
Jonathan B. Scott

Texas A&M University

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Abstract

I analyze a vehicle arms race as a potential contributing factor to the rise in the size of the U.S. vehicle fleet. In order to identify an arms race effect, I analyze salient outcomes in which large vehicles produce in vehicle accidents. Using a difference-in-differences approach, I exploit the quasi-randomness of deaths in fatal accidents to identify changes in household vehicle purchasing behavior. I find that households neighboring an individual who died in an accident respond by purchasing significantly larger and safer vehicles. Exploiting the relationship between the size of the cars involved in the accident and the severity of the outcome, I explore how these weights may impact vehicle adoption through means other than victim fatality. I test for an indirect response by neighbors to the weight of the opposing vehicle involved in the accident. I find that neighbors of those involved in the accident respond to a 1,000 pound increase in opposing vehicle weight by increasing average household weight by up to 20 pounds.
1 Introduction

The average size of the U.S. vehicle fleet has risen steadily over the years. According to the National Highway Traffic Safety Administration (NHTSA), passenger cars have seen a 12.5% increase in equivalent test weight, and light trucks have increased by 26% from model year 1984 to 2004. These increases in vehicle sizes could be driven by sustained shocks to fuel prices or income growth. Another factor may be strong preferences for vehicle size, or in particular, a strategic response by consumers to other heavy cars on the road.

People often purchase larger vehicles in response to preferences for safety. The literature on the vehicle “arms race” (e.g., White, 2004[14]; Li, 2012[10]) discusses these preferences for safety, wherein consumers buy increasingly larger vehicles to protect themselves from the larger vehicles on the road. This type of contagion effect, inherent in a vehicle arms race, produces higher emissions and increased risk on the road as vehicles increase in size. If these types for strategic responses from consumers are not welfare increasing, it creates a feedback loop that causes the fleet to be heavier than it would otherwise be absent the strategic motive by households to purchase large vehicles.

The traditional view is that heavier vehicles provide more safety when experiencing an accident (Crandall and Graham, 1989[6]). However, consumers who protect themselves by buying small trucks and SUVs create an increased risk on the road to other drivers. It is well documented that large vehicles create an increased risk of fatality to the opposing driver when involved in an accident. For example, Jacobsen (2013)[9] finds that a 1,000 pound increase in the weight of a vehicle involved in an accident increases the number of fatalities in other vehicles by about 46 percent, but reduces own risk by 53 percent. Anderson and Auffhammer (2014)[2] find, conditional on the occurrence of an accident, a 1,000 pound increase in vehicle weight is associated with a 47% increase in the baseline fatality probability. Evans (2001)[7] finds that the effects of adding mass, in the form of a passenger, adds to the increased risk of fatality in head on collisions. He finds that adding a passenger to a car leads to a 7.5% reduction in driver’s risk of fatality, while increasing the risk of fatality to the other driver by 8.1%.

These types of safety attributes create strong preferences for large vehicles. Because of the positive relationship between vehicle size and fuel efficiency, increased demand for heavier vehicles have a countering effect on policies set
forth to improve vehicle fuel economy. The Corporate Average Fuel Economy (CAFE) standards, enacted in 1975, aim to reduce energy consumption by increasing the fuel economy of the U.S. vehicle fleet. The CAFE standards have had positive effects on improving average vehicle miles-per-gallon (MPG), however, the vehicle arms race creates an opposing dynamic, reducing the overall potential of CAFE.

There has been a lot of conjecture that the vehicle arms race has played a major role in the growth in size of the vehicle fleet presented in Figure 1. However, there has been no credible evidence of this. The reason is in the difficulty in identifying such an effect using naturally occurring field data. The thought experiment would be to randomly assign different households in a region large vehicles and observe take-up among other households. Such an experiment would isolate confounding factors such as common shocks and unobserved characteristics that are common across households, and also predict preferences for vehicle size. Random assignment would also mitigate concerns of simultaneity bias; we would be able to properly identify the causal direction of the treated households impact on other households. Such large scale experiments are often infeasible in practice.

Figure 1: U.S. Average Equivalent Test Weight (ETW)
In this paper, I show that preferences for vehicle size can be pronounced in the presence of a salient shock to consumers’ perception about vehicle safety. That is, I investigate whether people respond directly to the salient outcomes that large vehicles produce, for instance, the death of an individual in a vehicle accident. I examine accidents involving at least one fatality and use the quasi-experimental nature of death in the accident to estimate its impact on the purchasing behavior of the neighbors nextdoor to the involved vehicle owner. I will then argue that the neighbor should respond to the weight of the opposing vehicle in the accident, only through its salient effects on the nearby household involved (e.g., a fatality). This effect of the opposing vehicle’s weight may be visible to the household of interest in terms of severity of the accident, such as injuries or death of the nearby neighbor. That is, on average, a larger opposing vehicle invokes more harm on the neighbor involved in the accident.

When examining the effects coming from a neighbor’s fatality, it is important to recognize that the assignment of death between parties involved in the accident is not purely random. As discussed, there is a strong relationship between the mass of a vehicle and the likelihood that a fatality takes place in it. This means, neighborhoods with small vehicles may be more likely to be treated with the death of a neighbor in an accident. This non-random assignment of treatment can lead to selection problems.

To account for these confounders, I utilize the entire time series of my data, and the multiple vehicle purchases a household makes over time. Through a difference-in-differences approach, I account for the heterogeneity over households that is constant over time. Using the neighbors of those who survived a fatal accident as a control group, I can predict the consumption patterns of the households treated with a neighbor vehicle fatality in absence of treatment. An accurate prediction of the counterfactual consumption path of the treatment group depends on how similar they are to the control group prior to treatment, in a parallel sense. That is, identification of the average treatment effect on the treated (ATET) requires that, without treatment, the difference in consumption between these two groups are constant over time.

I show evidence supporting parallel trends between the two groups and I examine the impact of a neighbor dying in an accident on preferences for vehicle safety. I test whether these fatalities affect the mass of household vehicle fleets. I show evidence that average weight of the household vehicle...
stock increases by 20–30 pounds following the death of a neighbor involved in an accident. Similarly, the probability of a household owning at least one passenger car—as opposed to owning all vans, SUVs, or trucks—decreases by about 2%. I also present evidence that these households increase their stock of vehicles with airbags.

Because I use households neighboring a survivor of an accident as the control group—who are most likely also affected by a neighbor’s accident—I consider my estimates of these treatment effects to be a lower-bound of the true effect. Another reason these estimates should be considered a lower-bound effect is because of the measurement error entering my treatment variable. In order to match data on vehicle accidents to vehicle ownership, I must match probabilistically on vehicle and owner characteristics. This approach can lead to significant measurement error, possibly assigning treatment to households who were not treated. This measurement error on the right-hand side can lead to significant attenuation bias in my estimates of treatment effect.

I conclude my estimation results by presenting evidence of an indirect arms race effect. That is, because of the relationship between opposing vehicle weight and severity of accident, I can estimate the reduced form effect of opposing vehicle weight on neighbor weight. This effect will be indirect in the sense that this vehicle's weight is not salient to the household neighboring the crash victim, but only visible to the household of interest in terms of its impact on the outcome of the accident. I show that there is a significant indirect response by households to the size of the vehicle that collided into the neighbor. This exercise suggests that the control group (households who’s neighbor survived the accident) are affected as well.

I consider these responses by consumers to be evidence of information shocks to perceived safety. This is primarily due to the salient information that these households are treated with. That is, a household now becomes aware of a neighbor’s fatality in a vehicle accident. This type of information presumably and evidently creates an adjustment in preferences for vehicle attributes related to safety. The primary driver of these changes in preferences is plausibly due to a change in beliefs about the safety of their current vehicles.

In this paper, I do not take a stand on potential behavioral mechanisms that could be driving these changes in preferences. One can speculate that these
responses may be a result of projection bias (Lowenstein et al., 2003[11]; Conlin et al., 2007[5]; Busse et al., 2014[4]), anchoring heuristics (Slovic, 1967[12]; Tversky and Kahneman, 1974[13]; Crandall and Graham, 1989[6]; Furnham, 2011[8]), or other assimilative biases. Identifying any behavioral mechanisms that may be driving these responses is not feasible given the information that I have. Furthermore, I cannot reject the possibility that consumers are simply rationally updating beliefs from incorrect priors.

The paper proceeds as follows. In section I, I lay out the groundwork for my empirical identification strategy and identification assumptions. In section II, I will discuss the data and data construction. I continue on to my primary estimation results of the effect on neighbor death in section III for various outcomes, including household vehicle size. Section IV verifies the robustness of these findings by presenting various falsification tests. In section V, I present evidence of an indirect arms race effect. Finally, I conclude my paper in section VI with a brief discussion of the implications of my findings.

2 Empirical Strategy

To study the effects of a vehicle arms race, I narrow my focus to salient outcomes which large vehicles produce in an accident. Specifically, I study nextdoor neighbors of individuals involved in fatal accidents. Among those involved in an accident with a fatality, some will live and others will die. Thus, some of the nextdoor neighbors will be “treated” with the knowledge that someone who they likely knew died in a vehicle accident. I view this treatment as an information shock to perceived vehicle safety, which is most likely influenced by the size of the vehicles involved in the accident. The control group include those who had a neighbor involved in accident, but whom did not die. I view the control group, therefore, as a good representation of the treatment group absent a neighbor’s death.

My empirical strategy is to implement a difference-in-differences estimator of the treatment effect of neighbor’s death, as an information shock to perceived safety. The key identifying assumption is that subsequent vehicle purchasing behavior of the treated group would have followed a “parallel” pattern of the control group absent treatment. I test if the two groups are “parallel” absent treatment by testing if trends in outcomes were similar prior to the accident.
The assignment of death in a vehicle accident which involves at least one
death is plausibly random. However, my results will not hinge on this as-
sumption. There are multiple reasons why this assignment may indeed be
endogenous. For instance, Anderson and Auffhammer (2014)[2] find, condi-
tional on being in an accident, being hit by a vehicle 1,000 pounds heavier
generates a 40-50% increase in risk of a fatality. This finding implies a neg-
ative correlation between own vehicle weight and the likelihood of death in
an accident. This correlation could be a confounding factor in an empirical
design which assumes exogeneity of treatment. Given a high degree of simi-
laritiy and homophily within a neighborhood, this would lead to a higher rate
of treatment within neighborhoods driving smaller vehicles. In this sense,
treating a neighbor’s death as purely exogenous could lead to a biased and
inconsistent estimate of the impact on neighbor vehicle size.

Fortunately, for most households, I observe multiple vehicle purchases be-
tween 2004 and 2010. Therefore, any confounding factors that are con-
stant over time, within households, can be controlled for. I implement a
difference–in–differences approach to estimate the effect of neighbor deaths
on household purchasing behavior. I estimate this effect using a fixed effect
estimator, where my identification assumption is conditional independence.
That is, after controlling for constant effects within households and over
time, the remaining variation in treatment status is independent of unob-
servables. This assumption implies that we can identify the average treat-
ment effect on the treated (ATET) by estimating the following equation.

\[
y_{it} = \alpha_i + \gamma_t + \beta \cdot \text{neighbor\_died}_{it} + \varepsilon_{it}
\]  \hspace{1cm} (1)

where \(y_{it}\) is the outcome of interest for household \(i\) in period \(t\) and \text{neighbor\_died}_{it}
is an indicator which equals one if household \(i\) died in a vehicle accident in
some period \(s \leq t\), and zero otherwise. The parameter of interest is \(\beta\)
which estimates the ATET. Key outcomes examined in this paper include
the adoption of vehicle safety attributes, such as weight and airbags, as well
as other characteristics such as vehicle fuel efficiency. This design implies
that we can predict the counter-factual pathway of these outcomes in treated
households in absence of treatment. This requires a control group that can
track the treatment group well.

Notice that \text{neighbor\_died}_{it} is a function of the size vehicles involved in the
accident (Anderson and Auffhammer (2014)[2]). That is, all else equal, the
heavier the opposing vehicle in a two-car collision, the higher risk of death to the neighbor. However, the size of this opposing vehicle may influence the outcome variable (e.g., choice of vehicle size) in ways other than the fatalities. It may influence other salient outcomes, such as injuries to the neighbor, which may also trigger a response by the household. I will examine this relationship between opposing vehicle weight and household choice of vehicle size in section 6.

I will estimate equation 1 using, as my control group, households neighboring an individual who was involved in a fatal accident, but who survived it. The intuition is that this group of individuals will likely behave similar to the treatment group on average. Notice that it is very likely that the control group may have larger vehicles on average than those of the treatment group. This might be the case because of the correlation between vehicle size and likelihood of death in an accident, as well as the correlation in characteristics among neighbors. This alone does not violate the identifying assumption. It is only required that these differences remain fixed over time, pre-treatment.

Using the neighbors of the survivors as the control group could lead to smaller estimates than if the control group included households who had no neighbor experience a car accident at all. This could be the case if the control group was affected (in the same direction) by their neighbor’s accident too, even though that neighbor survived. However, using the survivors as the control group should provide robust estimates because of the presumed similarity they have with the treatment group, but with a distinguishing feature (i.e., surviving) that will help disentangle the effects of a neighbor’s death.

It is also possible that using the survivors as my control group could make the effect seem larger than it actually is. This could be the case if the neighbor’s of these survivors decided to purchase smaller vehicles following the accident. Therefore, to lead to an over-sized estimate of the effect on the treatment group, I would need the control households to be unrealistically conscious of the accidents and willing to reduce the size of their vehicle to “save” others’ lives. I view this social utility hypothesis as unlikely to be the case, and thus, posit that my estimates will be a lower-bound of the true effect of treatment. With that said, in section 5.4 I will explore an alternative control group and show that my choice of control group does not seem to matter.
3 Data

The data primarily come from a combination of vehicle registrations and accident records. For vehicle accidents, I use information on accidents involving at least one death. These data come from the Fatality Analysis Reporting System (FARS) and are supplemented with the Texas Transportation Institute (TTI) dataset. All accidents data are observed over the span of 2006 through 2010. For household vehicle ownership, I use confidential administrative records maintained by the Texas Department of Motor Vehicles (DMV). These data include registration records for 2004 to 2010. I decode each VIN in the DMV dataset using a database obtained from DataOne Software. This provides me with various attributes associated with the vehicles, such as make, model, year, weight and MSRP.

The DMV records provide me with household addresses, which I use to match neighbors to the individuals involved in a car crash. For these neighbors, I look at multiple outcome variables related to their stock of vehicles. These include transactions, household vehicle fleet attributes, and a measurement of fleet value. To measure the value of a household’s vehicle fleet in a given month in time, I take the average transaction price at that time for those particular vehicles. Narrowing the description of the vehicle to the 10-digit substring of the VIN allows me to account for characteristics beyond make and model, such as trim.

Since the primary focus of this paper is to estimate the impact of specific safety-related information shocks, I will be focusing on various outcomes associated with safe vehicle adoption. These include attributes such as vehicle size, MPG, vehicle type, and airbag systems. For vehicle size, I use the vehicle’s listed curb weight from the DataOne Software. This dataset also provides me with a variable describing each vehicle’s restraint system. I query each vehicles restraint system description for whether it includes the word *airbag*. I will use this indicator as a proxy for whether a given car has at least one airbag.

3.1 Fuzzy Matching Accidents to Owners

Due to an absence of VIN or household-level information in the FARS dataset, I make use of additional information in the TTI data to “fuzzy” match fatal vehicle accidents to the households in the DMV dataset in which
the victims belong to. This process involves two layers of “fuzzy” matching, in which I first match key TTI information to FARS observations, and then match fatal accidents to vehicle owners in the DMV data.//

The Texas fatal vehicle accidents in the FARS dataset are a small subset of the TTI dataset, which records information on all vehicle accidents in Texas, whereas FARS records only accidents involving a death. I can match these two datasets together using information on the time and location of the accident, as well as the make, model and year of the cars involved. I merge these two datasets together in order to supplement the FARS dataset with the vehicle owner names recorded in the TTI data. With this supplemented information, I will be able to more accurately match victims of fatal accidents to households in the DMV.

Due to minor inconsistencies in the data, I must fuzzy match the TTI to FARS. For instance, geo-coordinates, as well as the exact time of the crash, may differ slightly between datasets. Therefore, my first condition for a vehicle listed in FARS to be a candidate match for a vehicle listed in TTI is an exact match in vehicle model year. Second, I set an arbitrary maximum distance of 4,000 meters between crashes in each dataset that must be satisfied in order to be considered a match. After these two conditions are met, I am left with a much smaller set of candidate TTI vehicles for each FARS vehicle. I then sort the remaining candidates by distance in time of crash. After this procedure, if I am left with a non-unique match on time, then I also sort by geographical distance of crash. It is rarely the case that I am left with a non-unique match following these conditions. When the result here is non-unique, it is more than likely the case that these vehicles were involved in the same accident, and each vehicle had the same model year.

To narrow it down to a unique match, I then choose the match with the longest common substring in terms of vehicle make. I store all unique matches between the two datasets and proceed to match these fatal accidents to DMV records. From the TTI dataset I recovered a key element of this matching procedure—vehicle owner first and last name. From the FARS data, I have the 12-digit VIN number. This identifies most vehicle attributes (e.g., trim) though does not identify the exact vehicle.

Therefore, I first require an exact match on 12-digit VIN. I proceed by fuzzy matching to the DMV dataset on first and last name jointly using the Lev-

\[1\] Vehicle make in the two datasets may differ slightly. For instance “Ford” versus “Ford Motor Company”.

10
enhstein distance, with a critical value of 0.25\(^2\). I also add a condition on dates, requiring candidate vehicle matches in the DMV dataset to be owned prior to the crash. Often, I am still left with a list of candidate matches in the dataset. Therefore, I narrow my results down to the household who’s address is closest to the accident. This last criteria is based on the common statistic that most vehicle accidents occur close to home.

At the end of the matching procedure, I am left with about 46% of the original FARS dataset. For these households, I now have information from the DMV dataset such as vehicle ownership and addresses. I will therefore be able to locate the neighbors of these households, and their corresponding ownership histories. It is important to recognize that this matching procedure will most likely lead to significant measurement error in the treatment variable. That is, I may incorrectly identify a household as being involved in a fatal accident. The result of my estimates should therefore be attenuated.

For the purposes of estimating the effect of a neighbor’s death on nearby neighbors, I will restrict the set of neighbors to those located on the same street as the individual involved in the accident, and within 100 meters. Throughout this paper, I will focus on different samples of neighbors, usually focusing on only the nearest two nextdoor neighbors.

3.2 Summary Statistics

A summary of statistics for relevant variables are presented below. Each column in the table corresponds to a different sample, containing a different number of nearest neighbors. These are the \( k \) closest households neighboring the individual who experienced a vehicle accident. Note that as I go from one to two neighbors, that the sample of households does not double exactly. This will be either due to missing data, or neighborhoods with spaced out houses (since only houses on the same street and within 100 meters are included in my data).

About 56% of households are in my sample. These are households who live next to a neighbor who died in a vehicle accident. The average vehicle curb weight in this sample hovers around 3,700 pounds. This is about the mass

\(^2\)The Levenhstein distance is the minimum number of insertions, deletions, or substitutes of characters required to change one string into another. For a joint twelve letter first and last name, a Levehnstein distance of 3 requires a critical value of 0.25.
of a medium-sized SUV. One variable I will use as an outcome in this paper is an indicator for whether the household owns a passenger car. Note that this refers to the household owning at least one passenger car, as opposed to owning all trucks, vans, or SUVs.

The households’ vehicle value here refers to the estimated average worth of the vehicle fleet. For a given vehicle, this is estimated by taking the average price of all vehicles with the same VIN, sold at the month being observed. This requires that I observe at least one sale of a vehicle with that VIN for each month that the household is observed. Therefore, using this approach, some observations for this variable will be missing.

Another key point to note in this table is that the average household purchases just over two vehicles in this dataset. That’s two vehicles over the course of seven years (2004-2010). Meaning, a household in this dataset, on average, purchases one vehicle every 3.5 years. Because of these quite infrequent purchases, any adjustment to household vehicle characteristics will most likely be a slow and gradual process.

Table 1: Summary Statistics

<table>
<thead>
<tr>
<th>Neighbors</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>5</th>
<th>10</th>
<th>ALL</th>
</tr>
</thead>
<tbody>
<tr>
<td>Curb Weight</td>
<td>3,753.145</td>
<td>3,747.29</td>
<td>3,739.05</td>
<td>3,729.71</td>
<td>3,708.61</td>
<td>3,687.70</td>
</tr>
<tr>
<td></td>
<td>(800.37)</td>
<td>(795.77)</td>
<td>(789.89)</td>
<td>(784.06)</td>
<td>(768.81)</td>
<td>(760.11)</td>
</tr>
<tr>
<td>Passenger Car*</td>
<td>0.64</td>
<td>0.65</td>
<td>0.65</td>
<td>0.65</td>
<td>0.66</td>
<td>0.67</td>
</tr>
<tr>
<td></td>
<td>(0.48)</td>
<td>(0.48)</td>
<td>(0.48)</td>
<td>(0.48)</td>
<td>(0.47)</td>
<td>(0.47)</td>
</tr>
<tr>
<td>Airbag</td>
<td>0.81</td>
<td>0.81</td>
<td>0.80</td>
<td>0.81</td>
<td>0.81</td>
<td>0.81</td>
</tr>
<tr>
<td></td>
<td>(0.32)</td>
<td>(0.32)</td>
<td>(0.32)</td>
<td>(0.32)</td>
<td>(0.32)</td>
<td>(0.32)</td>
</tr>
<tr>
<td>MPG</td>
<td>18.99</td>
<td>18.99</td>
<td>19.00</td>
<td>19.03</td>
<td>19.10</td>
<td>19.16</td>
</tr>
<tr>
<td></td>
<td>(3.81)</td>
<td>(3.80)</td>
<td>(3.81)</td>
<td>(3.80)</td>
<td>(3.80)</td>
<td>(3.80)</td>
</tr>
<tr>
<td>Purchases</td>
<td>2.16</td>
<td>2.17</td>
<td>2.16</td>
<td>2.15</td>
<td>2.15</td>
<td>2.13</td>
</tr>
<tr>
<td></td>
<td>(2.24)</td>
<td>(2.20)</td>
<td>(2.17)</td>
<td>(2.15)</td>
<td>(2.14)</td>
<td>(2.13)</td>
</tr>
<tr>
<td>Households</td>
<td>7,923</td>
<td>15,297</td>
<td>22,081</td>
<td>34,039</td>
<td>56,159</td>
<td>78,220</td>
</tr>
<tr>
<td>Treated</td>
<td>0.58</td>
<td>0.58</td>
<td>0.57</td>
<td>0.57</td>
<td>0.56</td>
<td>0.56</td>
</tr>
</tbody>
</table>

*This refers to the household owning at least one passenger car, in contrast to all trucks, vans, or SUVs. This table presents the sample mean for each variable, for each sample corresponding to the number of nextdoor neighbors included. Standard deviations are in parentheses.

4 Effect of a Neighbor’s Fatality

In this section, I look at the effects of these information shocks on various outcomes. Specifically, I test whether a neighbor’s death in a vehicle acci-
dent has an effect on the revealed preferences for vehicle size. I will also look at other vehicle characteristics, such as whether the vehicles purchased have airbags, as well as the implications of these purchases in terms of fuel efficiency.

Throughout this paper, I look at consumer behavior at the household level, on a monthly-level panel. I estimate equation 1, where the outcome is a snapshot of some characteristic of the household fleet at a given month in time\(^3\). Note that the characteristics of a household fleet will only change when the household purchases a new vehicle. For these new purchases, I observe the exact date, however, for simplicity, I group these into a monthly observation.

Estimates from a household-month panel are interpreted as effects on the “stock” of vehicles, which are smaller than the direct effects on “flow”. For example, suppose a household on average owns two vehicles in any given period, and purchases a new vehicle every four years. We should expect very small changes over time when observing average household vehicle sizes from month-to-month. Following treatment, we also should not expect that the household will replace all of their vehicles. Therefore, in looking for a treatment effect at the household-month level, we should not expect to see a big effect on vehicle stock. Furthermore, on aggregate, these changes in vehicle “stock” will be slow given the varying timing of purchases.

Evidence of a treatment effect on neighbor’s death can be seen in Figure 2. This figure plots the average vehicle size for the treatment and control groups by months away from treatment. I have included only the two nearest nextdoor neighbors in this figure, though I will check the robustness across different samples of neighbors in section 5.1. As expected, the neighbor’s of those who died tend to track well against the neighbors of the survivors in the periods preceding treatment. At the treatment period, their paths begin to diverge. This figure illustrates that the treatment group begin to increase the relative size of their vehicles following neighbor’s death.

\(^3\)I use this approach as opposed to conditioning on only new purchases for two reasons: 1) To avoid potential selection issues when conditioning on only new purchases, and 2) the trends displayed in Figure 2 are much smoother for vehicle stocks and lead to less noisy estimates.
I will be using a difference-in-differences estimation approach in this paper to correct for potential household specific factors which could bias my estimates. Table 2 presents the least squares estimated treatment effect, where the outcome variable, $y_{it}$, is household $i$’s average vehicle curb weight at time $t$, and $t$ is a month-year. The sample includes the nearest two neighbors to the individual involved in the accident.

Column 1 estimates the treatment effect using the traditional difference-in-differences estimator, which controls for a post accident indicator, as well as a treatment group indicator. In column 2, I control for time-invariant census tract-level demographic variables, such as median income, percent white, percent black, and median age. Column 3 includes time fixed effects, and in column 4, I estimate equation 1—my primary specification. This includes a full set of household and time fixed effects. In the last column, I include a county-specific linear time trend, allowing each county to have differing trends.
Table 2: Effect of Neighbor’s Death on Household Vehicle Size

<table>
<thead>
<tr>
<th>Outcome: Weight (1) (2) (3) (4) (5)</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>neighbor died</td>
<td>32.16**</td>
<td>33.48**</td>
<td>34.50**</td>
<td>19.95**</td>
<td>16.37**</td>
</tr>
<tr>
<td></td>
<td>(14.73)</td>
<td>(14.56)</td>
<td>(14.52)</td>
<td>(8.367)</td>
<td>(8.319)</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Month-Yr FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
<td></td>
</tr>
<tr>
<td>HH FE</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>County Time Trend</td>
<td></td>
<td></td>
<td></td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>964,824</td>
<td>964,824</td>
<td>964,824</td>
<td>964,824</td>
<td>964,824</td>
</tr>
</tbody>
</table>

Standard errors in parentheses. Standard errors are clustered at the street level.
* p < 0.1, ** p < .05, *** p < .01

These estimates indicate that households residing near a neighbor who died in an accident, tend to increase their average vehicle size by about 20 pounds on average. This implies that, for a two-vehicle household purchasing only one new car in the post period, this vehicle should on average be about 40 pounds heavier. This is equivalent to a 400 pound upgrade for one in ten two-car neighbors—roughly the same as a switch from a sedan to a mid-sized SUV.

This effect may be viewed as a shock to consumers information regarding vehicle safety. There are several reasons why consumers may respond in such a way. First, these consumers may have had incorrect priors on the likelihood of dying in a vehicle accident. This response could simply be correcting for their imprecise prior beliefs. On the other hand, consumer may have had correct priors, but over updated their posterior beliefs. With rational updating, the effect of this treatment on posterior probabilities would need to be very large to trigger such a response.

A potential mechanism driving this apparent change in preferences could be projection bias (Loewenstein, O'Donoghue, and Rabin, 2003[11] and Busse et al., 2014[4]). Projection bias occurs when consumers incorrectly predict the extent to which future states of the world will resemble the current state of the world. If this were the mechanism driving this type of behavior, it would be the case that a consumer observing a neighbors death incorrectly projects future states as having an increased level of risk. In this paper, I will not take a stance as to why consumer change their revealed preferences for vehicle size.
In a vehicle arms race, consumers strategically respond to larger cars on the road by increasing the relative size of their own vehicles. Fatality is just one potential consequence of being hit by a large vehicle. Other salient outcomes could include injuries, totaled vehicles, or a “close call”. I have just shown that these types of salient outcomes have significant effects on the purchasing behaviors of the neighbors of the vehicle accident victims. It is plausible to think that these consumers respond as a function of accident severity, which is influenced by vehicle size. In section 6, I will show that these types of salient outcomes are affected by the size of the opposing vehicle in a vehicle collision. I will then show how neighbors respond as a function of these opposing vehicle weights.

4.1 Additional Outcomes

I test whether a neighbor dying in a vehicle accident has an effect on other attributes of consumer decision making beyond increasing the size of their vehicles. Figure 3 presents the analogue to Figure 2 for three additional outcomes.

In Figure 3, panel a, I examine the extent to which this transition to heavier vehicles is translated into a switch away from passenger cars. This figure illustrates that treated households decrease their consumption of passenger cars. This decrease in probability of owning a passenger car implies an increase in adoption of SUVs and pickup trucks.

In panel b, I look at the effects on vehicle safety, as measured by the fraction of vehicles a household owns that have at least one airbag. This variable was constructed using a string variable in my data which describes the restraint system in a vehicle. This description will normally include information regarding the types of seatbelt and airbag systems installed in the car. I assign a one to vehicles that contain “airbag”, “Airbag”, or “AIRBAG” in this description, and a zero otherwise. In the figure, following treatment, it appears that the treatment group increases their share of vehicles with

4Additionally, I look at whether the timing of purchase changed and the value of the vehicle stock—which I define as the average price that VIN10 was sold for in that month. The timing only changes slightly, and insignificantly (for the two neighbor sample). The vehicle value measure was not very precise and lead to noisy estimates. This is mostly due to insufficient number of the same VIN10s being sold in any given month.
at least one airbag. Though the crossed paths in the pre-period could be a reason for concern.

Panel c illustrates the effect on household average combined MPG. Because of the relationship between vehicle size and fuel efficiency, we would expect to see an effect here. The figure illustrates that the control and treatment group seem to track each other well in the pre-period. Following treatment, the treated group shows a significant drop in MPG.

In Table 3, I present the estimated treatment effects for these additional outcomes. As in Table 2, these effects are estimated using the nearest two neighbors on the same street. The estimates in panel a are the results of a linear probability model on whether the household owns at least one passenger car. These estimates imply a reduction in probability of owning a passenger car of about two percentage points. The estimated effect on household airbags implies that a neighbor’s death in an accident produces a near one percentage point increase in proportion of vehicles with airbags. Such a small effect should not be surprising since most households already own
vehicles with airbags. The effect of a neighbor’s death on average vehicle fleet MPG is about a 0.1 reduction. Since this is the effect on household average MPG, this implies about a 0.2 reduction in MPG for a two-vehicle household replacing only one vehicle in the post period.

Table 3: Effect of Neighbor’s Death in a Vehicle Accident

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel a</strong>: Passenger Car</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>neighbor died</td>
<td>-0.0230***</td>
<td>-0.0210**</td>
<td>-0.0212**</td>
<td>-0.0173***</td>
<td>-0.0171***</td>
</tr>
<tr>
<td></td>
<td>(.0087)</td>
<td>(.0086)</td>
<td>(.0086)</td>
<td>(.0062)</td>
<td>(.0062)</td>
</tr>
<tr>
<td>N</td>
<td>1,003,162</td>
<td>1,003,162</td>
<td>1,003,162</td>
<td>1,003,162</td>
<td>1,003,162</td>
</tr>
<tr>
<td><strong>Panel b</strong>: Airbag</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>neighbor died</td>
<td>0.0029</td>
<td>0.0052</td>
<td>0.0055</td>
<td>0.0072*</td>
<td>0.00702*</td>
</tr>
<tr>
<td></td>
<td>(.00579)</td>
<td>(.00581)</td>
<td>(.00580)</td>
<td>(0.00416)</td>
<td>(0.00411)</td>
</tr>
<tr>
<td>N</td>
<td>1,003,162</td>
<td>1,003,162</td>
<td>1,003,162</td>
<td>1,003,162</td>
<td>1,003,162</td>
</tr>
<tr>
<td><strong>Panel c</strong>: MPG</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>neighbor died</td>
<td>-0.131*</td>
<td>-0.111</td>
<td>-0.111</td>
<td>-0.0895**</td>
<td>-0.0744*</td>
</tr>
<tr>
<td></td>
<td>(.0706)</td>
<td>(.0692)</td>
<td>(.0692)</td>
<td>(0.0438)</td>
<td>(0.0437)</td>
</tr>
<tr>
<td>N</td>
<td>972,893</td>
<td>972,893</td>
<td>972,893</td>
<td>972,893</td>
<td>972,893</td>
</tr>
</tbody>
</table>

Controls: Yes Yes  
Month × Year FE: Yes Yes Yes  
Household FE: Yes  
County Time Trend: Yes

Standard errors in parentheses. Standard errors are clustered at the street level.  
* p < 0.1, ** p < 0.05, *** p < 0.01

5 Robustness Checks

5.1 Treatment Effects by Nextdoor Neighbors

Next I estimate these treatment effects using different samples of neighbors. In each specification, I include the nearest one through $k$ nearest nextdoor neighbors (on the same street), as listed below in Table 4. This illustrates a trade-off between size and power. That is, as I include more neighbors in the sample, I add more households that are further away from the neighbor involved in the accident. Thus, this serves as a robustness check, as one would expect the estimates to be attenuated as I add neighbors who live further away. However, including more households adds to the size of my
sample, which overall reduces the size of my standard errors.

Examining the results in Table 4, the two neighbor sample seems to capture the largest effect in terms of household vehicle weight. These estimates show a gradual decay in size following the second neighbor. For household weight, the effect of neighbor’s death is no longer significant by the tenth neighbor sample. The trade-off between size and power becomes apparent when examining the other outcomes. Though these estimates still show statistical significance by the ten neighbor sample, these estimates become much smaller as neighbors further away are added to the sample.

Table 4: Effect of Neighbor’s Death by Neighbors

<table>
<thead>
<tr>
<th>Neighbors</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>5</th>
<th>10</th>
</tr>
</thead>
<tbody>
<tr>
<td>Panel a: Weight neighbor died</td>
<td>10.34</td>
<td>19.95**</td>
<td>15.21**</td>
<td>5.479</td>
<td>4.815</td>
</tr>
<tr>
<td></td>
<td>(11.67)</td>
<td>(8.367)</td>
<td>(6.901)</td>
<td>(5.627)</td>
<td>(4.360)</td>
</tr>
<tr>
<td>N</td>
<td>495,278</td>
<td>964,824</td>
<td>1,397,654</td>
<td>2,159,147</td>
<td>3,590,583</td>
</tr>
<tr>
<td>Panel a: Passenger Car neighbor died</td>
<td>-0.00734</td>
<td>-0.0173***</td>
<td>-0.0128**</td>
<td>-0.00786*</td>
<td>-0.00735**</td>
</tr>
<tr>
<td></td>
<td>(0.00872)</td>
<td>(0.00618)</td>
<td>(0.00509)</td>
<td>(0.00412)</td>
<td>(0.00325)</td>
</tr>
<tr>
<td>N</td>
<td>514,956</td>
<td>1,003,162</td>
<td>1,451,992</td>
<td>2,239,699</td>
<td>3,720,048</td>
</tr>
<tr>
<td>Panel b: Airbag neighbor died</td>
<td>0.00221</td>
<td>0.00719*</td>
<td>0.00489</td>
<td>0.00568**</td>
<td>0.00391*</td>
</tr>
<tr>
<td></td>
<td>(0.00574)</td>
<td>(0.00416)</td>
<td>(0.00340)</td>
<td>(0.00274)</td>
<td>(0.00214)</td>
</tr>
<tr>
<td>N</td>
<td>514,956</td>
<td>1,003,162</td>
<td>1,451,992</td>
<td>2,239,699</td>
<td>3,720,048</td>
</tr>
<tr>
<td>Panel c: MPG neighbor died</td>
<td>-0.0612</td>
<td>-0.0895**</td>
<td>-0.0772**</td>
<td>-0.0537</td>
<td>-0.0488**</td>
</tr>
<tr>
<td></td>
<td>(0.0621)</td>
<td>(0.0438)</td>
<td>(0.0368)</td>
<td>(0.0298)</td>
<td>(0.0232)</td>
</tr>
<tr>
<td>N</td>
<td>498,744</td>
<td>972,893</td>
<td>1,409,593</td>
<td>2,179,309</td>
<td>3,629,256</td>
</tr>
</tbody>
</table>

Month × Year FE Yes Yes Yes Yes Yes
Household FE Yes Yes Yes Yes Yes

Standard errors in parentheses. Standard errors are clustered at the street level.
* p < 0.1, ** p < .05, *** p < .01

5.2 Dynamic Treatment Effects

Next, I estimate a fully dynamic version of equation 1. I include in the regression, six lagged and twenty-four leading indicators of treatment. The lagged terms serve as a placebo test, as I shouldn’t expect to see an effect
in periods preceding treatment. This formally tests the “parallel trends” assumption, as displayed in Figure 2. The dynamic treatment effects fully characterizes the gradual effects of neighbor’s death on the average weight of the households’ vehicle stock overtime. These effects are gradual due to non-immediate adoption by households. The 24th post-treatment dummy is one for all treated groups twenty-four months or more after treatment. All of these estimates should therefore be interpreted as the treatment effects relative to the pre-treatment period.

![Figure 4: Dynamic Estimates for Weight](image)

Since I do not condition on purchase in this paper, the estimates are interpreted as the effects on household vehicle stock. Many households may only upgrade one of their vehicles following treatment. It is not surprising then that Figure 4 illustrates a slow and gradual increase in household vehicle size over time. These estimates indicate that, a year out from the treatment period, treated households are now consuming a vehicle fleet that is roughly 20–25 pounds heavier.

As my accidents data span only five years, it is difficult to look at the long-term effects on future vehicles. The 24th post-treatment coefficient best
estimates this effect within my data. Being able to examine the longer-run effects of a neighbor’s death would be key in evaluating possible mechanisms driving these effects.

One of the reasons it is difficult to point out a potential behavioral mechanism driving these effects, is that, in the vehicle market, I usually observe a small number of transactions for each household within the dataset. For instance, as discussed by Loewenstein, O’Donoghue, and Rabin (2003)[2], projection bias can lead to dynamic inconsistency in preferences. In this case, a consumer purchases a larger vehicle because he incorrectly projects a higher level of risk in future states. Once these future states of the world are realized, they adjust their projections accordingly. These findings could provide evidence in favor of projection bias if I observed many vehicle purchases. Projection bias would then predict that consumers would quickly revert back to their original purchasing habits, buying vehicles of similar size to their pre-treatment vehicles. Since I often only observe one purchase per household in the post-treatment period, a true long-run analysis is not feasible.

5.3 Placebo Tests

I further check the robustness of the findings in Table 2 by estimating the treatment effect for a series of placebo estimates. This procedure is similar to the one outlined by Bertrand et al. (2004)[3]. Using the same sample as that in Table 2 (two neighbor sample), I randomly assign each of 15,723 households to the treatment or control group (with the same probabilities as the empirical probabilities). I randomly assign a treatment period to each household, from the periods in which the household was observed. For most households, I observe their vehicle stock for the duration of the accidents data, which begins in 2006 and ends in 2010. I proceed by estimating equation 1 for these placebo treatments. I repeat this process 1,000 times and record the estimates for each.

This process leaves me with a distribution of estimates. In a manner similar to that of synthetic control methods (e.g., Abadie et al., 2010[1]), I can then calculate p-values for my estimates in Table 2 from the resulting permutation distribution. A kernel distribution of the resulting placebo estimates is presented in Figure 5.
As expected, this distribution is centered on zero, which indicates a mean zero effect for a randomly assigned placebo treatment. My estimate for the effect of neighbor’s death on average household vehicle weight ranks at the top of the distribution, with a resulting p-value of 0.3%. This implies less than half a percent of placebo treatments resulted in a greater estimate than my estimate of 19.95 from Table 2. This suggests that it is highly unlikely that this size of effect was estimated by chance.

Figure 5: Distribution of Placebo Estimates on Household Weight

![Figure 5](image)

Note: This is the resulting distribution of 1,000 simulated placebo estimates. The vertical line corresponds to the estimated value of 19.95. An Epanechnikov kernel was used to estimate this distribution, with bandwidth 1.75.

I use the same process to generate the permutation distributions for other outcomes. The results suggest robust estimates for whether a household owns a passenger car, with a p-value of 0.006. For the fraction of a household fleet with airbags, the permutation approach suggests this estimate is significant at the 3.5% level. Results for MPG indicates only 2.2% of placebo estimates rank lower than my estimate.
5.4 An Alternative Control Group

The trend break for the control group illustrated in Figure 2 could be cause for concern. I might worry that this would lead to an over-estimate of the true effect on the treatment group. To test whether my choice of control group makes a difference in estimating these effects, I will form an alternative control group. The results from Table 4 form the basis of this control group. As neighbors further away from the involved household are added to my sample, the treatment effect becomes attenuated. This implies that neighbors further away (but on the same street) are less effected by treatment.

I construct an alternative control group by assigning the tenth or greater treated neighbors to the control, while the treatment group remains the nearest two neighbors to the accident involved household. It is important to note that these far away neighbors are still treated. Therefore, I may run into the same problem as my original control group—both increase weight following their neighbor’s accident.

The analogue to Figure 4 for the new control group is presented in Figure 6 below. Comparing these estimates to those with the original control group demonstrates a near-identical relationship. Similar to the original control group estimates, these estimates imply an overall treatment effect of between 20–25 pounds. The zero effect in the pre-periods also demonstrates that this control group tends to track the treatment group well.
6  Indirect Responses to Opposing Vehicle Size

Thus far I have shown evidence of a change in preferences for safety in response to salient information shocks concerning the perception of vehicle safety. I have been able to isolate the effects of a neighbor’s death, using a control group who’s neighbor did not die. However, it may be true that the control group is affected, not by a neighbor’s death, but through some alternative outcome of their accident. For example, it may be the case that a control’s neighbor incurred significant injuries. The control group may respond to the accident because their neighbor had a “close call”. It is also reasonable to suggest that these salient outcomes are affected by the size vehicles involved in an accident.

In identifying the effect of a vehicle arms race, we can easily run into multiple confounding factors using traditional data from the field. In an ideal experiment, we could isolate these confounders through random assignment of large vehicles to different homes. We would then observe the adoption of larger vehicles by other homes. Through random assignment, we would be isolating the effects of common shocks, as well as eliminating simultaneity
concerns, since assignment does not depend on others’ choice of vehicle size.

An alternative approach to examine a vehicle arms race is to look at the
effects of the salient outcomes which large vehicles produce on consumer
purchasing behavior. This is what the previous part of the paper examined.
That is, we would expect a larger opposing vehicle in a two car collision to
contribute to a higher probability of death. This is just one salient outcome
in which I have shown that neighboring households do in fact respond to.

In this section, I will look at how neighboring households to crash victims
respond indirectly to the size vehicle that hit their neighbor. The idea is
that a larger opposing vehicle, the more severe and salient the outcomes.
The neighbor to the victim then directly responds to this outcome. This
effect implies a feedback loop, as in a vehicle arms race, in which a larger
opposing vehicle generates adoption of larger vehicles by households.

Given the quasi-random nature of a vehicle accident, I can avoid issues re-
lated to common shocks. Also note that the opposing vehicle matched to
the victim is chosen from the population essentially at random. This implies
that the chance that the opposing individual responds directly to the victims
neighbors’, in terms of choice of vehicle size, is very small. Therefore, this
should mitigate concerns of simultaneity bias, and estimates should reflect
the correct causal direction of the opposing vehicle on the neighbor.

Intuitively, one would expect that the severity of an accident is a function
of the weights involved in the accident. Existing literature supports this
notion (e.g., Jacobsen (2013)[9]; Anderson and Auffhammer (2014)[2]). If
neighbors respond to this severity, then in the reduced form, it must be
ture that they respond to opposing vehicle weights. To illustrate, suppose
that the neighbor of household \(i\) is involved in an accident, with severity
\(S_i\), where I omit the \(t\) subscript for simplicity. Suppose that household \(i\) adjusts
their vehicle weight, \(W_i\) linearly to this level of severity. That is:

\[
W_i = \gamma_0 + \gamma_1 \cdot S_i + v_i
\]  

(2)

where \(\gamma_0\) is a constant, \(\gamma_1\) is the constant marginal effect of accident severity
on weight, and \(v_i\) is the unobserved error. Suppose that accident severity,
\(S_i\), is an increasing function of the opposing vehicle’s weight. For example:
\[ S_i = \alpha_0 + \alpha_1 \cdot W_{i}^{\text{opp}} + u_i \]  

(3)

where \( \alpha_0 \) and \( \alpha_1 \) are constants, \( u_i \) is the unobserved term, and \( W_{i}^{\text{opp}} \) is the weight of the vehicle involved in the two-car collision, opposite the neighbor. We can write the reduced form equation, which illustrates the indirect effect of \( W_{i}^{\text{opp}} \) on \( W_i \) as:

\[ W_i = \delta_0 + \delta_1 \cdot W_{i}^{\text{opp}} + e_i \]  

(4)

where \( \delta_0 \) and \( \delta_1 = \gamma_1 \cdot \alpha_1 \) are constants and \( e_i \) is the unobserved disturbance. This equation illustrates a feedback loop in the sense that increases in the opposing vehicle size lead to increases in household \( i \)'s vehicle size. However, this equation is just the reduced form of an alternative mechanism (equation 2), and thus follows an indirect causal route. Therefore, this equation describes somewhat of an indirect arms race, in the sense that household \( i \) only responds to \( W_{i}^{\text{opp}} \) through its salient effects on the neighbor (i.e., equation 2).

The outcome is the same as a traditional arms race—larger vehicles influence \( i \) to purchase larger vehicles for safety—but the response occurs through the mechanism of a vehicle accident. I can identify \( \delta_1 \) in equation 4 when \( W_{i}^{\text{opp}} \) enters \( W_i \) through the mechanism of a quasi-random neighbor car accident. When \( W_{i}^{\text{opp}} \) effects \( W_i \) outside of the mechanism of a vehicle accident, then we would be concerned about endogeneity problems common to measuring peer effects, i.e., simultaneity, common shocks.

More specifically, to identify the effect of \( W_{i}^{\text{opp}} \) on \( W_i \) through the mechanism of a neighbor vehicle accident, I estimate the following equation:

\[ W_{it} = \alpha_i + \gamma_t + \lambda \cdot \text{post\_crash}_{it} + \beta \cdot W_{i}^{\text{opp}} \times \text{post\_crash}_{it} + \varepsilon_{it} \]  

(5)

where \( \alpha_i \) and \( \gamma_t \) are household and time specific fixed effects, respectively, \( \text{post\_crash}_{it} \) is an indicator for \( i \)'s neighbor incurring a vehicle accident at some time \( s \geq t \), and \( \varepsilon_{it} \) is the unobserved error. The coefficient of interest, \( \beta \) measures how \( i \) adjusts their household vehicle weight in response to the opposing vehicle's weight. The arms race mechanisms implies \( \beta > 0 \). This mechanism further suggest that the household makes this upward adjustment in vehicle size for the purpose of individual protection.
Using this approach, any time-specific common shocks will be absorbed into $\gamma_t$. Furthermore, $W_{i}^{opp}$ is essentially chosen from the population at random (and does not vary over time) which mitigates concerns of simultaneity. Finally, we should expect that the timing of this vehicle accident will be as-good-as-random.

6.1 Results and Robustness

In the following analysis of an arms race effect, my sample is split substantially. This primarily has to do with the loss of one car collisions. In addition, weights for some opposing vehicles were missing from the DataOne vehicle characteristics data. Therefore, the resulting estimates include only the subsample of two-vehicle collisions.

The least squares estimates of equation 4 are presented in Table 5. These estimates report the effect of each 1,000 pounds of the opposing vehicle’s weight. The first two columns estimate the effect of opposing vehicle weight on neighbor death, using a linear probability model. This is simply the cross section, with the same number of observations as two-car collisions. The results indicate that a 1,000 pound increase in the opposing vehicle weight increases the probability of neighbor death by about 6.5%. In the second column, I include census tract-level demographics, such as median income, median age, percent white, and percent black. I also control for neighbor’s weight in column 2.

Because the weight effects not only risk of fatality, but also the severity of accident (and potentially other salient factors), using opposing weight as an instrument for neighbor death would violate the exclusion restriction. Therefore, inference should only be made from the reduced form effect of opposing vehicle weight on neighbor of victim weight. These estimates are presented in columns 3-6 of Table 5.

The coefficients in Table 5 were estimated using the two neighbor sample. These results indicate a statistically significant estimate of about 18 pounds. That is, a 1,000 pound increase in the opposing vehicle’s weight leads to household $i$ increasing their weight by 18.
Table 5: Effect of Opposing Vehicle Weight

<table>
<thead>
<tr>
<th>Opposing Weight (1,000s)</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yr-Month FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>HHI FE</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
<td></td>
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</tr>
<tr>
<td>County Time Trend</td>
<td></td>
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<td>Yes</td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>3,775</td>
<td>3,775</td>
<td>414,817</td>
<td>414,817</td>
<td>414,817</td>
<td>414,817</td>
</tr>
</tbody>
</table>

Standard errors in parentheses. In columns 1 and 2, standard errors are clustered at the level of the county of residence. Columns 3-6 are clustered at the street level.

I present the estimates for samples containing differing numbers of nearest nextdoor neighbors. These results are presented in Table 6 below. Similar to previous results, these estimates are decaying as I include neighbors further away. However, these estimates don’t fully go to zero in the 10 neighbor sample. This result differs from the estimates in Table 4 where the estimates on weight converge to zero quickly.

Table 6: Effect of Opposing Vehicle Weight by Neighbors

<table>
<thead>
<tr>
<th>Neighbors</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>5</th>
<th>10</th>
</tr>
</thead>
<tbody>
<tr>
<td>Opposing Weight (1,000s)</td>
<td>16.42**</td>
<td>17.36***</td>
<td>10.18**</td>
<td>5.997</td>
<td>6.105**</td>
</tr>
<tr>
<td>(6.811)</td>
<td>(5.077)</td>
<td>(4.243)</td>
<td>(3.361)</td>
<td>(2.580)</td>
<td></td>
</tr>
<tr>
<td>Month × Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Household FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>N</td>
<td>212,762</td>
<td>414,817</td>
<td>598,491</td>
<td>928,146</td>
<td>1,551,218</td>
</tr>
</tbody>
</table>

Standard errors in parentheses. Standard errors are clustered at the street level.

I present the estimates for samples containing differing numbers of nearest nextdoor neighbors. These results are presented in Table 6 below. Similar to previous results, these estimates are decaying as I include neighbors further away. However, these estimates don’t fully go to zero in the 10 neighbor sample. This result differs from the estimates in Table 4 where the estimates on weight converge to zero quickly.

In the event that these estimates remain positive for neighbors very far away, we may worry that I am picking up the effects of common shocks. To mitigate concerns that these effects exist across neighbors very far away from the household involved in the accident, I could look at neighbors located on a street behind the household. We would not expect these neighbors to be affected much, since it is less likely that they know the household involved in the accident. This process of finding neighbors a block over, however, is very tedious. The simpler approach would be to randomly assign neighbors
to opposing vehicle weights from the sample. This is similar to the permutation approach, except performed in one dimension. That is, I maintain the same crash time (the true crash time), which is a source of random variation in the timing of the accident, but assign randomly an opposing vehicle weight from the sample of two-car crashes. If I am picking up confounding factors common across all vehicles, I would expect to see that the household not only responds to the opposing vehicle hitting the neighbor, but also any opposing vehicle in the sample.

I randomly draw opposing vehicle weights from the sample and estimate the corresponding difference-in-differences coefficients. I run the simulation 1,000 times and plot the distribution of these placebo estimates. Using this approach resulted in zero estimates larger than the estimated effect in Table 6. This should reassure us that I am only picking up the effect of opposing vehicle size through the mechanism of a quasi-random car crash with the household’s neighbor. The distribution of the placebo estimates are presented in Figure 7.

Figure 7: Distribution of Placebo Estimates: Effect of Opposing Vehicle Weight (Same Treatment Period, Random Neighbor)
I further verify the robustness of these estimates by performing the permutation procedure in both dimensions. For each iteration I randomly draw, with replacement, opposing vehicle weights from the true sample of opposing vehicle weights for each household. I then randomly draw a treatment period for each household and estimate the effect on these placebo treatments using my primary specification in equation 5. The distribution of these placebo estimates are presented in Figure 8.

Figure 8: Distribution of Placebo Estimates: Effect of Opposing Vehicle Weight

The vertical line in Figure 8 corresponds to the estimate in column 5 of Table 5. The results of the placebo simulations suggests a p-value of 0.003. This is consistent with the implied p-value from the clustered standard error estimates.

7 Alternative Mechanism

In this paper, I argue that consumers respond to a salient vehicle accident of a neighbor by increasing their relative vehicle size due to an increased
preference for safety. Such a response is consistent with a vehicle arms race. However, it is possible that consumers are increasing their vehicle size due to reasons other than preferences for safety. An alternative hypothesis is that consumers observe a neighbor’s death, which triggers a belief that life is short, and thus they should treat themselves to a larger, more luxurious vehicle.

In this section, I test whether this “life is short” hypothesis could be driving these responses. I will look at the transaction prices of the vehicles in a household’s fleet, and test whether these households increase the value of their fleet by means other than purely increasing the size of the fleet. That is, post-neighbor death, do households increase the value of their fleet, conditional on fleet size?

I’ve constructed two new variables, that vary by household at a monthly frequency. \textit{Transaction price} is the household’s average price paid for each vehicle in the fleet. This outcome only changes following a purchase, and therefore, remains constant across most observations. \textit{Car value} calculates the average value of the household’s fleet, assuming a 1.3\% depreciation rate per month\(^5\).

Figure 9 plots the value of a vehicle fleet over time, according to these two separate metrics. Both measures show an increase in the treatment group’s fleet value following the accident. This makes sense due to the positive relationship between a vehicle’s size and its price.

\(^5\)This corresponds to the estimated average depreciation rate of a vehicle, as reported by Edmunds.com.
In Table 7, I present the results of the difference-in-differences regression, with fleet value as the outcome variable. I show these estimates before and after controlling for weight. Though not statistically significant, the estimates suggest a positive effect of a neighbor’s death on vehicle value. These estimates decrease in size after controlling for fleet weight.

Table 7: Effect of Neighbor Dying on Vehicle Price

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel b: Transaction Price</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>neighbor died</td>
<td>135.4</td>
<td>-167.6</td>
<td>64.59</td>
<td>-63.28</td>
</tr>
<tr>
<td></td>
<td>(206.9)</td>
<td>(209.4)</td>
<td>(94.07)</td>
<td>(89.59)</td>
</tr>
<tr>
<td>weight</td>
<td>6.171***</td>
<td>4.584***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0916)</td>
<td>(0.118)</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Panel b: Car Value</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>neighbor died</td>
<td>159.3</td>
<td>-16.49</td>
<td>99.76</td>
<td>-2.461</td>
</tr>
<tr>
<td></td>
<td>(149.7)</td>
<td>(154.7)</td>
<td>(97.63)</td>
<td>(103.8)</td>
</tr>
<tr>
<td>weight</td>
<td>4.525***</td>
<td>4.352***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0679)</td>
<td>(0.109)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yr-Month FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>HH FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>884,416</td>
<td>540,628</td>
<td>884,416</td>
<td>540,628</td>
</tr>
</tbody>
</table>

Standard errors in parentheses. Standard errors are clustered at the street level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Next, I present these results graphically. I regress the fleet value variables on household average weight. I plot the residuals from this regression in Figure 10. This Figure is the analog to Figure 9, after controlling for fleet weight. This figure suggests that the increase in fleet value seen in Figure 9 was primarily due to an increase in size.
If households increased the fleet size due to a “life is short” mechanism, we would suspect to see an increase in vehicle value by means other than that of just vehicle size. Therefore, this evidence suggests that this sort of mechanism is not a driving force, further supporting the notion that consumers increase their preferences for safety—behavior consistent with a vehicle arms race.

8 Conclusion

There is an obvious relationship between preferences for safety and preferences for size, with respect to vehicle demand. Measuring the extent to which consumers favor these attributes is important for environmental policy. As larger vehicles on the road create higher levels of carbon emissions and an increased risk to others, policy efforts need to be focused on the incentives that drive the adoption of these vehicles. I examine an apparent motivating factor in the form of a vehicle arms race, identified through salient outcomes of large vehicles.

This paper has presented strong evidence that households respond to salient information regarding vehicle safety. This type of information was presented in the form of fatal accidents harming a nextdoor neighbor. The consumer response identified was a heavier household vehicle fleet with lower fuel economy. These responses were most likely made with the purpose of protection from opposing drivers; however, this type of behavior produces very costly externalities.
In the event of a vehicle arms race, households strategically respond to heavy cars on the road by increasing the size of their vehicle fleet. These types of responses are not necessarily welfare increasing. That is, these behaviors lead households into a prisoner’s dilemma, stuck on an inefficient outcome.

Presenting evidence of a vehicle arms race is not an easy task, as there are many confounding factors in identification. In this paper, I identified an arms race effect through the salient outcomes in which large vehicles produce. The type of outcome examined in this paper was the death of a neighbor in a vehicle accident. However, neighbors of crash victims may be affected in other salient outcomes of the accident other than the victim’s death. They may also be affected by knowledge of the severity of the crash. The weights of the vehicles involved in the accidents directly contribute to this accident severity. I’ve shown evidence of an arms race effect, in which opposing vehicle weight influences neighbors to purchase larger vehicles. This contagion-type effect, inherent in an arms race, further exacerbates the costs associated with large vehicle adoption.

Policy makers need be concerned with the externalities formed from this type of consumer behavior. A higher demand for larger vehicles means increased adoption of gas guzzlers that produce higher levels of carbon pollution. This is apparent in the transition by consumers from passenger cars to small trucks and SUVs. These are preferences which work against the federal CAFE standards, which set mandates on minimum fuel economy. These types of misalignments in preferences between consumers and regulators increase market inefficiencies while reducing the overall impact of the CAFE standards.
References


