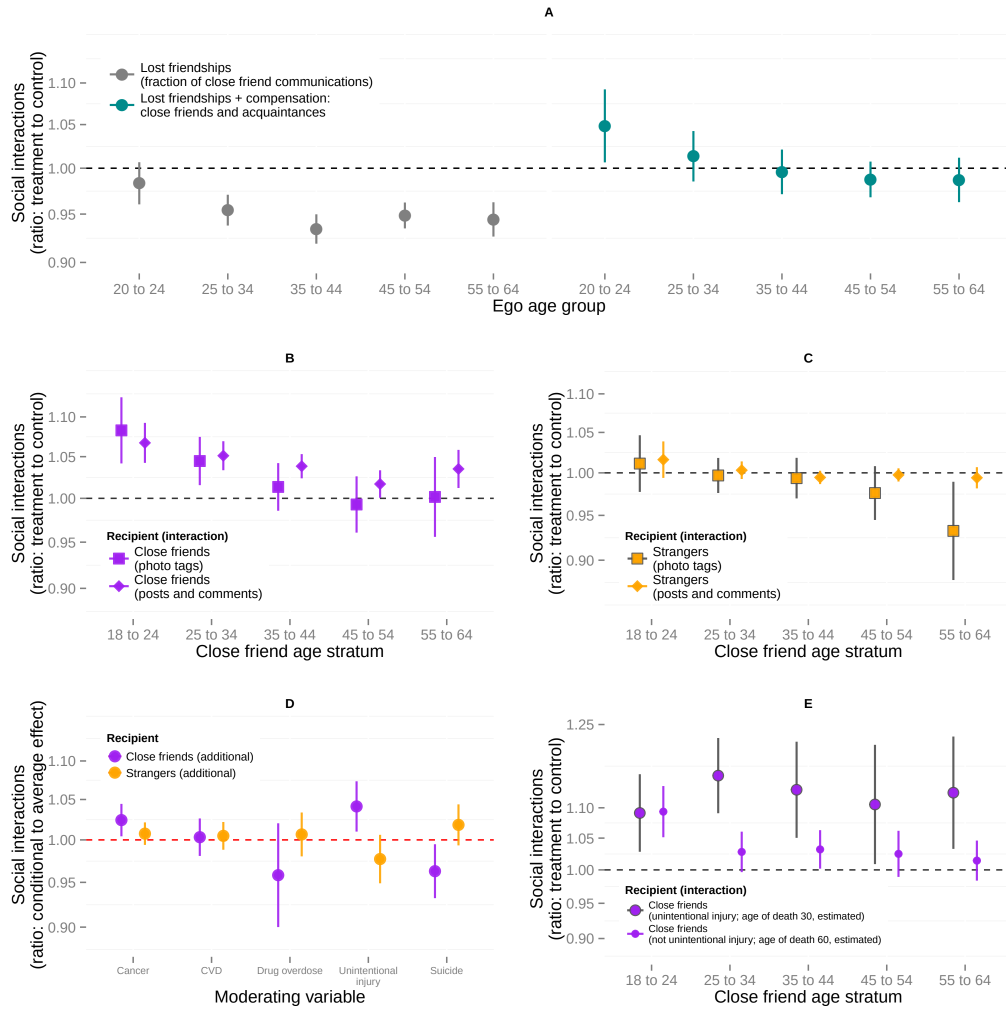

Figure 3.3: *Variation in plasticity by subject age, close friend age, interaction type, and cause of mortality.* This figure displays variation in compensatory social interactions in networks that experienced a death. In all panels, the x-axis is the strata or moderating variable and the y-axis is a rate ratio. Panels A, B, C, and E display difference- in-difference estimates from quasi-Poisson models. In Panel A, the turquoise dots are the difference-in-difference estimates subtracting estimated lost interactions (from the death of a friend – details in text) and the gray dots are the estimated lost interactions. Panel D displays additional effects (on top of the average compensation) for moderating variables in interaction models (additional moderating variables results are shown in the SOM). 95% confidence intervals are point-wise and calculated with robust standard errors clustered at the ego network level.

mutual friend interactions by interaction type at young ages, suggesting that support is happening both online (e.g., exchanging supportive wall posts and comments) and offline (getting together in person and being photographed). However, older individuals increase interactions with the decedent's mutual friends through posts and comments without a corresponding increase in photo tags.

This suggests that compensation among older individuals might only occur online. In Panel C, we show that photo-based interactions with people who did not share the loss of a friend (i.e., strangers to the decedent) decrease among older people. At ages 55 to 64, photo-based interactions with individuals who did not know the friend who died were only 93% (point-wise 95% CI 88% to 99%) of their expected level. We did not observe this effect among younger people, suggesting that this decrease in face-to-face interactions, which may reflect increased isolation, occurs only in older populations.

Finally, in Panel D, we describe variation in these effects by cause of death. In the panel, cause-specific estimates are from separate models, each of which were stratified by the age of death groups used in Panel A. The red line at 1.0 is the average change in interaction edges after a death for all causes other than the one shown, and the estimates are changes in interaction edges above or below that average increase in interaction edges after a death for the specific cause. Some causes of death are linked to stronger changes in close friends' social interactions; close friends of individuals who die suddenly and unexpectedly, such as from an unintentional injury, interact more with each other. Friends of suicide victims, however, are less likely than friends of people who died of other causes to form new interaction edges with the decedent's other friends. Friends of people who die from drug overdoses exhibit a similar pattern, though not at a statistically significant level.

3.4 Discussion

This is the first large-scale study of social network adaptations to structural trauma. We found that, on average, social networks fully recovered the volume of interaction connections lost from a death. Healing occurred through connective recovery and friends were more closely connected to each other years after the shared loss. Plasticity effects were highly localized and, by and large, did not spread beyond the immediate social network.

While we observed some age variation in close friends' adaptations to a death, these differences appeared to be limited to close friends ages 18-24 and, after twenty four, could be explained by cause of mortality and the age of the person who died. In other words, the youngest people in these networks – conceivably those with the most fluid lives and ties – tended to contribute a disproportionate amount to connective recovery, but individuals of all ages adapted and greatly contributed to recovery when a young person died unexpectedly.

We leave many questions unanswered. Networks likely do not always adapt to a loss and network-level recovery might not translate into recovery at the individual level – notably, we were not able to evaluate the subjective experience of loss here. Evidence on recovery among widows, for example, suggests that recovery may depend on a shared loss, as widowed individuals fare better when they live close to others who have experienced the death of a spouse [46], and the apparently smooth trajectories of network recovery seen here might correspond to large oscillations (i.e. swings) in emotional well-being at the individual level [122]. Further, even with full connective recovery, the networks might function differently than before – that is, in a sense, have a changed personality. The new edges forming between friends of the person who died did not replace that person; while levels of connectivity were the same, the networks restructured to accomplish it.

In terms of practical implications of these findings, we note that the typical response to loss appears to be to immediately develop new connections with others. Although this increased connectivity might not be reflected in immediate perceptions of closeness, it suggests that the precursors to new or strengthened close friendships manifest immediately after a loss. While psychiatric maladaptation to a loss is diagnosed at fourteen months after a death [123], the findings here suggest that *social* interventions might take place substantially earlier.

We note two possible explanations for the quick and nearly complete recovery, in terms of social interactions, that we observed. First, the compensation effects might be driven by a lower bound on individuals' level of social connection. Since individuals might have a carrying capacity in their social activity [124], we might expect them to be driven to replace lost friendships more quickly than they are driven to establish them in general.

Second, compensatory social interactions could result from bonding during crisis. The extent of recovery observed here would imply that grief responses tend to produce a level of increased social interaction that compensates for the loss of a single individual.

Finally, recovery dynamics here did not clearly correspond to hypothesized stages of emotional recovery after loss (i.e. five stages of loss [125]) and instead corresponded to resilient psychiatric responses to grief and trauma [126]. More surprisingly, network responses to loss mathematically resembled responses to shock in small-scale biological networks. As an example of a network-focused model of *social* recovery, we highlight in Figure 3.4 that the dynamics here closely resemble separable patterns observed in synaptic potentiation – the set of processes thought to underlie learning and memory in the brain [127]. Synaptic potentiation is thought to involve both biphasic short-term adaptation and long-term adaptation [128]. Similarly, human social networks exhibit these distinct short and long-term adaptations, though at much longer time scales.

We hope that these findings spur greater interest in how social networks adapt to trauma and crisis. Better understanding of social network plasticity could help us identify why social networks succeed or fail in recovery – and how social network failures might be prevented. The findings here, we believe, are an important first step in this direction.

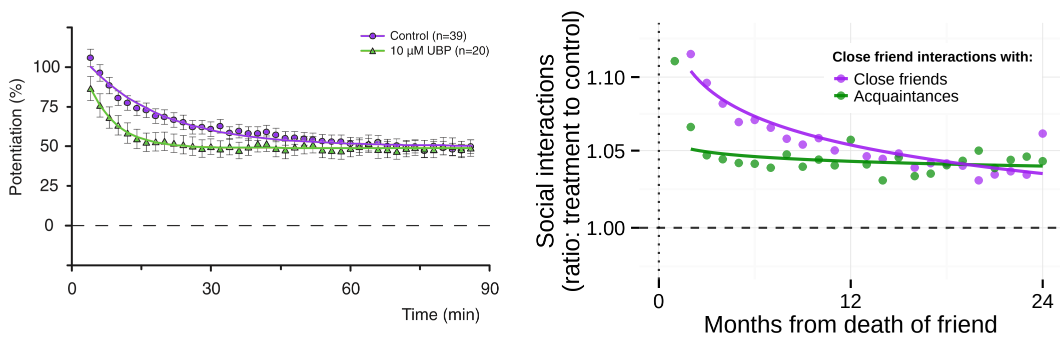

Figure 3.4: *Comparison of synaptic potentiation processes and social network processes.* Network recovery was not reflected in a single, exponential decline of social interaction over the grieving period (commonly observed in network responses to crisis) or in a clear five-stage process of emotional recovery. Instead, we observed a separable, two pattern decline in social interaction, on top of a stable increase in social interaction. These biphasic, short-term responses mathematically resemble small-scale biological network dynamics. Panel A displays short-term potentiation (two patterns) and long- term potentiation (one pattern) – the set of processes thought to underlie learning and memory in the brain [127] - in hippocampus synapses. The green line displays potentiation without the second short-term potentiation present in the purple line. This figure is adapted from Volianskis et al. [128] / CC BY 4.0. Panel B displays the three pattern response in our study. The purple displays all patterns of recovery, while the green line exhibits only the first (more clearly visible in Figure 2) and third patterns.

3.5 Additional information

3.5.1 Data

Eligible population and linking

Prior analyses of social media usage have typically restricted their focus to relatively active users (e.g. active on a specific day [3]) and/or self-reports of activity [129]. Rather than selecting on activity levels closely related to our outcome of interest, we required only that individuals 1) communicated with two or more people on the site between January and June 2011; and 2) listed a first name (or, based on a publicly available database, associated nickname) and last name independently present in the California voter record (e.g. we included users named 'Jenny' if anyone in the California voter record was named 'Jenny' or 'Jennifer'). To further confirm that users listed real names, we segmented our analyses based on whether individuals listed a combined first name (or nickname), last name, and date of birth on Facebook that was also present in the California voter record, and we omitted users who listed a January 1st birthday because this is the default value when signing up for the site. 12,689,047 profiles fit these criteria (the 'full' population), of whom 4,011,852 were present in the California voter record (the 'validated' subpopulation). This match rate of 32% is similar to the match rate reported in previous analyses of California Facebook users [3] and is consistent with the observations that 1) younger people are less likely to be registered to vote, and 2) California has the 2nd lowest voter registration rate in the United States because of its large non-citizen population. Public voting records were only used for this sample validation step; no information related to voting was part of the study.

Once we identified the eligible population, we compared first name or nickname, last name, and date of birth to California Department of Public Health vital records for 2012 and 2013 to ascertain mortality status and cause of mortality. We then linked users who were living in January 2012 to their aggregated Facebook usage (see below) for the six-month impanel period January 2011 through June 2011, as well as basic demographic information: year of birth, gender, date signed up on Facebook, highest education listed on profile, marital status listed on profile, and type of device used to access Facebook, along with the same information for all Facebook friends of the subjects. We excluded deaths that occurred in the six months prior to the impanel period so that the impanel period was less likely to include acute periods of illness and disability. All data were de-identified and aggregated after linking, and no individual activity was viewed by the researchers.

We categorized underlying causes of mortality in 17 specific categories, as well as 5 broader categories (cancer, cardiovascular disease, drug overdose, suicide, and unintentional injury), based on codes of the *International Classification of Diseases, Tenth Revision* [130]. Cause of death categorizations differ from standard categorizations seen in prior works [26] only in that there are fewer old age categories (e.g. no dedicated prostate cancer category) and more young age categories (e.g. distinguishing between drug overdoses and unintentional injuries).

Variable summaries and categorizations

In our analysis sample, the average age as of January 2011 was 49 (sd 12) and 43% listed female gender. Of those who made any social action on the website for January 2011 through June 2011, 27% used smartphone applications on iOS, Android, or Blackberry operating systems (for comparison, others have reported that 35% of Americans owned a smartphone in 2011 [131]).

The median number of close friends (including those co-tagged in a photo with the subject) was 27 (25th percentile 10, 75th percentile 69; mean 56, sd 87) and Facebook friends 64 (25th percentile 30, 75th percentile 138; mean 127, sd 232). These numbers are lower than those for all Facebook users, but note that social connections and social media activity are typically lower in older populations.

After taking geometric means of posts, comments, and photo tags, there were a median of 113 (25th percentile 16, third quartile 463; mean 498, sd 1249) total monthly edges between close friends and other close friends, 87 (25th percentile 22, third quartile 261; mean 278, sd 711) to acquaintances, and 4,049 (25th percentile 1,117, third quartile 12,041; mean 10,593, sd 19,041) to strangers per network. A median of 15 (25th percentile 3, 75th percentile 50; mean 35, sd 50) close friend interactions were sent to the comparison egos over the full panel.

We linked these individuals (the ‘egos’) to aggregate counts of the Facebook activity of their close friends (anyone who interacted with the individual using Facebook comments, wall posts, or photo tags), and separated these interactions by communication type (comment, post, photo tag), as well as whether the communications were sent to other close friends, Facebook friends who had not interacted with the egos, and strangers who were not Facebook friends and had not interacted with the egos. For technical reasons (related to our IRB approvals), we counted all interactions by month and time from death for each monthly count by days from the middle of that month. In the fixed effects models, we run fixed effects for monthly aggregates of the approximate days from death.

The counts of interactions were sums of directed indicators for whether one individual interacted with another individual in a social network in a given month (i.e. total directed edges in a network). We summed the unique close friend $\rightarrow$ close friend/acquaintance/stranger pairs in each network in a given month per interaction type. To combine different Facebook interactions while making few assumptions about their importance, we used the geometric averages of post, comment, and photo tag interactions per month as our outcome variables. To account for zeroes, we added one to each count before taking the geometric mean and subtracted one again after taking the average.

We distinguished text-based interactions (wall posts and comments) from photo-based interactions (photo tags). The photo tags were more likely to reflect strong ties [119]

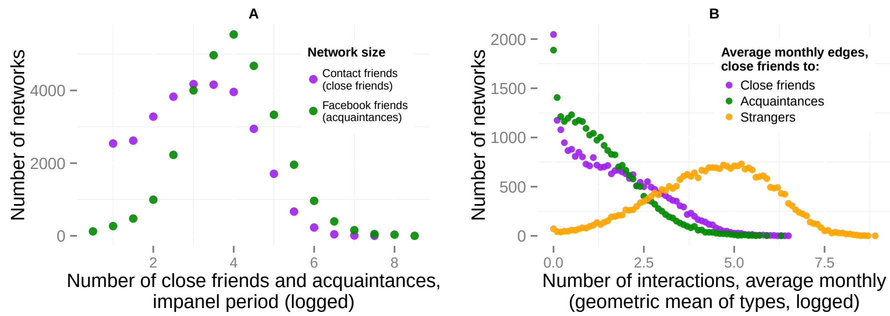

Figure 3.5: *Distributions of network sizes and average monthly interactions edges.*

and, because they are based on photos, offline interactions. This categorization correspond to loadings on major variance dimensions in a principal component analysis [132] of subject Facebook activities in the observation period. Loadings (i.e. transformation coefficients) in Figure 3.6 are the eigenvectors of our sample's Facebook activity covariance matrix and they can be multiplied by the original activity counts to produce a transformation of the data which preserves correlated information in a smaller number of composite, orthogonal variables. The eigenvector corresponding to the leading eigenvalue contains each variable's contribution to the matrix's largest variance component (and the eigenvector correponding to the nth largest eigenvalue is each variable's contribution to the nth largest variance component). In the principal component analysis here, we $log(x+1)$ and scale each variable by its standard deviation and then center at zero.

The first component (i.e. the largest variance component, accounting for 70% of the proportion of variance explained) in this principal component analysis is overall activity (not shown because it is similar for all activity variables), the second is undirected/outgoing activity versus incoming activity (including network size – this component explains 7% of the variance), and the third is text-based versus photo-based activity (5% of the variance). In other words, it appears that users vary primarily in their level of activity, the extent to which the send or receive interactions, and the extent to which they use photos or text to interact with others on the site.

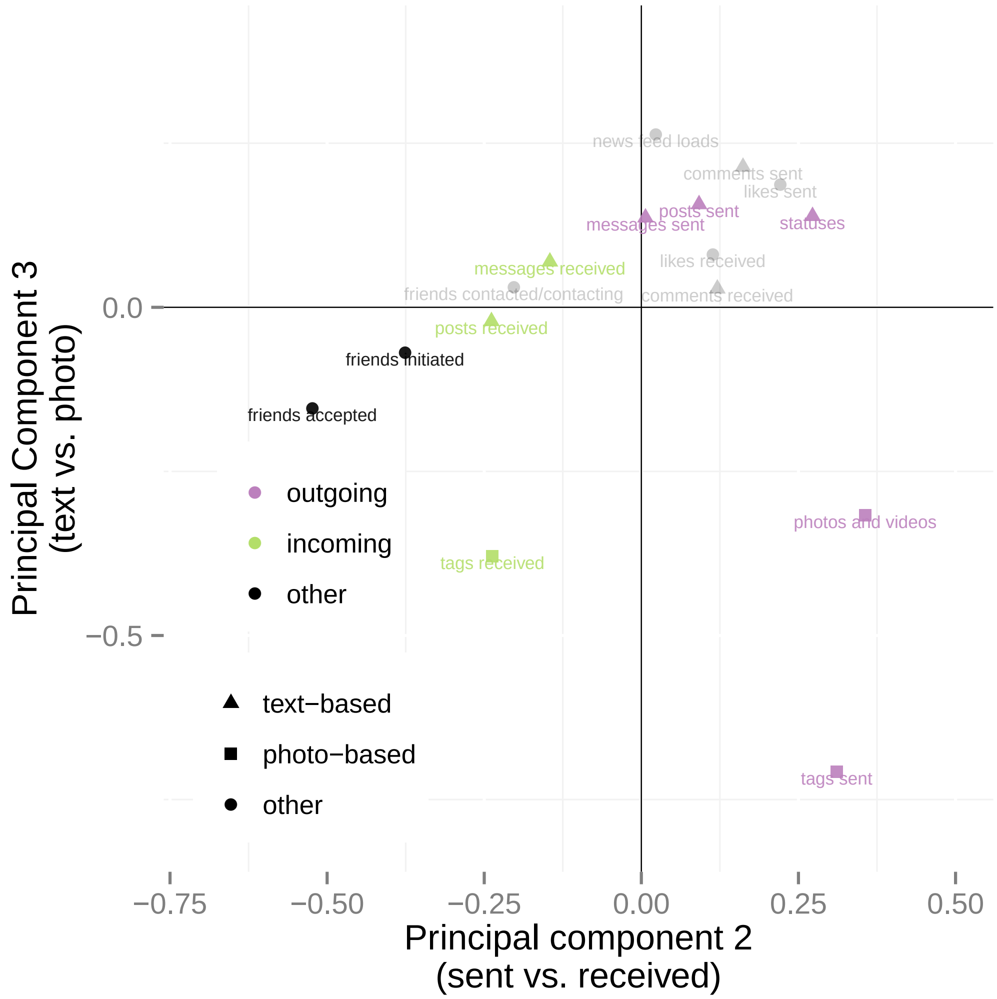

Figure 3.6: *Principal component analysis of common Facebook activities.* This figure shows the second and third components of a principal component analysis on major Facebook activities. The first component (not shown) is the overall level of activity, and the activities of interest do not clearly differ on it. The second component corresponds to sent versus received messages, and the third component corresponds to text versus photo activities.

3.5.2 Matching and controls

The general Facebook population was younger than the population of individuals who died. Therefore, we created a comparison dataset matched on year of birth, gender, and name validation (full name and date of birth present in the California voter record) so that our full sample was composed of two California Facebook users for each deceased California Facebook user and was perfectly balanced on these covariates.

Propensity scores

Propensity scores were estimated using an logit link elastic net penalized regression [133] on network characteristics (counts of subject Facebook activity, counts of close friend Facebook activity, Facebook friend self-reported education, self-reported marital status, whether they used a smartphone, and a set of 'like' space derived latent social characteristics) with α set to 0.99 (i.e. very close to LASSO) and λ set by cross-validation. Penalized regression avoids over-fitting in models including many independent variables.

This propensity score method was validated using an experimental baseline by Eckles and Bakshy [121]. Ours differs from their approach by using a penalty close to the l1 norm, including like behaviors of friends, and by reducing the dimensions of the network average 'like' space prior to inclusion in the model, thus reducing the number of like-based predictors from 1,556 to 100 variables. This pre-processed dimensionality reduction is similar to a ridge regression [134] on the full 'like' variables, as implemented by Eckles and Bakshy, but allows us to describe the major variance components of the input variables. We note that while Eckles and Bakshy stratified their models on the propensity scores (so that their estimates depended on only prediction rank), the treatment and control groups here, after sampling to balance on age and gender, are much less imbalanced than their sample.

Subject networks received stabilized weights of $(P(treated))/(p_i(treated))$ and the stabilized weights for controls were $(1-P(treated))/(1-p_i(treated)))$, where $P(treated)$

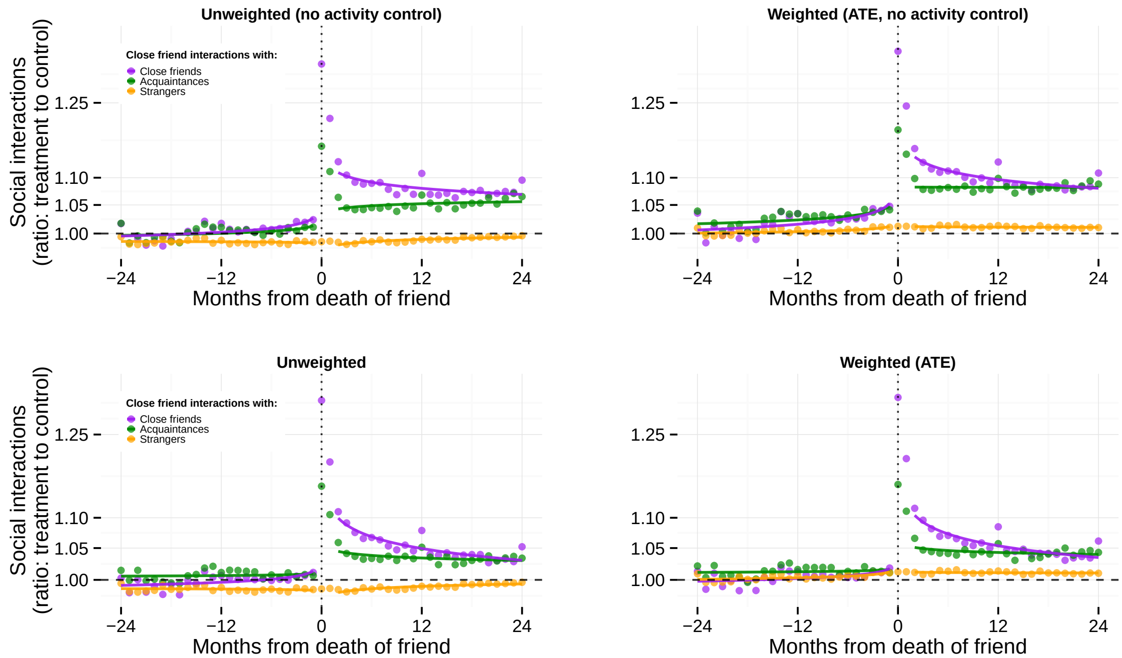

Figure 3.7: *Changes in social interactions following the death of a friend, showing weighted and unweighted data.* Most balanced improvement occurred in the pre-sampling stage, where treatment and control networks were matched exactly on age, gender, and name validation. After this pre-sampling, inverse stabilized weights using high-dimensional propensity scores decreased pre-death differences between treatment and control groups primarily in overall social interactions on Facebook (orange line). Controlling for overall activity, there was little difference between unweighted and weighted estimates of crisis response and compensation effects (purple and green lines).

is the overall probability of a death in our sample and $p_i(treated)$ is an individual's predicted probability.

Comparison networks were randomly paired, given same age, gender, and name validation, to networks in which the central individual died. The comparison networks were assigned counterfactual dates of "death" from the paired networks.

Like space dimensions

To construct our 'like' space measure of latent social characteristics, we decomposed an affiliation matrix of likes of popular Facebook page content. We selected the top 1,000 pages for each month January through June 2011 and used the likes of all California based Facebook users over this period to construct the affiliation matrix. To reduce skew in our matrix, we applied a count normalization (the square root) prior to normalizing to the Laplacian matrix. This additional normalization produced interpretable dimensions and slightly improved predictions in the propensity score models. We ran a singular value decomposition on a normalized Laplacian of this affiliation matrix to estimate 'ideal points' in the Facebook like space, and then took the average of these ideal points per individual. Once we obtained these like space ideal points per individual, we then further averaged the ideal points of Facebook friends for each subject in our study.

The singular value decomposition of the matrix normalized Laplacian returned dimensions corresponding to major sources of polarization and homophily in social networks. The first dimension of this decomposition was the eigenvector centrality of a Facebook page (34% variance) and the subsequent dimensions described like polarization by language (15%), age (8% variance), social values (7% variance), race/ethnicity (5% variance), and gender (4% variance). A similar scaling method on political page likes (producing a liberal-conservative dimension highly correlated with our 'social values' dimension) was validated by Bond and Messing [135].

Imputation

Because a small number (4.6%) of social networks contained no individuals who had liked pages, we imputed missing values using multivariate imputations by chained equations [136]. Because the imputation was used only for the propensity score estimation, we used single imputation and used all variables in the imputation models that would be later used in constructing the propensity scores.

3.5.3 Statistical models

We used quasi-Poisson generalized estimating equations with independent working correlation. Our estimating equation relates counts of interactions in the ego social network i at month t from our observation period:

$$E[y_{it}|X_{it}] = exp[\beta_0 + \beta_1 ln(y_{it_0}) + \beta_2 ln(t) + \beta_3 post_t + \beta_4 D_i + \beta_5 post_t * D_i],$$

where y_{it} is the number of interactions in the local social network (a geometric average if the outcome is a composite of different types of Facebook interactions), $ln(y_{it_0})$ is the monthly average count of interactions in the observation period, t is the number of months from the observation period, $post_t$ is an indicator variable indicating post-treatment period (regardless of treatment status), and D_i is an indicator for treatment status (both before and after treatment). Because comparison networks were assigned a counterfactual date of death, our treatment estimate is the interaction $post_t * D_i$ (estimate β_5).

For models measuring the counts of interactions from close friends to close friends and close friends to acquaintances, we added controls for counts of interactions sent from close friends to others outside the network:

$$E[y_{it}|X_{it}] = exp[... + \beta_6 ln(s_{it_0}) + \beta_7 ln(s_{it})...],$$

This added control slightly attenuated our effect sizes, but increased the precision of our compensation estimates. In our moderation analyses, we stratified our estimates based on the age of close friends. These stratified analyses measure social network responses of close friends by age regardless of the age of the recipients of their interactions. In cause-by-cause analyses, we stratified our models based on the age of death of the central individuals, using the age of death groups presented in our main results.

3.5.4 Moderating variables (other)

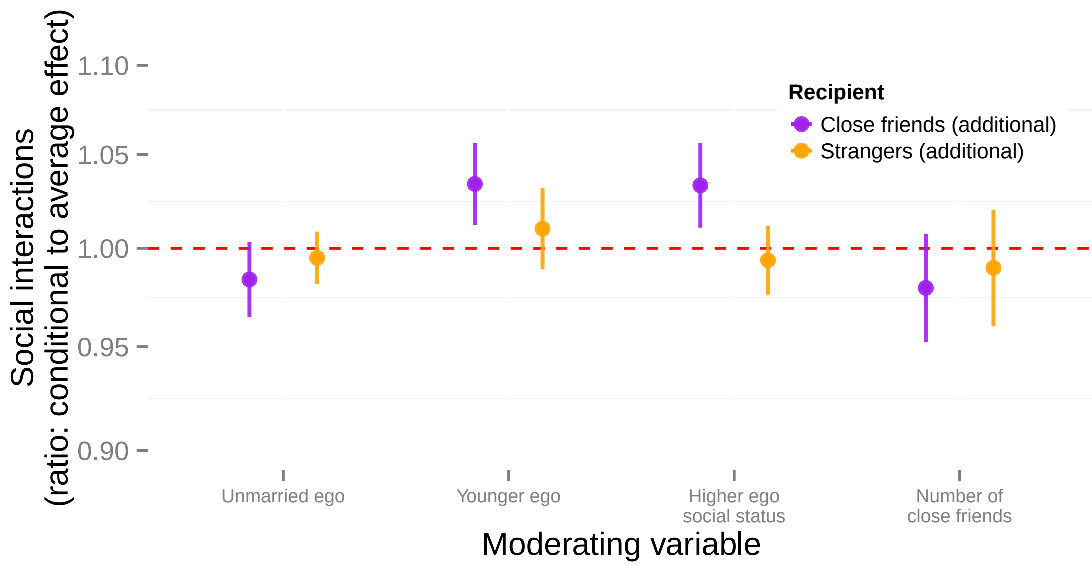

Figure 3.8: *Variation by marital status, age, social status, and number of close friends.* This figure displays variation in social interaction effects by marital status, age, our measure of social status (number of sent and accepted Facebook friendships minus number of received and accepted Facebook friends), and number of close friends. We estimate this variation by interacting moderating variables with the difference in difference variable Deceased:After death. The model included interactions for causes of death considered in the main text (cancer, cardiovascular disease, drug overdose, suicide, and unintentional injury).

3.5.5 Overall estimates

Table 3.1: Overall estimates

	Close friends	Acquaintances	Strangers
(Intercept)	1.149	0.831	4.362
	(0.005)	(0.004)	(0.005)
	0.000	*0.000*	*0.000*
Close friend interactions (impanel)	1.282		
	(0.008)		
	0.000		
Stranger interactions (impanel)	-1.406	-1.290	1.678
	(0.016)	(0.015)	(0.005)
	0.000	*0.000*	*0.000*
Stranger interactions	1.724	1.611	
	(0.018)	(0.015)	
	0.000	*0.000*	
Deceased	0.004	0.013	0.004
	(0.005)	(0.005)	(0.004)
	0.417	*0.016*	*0.320*
After death	-0.232	-0.197	-0.069
	(0.003)	(0.003)	(0.003)
	0.000	*0.000*	*0.000*
Deceased:After death	0.045	0.026	0.005
	(0.006)	(0.005)	(0.006)
	0.000	*0.000*	*0.369*
Acquaintance interactions (impanel)		1.058	
		(0.004)	
		0.000	
Observations	2,043,837	2,043,837	2,043,837

Note: Excludes observations less than two months from death. Robust standard errors shown in parentheses and p-values in italics. Continuous variables logged, scaled by standard deviation, and centered at logged mean, unless otherwise noted. “Impanel” variables are means over the January through June 2011 impanel period.

3.5.6 Crisis-response slope models

Table 3.2: Crisis-response slope models

	Close friends	Acquaintances	Close friends, control acquaintances
(Intercept)	1.204	0.880	1.248
	(0.006)	(0.005)	(0.005)
	0.000	*0.000*	*0.000*
Close friend interactions (impanel)	1.282		1.460
	(0.008)		(0.005)
	0.000		*0.000*
Stranger interactions (impanel)	-1.419	-1.298	
	(0.016)	(0.015)	
	0.000	*0.000*	
Stranger interactions	1.737	1.619	
	(0.018)	(0.015)	
	0.000	*0.000*	
Deceased	0.001	0.010	0.001
	(0.006)	(0.007)	(0.007)
	0.904	*0.154*	*0.853*
After death	-0.339	-0.291	-0.192
	(0.005)	(0.005)	(0.006)
	0.000	*0.000*	*0.000*
Months from death (centered at 18)	0.060	0.053	0.046
	(0.002)	(0.002)	(0.002)
	0.000	*0.000*	*0.000*
Deceased:After death	0.032	0.024	0.022
	(0.009)	(0.009)	(0.009)
	0.000	*0.011*	*0.020*
Deceased:Months from death	-0.004	-0.004	-0.003
	(0.004)	(0.004)	(0.004)
	0.230	*0.298*	*0.447*
After death:Months from death	-0.146	-0.128	-0.067
	(0.003)	(0.003)	(0.003)
	0.000	*0.000*	*0.000*
Deceased:After death:Months from death	-0.022	-0.005	-0.019
	(0.005)	(0.006)	(0.006)
	0.000	*0.432*	*0.001*
Acquaintance interactions (impanel)		1.058	-0.725
		(0.004)	(0.008)
		0.000	*0.000*
Acquaintance interactions			0.685
			(0.008)
			0.000
Observations	2,043,837	2,043,837	2,043,837

Note: Excludes observations less than two months from death. Robust standard errors shown in parentheses and p-values in italics. Continuous variables logged, scaled by standard deviation, and centered at logged mean, unless otherwise noted. "Impanel" variables are means over the January through June 2011 impanel period.

3.5.7 Models included in Figure 3.3

Table 3.3: Figure 3.3, Panel A

	20 to 24	25 to 34	35 to 44	45 to 54	55 to 64
(Intercept)	2.755	2.488	2.168	1.738	1.342
	(0.013)	(0.010)	(0.008)	(0.006)	(0.007)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Close friend, acquaintance, and subject interactions (impanel)	1.369	1.333	1.312	1.239	1.166
	(0.025)	(0.019)	(0.016)	(0.015)	(0.012)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Stranger interactions (impanel)	-1.845	-1.528	-1.228	-1.087	-0.959
	(0.041)	(0.039)	(0.039)	(0.031)	(0.026)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Stranger interactions	2.082	1.742	1.533	1.367	1.200
	(0.032)	(0.041)	(0.045)	(0.042)	(0.029)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Deceased	-0.017	-0.047	-0.068	-0.053	-0.057
	(0.012)	(0.009)	(0.008)	(0.007)	(0.010)
	0.161	*0.000*	*0.000*	*0.000*	*0.000*
After death	-0.272	-0.262	-0.233	-0.206	-0.185
	(0.012)	(0.008)	(0.006)	(0.005)	(0.005)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Deceased:After death	0.064	0.060	0.064	0.040	0.044
	(0.018)	(0.013)	(0.010)	(0.009)	(0.010)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Observations	79,572	218,586	324,804	596,844	761,904

Note: Excludes observations less than two months from death. Robust standard errors shown in parentheses and p-values in italics. Continuous variables logged, scaled by standard deviation, and centered at logged mean, unless otherwise noted. "Impanel" variables are means over the January through June 2011 impanel period.

Table 3.4: Figure 3.3, Panel B

	18 to 24	25 to 34	35 to 44	45 to 54	55 to 64
(Intercept)	-0.375	0.023	-0.267	-0.711	-1.492
	(0.011)	(0.009)	(0.010)	(0.012)	(0.023)
	0.000	*0.014*	*0.000*	*0.000*	*0.000*
Close friend interactions 18 to 24 (tags, impanel)	0.675				
	(0.011)				
	0.000				
Stranger interactions 18 to 24 (tags, impanel)	-0.526				
	(0.013)				
	0.000				
Stranger interactions 18 to 24 (tags)	0.988				
	(0.009)				
	0.000				
Deceased	-0.004	0.006	0.013	0.015	-0.018
	(0.017)	(0.011)	(0.014)	(0.020)	(0.037)
	0.827	*0.574*	*0.360*	*0.445*	*0.623*
After death	-0.368	-0.227	-0.135	-0.086	-0.062
	(0.011)	(0.008)	(0.007)	(0.008)	(0.013)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Deceased:After death	0.080	0.044	0.013	-0.007	0.002
	(0.020)	(0.014)	(0.014)	(0.017)	(0.024)
	0.000	*0.002*	*0.349*	*0.669*	*0.945*
Close friend interactions 25 to 34 (tags, impanel)		0.761			
		(0.010)			
		0.000			
Stranger interactions 25 to 34 (tags, impanel)		-0.534			
		(0.011)			
		0.000			
Stranger interactions 25 to 34 (tags)		0.943			
		(0.008)			
		0.000			
Close friend interactions 35 to 44 (tags, impanel)			0.691		
			(0.010)		
			0.000		
Stranger interactions 35 to 44 (tags, impanel)			-0.481		
			(0.012)		
			0.000		
Stranger interactions 35 to 44 (tags)			0.971		
			(0.010)		
			0.000		
Close friend interactions 45 to 54 (tags, impanel)				0.623	
				(0.010)	
				0.000	
Stranger interactions 45 to 54 (tags, impanel)				-0.465	
				(0.014)	
				0.000	
Stranger interactions 45 to 54 (tags)				0.982	
				(0.010)	
				0.000	
Close friend interactions 55 to 64 (tags, impanel)					0.457
					(0.016)
					0.000
Stranger interactions 55 to 64 (tags, impanel)					-0.418
					(0.016)
					0.000
Stranger interactions 55 to 64 (tags)					0.936
					(0.010)
					0.000
Observations	1,065,480	1,338,566	1,329,977	1,279,195	1,011,755

Note: Excludes observations less than two months from death. Robust standard errors shown in parentheses and p-values in italics. Continuous variables logged, scaled by standard deviation, and centered at logged mean, unless otherwise noted. "Impanel" variables are means over the January through June 2011 impanel period.

Table 3.5: Figure 3.3, Panel B

	18 to 24	25 to 34	35 to 44	45 to 54	55 to 64
(Intercept)	0.323	0.864	0.878	0.824	0.477
	(0.009)	(0.006)	(0.005)	(0.009)	(0.017)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Close friend interactions 18 to 24 (posts and comments, impanel)	1.065				
	(0.009)				
	0.000				
Stranger interactions 18 to 24 (posts and comments, impanel)	-1.073				
	(0.016)				
	0.000				
Stranger interactions 18 to 24 (posts and comments)	1.387				
	(0.018)				
	0.000				
Deceased	0.008	0.006	-0.003	-0.009	-0.026
	(0.008)	(0.007)	(0.007)	(0.009)	(0.020)
	0.310	*0.399*	*0.629*	*0.359*	*0.176*
After death	-0.337	-0.299	-0.232	-0.183	-0.159
	(0.008)	(0.004)	(0.004)	(0.004)	(0.005)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Deceased:After death	0.065	0.050	0.038	0.017	0.034
	(0.012)	(0.009)	(0.007)	(0.008)	(0.011)
	0.000	*0.000*	*0.000*	*0.041*	*0.003*
Close friend interactions 25 to 34 (posts and comments, impanel)		1.141			
		(0.007)			
		0.000			
Stranger interactions 25 to 34 (posts and comments, impanel)		-0.933			
		(0.012)			
		0.000			
Stranger interactions 25 to 34 (posts and comments)		1.148			
		(0.013)			
		0.000			
Close friend interactions 35 to 44 (posts and comments, impanel)			1.094		
			(0.006)		
			0.000		
Stranger interactions 35 to 44 (posts and comments, impanel)			-0.741		
			(0.010)		
			0.000		
Stranger interactions 35 to 44 (posts and comments)			0.984		
			(0.010)		
			0.000		
Close friend interactions 45 to 54 (posts and comments, impanel)				1.059	
				(0.008)	
				0.000	
Stranger interactions 45 to 54 (posts and comments, impanel)				-0.699	
				(0.010)	
				0.000	
Stranger interactions 45 to 54 (posts and comments)				0.927	
				(0.011)	
				0.000	
Close friend interactions 55 to 64 (posts and comments, impanel)					0.901
					(0.012)
					0.000
Stranger interactions 55 to 64 (posts and comments, impanel)					-0.678
					(0.013)
					0.000
Stranger interactions 55 to 64 (posts and comments)					0.917
					(0.014)
					0.000
Observations	1,065,480	1,338,566	1,329,977	1,279,195	1,011,755

Note: Excludes observations less than two months from death. Robust standard errors shown in parentheses and p-values in italics. Continuous variables logged, scaled by standard deviation, and centered at logged mean, unless otherwise noted. "Impanel" variables are means over the January through June 2011 impanel period.

Table 3.6: Figure 3.3, Panel C

	18 to 24	25 to 34	35 to 44	45 to 54	55 to 64
(Intercept)	2.314	2.595	2.453	2.109	1.163
	(0.014)	(0.008)	(0.012)	(0.012)	(0.019)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Stranger interactions 18 to 24 (tags, impanel)	1.321				
	(0.012)				
	0.000				
Deceased	-0.008	-0.010	-0.024	-0.006	0.062
	(0.017)	(0.010)	(0.012)	(0.018)	(0.031)
	0.618	*0.319*	*0.038*	*0.728*	*0.048*
After death	-0.366	-0.029	0.146	0.142	0.199
	(0.011)	(0.007)	(0.007)	(0.009)	(0.016)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Deceased:After death	0.011	-0.003	-0.006	-0.024	-0.070
	(0.017)	(0.011)	(0.013)	(0.017)	(0.030)
	0.521	*0.770*	*0.611*	*0.141*	*0.021*
Stranger interactions 25 to 34 (tags, impanel)		1.324			
		(0.006)			
		0.000			
Stranger interactions 35 to 44 (tags, impanel)			1.301		
			(0.010)		
			0.000		
Stranger interactions 45 to 54 (tags, impanel)				1.214	
				(0.010)	
				0.000	
Stranger interactions 55 to 64 (tags, impanel)					1.019
					(0.013)
					0.000
Observations	1,065,480	1,338,566	1,329,977	1,279,195	1,011,755

Note: Excludes observations less than two months from death. Robust standard errors shown in parentheses and p-values in italics. Continuous variables logged, scaled by standard deviation, and centered at logged mean, unless otherwise noted. “Impanel” variables are means over the January through June 2011 impanel period.

Table 3.7: Figure 3.3, Panel C

	18 to 24	25 to 34	35 to 44	45 to 54	55 to 64
(Intercept)	3.782	4.162	4.188	4.032	3.384
	(0.006)	(0.004)	(0.003)	(0.003)	(0.005)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Stranger interactions 18 to 24 (posts and comments, impanel)	1.369				
	(0.004)				
	0.000				
Deceased	-0.005	0.006	-0.001	0.003	0.007
	(0.006)	(0.004)	(0.004)	(0.004)	(0.006)
	0.422	*0.147*	*0.718*	*0.407*	*0.283*
After death	-0.523	-0.162	0.013	0.094	0.169
	(0.006)	(0.003)	(0.002)	(0.002)	(0.003)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Deceased:After death	0.016	0.003	-0.006	-0.002	-0.006
	(0.011)	(0.005)	(0.004)	(0.004)	(0.007)
	0.157	*0.555*	*0.180*	*0.540*	*0.363*
Stranger interactions 25 to 34 (posts and comments, impanel)		1.350			
		(0.003)			
		0.000			
Stranger interactions 35 to 44 (posts and comments, impanel)			1.380		
			(0.002)		
			0.000		
Stranger interactions 45 to 54 (posts and comments, impanel)				1.398	
				(0.002)	
				0.000	
Stranger interactions 55 to 64 (posts and comments, impanel)					1.338
					(0.004)
					0.000
Observations	1,065,480	1,338,566	1,329,977	1,279,195	1,011,755

Note: Excludes observations less than two months from death. Robust standard errors shown in parentheses and p-values in italics. Continuous variables logged, scaled by standard deviation, and centered at logged mean, unless otherwise noted. "Impanel" variables are means over the January through June 2011 impanel period.

Table 3.8: Figure 3.3, Panel D. Stratified on age. 18 to 24 age group coefficients shown.

	Unintentional injury	Suicide	Cancer	CVD	Drug overdose
(Intercept)	1.225	1.225	1.224	1.224	1.226
	(0.021)	(0.021)	(0.022)	(0.022)	(0.022)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Close friend interactions (impanel)	1.220	1.221	1.222	1.222	1.222
	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Stranger interactions (impanel)	-1.952	-1.950	-1.950	-1.952	-1.953
	(0.045)	(0.046)	(0.046)	(0.046)	(0.046)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Stranger interactions	2.300	2.299	2.296	2.298	2.299
	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Deceased	0.018	0.026	0.018	0.023	0.026
	(0.014)	(0.013)	(0.014)	(0.013)	(0.013)
	0.196	*0.058*	*0.180*	*0.095*	*0.050*
After death	-0.247	-0.248	-0.248	-0.248	-0.247
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Unintentional injury	0.010				
	(0.012)				
	0.375				
Deceased:After death	0.066	0.085	0.075	0.079	0.084
	(0.021)	(0.021)	(0.021)	(0.021)	(0.022)
	0.002	*0.000*	*0.000*	*0.000*	*0.000*
After death:Unintentional injury	0.040				
	(0.016)				
	0.009				
Suicide		-0.040			
		(0.015)			
		0.006			
After death:Suicide		-0.038			
		(0.017)			
		0.023			
Cancer			0.021		
			(0.009)		
			0.028		
After death:Cancer			0.024		
			(0.010)		
			0.017		
Cardiovascular disease				-0.008	
				(0.010)	
				0.403	
After death:Cardiovascular disease				0.003	
				(0.012)	
				0.778	
Drug overdose					-0.043
					(0.012)
					0.001
After death:Drug overdose					-0.043
					(0.032)
					0.183
Observations	2,043,837	2,043,837	2,043,837	2,043,837	2,043,837

Note: Excludes observations less than two months from death. Robust standard errors shown in parentheses and p-values in italics. Continuous variables logged, scaled by standard deviation, and centered at logged mean, unless otherwise noted. "Impanel" variables are means over the January through June 2011 impanel period.

Table 3.9: Figure 3.3, Panel D. Stratified on age. 18 to 24 age group coefficients shown.

	Unintentional injury	Suicide	Cancer	CVD	Drug overdose
(Intercept)	4.291	4.291	4.291	4.291	4.292
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Stranger interactions (impanel)	1.644	1.645	1.644	1.644	1.644
	(0.019)	(0.018)	(0.018)	(0.019)	(0.019)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Deceased	0.002	0.002	-0.002	0.004	0.006
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)
	0.904	*0.865*	*0.876*	*0.768*	*0.643*
After death	-0.446	-0.446	-0.446	-0.446	-0.446
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
	0.000	*0.000*	*0.000*	*0.000*	*0.000*
Unintentional injury	0.004				
	(0.009)				
	0.701				
Deceased:After death	0.021	0.011	0.014	0.014	0.013
	(0.026)	(0.025)	(0.025)	(0.025)	(0.025)
	0.417	*0.654*	*0.584*	*0.577*	*0.596*
After death:Unintentional injury	-0.023				
	(0.015)				
	0.121				
Suicide		0.004			
		(0.009)			
		0.627			
After death:Suicide		0.018			
		(0.013)			
		0.151			
Cancer			0.026		
			(0.006)		
			0.000		
After death:Cancer			0.008		
			(0.007)		
			0.277		
Cardiovascular disease				-0.018	
				(0.007)	
				0.008	
After death:Cardiovascular disease				0.005	
				(0.009)	
				0.571	
Drug overdose					-0.027
					(0.011)
					0.013
After death:Drug overdose					0.007
					(0.014)
					0.627
Observations	2,043,837	2,043,837	2,043,837	2,043,837	2,043,837

Note: Excludes observations less than two months from death. Robust standard errors shown in parentheses and p-values in italics. Continuous variables logged, scaled by standard deviation, and centered at logged mean, unless otherwise noted. "Impanel" variables are means over the January through June 2011 impanel period.

Table 3.10: Figure 3.3, Panel E

	18 to 24	25 to 34	35 to 44	45 to 54	55 to 64
(Intercept)	-0.103 (0.018) *0.000*	0.460 (0.016) *0.000*	0.463 (0.019) *0.000*	0.471 (0.020) *0.000*	0.076 (0.021) *0.000*
Close friend interactions 18 to 24 (impanel)	0.074 (0.008) *0.000*				
Stranger interactions 18 to 24 (impanel)	-0.062 (0.008) *0.000*				
Stranger interactions 18 to 24	1.523 (0.016) *0.000*				
After death	-0.246 (0.010) *0.000*	-0.267 (0.010) *0.000*	-0.245 (0.010) *0.000*	-0.207 (0.012) *0.000*	-0.187 (0.015) *0.000*
Deceased (not unintentional injury)	-0.076 (0.046) *0.099*	-0.051 (0.046) *0.263*	-0.021 (0.048) *0.659*	-0.005 (0.072) *0.941*	-0.020 (0.081) *0.804*
Deceased	0.052 (0.044) *0.233*	0.033 (0.042) *0.424*	0.012 (0.047) *0.796*	0.048 (0.067) *0.468*	-0.016 (0.078) *0.834*
Age (centered at 30)	-0.001 (0.012) *0.953*	0.009 (0.015) *0.530*	-0.069 (0.014) *0.000*	-0.185 (0.017) *0.000*	-0.110 (0.021) *0.000*
Deceased (not unintentional injury):After death	-0.008 (0.032) *0.796*	-0.106 (0.031) *0.001*	-0.087 (0.037) *0.019*	-0.062 (0.046) *0.176*	0.013 (0.039) *0.747*
Deceased:After death	0.087 (0.030) *0.004*	0.145 (0.029) *0.000*	0.123 (0.038) *0.001*	0.100 (0.047) *0.032*	0.118 (0.044) *0.007*
Deceased:Age (centered at 30)	-0.012 (0.015) *0.442*	0.030 (0.027) *0.275*	0.010 (0.028) *0.721*	-0.040 (0.034) *0.237*	0.013 (0.042) *0.762*
After death:Age (centered at 30)	0.096 (0.008) *0.000*	0.067 (0.010) *0.000*	0.018 (0.011) *0.092*	0.003 (0.012) *0.774*	-0.037 (0.015) *0.015*
Deceased:After death:Age (centered at 30)	0.008 (0.012) *0.511*	-0.009 (0.021) *0.681*	-0.003 (0.021) *0.880*	-0.011 (0.022) *0.603*	-0.099 (0.029) *0.001*
Close friend interactions 25 to 34 (impanel)		0.093 (0.011) *0.000*			
Stranger interactions 25 to 34 (impanel)		-0.094 (0.015) *0.000*			
Stranger interactions 25 to 34		1.514 (0.015) *0.000*			
Close friend interactions 35 to 44 (impanel)			0.106 (0.013) *0.000*		
Stranger interactions 35 to 44 (impanel)			-0.128 (0.009) *0.000*		
Stranger interactions 35 to 44			1.561 (0.028) *0.000*		
Close friend interactions 45 to 54 (impanel)				0.096 (0.010) *0.000*	
Stranger interactions 45 to 54 (impanel)				-0.101 (0.015) *0.000*	
Stranger interactions 45 to 54				1.453 (0.038) *0.000*	
Close friend interactions 55 to 64 (impanel)					0.144 (0.015) *0.000*
Stranger interactions 55 to 64 (impanel)					-0.200 (0.023) *0.000*
Stranger interactions 55 to 64					1.281 (0.018) *0.000*
Observations	1,065,480	1,338,566	1,329,977	1,279,195	1,011,755

Note: Excludes observations less than two months from death. Robust standard errors shown in parentheses and p-values in italics. Continuous variables logged, scaled by standard deviation, and centered at logged mean, unless otherwise noted. "Impanel" variables are means over the January through June 2011 impanel period.

Table 3.11: Figure 3.3, Panel E

	18 to 24	25 to 34	35 to 44	45 to 54	55 to 64
(Intercept)	-0.104 (0.017) *0.000*	0.472 (0.014) *0.000*	0.376 (0.016) *0.000*	0.245 (0.015) *0.000*	-0.053 (0.014) *0.000*
Close friend interactions 18 to 24 (impanel)	0.074 (0.008) *0.000*				
Stranger interactions 18 to 24 (impanel)	-0.062 (0.008) *0.000*				
Stranger interactions 18 to 24	1.523 (0.016) *0.000*				
After death	-0.125 (0.013) *0.000*	-0.183 (0.008) *0.000*	-0.222 (0.008) *0.000*	-0.203 (0.010) *0.000*	-0.231 (0.009) *0.000*
Unintentional injury	0.076 (0.046) *0.099*	0.051 (0.046) *0.263*	0.021 (0.048) *0.659*	0.005 (0.072) *0.941*	0.020 (0.081) *0.804*
Deceased	-0.039 (0.025) *0.114*	0.020 (0.022) *0.374*	0.003 (0.022) *0.876*	-0.005 (0.032) *0.863*	-0.021 (0.025) *0.393*
Age (centered at 60)	-0.001 (0.011) *0.953*	0.008 (0.013) *0.530*	-0.053 (0.011) *0.000*	-0.127 (0.011) *0.000*	-0.064 (0.013) *0.000*
After death:Unintentional injury	0.008 (0.032) *0.796*	0.106 (0.031) *0.001*	0.087 (0.037) *0.019*	0.062 (0.046) *0.176*	-0.013 (0.039) *0.747*
Deceased:After death	0.089 (0.020) *0.000*	0.028 (0.016) *0.081*	0.031 (0.015) *0.037*	0.025 (0.018) *0.173*	0.014 (0.016) *0.364*
Deceased:Age (centered at 60)	-0.011 (0.015) *0.442*	0.026 (0.023) *0.275*	0.008 (0.021) *0.721*	-0.027 (0.023) *0.237*	0.007 (0.025) *0.762*
After death:Age (centered at 60)	0.092 (0.008) *0.000*	0.058 (0.008) *0.000*	0.014 (0.008) *0.092*	0.002 (0.008) *0.774*	-0.022 (0.009) *0.015*
Deceased:After death:Age (centered at 60)	0.008 (0.012) *0.511*	-0.007 (0.018) *0.681*	-0.002 (0.017) *0.880*	-0.008 (0.015) *0.603*	-0.058 (0.017) *0.001*
Close friend interactions 25 to 34 (impanel)		0.093 (0.011) *0.000*			
Stranger interactions 25 to 34 (impanel)		-0.094 (0.015) *0.000*			
Stranger interactions 25 to 34		1.514 (0.015) *0.000*			
Close friend interactions 35 to 44 (impanel)			0.106 (0.013) *0.000*		
Stranger interactions 35 to 44 (impanel)			-0.128 (0.009) *0.000*		
Stranger interactions 35 to 44			1.561 (0.028) *0.000*		
Close friend interactions 45 to 54 (impanel)				0.096 (0.010) *0.000*	
Stranger interactions 45 to 54 (impanel)				-0.101 (0.015) *0.000*	
Stranger interactions 45 to 54				1.453 (0.038) *0.000*	
Close friend interactions 55 to 64 (impanel)					0.144 (0.015) *0.000*
Stranger interactions 55 to 64 (impanel)					-0.200 (0.023) *0.000*
Stranger interactions 55 to 64					1.281 (0.018) *0.000*
Observations	1,065,480	1,338,566	1,329,977	1,279,195	1,011,755

Note: Excludes observations less than two months from death. Robust standard errors shown in parentheses and p-values in italics. Continuous variables logged, scaled by standard deviation, and centered at logged mean, unless otherwise noted. "Impanel" variables are means over the January through June 2011 impanel period.

3.5.8 Compensation within close friend network

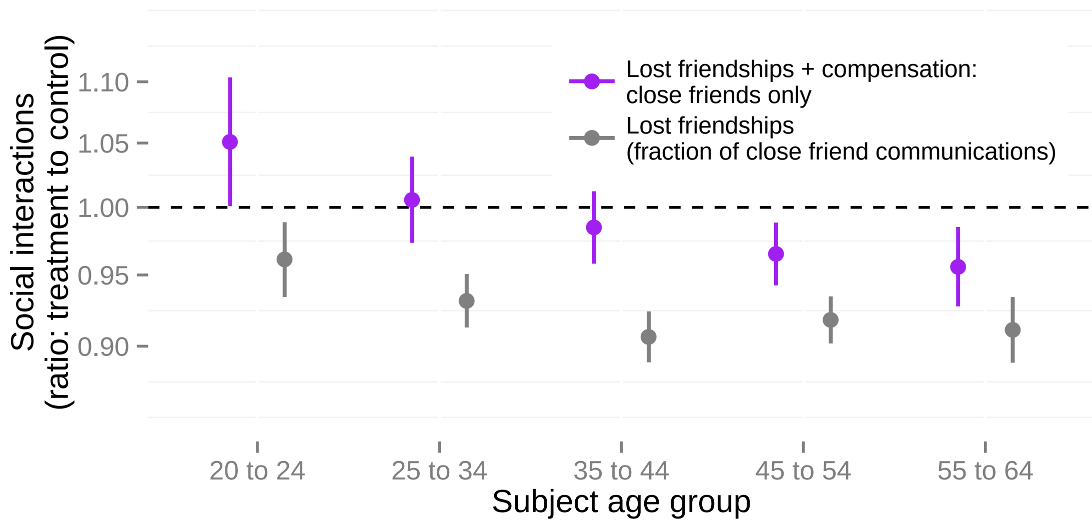

Figure 3.9: *Compensation effect, close friend network only.* Percent friendships recovered is the lost friendship coefficient divided by the compensation coefficient. Shown here, this is the purple (lost + compensation) estimate minus black (lost) estimate divided by black (lost) estimate.

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