Finding Love in the Wreckage:

Estimating Spousal Altruism with Data on Fatal Car Accidents

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Abstract. This paper estimates the degree of altruism among spouses by examining how often the driver of a car sacrifices himself or herself in order to save a spouse. Holding constant the magnitude of a collision, a driver can maneuver the car to distribute the risk from a collision between the driver and a passenger. We quantify spousal altruism by the degree to which drivers riding with their spouse redistribute the risk from a fatal accident to themselves – as measured by ex post mortality – as compared to drivers not traveling with their spouse. We find that drivers with their spouses are at least 37% more likely to sacrifice themselves. This implies that they value the lives of their spouses at least 37% more than the lives of other individuals.

A driver's position in a car provides him with exclusive control over the pedals and steering wheel. At the time of an impending accident, this control enables him to adjust the total amount of risk facing the car's occupants and how that risk is distributed. In other words, his maneuvers will have allocative and distributive effects on the occupants' risk. The driver's distributive choices reveal his relative preferences for himself over his passengers. The manifestation of injuries amongst the occupants allows observation of those relative preferences.

A theory of spousal altruism – indeed any romantic notion of marriage – would predict that a husband is more willing to sacrifice himself for his passenger when the passenger is his wife.[2] The central purpose of this paper is to test this prediction. We compare the probability that drivers travelling with spouses sacrifice their own life to the probability that drivers traveling with other women sacrifice their own life to estimate how much altruism drivers exhibit towards their spouses. We perform this comparison in a sample of two-occupant cars in which only the driver or the front seat passenger died. (We call these "one-death" cars). Assuming rational expectations, this selection guarantees that the drivers we compare are facing the same amount of risk, namely the death of one and only one occupant.

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[2] To keep the prose simple, we use the male pronoun to describe the driver and the female to describe the passenger.

Prior literature has proposed two motivations for interspousal transfers, reciprocity and altruism (Kolm & Ythier 2006). The former explains transfers as motivated by the transferor's expectation of compensation from his or her spouse, whereas the latter explains transfers as motivated by a transferor's "other regarding" preferences (i.e. preferences for others' utility). This paper offers an indirect test of these two theories. By focusing on a transfer that occurs at the end of a spouse's life, this paper rules out reciprocity as a motivation for altruism. If a driver is more willing to sacrifice himself for his spouse, then that transfer must be altruistic.

Our accident data comes from the Fatality Analysis Reporting System (FARS), which is a census of US auto fatalities that includes information on the context of the accident, the vehicles involved, and the occupants of those vehicles. The FARS data set does not identify the marital status of a car's occupants. To address this we merged the FARS data set with the Multiple Cause of Death (MCOD) data, which is a census of US decedents that includes their marital status. Because the MCOD is exclusively a census of decedents, we were unable to directly identify whether the surviving occupant in a one-death car was married. To solve this identification problem, we use a two-sample instrumental variable (TSIV) design (Angrist & Krueger 1992). In the first stage of the TSIV analysis, we estimate whether drivers and passengers are married using a sample of cars where both the driver and the front passenger died ("two-death cars"). Our excluded instrument is the square of the driver's age. Then we use that model to predict the probability that the driver and front passenger in one-death cars are married.

Our main second-stage structural equation is a linear probability model in which the dependent variable is whether the driver in a one-death car died and the independent variables include the predicted probability that the driver was traveling with his spouse. The spousal altruism hypothesis predicts that the coefficient on the probability of being married is positive. This is indeed what we find. In our base model, males are roughly 37 percent and females roughly 54 percent more likely to sacrifice themselves for spouses than for non-spouses. In more refined specifications, that treatment effect rises to approximately 53-83 percent for men and 92-105 percent for women. As a falsification test, we check if altruism disappears when a driver lacks control over his or her vehicle. We find that this is indeed the case when drivers are intoxicated, when the accident occurs in bad weather, when the car is struck from the rear (and the opposite if from the front), and when men drive on a wet surface.

Section 1 surveys the literature on spousal altruism. Section 2 describes the data we employ. Section 3 offers a theoretical justification for our method of estimating spousal altruism. Section 4 reports our empirical results. Section 5 concludes.

1. Literature review

Our study joins a great number that have looked at the distribution of resources within the family.[3] A brief survey of preceding theories and the results of their validations helps contextualize our own study as well as isolate its specific contributions to the field. Samuelson (1956) proposed a "unitary" model of intrahousehold exchange in which the head of household had exclusive control over resources that he then allocated. The allocation was directed by the head of household's utility function, which was itself dependent on the utilities of others in the household. A series of papers challenging the unitary model followed. (Lundberg 1988; Fortin and Lacroix 1997). These proposed a "collectivist" model under which household members separately controlled resources and transferred these resources through a process of negotiation with other members. Tests of the collectivist model have, for example, varied a household members' options outside the household (Chiappori et al 2002) or income within the household (Duflo and Udry 2003) to note that household expenditures shift to benefit the spouse with the decreased cost of exit, or increased income. These results are inconsistent with a unitary model under which only the aggregate household budget and the household members' preferences for consumption affect expenditures.

[3] Kolm and Ythier (2006) produced the Handbook of the Economics of Giving, Altruism and Reciprocity organizing much of the preceding literature on intrafamilial transfers and their explanations. A number of papers have studied interspousal exchange without making claims as to its motivation. Agarwal and Horowitz (2002) and de Laat (2005) present data on remittances from migrant workers. These studies do not seek to apportion the remittances between amounts due to altruism and amounts due to reciprocity. A second category of papers takes a different angle and examines domestic violence among married couples. For example, Dee (2003) and Stevenson and Wolfers (2006) find that easier access to divorce reduces domestic violence. These papers also do not distinguish between altruistic and egotistic motivations.

While the collectivist model may have displaced the unitary, the collectivist model without more does not predict intrafamilial transfers. That is because the collectivist model is consistent with both "altruistic" and "egotistic" preference structures (Chiappori 1992). The former allow for utility functions that depend on other family members' utilities such as that of Samuelson's original benevolent patriarch, whereas the latter only permit non-interdependent utilities amongst family members. Distinguishing between altruistic and egotistical motivations for transfers is difficult because compensatory transfers may occur at a different time or take a different form than the original transfer, or may never even manifest if the transfer functions as a premium paid for an informal insurance policy (Coate and Ravalion 1993, Thomas and Worral 1995, Ligon 2003). Disentangling the motivations is important because they determine the extent to which availability of market substitutes will crowd out intrafamilial transfers. If, for example, transfers are explained solely by an egotistic mutual insurance motivation the development of formal insurance markets will crowd them out when the benefits of diversification through contracts with unrelated parties overcome the information deficits of such unrelated parties (Foster and Rosenzweig 2001).

Because we choose a context where the transfer by nature is expected to take place near the end of life, our results provide evidence of interspousal altruism as distinct from reciprocity. In the intergenerational context, models of upward and downward indirect reciprocity have developed (Rapoport and Docquier 2006). Under the former model, children help their parents so that grandchildren help the children. Under the latter model, parents help children so that children help grandchildren. Either dynamic rests on generational pipelining, which has no obvious analog in the interspousal context.

Prior studies of interspousal transfers have rejected that spouses' preference structures reflect "perfect altruism." An agent exhibits perfect altruism when he is indifferent between personal consumption providing a unit of utility and another's consumption providing a unit of utility to that other. In other words, a husband will be perfectly altruistic if he vicariously experiences whatever his wife does. Papers have rejected the hypothesis of perfect interspousal altruism observing that exogenous increases of spousal income are followed by inadequate transfers from the affected spouse. For example, Chiappori et al (2002) estimate that a $1,740 ($2,240) increase in a wife's (husband's) annual income is followed by a $1,634 ($600) annual transfer from the wife (husband) to the husband (wife). Duflo and Udry (2003) report that rainfall patterns helpful to jointly controlled crops increase household expenditures on education and food consumption, whereas weather helpful to crops controlled separately by husbands (or wives) increases expenditures on adult and prestige goods (and food).[4] This paper adds to the preceding debate by being the first to provide evidence that perfect egotism (like perfect altruism) fails to explain interspousal tranfers.

2. Data

The results derive from two national censuses: the Fatality Analysis Reporting System (FARS) and the Multiple Cause of Death (MCOD) file.

Our treatment and control populations come from FARS, which reports all fatal traffic accidents occurring on public roadways that result in at least one fatality within 30 days of the crash so long as police were notified of the crash. (NHTSA 2005). The National Center for Statistics and Analysis of the National Highway Safety Administration (NHTSA/NCSA) administers FARS. Under agreements between the NHTSA/NCSA and individual states (as well as the District of Columbia and Puerto Rico), state agents trained in FARS protocols gather, translate and transmit data to NHTSA/NCSA, which publishes the data annually from 1975 until the present.

The FARS data include information on the location and circumstances of the accident, the vehicles involved in the accident and the occupants of each of those vehicles. Accident data include the date and time of day of the accident, as well as the weather, lighting and road conditions. Vehicle data include the car's model and year, its direction and speed of travel, and the angle of impact measured in clock angles (e.g. 12 o'clock for a frontal collision, 5 o'clock for back right). Person data include gender, age, seat position, whether an air bag and/or seat belt was used, and extent of injury. The data do not include the marital status of occupants. Though

[4] This class of studies is paralleled by several that reject perfect altruism between parents and children. For example, Altonji et al (1997) find that a $1.00 reduction in parental income reduces the amount transferred to children by only $.05 and a $1.00 increase in children's income reduces the transfer by only $.08.

unavailable to the public, the FARS dataset also includes the death certificate number of every decedent. A combination of the death certificate number and the state of death provided a unique identifier used to merge FARS data to MCOD data, which are described next.

The MCOD is a census of all deaths occurring within the United States. The National Center for Health Statistics (NCHS) maintains the MCOD dataset, publishing it annually since 1983. The dataset contains the cause(s) of death for each fatality, as well as personal information about the decedent including marital status. At our request, the Department of Transportation provided a dataset merging FARS and MCOD for each FARS fatality between 1990 and 1998.

3. Empirical method

An ideal field experiment to test for spousal altruism in car crashes would begin with a random sample of married individuals who are asked to drive a car. Each would be randomly assigned a passenger who was either (1) his or her spouse or (2) another person of the opposite gender. Each individual would then be exposed to a car crash where the aggregate mortality risk is held constant but the driver was able to distribute this mortality risk between him or herself and the passenger. The outcome would be the amount of risk the driver chose to retain. The treatment effect would be the difference between (1) the risk borne by the driver when driving with a spouse and (2) the risk borne by the driver when driving with a random partner of the opposite gender. The average treatment effect would amount to:

$$E(y_d \mid s_d, s_p, s_d \neq s_p, d = \text{married}, p = \text{spouse})$$

$$- E(y_d \mid s_d, s_p, s_d \neq s_p, d = \text{married}, p = \text{random non-spouse})$$

where y_d is the mortality risk borne by the driver, s_d and s_p are the sex of the driver and passenger, respectively, and d and p are the identity of the driver and front-right passenger, respectively. Fortunately for the subjects, we are not able to run this ideal experiment.

Our experiment departs from the ideal in four ways:

1. We do not observe a random sample of opposite-sex driving companions. Instead we have a sample of companions that were in car accidents: $E(y_d \mid s_d, s_p, s_d \neq s_p, d, p, \text{accident})$. Moreover, FARS only includes cars involved in accidents with at least one fatality. We cannot avoid the non-randomness due to selection of companions into accidents, fatal or not. But to limit the effect of the fatality selection criterion of the FARS data, some of our specifications will examine only cars involved in accidents in which there was at least one fatality in some other car. This subset of cars is one that is more random a selection from all cars in accidents than is the entire FARS dataset.

2. We do not observe the same person driving once with his or her spouse and once with some other, similar person of the opposite gender. What we have is observations on people who choose to drive together. This is a hurdle we cannot overcome. Therefore, our comparisons of spouses and non-spouses includes the effect of spousal status and selection effects due to the non-random sorting of driving partners. The selection bias is

$$E(y_d \mid s_d, s_p, s_d \neq s_p, d = \text{married}, p = \text{random non-spouse},)$$

$$- E(y_d \mid s_d, s_p, s_d \neq s_p, d = \text{married}, p = \text{chosen non-spouse})$$

Assuming that the altruism towards chosen driving partners is greater than altruism towards randomly-selected driving partners, this suggests that our estimate of altruism will underestimate spousal altruism.

3. We do not know the precise amount of mortality risk the driver thought he was free to distribute in an accident. We only observe an ex post realization of mortality, which is a mix of allocative effects, distributive effects, and uncontrollable effects. We bridge the gap between the driver's expectations about distributable risk and ex post realization of mortality in three steps. First, we assume the driver has rational expectations and rely on the law of large numbers. This implies that the driver's expectation of mortality risk is equal to the expected value of ex post realizations over a large number of accidents.[5] Second, we assume that the degree of control

[5] This is similar to the approach taken in Finkelstein and Poterba (2006), who assume ex post realizations of mortality are a good proxy for ex ante expectations of health risks in the context of annuity purchases. An implicit assumption embedded in our use of ex post mortality to proxy for ex ante expectation of mortality is that the

over the distribution of risks does not depend on spousal status. Third, we restrict our analysis to cars with two occupants and only one death. (We call these "one-death" cars.) The fact of one death is intended to control for the magnitude of risk that the driver distributed between herself and the passenger. Specifically, we assume that in every car accident where only one out of two total occupants died, the driver expected that he had the same amount of mortality risk to distribute between himself and the passenger and that this amount was less than one.[6]

4. The FARS data does not indicate whether the driver and passenger were married to each other. We address this problem in two steps. First, we merge the FARS data with the MCOD data to identify the marital status of the deceased occupant. Because the MCOD data do not provide the marital status of passengers who survived in one-death cars, it cannot identify spouses in these cars. We remedy this by using a two-sample instrumental variables (TSIV) design to identify whether the deceased occupant was married to the surviving occupant.

The first stage of the TSIV analysis uses a subsample of cars where both the driver and the passenger died ("two-death cars"). By definition each occupant is deceased, so the MCOD may be used to identify whether each is married. We define a driver and passenger paid as married *to each other* if they are both simply married.[7] Our excluded instrument for whether the driver and passenger are married is the square of the driver's age. The justification for this

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function that describes how mortality risk can be transferred from driver to passenger is linear. In other words, we have assumed that a 1% decrease in risk for the driver is associated with a 1% increase in risk for the passenger.

[6] Moreover, the nature of the risk distributed is not cumulative but alternative. In other words, although the driver is choosing p_0 for himself and p_1 for the passenger, p_0 (p_1) is not the probability that the driver (passenger) dies, but rather the probability that the driver (passenger) dies instead of the passenger (driver).

[7] This overestimates the number of driver-passenger pairs that are married to each other. Since the both-married variable is the left-hand side variable in stage one of the TSIV analysis, there is no bias so long as the overestimate does not depend on included or excluded instruments and there is a constant in the first stage model. Therefore, the important, implicit assumption in our first stage is that age or age-squared do not predict whether two people who are married, but not to each other, drive together. If this is not true, our second stage estimate will measure how much a married driver prefers another married passenger to a non-married passenger, whether or not the two are married to each other. If married drivers prefer their spouses to other married people, then our second stage result will underestimate the degree of spousal altruism.

instrument is that the fraction of cases in which the driver and passenger in a two-death car are both married rises and then falls with the age of the driver. This is illustrated in Figure 1. Moreover, while the probability that the driver dies in our second-stage model may depend on the age of the driver, it does not depend on the square of age.[8] To sum up, the first stage is a linear regression of whether the driver and passenger are married on, among other things, age squared in a subsample of two-death cars.

The second stage of the TSIV analysis returns to a sample of one-death cars. The outcome variable is whether the driver died. We use the estimated first stage parameters to predict the probability that the driver and passenger are married in one-death cars. This probability is the treatment variable. We employ a number of controls for characteristics of the driver and passenger (including other gender combinations, age, intoxication, and driving history), characteristics of the car, and characteristics of the accident (including whether the car rolled, the angle of collision, time of day, and location). Thus, our second stage is a linear regression of whether the driver died on the predicted probability of the occupants being married and controls. We adjust our second stage standard errors for the fact that our treatment variable is a predicted value.

We conduct our TSIV analysis separately for male and female drivers. To ensure our treatment and control groups are as similar as possible, we confine the sample to drivers who are married and driving with a passenger of the opposite gender. So, for example, our analysis compares male drivers with high probability of being married to their female passengers with male drivers with a low probability of being married to their female passengers to identify the effect of that probability on whether the driver dies, which we attribute to spousal altruism. Finally, to address well-known problems with the linear probability model, we also generate TSIV estimates using a logit model for both stages.

4. Empirical analysis

[8] We verify this by regressing the second stage residual on age squared and cannot reject the hypothesis that coefficient age squared is zero. Indeed, the estimated coefficient on age squared is very close to zero and has a small standard error (both usually orders of magnitude less than 1×10^{-5}).

Table 1 presents summary statistics for the control variables we include in our regressions. The columns present means (and standard errors) by subsamples, divided first by whether the driver was male (and passenger female) or the driver was female (and the passenger male) and subdivided further by whether the traveling companions are likely to be spouses or not.[9] The purpose of calculating means for these subpopulations is to determine whether the sorting of driving companions into the spouse and non-spouse groups and the sorting of spouses and non-spouses into accident scenarios is really random. It is apparent that spouses are likelier to drive younger, heavier cars, and use seat belts (but not airbags). Spouses are less likely to have traffic violations, drive drunk, drive fast, drive in the dark (but not bad weather). Spouses are more likely to be involved in head on or rear end collisions as well as side swipes in which vehicles were headed in opposite directions. On the other hand, spouses are less likely to be involved in side swipes with the vehicles heading in the same direction, less likely to be at fault in the accident, and less likely to attempt to avoid the collision by a maneuver other than a swerve. Spouses also tend to be older. Comparing spouses to non-spouses in cars with male drivers and female passengers (i.e. MF pairings), we observe that accidents are less likely to be caused by vehicular defects or involve angled collisions. MF pairs also tend to be driving faster relative to the velocity of the other vehicle involved in the accident, and more likely to swerve. The opposite tendencies are observed among FM pairs. These differences suggest significant non-random sorting that could affect the propensity of the driver (rather than the passenger) to die in a fatal crash. Fortunately we can control for the non-random sorting documented in Table 1. The question is whether there are variables we do not observe that are non-randomly distributed and are correlated with spousal altruism in a manner that is not captured by variables we do observe. This is a weakness we have previously identified and that Table 1 does not resolve.

A. Basic results and robustness checks

[9] The subsamples only include one-death cars where the driver and passenger are of opposite sex. For the purposes of table 1 only, male (female) drivers are deemed likely to be married to their passengers if their predicted probability of being married from our TSIV analysis is above the median predicted probability for all cars in the male (female) driver subsamples.

Our main, stage 2 regression results for FM and MF pairs are reported in Table 2. Specification (1) includes just the basic indicators for gender pairings and those pairings interacted with our spousal indicator. Specification (2) adds controls for whether the drive was intoxicated, whether the vehicle is an SUV or truck, and the age of the vehicle; (3) adds whether the road was dry and lit, whether driver and passenger were using safety devices, whether the driver was free from fault, whether the vehicle rolled over and the relative orientations of the vehicles involved in the accident; and (4) adds whether the weather was bad, whether the road was in an urban area, the number of traffic violations the driver received in the prior twelve months, whether the vehicle had a defect, and the speed and weight of the vehicle. Stage 1 results for each specification have R-squareds above 0.5 and pass weak instruments tests.

The second stage results confirm that both wives and husbands are likelier to sacrifice themselves for a spouse than another passenger. The first panel uses a spousal indicator based solely on the estimate from the first stage of the TSIV. Results from the first panel show that wives are 40 to 54 percent more likely to die when driving their husbands and husbands are 10 to 47 percent more likely to die when driving their wives. Subsequent panels and Table 3 act as robustness checks on these results.

The second panel reduces the false positives from the TSIV by allocating all cars where the decedent is not married according to the MCOD decisively to the control group (i.e. setting the parameter estimated in the first stage to zero). The correction reduces both the standard errors and amplifies the treatment effect, increasing the credence of the claim that the presence of spouses drives our results. Wives become 74 to 86 percent more likely to die when driving husbands and husbands become 56 to 76 percent more likely to die when driving wives.

The third and fourth panels repeat the two above after reducing the selection effect of the FARS sampling frame. These panels include only those vehicles that were involved in accidents in which at least one fatality occurred outside the vehicle, in other words, they only include those vehicles that would appear in FARS independent of whether an occupant died. Results in the third panel lose significance as sample size decreases by 95 percent. Coefficients are comparable in the first and third panels, but standard errors increase by a factor between two and three in the third panel. Significance returns in the fourth panel once false positives are removed. The fourth panel reports the highest estimate of spousal altruism with wives becoming 92 to 105 percent more likely to die when driving husbands, and husbands becoming 53 to 83 percent more likely to die when driving wives.

The fifth panel seeks to expand the sample size for the first stage by including cars with three and four occupants when estimating the likelihood of marriage.[10] Results in panel five are very close to panel one, with the measure of altruism decreasing up to three percent among wives and increasing up to four percent amongst husbands.

The sixth panel shows results for the sample from first panel using a logit regression rather than a linear regression. It presents coefficients expressed as odds ratios along with p-values. The results are consistent with those in the first panel, showing that they do not depend on a linear probability model.[11]

We can use the preceding results to estimate how much more spouses value each others' lives than the lives of other passengers. If we assume that drivers face risk exchange rates under which the sum of driver and passenger risk remains constant, the product of the additional risk borne by the driver when driving with a spouse with the driver's value of his own life provides an estimate of how much the driver would pay to preserve a spouse. If we use $10 million as the driver's value of his own life and apply that to the increased mortality observed under the third specification appearing in the base panel, a wife values her husband an additional $5.4 million over another passenger, while a husband values his wife $3.7 million more than another passenger.

B. Falisfication tests

A natural question is whether our findings of altruism are spurious. To address this concern, we run a series of falsification tests reporting their results in Table 3. Specifically, we repeat the analysis in contexts where drivers are likely to exercise less or more control over the distribution of risks. If spousal altruism does not increase with control, then one might suspect a

[10] The cost of increasing the sample size is introduction of a potential bias as married pairs driving with others in the back (e.g. children) may be older or younger than non-married pairs driving with passengers in the backseat.

[11] Estimation of logit regressions using samples from other panels yield similar results to corresponding linear regressions.

spurious correlation. On the other hand, if control increases the likelihood drivers die when driving their spouses, our results gain in credibility. Each panel in Table 3 focuses on a different subsample of observations taken from the first panel in Table 2.

The first panel focuses on intoxicated drivers. Our prior is that intoxication impairs a driver's responsiveness to risk thus decreasing his distributive activities and leading to a convergence of results between spouses and non-spouses. The results confirm our hypothesis, as the likelihood the driver dies when driving with a spouse falls between 20 and 33 percent for female drivers, and between 37 and 58 percent for male drivers.

The second panel focuses on vehicles struck in the rear. Drivers in such vehicles are less likely to be see the impact coming and be able to respond. Moreover, even if the driver is able to redirect the impact, the driver's and passenger's interests are aligned in that neither would benefit from the impact being shifted forward. In the absence of a conflict of interests between driver and passenger, the relative preference a driver may have for a spouse over another passenger would not be expressed. The results in panel two confirm our hypothesis, as the likelihood the driver dies when driving with a spouse falls by between 28 and 55 percent for female drivers, and 23 and 71 percent for male drivers. (Note that the sample size falls precipitously in these contexts because cars hit from behind rarely suffer fatalities and thus do not show up in our subsample of cars with at least one fatality.)

The third panel exploits the same intuitions by focusing only on those vehicles that were involved in head-on collisions. The orientation of the impact makes it more likely that the driver sees it. At the same time, there is a conflict between the interests of the driver who wants to rotate the impact towards the right and those of the passenger who would be likelier to survive when the impact was shifted to the left. The results in panel three confirm our hypothesis: the likelihood the driver dies when driving with a spouse increases by between 7 and 10 percent for female drivers, and 6 and 14 percent for male drivers.

The fourth panel is limited to vehicles that did not have a defect at the time of the accident. Assuming that a defect interferes with control, we would expect the limitation to amplify spousal altruism. These expectations are not met as the likelihood the driver dies when driving with a spouse decrease between 1 and 3 percent for female drivers, and between 0 and 3 percent for male drivers.

The fifth panel includes only those accidents that occurred on a wet surface. We would expect to see the measure of spousal altruism decrease as drivers enjoy less control on a wet surface. The results for female drivers contradict our expectations. The likelihood the driver dies when driving with a spouse *increases* by between 24 and 40 percent for female drivers, but *decreases* between -2 and 17 percent for male drivers.

The sixth panel uses only those accidents that occurred in bad weather, based on the same intuition as the fifth panel. Weather is identified as bad when there is rain, sleet, snow, or fog. In line with expectations, the loss of control in bad weather decreases the measure of altruism. The likelihood the driver dies when driving with a spouse decreases by between 2 and 6 percent for female drivers, and between 3 and 6 percent for male.

The seventh (last) panel looks only at those accidents occurring at night on unlit roads. The expectation is that in the absence of light, the driver will be less likely to see the threat and thus to respond to it by distributing risk. Though the effect of treatment is expected to decrease, the opposite occurs. The likelihood a driver dies when driving with a spouse increases by 20 to 40 percent for females drivers, and by 1 to 25 percent for male drivers.

Our falsification tests on balance support our finding of spousal altruism. Results observed when drivers lose control to intoxication or bad weather support our results by showing that altruism decreases as drivers lose the ability to act on their preferences. Results from rear-end and head-on collisions likewise support our hypothesis, whether because drivers are better able to see (and respond to) what is in front of the car than what is behind, or because alignment of interests removes distributive choices. On the other hand, results based on accidents occurring in the dark and on wet surfaces (with female drivers) suggests that reducing the driver's control may *increase* the gap between spouses and non-spouses.

5. Conclusion

It has been said that one death is a tragedy, a million are a statistic. Without denying that our data are an aggregate of tragedies, we present the aggregate as something more. A tragedy can be appreciated. A statistic can also be learned from. Like other papers before it (Klasen 1998; Dee 2003) this paper finds evidence of spousal exchange in death. We use that evidence to reconstruct tens of thousands of real life dictator games that played out before fatal car crashes. In such contexts, drivers are natural dictators having exclusive control over the amount of risk faced by the occupants and how it is distributed between them. We provide evidence that drivers' distributive choices depend on the identity of their passenger. Though our measures of altruism vary with panel and specification, a conservative model observes wives saving husbands 54 percent as often as other passengers, and husbands saving wives 37 percent as often as other passengers. On net, our theory survives a battery of falsification tests as the relative difference between spouses and non-spouses declines in contexts where the driver is less likely to distribute risk. If we assume that total risk faced by the occupants remains constant while the driver distributes it, our results show that wives would die for approximately two husbands, and husbands would die for approximately three wives. Even without an assumption as to the form of the risk exchange function, we provide evidence that spouses continue transfers in the moments before death – evidence that is inconsistent with a purely egotistic explanation of spousal transfers.

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Table 1. Descriptve statistics for outcome and control variables in base sample, by predicted probability that married driver is riding with spouse.

		Female driver				Male driver			
		Below median probability		Above median probability		Below median probability		Above median probability	
	Units	Mean	SE	Mean	SE	Mean	SE	Mean	SE
Driver died	0/1	0.37	0.48	0.40	0.49	0.35	0.48	0.43	0.50
Specification 1 includes									
Age of driver	Yrs	37.78	24.68	52.92	13.19	47.04	28.29	54.41	12.93
Specification 2 adds									
Driver intoxicatred	0/1	0.31	0.46	0.23	0.42	0.28	0.45	0.23	0.42
Vehicle is a car	0/1	0.68	0.47	0.60	0.49	0.67	0.47	0.53	0.50
Age of vehicle	Yrs	7.06	5.20	6.57	5.20	7.40	5.55	7.15	5.90
Specification 3 adds									
Vehicle rolled over	0/1	0.40	0.49	0.39	0.49	0.32	0.46	0.32	0.46
Driver-side airbag	0/1	0.22	0.42	0.22	0.41	0.21	0.41	0.20	0.40
Passenger airbag	0/1	0.17	0.37	0.16	0.37	0.16	0.36	0.15	0.35
Driver used seatbelt	0/1	0.47	0.50	0.56	0.50	0.46	0.50	0.51	0.50
Passenger belt use	0/1	0.38	0.48	0.46	0.50	0.44	0.50	0.50	0.50
Driver at fault	0/1	0.76	0.43	0.72	0.45	0.71	0.45	0.71	0.45
Driver not at fault	0/1	0.29	0.46	0.34	0.47	0.34	0.47	0.35	0.48
Dark out	0/1	0.31	0.46	0.24	0.43	0.28	0.45	0.26	0.44
Surface was dry	0/1	0.85	0.36	0.81	0.39	0.82	0.38	0.78	0.41
Rear-end collision	0/1	0.028	0.165	0.053	0.225	0.047	0.213	0.062	0.241
Head-on collision	0/1	0.104	0.305	0.118	0.323	0.104	0.305	0.172	0.377
Angled collision	0/1	0.164	0.370	0.181	0.385	0.231	0.421	0.204	0.403
Side-swipe, same direction	0/1	0.064	0.245	0.055	0.228	0.069	0.253	0.053	0.223
Side-swipe, opp direction	0/1	0.005	0.070	0.007	0.083	0.006	0.080	0.008	0.089
Specification 4 adds									
Speed	Mph	66.62	28.03	60.98	27.81	62.70	29.94	61.74	28.14
Vehicle defect	0/1	0.06	0.25	0.06	0.25	0.07	0.25	0.06	0.24
Vehicle weight	Lb	3035.7	717.3	3217.2	736.4	3111.6	698.1	3281.6	748.0
Bad weather	0/1	0.09	0.29	0.14	0.35	0.12	0.33	0.16	0.36
Urban road	0/1	0.32	0.47	0.27	0.44	0.33	0.47	0.28	0.45
Traffic viols. in last 12 mo.	#	0.30	0.73	0.21	0.63	0.36	0.82	0.25	0.69

Note. Dirver at fault and driver not at fault do not sum to one because there are cases where driver fault is uncertain.

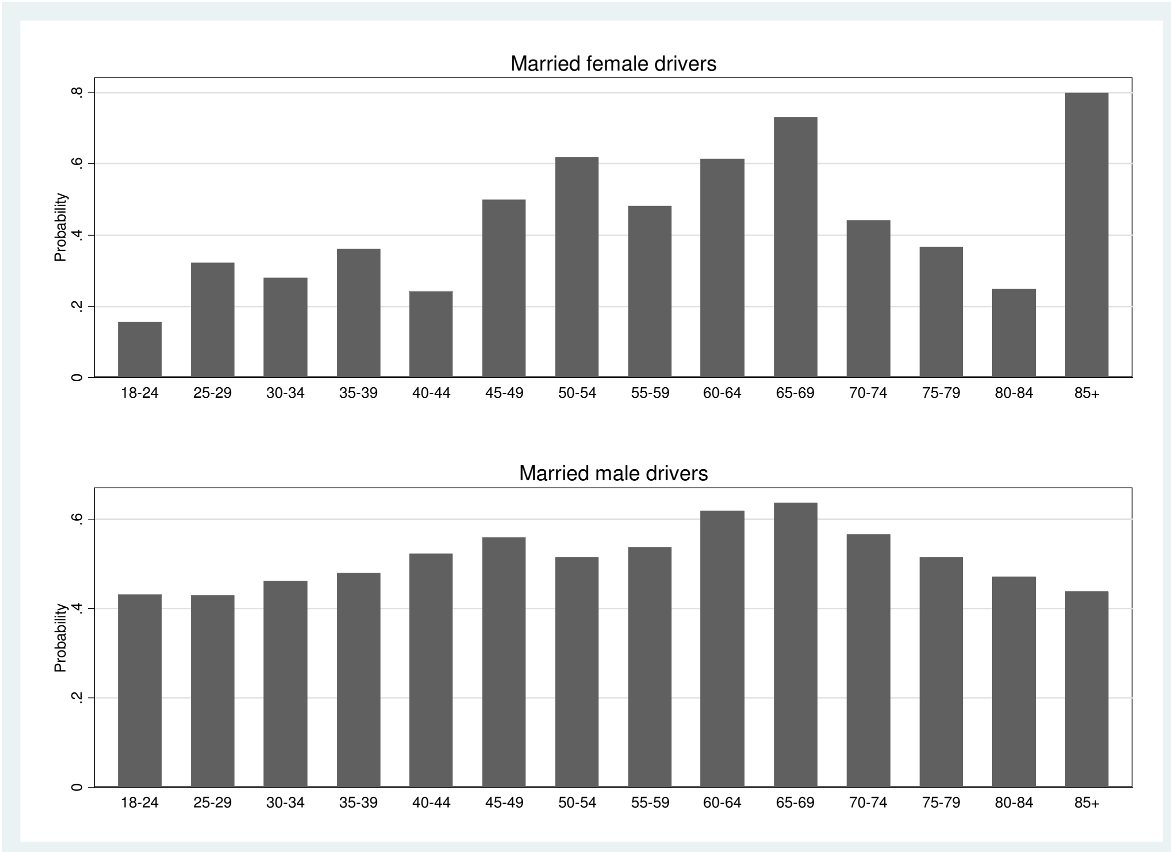
Table 2. Results from stage 2 of TSIV estimation.

	Female				Male			
	1	2	3	4	1	2	3	4
1. Base model (linear probability model)								
Coeff	0.45 ***	0.40 ***	0.54 **	0.52 **	0.47 ***	0.42 ***	0.37 ***	0.10
Std err	0.16	0.14	0.25	0.25	0.12	0.12	0.11	0.09
R-squared	0.40	0.40	0.44	0.47	0.40	0.40	0.44	0.48
N Stage 1	296	296	271	271	897	889	826	826
N Stage 2	6174	6134	4897	2008	16954	16864	14591	5229
2. Base model with refined prediction								
Coeff	0.86 ***	0.84 ***	0.86 ***	0.74 ***	0.76 ***	0.76 ***	0.69 ***	0.56 ***
Std err	0.10	0.10	0.11	0.16	0.05	0.04	0.04	0.06
R-squared	0.45	0.45	0.49	0.49	0.45	0.45	0.48	0.49
N Stage 2	6174	6134	4897	2008	16954	16864	14591	5229
3. Stage 2 excludes selected cars								
Coeff	0.58	0.48	0.43	0.16	0.40	0.38	0.33	0.15
Std err	0.41	0.39	0.50	0.68	0.26	0.28	0.27	0.39
R-squared	0.50	0.50	0.56	0.57	0.48	0.49	0.54	0.61
N Stage 2	244	243	232	102	888	882	859	348
4. Stage 2 excludes selected cars and uses refined predictions								
Coeff	1.02 ***	0.95 ***	1.05 ***	0.92 **	0.83 ***	0.81 ***	0.75 ***	0.53 ***
Std err	0.23	0.21	0.23	0.36	0.11	0.11	0.11	0.20
R-squared	0.54	0.55	0.61	0.60	0.52	0.52	0.57	0.62
N Stage 2	244	243	232	102	888	882	859	348
5. Base model with stage 1 including 3 and 4 occupant cars								
Coeff	0.44 ***	0.39 ***	0.52 **	0.49 **	0.51 ***	0.46 ***	0.41 ***	0.11
Std err	0.14	0.12	0.21	0.22	0.13	0.13	0.12	0.11
R-squared	0.40	0.40	0.44	0.47	0.40	0.40	0.44	0.48
N Stage 1	336	336	309	309	1056	1047	978	978
N Stage 2	6174	6134	4897	2008	16954	16864	14591	5229
6. Base model estimated with logit regression								
Odds ratio	1.65	1.54	1.85	1.80	1.64	1.57	1.50	1.11
P-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.27
N Stage 2	6174	6134	4897	2008	16954	16864	14591	5229

Table 3. Stage 2 results from falsification tests.

	Female					Male				
	1	2	3	4	5	1	2	3	4	5
1. Intoxicated drivers										
Coeff	0.17	0.20	0.34	0.19	0.05	-0.11	-0.10	0.00	-0.33	-1.40
Std err	0.20	0.25	0.26	0.37	1.04	0.14	0.12	0.13	0.27	0.79
R-squared	0.46	0.46	0.53	0.59	0.86	0.40	0.40	0.46	0.56	0.64
Obs	1670	1660	1270	464	47	4255	4243	3396	1039	102
2. Car struck in back										
Coeff	0.13	0.12	0.00	0.08	-3.84	0.22	0.19	0.13	-0.61	-0.76
Std err	0.33	0.32	0.41	0.61	0.00	0.26	0.29	0.27	0.42	0.79
R-squared	0.36	0.37	0.47	0.53	1.00	0.44	0.44	0.51	0.56	0.71
Obs	231	230	226	103	27	709	702	693	296	91
3. Cars in straight-on collisions										
Coeff	0.53 ***	0.50 ***	0.64 **	0.62 **	0.33	0.59 ***	0.56 ***	0.45 ***	0.16	-0.22
Std err	0.19	0.18	0.30	0.31	0.45	0.15	0.16	0.14	0.14	0.29
R-squared	0.39	0.39	0.44	0.50	0.58	0.41	0.42	0.46	0.50	0.54
Obs	2286	2275	2242	866	175	6806	6776	6692	2346	541
4. Excluding cars with defects										
Coeff	0.45 ***	0.39 ***	0.52 **	0.51 **	0.43	0.46 ***	0.42 ***	0.36 ***	0.08	-0.04
Std err	0.15	0.13	0.24	0.25	0.33	0.12	0.12	0.11	0.09	0.19
R-squared	0.40	0.40	0.44	0.47	0.51	0.41	0.41	0.44	0.48	0.51
Obs	5775	5738	4618	1900	408	15860	15775	13741	4969	1209
5. Wet surface										
Coeff	0.76 ***	0.65 ***	0.94 **	0.90	1.43	0.49 ***	0.35 **	0.37 **	-0.08	0.23
Std err	0.28	0.24	0.43	0.69	0.94	0.16	0.15	0.15	0.33	0.49
R-squared	0.41	0.42	0.46	0.56	0.77	0.40	0.41	0.44	0.47	0.50
Obs	1045	1041	918	112	61	3308	3293	2944	378	212
6. Bad weather										
Coeff	0.43 ***	0.38 ***	0.49 **	0.46 *	0.35	0.44 ***	0.39 ***	0.32 ***	0.04	-0.09
Std err	0.15	0.13	0.23	0.24	0.33	0.11	0.11	0.10	0.10	0.20
R-squared	0.40	0.40	0.45	0.48	0.51	0.40	0.41	0.44	0.49	0.51
Obs	5457	5420	4258	1757	371	14619	14542	12494	4519	1110
7. Accident was in darkness										
Coeff	0.70 ***	0.60 ***	0.90 **	0.92 *	1.78	0.47 ***	0.46 ***	0.53 ***	0.34 *	0.58
Std err	0.26	0.23	0.43	0.53	1.66	0.15	0.16	0.18	0.21	0.45
R-squared	0.42	0.42	0.47	0.52	0.74	0.40	0.40	0.45	0.51	0.53
Obs	1671	1662	1286	484	74	4550	4527	3709	1321	236

Figure 1



Married female drivers
Probability
0
.2
.4
.6
.8
18-24
25-29
30-34
35-39
40-44
45-49
50-54
55-59
60-64
65-69
70-74
75-79
80-84
85+
Married male drivers
Probability
0
.2
.4
.6
18-24
25-29
30-34
35-39
40-44
45-49
50-54
55-59
60-64
65-69
70-74
75-79
80-84
85+