Formative Experiences and the Price of Gasoline*

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Abstract

Formative experiences can shape people's behavior for decades. We document a striking feature about those who came of driving age during the oil crises of the 1970s: they drive to work less in the year 2000. The effect is not specific to these cohorts; we exploit price variation over time and across states to show that gasoline price changes experienced between ages 15 and 18 generally shift several margins of later-life travel behavior. Effects are not explained by recessions, income, or costly skill acquisition. Instead, they likely reflect early formation of preferences for driving or persistent changes in its perceived cost. These findings are inconsistent with recency bias, habit formation, and mental plasticity.

Keywords: formative experiences, preference persistence, path dependence, driving behavior, gasoline price

JEL Codes: D12, D90, L91, Q41, R41

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1 Introduction

Behavior is often modeled as an economic choice that depends on preferences and the current economic environment, yet observable characteristics of agents and of the economic environment often fail to explain significant variation in behavior. In many consumer choice models, remaining variation is cast as idiosyncratic preference for particular goods or outcomes (e.g., Manski and McFadden 1981; Berry, Levinsohn, and Pakes 1995). Where do these observably idiosyncratic differences in behavior come from? A growing literature in economics argues that cumulative and recent experiences exert a significant influence on preferences (Bronnenberg, Dubé, and Gentzkow 2012; Malmendier and Nagel 2011; Simonsohn 2006).

We add to this narrative by providing micro-level evidence that "formative experiences" with an economic activity shape individual behavior for decades to come. We document two striking facts: commuters in the United States who experience a positive shock to the price of gasoline while coming of driving age—and thus first experiencing driving—are less likely to drive to work in a private automobile decades later in life. If they do drive, they drive substantially less. In many economic models, such path-dependent behavior appears as individual preference heterogeneity.

We first motivate this with a graphical and quantitative case study. Individuals who experienced the oil crises of the 1970s during their formative driving years (around age 15) appear less likely to drive to work 20 years later (at the time of the 2000 census) than preceding and following cohorts. These cohorts see a similar decrease in vehicle access and a corresponding increase in commuting by public transit. Event study analysis puts the magnitude of this difference between 0.2–0.5 percentage points for the 1979 oil crisis. The effect is larger in magnitude in urban settings with transportation alternatives and for lower-income workers.

In our main analysis, we carefully identify the effect of gasoline price movements during formative years on two aspects of driving behavior: mode of commuting (an extensive margin) and miles traveled (an intensive margin). We study many cohorts across many cross sections of the U.S. population and exploit annual variation in gasoline prices across states and time over a long horizon to flexibly control for time-invariant differences across locations, life-cycle (age) effects, and contemporaneous factors that influence transportation choices (e.g., current gasoline prices). Individuals respond to price changes during their formative driving years much more so than to price levels. A doubling of the real price of gasoline between the ages of 15 and 17 leads to a 0.3–0.4 percentage point reduction in the probability of driving to work later in life and a 0.2–0.3 percentage point increase in transit usage. There is some evidence of a smaller effect on household access to a vehicle, indicating that durable goods consumption may respond to price shocks far in the past.

In contrast to a notable but small extensive margin effect, we find a large intensive-margin response. Combining several waves of a national travel survey, we show that drivers who experience a doubling of real gasoline prices between ages 15 and 17 drive 3.4–8.2 percent fewer annual miles as adults. This effect corresponds to roughly 900–1,100 fewer average annual miles traveled for drivers in affected cohorts, conditional on having access to a vehicle. Furthermore, we find that drivers that were exposed to gas price hikes early in life are somewhat less likely to own fuel-inefficient light-duty trucks.

It is precisely the effect of gasoline price shocks during formative years that matters. We show that only gas price movements between ages 15 and 18 shape later-life behavior—neither gas price movements earlier nor later matter. We also allow the formative window to vary according to state-specific driving age restrictions. An effect is present from 1 year before to 2 years after the minimum age at which teenagers can obtain a full-privilege license, which covers ages 15 to 18 in many states. Variation in these restrictions across states and over time strengthen identification and lead to slightly larger effects of gasoline price movements during formative driving years.

We rule out a primary role for two obvious explanatory mechanisms: income and costly skill acquisition. Gasoline price movements are associated with recessions, and entering the labor market during a recession can decrease permanent income and, therefore, change later-life driving behavior. Our results are robust to including measures of graduating into a recession and of contemporaneous income. A more formal mediation model reveals that the income channel explains at most 24% of the observed effect, but likely much less. We also study the effects of changes in minimum driving age to determine whether restrictions on learning to drive during formative years discourage take up of driving; it does not appear to. Nor can these patterns patterns be explained by delays in driver license take up. Further, delayed skill acquisition is unlikely to lead to the intensive-margin effect. Together, these results indicate that driving behavior is imprinted by formative experiences with gasoline prices, likely either through a change in preferences or in the perceived cost of driving and volatility of gas prices.

Our results point to a different source of experiential learning than commonly put forward. Related literature shows that recent exposure to violence or cumulative experience of poor economic conditions can impact risk attitudes and expectations about inflation and stock returns later in life. For example, Callen et al. (2014) find that individuals exposed to traumatic violence exhibit an increased preference for certainty. Malmendier and Nagel (2011) demonstrate that those who experienced lower stock returns during their lifetimes exhibit less willingness to take financial risk and invest less in the stock market, with more recent stock market experiences mattering most.[1]

In contrast to this existing literature—which often finds evidence for recency bias, habit formation, or mental plasticity during the decade of early adulthood—we show that early-life experiences during a narrow, formative window affect long-run driving behavior. The lack of an effect of gasoline price shocks outside this formative window suggests that initial experiences are more important than cumulative experience in some settings. "First impressions," in which agents first interact with a good and form initial impressions and information sets, matter a lot. We also confirm that earlier experiences matter more than recent ones by estimating an oft-used cumulative weighting function (Malmendier and Nagel 2011); we find that this function slopes in the direction opposite than in most other studies, which find a form of recency bias.

The economics literature also provides theoretical foundations for how current prices and consumption may have long-term impacts. Seminal work on 'habit formation' goes back to Pollak (1970) and Becker and Murphy (1988). With habit-forming goods, an individual's current consumption depends on past consumption levels to which she has become accustomed, and thus indirectly on past prices.[2] This model suggests that what matters is a cumulative average (with diminishing weights on earlier years) over the level of consumption, and that there should be no critical formative periods.

Nor are our results consistent with theories from social psychology that economic preferences and beliefs are formed during the first decade of adulthood—a period of "impressionable years" during which people exhibit "mental plasticity," after which beliefs

[1] Malmendier and Nagel (2016) find a similar effect for inflation expectations, which are mostly driven by people's recent inflation experiences. Malmendier and Shen (2018) show that exposure to severe unemployment spells leads to consumption deviations inconsistent with the permanent income hypothesis, and Fujiwara, Meng, and Vogl (2016) find evidence that rainy election days decrease voter turnout in current and future elections. Another strand of the literature links later-life purchasing decisions of migrants to brand availability or relative prices in places of origin (Bronnenberg, Dubé, and Gentzkow 2012; Logan and Rhode 2010), while Luttmer and Singhal (2011) show that preferences for redistribution of second-generation immigrants in Europe tend to mirror those of their countries of origin.

[2] A simple example starts with a utility function over n goods: $U(X) = \sum_{k=1}^{n} a_k \log(x_{kt} - \beta_k x_{kt-1})$ for $a_i > 0$, $\beta_i x_{it-1} > 0$, $x_i > \beta_i x_{it-1}$, and $\sum_k a_k = 1$. The term $\beta_i x_{it-1}$ denotes the "psychologically necessary" amount of good i and depends on past consumption. The $0 < \beta_i \leq 1$ are habit formation coefficients. See Pollak (1970) for different and more general versions of this model.

become more persistent and less mutable (Krosnick and Alwin 1989). While we provide evidence that behaviors and preferences are influenced by long-ago events, our finding of a narrow formative time window argues that early, initial experiences matter more than general mental plasticity over a wider age range in this context. Giuliano and Spilimbergo (2013) instead find empirical evidence for mental plasticity generally by analyzing attitudes for redistribution of cohorts that reached adulthood in a recession.[3] Malmendier and Nagel (2011) and 2016 and Malmendier and Shen (2018) also explain their empirical results using social psychology: personal experiences, especially recent ones, matter more for decision making than statistical information (Tversky and Kahneman 1974; Hertwig et al. 2004). We confirm that personal experiences matter for preference formation, but there does not appear to be a "universal law" about when they matter most.

Our results show that price volatility (rather than price level) can imprint later behavior. A cohort that experiences a price increase to a high level during their formative window exhibits decreased later-life driving relative to previous or following cohorts for whom prices were consistently low or consistently high during their formative window. The significance of shocks rather than levels suggests reference dependence does not explain this behavior, and is consistent with recent experimental research by Haushofer and Fehr (2019), who find that income shocks affect discounting behavior, even conditional on post-shock income levels. The finding that price changes during formative years determine later-life behavior suggest that standard habit formation or reference-dependence models cannot explain long-run consumer behavior in our context.

Finally, we demonstrate that relatively mundane experiences (e.g., interactions with gasoline prices) can be important. Formative experiences need not be particularly life-changing or extreme (such as violence or unemployment as in, for example, Callen et al. (2014) and Malmendier and Shen (2018)).

We also speak to the literature in urban and environmental economics that seeks to understand the short-run relationship between gasoline prices and driving behavior, by adding a new, long-run response to macroeconomic energy price shocks, which uncovers a channel that gives rise to heterogeneous driving behavior.

We briefly describe this related literature in urban, transportation and environmental economics below. Section 2 then describes research setting and data sources. In Section 3, we use the oil crises of the 1970s as a case study to illustrate that long-ago price volatil-

[3]Relatedly, Brown, Cookson, and Heimer (2019) find that children growing up in financially underserved Native American reservations later make less use of financial products and have lower credit scores than children growing up in better-served reservations.

ity continues to influence driving patterns. Section 4 identifies the long-run effects of gasoline prices on the intensive and extensive margins of driving using many cohorts of drivers. We then examine mechanisms and show the long-run effects of changes in the minimum driving age in Section 5. Section 6 offers conclusions and implications.

Related Literature

A literature in urban and transportation economics studies impacts of recent experiences, particularly to understand the behavioral determinants of location choice, commuting, and vehicle choice.[4] Busse et al. (2015) find that weather at the time of purchase influences vehicle choice. Simonsohn (2006) and Simonsohn and Loewenstein (2006) show that people that move to a new city exhibit reference dependence with respect to their former city of residence regarding housing prices and commuting. Another set of papers finds benefits of experimentation, studying the effects of incentivizing households to use public transit shortly after they move to a new location (Bamberg 2006; Yang and Lim 2017) or of commuting behavior after being forced to travel along new routes (Larcom, Rauch, and Willems 2017). These papers show short- or medium-run path dependence and rely on the experience of recent events, often referred to as "recency bias", which might be transitory (Tversky and Kahneman 1974). In contrast, our results show persistence over the life cycle from the price variation experienced during a three-year period in which consumers first interact with a consumption good.

Several papers study how fleet composition responds to changes in gasoline prices (Puller and Greening 1999) or efficiency mandates (West et al. 2017) and whether this is consistent with standard models of rational expectations about gasoline prices and consumer valuation of future fuel costs (Allcott and Wozny 2014; Busse, Knittel, and Zettelmeyer 2013; Gillingham, Houde, and Benthem 2019; Jacobsen and van Benthem 2015; Li, Timmins, and von Haefen 2009). Hughes, Knittel, and Sperling (2008) show that gasoline usage has become more inelastic in the twenty-first century; Small and van Dender (2007) attribute this to rising incomes. Gillingham, Jenn, and Azevedo (2015) explore heterogeneity in fuel price elasticities by geography and the fuel economy and age of the vehicle. However, there is little evidence on how differences in behavior and preferences arise.[5]

[4]There is a large literature on path dependence based on the supply of transportation infrastructure—it influences the location of cities (Davis and Weinstein 2008; Bleakley and Lin 2012; Michaels and Rauch 2018), urban form (Brooks and Lutz 2019), and regional growth (Donaldson and Hornbeck 2016).

[5]One exception is Anderson et al. (2015), who document that preferences for particular automobile

2 Context and Data

The United States is a notably automobile-friendly nation: about 76 percent of workers commute alone in a private vehicle (85 percent including carpoolers), compared with 56 percent (64 percent) in the United Kingdom. Laws regulating driving tend to provide few barriers, and people start driving at relatively young ages. In 30 U.S. states, it is today possible to obtain an unrestricted (full-privilege) driver license before the age of 18, the standard minimum unrestricted age in most of Europe. In 1980, only in seven states was the minimum full-privilege driving age greater than 16, and in five states it was less than 16. Learner's permits have traditionally been granted between the ages of 14 and 16; in 1980, eight states allowed those 14 years old to begin supervised driving and twenty-one more states allowed those between 14.5 and 15.5 years old to begin supervised driving (see Appendix Table A.1 for details).

Many teenagers begin driving soon after reaching the minimum legal age. In 1980, roughly 44 percent of those aged 16, 66 percent of those aged 17, and 77 percent of those aged 18 had a full-privilege driver license. These numbers have been falling as states started implementing graduated licensing programs that delay full-privilege licenses until age 17 or 18. By 2010, only 28 percent of those aged 16, 46 percent of those aged 17, and 61 percent of those aged 18 had full-privilege licenses.[6]

This age distribution matters, as there are several reasons to believe it is substantially easier to learn to drive when young than when older (at least in the United States). Young people often have access to vehicles, as well as training and supervision, while living with parents. It is the norm in most non-urban and many urban communities to learn to drive during one's teenage years. Further, many high schools have traditionally offered subsidized driver training programs. Finally, the opportunity cost of time for this age group is likely lower than for older people.

2.1 Data

We draw our primary data from several sources and discuss each below. See Appendix A.1 for additional data details, and Appendix Table A.2 for summary statistics.

Commuting Behavior and Vehicle Ownership. The decennial census asks questions

brands are transmitted across generations, leading to an interesting source of brand loyalty.

[6]Data from the 2016 release of the Highway Statistics published by the Federal Highway Administration, Table DL-220; see Appendix A.1 for details.

about commuting mode and time. 'Journey to Work' questions appear in the 1980, 1990, and 2000 censuses, and in the American Community Survey (ACS). We use data from these three censuses, as well as the 2006/10, 2011/15, 2016, and 2017 ACS. Key variables of interest are: (i) the primary mode of commute for each worker in the household; (ii) whether a household keeps a vehicle at home for use by members of the household; and (iii) public transit ridership for each worker in the household. The census data include rich demographic and economic controls, including contemporaneous income.

Age plays a central role in our analysis, but interpreting age in census data requires qualification. Census microdata report both age and birth year for each person in the household. Age is understood in terms of a particular *reference day*, which is April 1 of the enumeration year. Birth year is defined as sample year less age. Thus, someone born in May reporting 35 years of age in the 2000 census was born in 1964, whereas someone born in March reporting an age of 35 in 2000 was born in 1965. We use birth year to define cohorts at age 15, recognizing that there is some spillover across years. The ACS is conducted on a rolling basis, and there is no constant reference day. Although the sampling year is reported in the multi-year ACS data, it is not possible to precisely recover the birth year because the sampling date is not reported. However, in both census and ACS data, errors are consistent within observation years (and so captured by fixed effects).

Travel Surveys and Vehicle Data. We draw on five waves of the National Household Travel Survey (NHTS) and its predecessors from 1990, 1995, 2001, 2009, and 2017. Our main variable of interest is miles driven in each vehicle, combined with information on which household member is the main driver of each vehicle. We aggregate vehicles across primary drivers to develop a person-specific measure of annual miles traveled. There are fewer demographic details available in these data; we use sex, race, urban/rural status, and family size. The data also contain vehicle-level information on make, model, and vintage, which allows us to identify a vehicle's type (passenger car or light-duty truck). We merge make, model, and vintage information with EPA data on fuel economy from Allcott and Knittel (2019).

Gasoline Prices. The Energy Information Administration reports nominal tax-inclusive, state-level gasoline price data starting in 1983. For the years 1966-1982, we use data from annual Federal Highway Administration (FHWA) *Highway Statistics* publications (Small and van Dender 2007; Li, Linn, and Muehlegger 2014).[7] Appendix Figure A.1 plots these

[7]We thank Erich Muehlegger for sharing these data with us.

data.

Driver Licensing. We develop a database of driver licensing requirements by drawing from several sources, including the FHWA's publication *Driver License Administration Requirements and Fees*, records from the Insurance Institute for Highway Safety, records from several state Departments of Motor Vehicles, and newspapers (see Appendix A.1 for more details). We also use aggregate data on driver licensing published by the FHWA in *Highway Statistics* that list the number of driver licenses held by people of each age from 16 to 24 in each year. To estimate rates of driver license adoption, we construct age-specific population estimates from the National Cancer Institute's SEER Population database.

3 Case Study: Oil Crises and Later-Life Driving

We first exploit the gasoline price volatility of the 1970s to provide a case study and show graphically the link between early-life experience and later-life driving behavior. We provide some estimates of the magnitude of this effect in this section, but defer our main quantitative results until Section 4, in which we leverage additional gasoline price data and variation over a time period of nearly four decades.

The price of gasoline in the United States was relatively stable during the 1950s and 1960s. Beginning in the 1970s, the global oil market entered a phase of increased volatility. The United States experienced two large shocks to gasoline prices related to the 1970s oil crises in the Middle East: one in late 1973 and 1974 and another from late 1978 through early 1980. Figure 1 highlights the period of dramatically increasing gasoline prices.[8]

These gasoline price shocks are notable for three reasons. First, the increases were large, sudden, and well-publicized in the media.[9] The nominal price changes during these episodes were substantial, and in the 1979 crisis prices doubled over the course of a year. These increases were unexpected and likely exacerbated by unpredictable demand-side responses (Baumeister and Kilian 2016).[10] In fact, average consumer beliefs are often best reflected by a no-change forecast, so shocks to prices can be modeled as unexpected (Anderson, Kellogg, and Sallee 2013). Second, nominal prices had never been so high, and real prices had not seen such levels since the 1930s. This was the first time since

[8]Nominal prices are from the Bureau of Labor Statistics' 'Consumer Price Index for All Urban Wage Earners and Clerical Workers: Gasoline' series, and are deflated using the CPI-U and rescaled in $2017.

[9]See, for instance, `https://books.google.com/ngrams/graph?content=gas+price&year_start=1960&year_end=2000&corpus=15&smoothing=0`.

[10]See Hamilton (2008) for more discussion of the proximate causes of oil shocks and their effect.

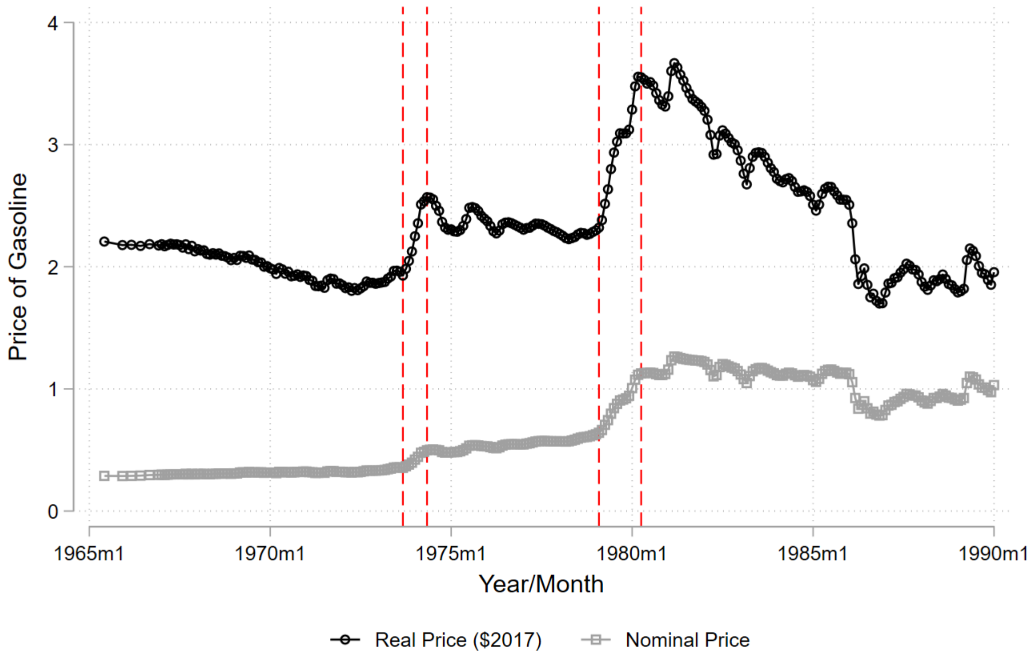

Figure 1: Gasoline prices spikes in the United States (1965-1990)

the U.S. became an automobile-dependent society that nominal gasoline prices exceeded $1 per gallon and real gasoline prices were higher than $3 per gallon (in 2015 dollars). The $1 price level may have been particularly salient. Third, not only was the cost of gasoline high, queuing at the gasoline pump meant that an additional time expenditure was required to obtain gasoline.[11]

The price shocks were likely consequential for those just coming of driving age. Two elements are particularly relevant for long-run driving behavior: (i) preferences for driving or perceptions of its cost may have changed[12] and (ii) learning to drive may have become more costly (in terms of time, money, or other inputs). Both factors could plausibly lead to a reduction in driving later in life. Given that most drivers in the U.S. learn to drive before the age of 18, those who do not may face more difficulties and higher opportunity costs learning to drive later in life and may even forgo driving altogether. We later

[11]These queues could be quite substantial: Frech and Lee (1987) and Deacon and Sonstelie (1989) highlight the negative consequences of time wasted by queuing.

[12]Media coverage of the oil crisis was ubiquitous. Even though some teens may not have paid directly for gasoline or driving, media coverage and family interactions discussing gasoline price fluctuations likely meant that prices were salient and played a role in shaping perceptions and expectations.

show that such a costly skill acquisition channel does not appear to explain our results.

3.1 Later-Life Driving

Figure 2 plots driving, public transit use, and household vehicle access more than two decades after the oil crises of the 1970s, as reported in the 2000 census. We focus on the 2000 census because commuting behavior among those in their early and mid 20s is highly mutable. Indeed, the steeper slopes to the right in Figure 2 represent people progressively younger when observed in 2000. Travel patterns are more settled among those in their 30s and their life-cycle trends have mostly smoothed out. Commuters are matched to cohorts at age 15 (displayed along the horizontal axis). For example, the year-2000 behavior (at age 35) of those born in 1965 is indexed to cohort year 1980 (when they turned 15), while the year-2000 behavior (at age 32) of those born in 1968 in 2000 is indexed to cohort year 1983. Vertical bars bound the two periods of rapid increases in gasoline prices shown in Figure 1.

Because census data imperfectly report birth year, Figure 2 slightly adjusts the horizontal location of each cohort. Specifically, note that someone born on April 2, 1964, is 35 years old during the 2000 census. Census data thus assign this person a birth year of 1965, and this person would appear to turn 15 in 1980 (instead of 1979). Assuming a uniform distribution of births throughout the year, one-quarter of those who appear to turn 15 in 1980 do actually turn 15 in 1980; three-quarters turn 15 some time in the last nine months of 1979. Figure 2 plots this cohort at the midpoint of the distribution: 1979.75.

Driving behavior appears to respond to the gas price shocks of the 1970s in each of the series shown in Figure 2. The probability that a commuter drives to work decreases and the probability that a commuter takes mass transit increases during or following the oil crises. For the 1979 oil crisis, the decrease appears to be nearly one-half of a percentage point and marks a jump in behavior between cohorts turning 15 in 1980 and those that came before. Furthermore, the bottom panel illustrates that individuals in these cohorts are less likely to have access to a vehicle.[13] The trends starting in 1980 potentially reflect both a longer-run response to the crises and life-cycle differences of younger cohorts (those turning 15 in 1990 are only 25 during the 2000 census, and have different transportation behaviors than more established workers). We control for age trends in the following analysis; in Section 4 we explicitly control for both current age (at time

[13]For vehicle access, the sample is not limited to workers.

of observation) and the gasoline price environment during teenage years to distinguish between these possibilities.

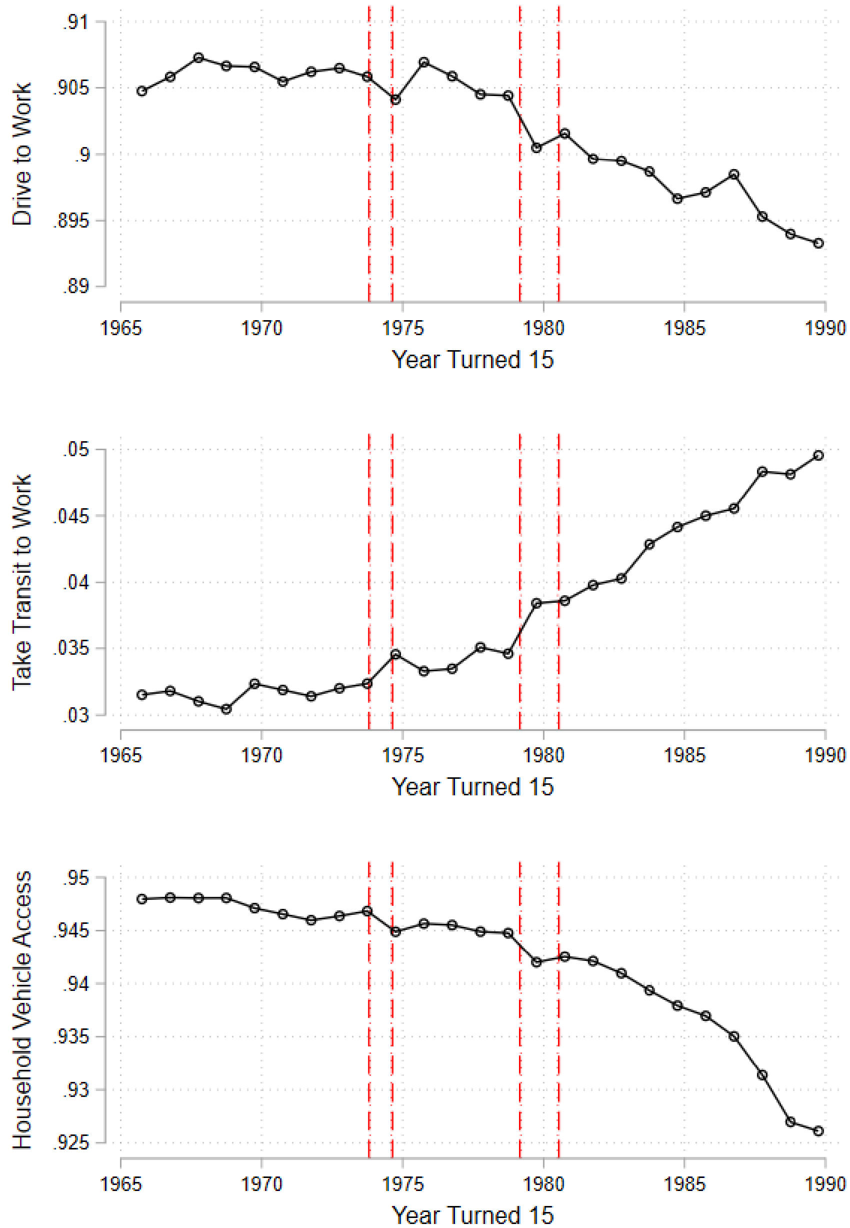

Figure 2: Commuting behavior and vehicle ownership by cohort in 2000. Age in year 2000 goes from 50 (left) to 25 (right).

Gasoline price increased from September 1973 until mid 1974, and from late 1978 until mid 1980, then bumped up further in 1981. We do not have a strong prior for the precise age at which gasoline prices become salient; in most states, learner's permits could be awarded at age 15 in 1980. Nonetheless, it is clear from Figure 2 that (i) the response to the 1979 oil crisis appears larger, and (ii) a break, if it exists, most likely occurs between either the 1974 and 1975 or the 1979 and 1980 cohorts.

We turn to an event study framework to quantify the size of the larger of these breaks, in 1979.[14] (The empirical design resembles a 'regression discontinuity in time' with birth year as the running variable; we refer to this setup as 'RD' for simplicity.) Though we discuss under what conditions these RD estimates have a causal interpretation below, our preferred estimates adopt a more comprehensive repeated cross-section approach that lets us characterize which ages are formative (see Section 4).

Specifically, we quantify the break by estimating variants of the following equation:

$$Y_i = \alpha + g(S_i) + \tau D_i + X_i'\lambda + \varepsilon_i \tag{1}$$

where Y_i is an outcome of interest for individual i in the 2000 census, S_i is the year that i turned 15, and X_i are other characteristics of i. Treatment is the binary variable D_i, which is equal to one if i turned 15 on or after April 2, 1979. The function $g(\cdot)$ is segmented before and after 1979 and captures trends in driving behavior; results are mostly robust to different functional form specifications. Data are limited to a symmetric bandwidth around the treatment year.[15] We interpret gas price at age 15 or 16 as exogenous, as demand shocks to early teen driving in the United States are unlikely to shift global gasoline prices.

Results indicate a significant response to the oil crisis (details are discussed in Appendix A.2 and shown in Appendix Tables A.3-A.6).[16] There is a sharp decrease in the likelihood of driving to work of 0.2–0.5 percentage points that persists roughly twenty years after turning 15 (Appendix Table A.3). The effect cannot be explained away by demographics or observable, contemporaneous characteristics. Those coming of age in

[14]We see a similar pattern of response to the earlier oil shock of 1973-74. Significant estimates of the effect vary from -0.35 to -0.45 percentage points, but results are generally less significant due to less precise timing and measurement error in census-reported birth year (see Section 2). Such an effect could contaminate wide-bandwidth estimates of the effects of the 1979 oil crisis. Rather than correct for this or estimate the effects of every significant oil crisis, we comprehensively use all available gasoline price variation in Section 4. Because the empirical approach in that section does not rely on bandwidth-based estimators, contamination is not a concern.

[15]We define the treatment time as just after 1979. Thus, a bandwidth of two includes cohorts that turn 15 in 1978, 1979, 1980, and 1981.

[16]We estimate τ using linear and quadratic trends varying the bandwidth from two to ten years.

1980 are also 0.2–0.4 percentage points more likely to take transit to work. The magnitude of the transit effect is between 50 and 100 percent of the driving effect, suggesting that transit is the primary substitute for driving (relative to working at home, carpooling, or self-powered means). Those coming of age in 1980 are also 0.2–0.3 percentage points less likely to have access to a vehicle (Appendix Table A.4). Though these effects may seem small, it is surprising to see any detectable effect given the stability of the driving share in the U.S. over time.[17] Further, though the causes and consequences (e.g., queuing and rationing) of this oil crisis may make it unique, we show in Section 4 that the magnitude of the estimates is consistent with our main analysis that leverages variation in gasoline prices over nearly four decades.[18]

The relationship between the gasoline price jump in 1979-80 and reduced later-life driving of cohorts coming of age in 1980 can be assigned a causal interpretation if no other observable or unobservable confounding factors experience a discontinuous break at the same point in time. Observable covariates are typically smooth across a range of demographic, employment, and housing characteristics (Appendix Figures A.2-A.4). There are no large discontinuities in these graphs, though some display more curvature than those in Figure 2. We also report results from 'donut' regression discontinuity tests that omit the 1980 cohort to alleviate measurement error due to the gradual change of prices throughout the years 1979-80. Results are similar in magnitude, though slightly less significant (Appendix Table A.5). We also show later that graduating into a recession does not explain these results. Taken together, this suggests that other factors are not likely responsible for the jump visible in Figure 2.

As a further check, we test for heterogeneity in the size of this effect along several dimensions (Appendix Table A.6). Some of these characteristics are determined after, and therefore potentially endogenous to, the 1970s oil crises, so we interpret results as suggestive. Whether or not commuters are able to substitute away from the automobile depends on the choices available to them. Therefore, we expect effects to be stronger in urban settings where there are plausible alternatives to driving. Among commuters

[17] For example, recent papers investigate if millennials have different preferences for driving than the previous generation (Klein and Smart 2017; Knittel and Murphy 2019; Leard, Linn, and Munnings 2019). These papers find similar generational patterns once income and demographics are accounted for. In contrast, we find a source of preference heterogeneity that persists over the life cycle and does not depend on income.

[18] Another specific feature of the first oil crisis is that it triggered the National Maximum Speed Law, which reduced the speed limit on freeways to 55 miles per hour in early 1974. This falls outside the bandwidth used for many estimates of the effects of the 1979-80 crisis presented in Appendix A.2 Moreover, we expect the effect of speed limit changes to be quite small relative to experiences with rationing and queuing.

that live within the 'principal city' of an MSA, those turning 35 in 2000 are 0.6 to 1.9 percentage points less likely to drive to work than someone (slightly older) turning 36. Conversely, there is little effect on workers who live outside of metropolitan areas. We also test for differences across the (contemporaneous) income distribution. Estimates for the lowest decile are negative (about -1.4 percentage points) and significant, and there is no or positive effect for the two highest deciles (Appendix Figure A.5).

The preceding analysis strongly suggests that cohorts turning 15 after 1979 exhibit different driving behavior later in life. This is not due to gasoline prices immediately leading up to 2000: everyone faced the same price profile in the preceding years. Further, there is a logical link between the high gas prices and reduced driving. While these results are suggestive, we now turn to more comprehensive and generalizable analysis.

4 Long-Run Driving Effects of Gasoline Prices

To more precisely link long-run behavior with formative exposure to gasoline prices, we use all available variation in gasoline prices across time and states with a fixed effects research design that uses repeated cross sections over nearly four decades. We tie the price of gasoline that someone likely experiences during their formative driving years to later-life driving behavior. We show results for different treatment age windows (such as between ages 15 and 17) and for the years around a state's minimum driving age. We use all available public-use census/ACS microdata since 1980 on commuting mode to study the extensive margin of driving behavior, and all NHTS data since 1990 to study the intensive-margin response and vehicle choice.

Our primary specification models outcome Y_{icst} for person i in cohort c born in state s observed in sample year t as:

$$Y_{icst} = \theta T_{cs} + \kappa_s + \delta_t + \eta_{t-c} + X'_{it}\lambda + \varepsilon_{icst} \tag{2}$$

where the treatment variable T_{cs} is either the price of gasoline or change in the price of gasoline during formative driving years for cohort c in state s. θ is the parameter of interest and measures the response in the outcome variable to gasoline price exposure at early driving ages. Different states may exhibit different behavior on average due to different provision of infrastructure, social norms, etc.; state fixed effects, κ_s, capture these differences. Sample year fixed effects, δ_t, control for current gas prices, business-cycle trends in employment, etc.[19] We also add age-at-time-of-sample fixed effects η_{t-c} to capture important life-cycle trends in transportation behavior. We include a vector of individual and household characteristics, X_{it}. We limit the sample to prime-age (25- to 54-year-old) native-born adults, and exclude residents of Alaska and Hawaii as we do not have a complete series of gasoline prices for these states. We discuss other sample adjustments that are particular to each dataset below. Standard errors are clustered by state.

Equation (2) is conceptually similar to a fixed effects difference-in-differences estimator. We observe cohorts, who have different formative experiences in the years around their initial driving age, at several points in time as they progress throughout the life cycle. Thus we are comparing, e.g., the driving behavior of a 36-year-old person in a state at a particular time with other 36-year-old people in the same state at earlier or later points in time.

We experiment with different definitions of treatment based on exposure to real gasoline prices during one's formative driving years. We use a data-driven approach to establish which ages are most formative, i.e., have the largest effects on later behavior. We consider both absolute 'calendar' age and age relative to the minimum driving age in state s. Finally, we test if later-life driving behavior is best explained by the *level* of gasoline prices during formative driving years or by *changes* in gasoline prices. Specifically, P_{cs}^{a} is the price of gasoline that cohort c in state s experienced at age a, and $P_{cs}^{m_{cs}}$ is the gasoline price that cohort c faced at the minimum full-privilege driving age in state s. We show results using several definitions of T_{cs}:

$$(i)\ P_{cs}^{\Delta a,(a-h)} = \frac{P_{cs}^{a} - P_{cs}^{a-h}}{P_{cs}^{a-h}}, \quad (ii)\ P_{cs}^{a},$$

$$(iii)\ P_{cs}^{\Delta(m_{cs}+j),(m_{cs}-k)} = \frac{P_{cs}^{m_{cs}+j} - P_{cs}^{m_{cs}-k}}{P_{cs}^{m_{cs}-k}}, \quad (iv)\ P_{cs}^{m_{cs}},$$

where we choose different combinations of $h \in \{1,2\}$ and $j,k \in \{0,1,2\}$. Choice (i) gives the percentage change in price between age a and $a-h$ and choice (ii) gives the price at age a. Choice (iii) represents the percentage price change during a window of ages around the minimum driving age; choice (iv) gives the price at the minimum driving age. Results (in Table 4 below) will reveal that gasoline price shocks between the ages of 15 and 18 (or between one year before and two years after the minimum driving age) matter

[19] We include state-by-sample year fixed effects in some specifications to flexibly control for local differences in these contemporaneous factors.

much more than levels.

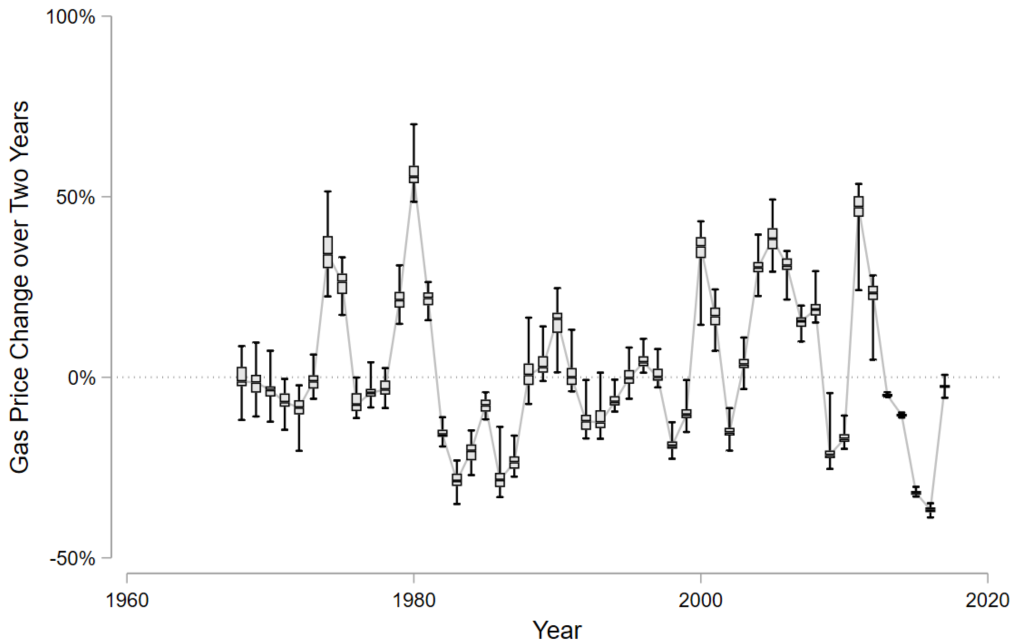

Figure 3: Box plot of 2-year lagged percentage changes in state-level gasoline prices

Figure 3 plots gasoline price exposure variable (i) for $h = 2$, showing the price changes for each calendar year rather than by cohort or age. The boxes and whiskers in Figure 3 indicate the quartiles of variation across states. Appendix Figure A.1 adds plots for price exposure variables (i) for $h = 1$ and (ii). These figures showing annual and biennial gasoline price changes highlight the variation used to identify long-run effects. Our preferred specification uses $P_{cs}^{\Delta 17,15}$, as the window from 15 to 17 covers most driver licensing uptake (see Section 2 and Section 5).[20]

Ideally, we would observe everyone's residential location in these formative years. However, census data only provide information on state of birth and current residence. We therefore define a sample of *stayers* who currently reside in their state of birth (about 64 percent of the full sample) as our primary population of analysis. However, we provide robustness analysis showing that our results are not sensitive to using the full sample (merged either on state of birth or state of current residence). NHTS data do not contain information about place of birth or prior migration decisions, so we merge on current state of residence.[21]

[20] See Appendix Table A.7 for summary statistics of the treatment variables in the sample.

[21] We are concerned about incorrectly matching people to gasoline prices experienced earlier in life (we

4.1 Extensive Margin

We first explicitly incorporate gasoline prices into the analysis of commuting mode. We merge data on driving behavior from public use census/ACS microdata available between 1980 and 2017 with gasoline prices based on respondent age and birth state. In most specifications, we only include those who still reside in the state of their birth. Our state-level gasoline price data begin in 1966, so our sample includes those for whom we can calculate our primary definition of treatment: $P_{cs}^{\Delta 17,15}$. This corresponds to cohorts born between 1951 and 1992.

4.1.1 Driving to Work

Estimates of Equation (2) on an indicator of driving to work are shown in Table 1. Note that each regression coefficient is from a separate linear probability model. Rows correspond to different definitions of treatment. We show specifications with gasoline price levels vs. changes, where the change is taken over either the ages 15 to 17 ($P_{cs}^{\Delta 17,15}$) or the two-year window around the minimum full-privilege driving age ($P_{cs}^{\Delta(m_{cs}+1),(m_{cs}-1)}$). Appendix Table A.8 shows results for various alternative specifications of treatment. Column (1) uses census year, state, and age fixed effects to capture life-cycle trends in commuting for those who reside in their state of birth when observed in the census (*stayers*). Columns (2) and (3) alter the sample. In Column (2), all workers are included regardless of current state of residence; gasoline prices reflect those in the state of birth. In Column (3), all workers are again included, but gasoline prices reflect current state of residence. Columns (4) to (7) progressively add more controls, again restricted to the *stayers* sample. Column (4) adds demographic controls for sex, marital status, educational attainment, and race. Column (5) adds log household income, which could influence a number of transportation and residential location margins. Column (6) adds state-by-sample year fixed effects to control for contemporaneous differences in driving conditions (current gas prices, parking restrictions, etc.). Finally, Column (7) includes quadratic birth-year trends to control for smooth, secular time trends in preferences or the driving environment.

First, note that driving to work responds to changes in gasoline prices, but hardly to

do not have geographic life histories). Given the life cycle of migration decisions, we believe including those who still reside in their state of birth is one reasonable approach (Kaplan and Schulhofer-Wohl 2017), though our results are robust to several strategies. A particular concern with this strategy is that is that some leave their birth state to attend college but later move back (having faced different prices). While we cannot rule this out, this margin for concern is likely small: even elite students are significantly more likely to matriculate to same-state schools (Griffith and Rothstein 2009; Bostwick 2016).

Table 1: The effect of formative gasoline prices on driving to work.

	1[drive] (1)	1[drive] (2)	1[drive] (3)	1[drive] (4)	1[drive] (5)	1[drive] (6)	1[drive] (7)
Exposure defined by age							
$P_{cs}^{\Delta 17,15}$	-0.0038***	-0.0028**	-0.0031***	-0.0037***	-0.0039***	-0.0039***	-0.0043***
	(0.0010)	(0.0008)	(0.0009)	(0.0010)	(0.0010)	(0.0010)	(0.0009)
P_{cs}^{16}	-0.0007	0.0012+	-0.0029***	-0.0009	-0.0011	-0.0011	-0.0011
	(0.0010)	(0.0006)	(0.0007)	(0.0008)	(0.0009)	(0.0008)	(0.0008)
Exposure defined by minimum driver license age							
$P_{cs}^{\Delta(m_{cs}+1,m_{cs}-1)}$	-0.0041***	-0.0038***	-0.0040***	-0.0040***	-0.0040***	-0.0042***	-0.0045***
	(0.0010)	(0.0008)	(0.0008)	(0.0011)	(0.0010)	(0.0011)	(0.0010)
$P_{cs}^{m_{cs}}$	-0.0012	0.0006	-0.0012	-0.0013	-0.0015	-0.0015+	-0.0015+
	(0.0010)	(0.0006)	(0.0010)	(0.0009)	(0.0009)	(0.0008)	(0.0008)
Census year FEs	Y	Y	Y	Y	Y	-	-
State of birth FEs	Y	Y	Y	Y	Y	-	-
Age FEs	Y	Y	Y	Y	Y	Y	Y
Demographics	-	-	-	Y	Y	Y	Y
ln HH income	-	-	-	-	Y	Y	Y
State-X-year FEs	-	-	-	-	-	Y	Y
Quad. birth year	-	-	-	-	-	-	Y
Price in state of	Birth	Birth	Res	Birth	Birth	Birth	Birth
Sample	Stay	All	All	Stay	Stay	Stay	Stay

Each row and column represents the results from a different regression, for twenty-eight total. Dependent variable is a binary indicator of whether the respondent drove to work, as reported in the census. Sample includes all native-born persons actively working in the census between the ages of 25-54, and excludes farm workers and those coded N/A for transportation mode. Demographics include sex, marital status, educational attainment, and race. Observations weighted by person sample weights. Standard errors clustered by state of birth. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

levels. For the specifications using gasoline price changes in Columns (1) to (3), all coefficients are negative and significant, and the effect size varies from -0.3 to -0.4 percentage points. Estimates are similar both for the 15 to 17 calendar age range and for the window one year before to one year after the minimum driving age. The results in Column (1) indicate that a doubling in the price of gasoline between the ages of 15 and 17 ($P_{cs}^{\Delta 17,15} = 1$) leads to a 0.38 percentage point (0.43 percent reduction) in driving to work later in life.[22] For the two-year window around the minimum driving age, the effect is -0.41 percentage points (0.46 percent). The precise magnitude slightly varies across specifications, but is always statistically significant.

[22]In our sample, 88.3 percent of workers drive to work.

Columns (4) through (7) condition on contemporaneous covariates or fixed effects. These factors, such as income, educational attainment, or current state, could be influenced by economic conditions during formative years (which might be correlated with gasoline price shocks). If such channels are important, including these controls could lead to biased estimates of the effect. However, these controls could also account for other effects of coming of age during a period of increasing gasoline prices. For completeness, we include them in Columns (4) to (7). They make little difference, strongly suggesting that income and education are not primary mechanisms for our results. Furthermore, the magnitude of these estimates corresponds to the effect measured in Section 3 (reflecting the doubling of prices in between 1979 and 1981). The aggregate share of those commuting to work by car in the U.S. has shifted by no more than 4 percentage points since 1980. The effect we find shifts the average commuting behavior of entire cohorts by 10-12% of this aggregate movement.

Appendix Table A.8 shows that exposure to gasoline price changes during the 15-17 age range (or the window from one year before to one year after the minimum driving age) is associated with the strongest impact on later-life driving, but effects remain significant when using $P_{cs}^{\Delta(m_{cs}+2),(m_{cs}+1)}$ or $P_{cs}^{\Delta 18,17}$ (for the latter case, significance remains for only some of the specifications). Together these results suggest that the formative effect is strongest between the ages of 15 and 17.[23] We later show that there is no impact of gasoline price changes experienced at younger ages.

That gasoline price changes, not levels, matter most stands in contrast to the predictions generated by habit-formation models in economics (Pollak 1970; Becker and Murphy 1988), in which current preferences depend on past consumption and price levels. Such models suggest that a cumulative average (with diminishing weights on earlier years) over the level of consumption determines current preferences; price *changes* do not play a direct role. Hence, our finding that gasoline price changes during formative years determine later-life behavior suggests that habit formation models cannot explain long-run consumer preferences in our context. Our findings are consistent, however, with recent work by Haushofer and Fehr (2019), who show that price shocks—not levels—can determine subsequent behavior.

[23]We discuss the use of cohort fixed effects in Appendix A.2. Most gasoline price variation is temporal rather than cross sectional, and the use of cohort fixed effects absorbs much of this variation. Nonetheless, extensive-margin results are very similar to those presented here. Intensive-margin results are noisy and suffer from relatively small samples relative to the extensive-margin analysis.

4.1.2 Recession and Income Mediation

Gasoline price movements can be associated with recessions, and recessions experienced when first entering the labor market can have long-run consequences (Oreopoulos, von Wachter, and Heisz 2012; Stuart 2017). If a gasoline price shock during the formative window impacts labor market outcomes (through income, cohort effects, labor market entry timing, educational attainment, etc.), and labor market outcomes influence transportation behavior, then some portion of the effect shown in Table 1 may be *indirectly* due to recessions rather than *directly* from the price shock. Thus, the negative effects in Columns (1) to (4) in Table 1 could in principle represent effects mediated through other channels.

Three pieces of evidence argue against these mediated channels as the main explanation of the effect we find. First, Columns (5) to (7) in Table 1 control for contemporaneous income (and Columns (4) through (7) include controls for educational attainment). Results including a measure of recession at age 18 are also similar (see below and Appendix Table A.9). The stability of the estimates suggests that long-run, recession-driven changes in income cannot explain the observed effect. Second, results are similar when we exclude cohorts born in 1963 to 1965 (those that came of driving age during the 1979 oil crisis).[24] Finally, we conduct mediation analysis to quantify the indirect effect of these gasoline price shocks on later-life driving.

We formalize the mediation model in Appendix A.3. The analysis consists of two equations: The first equation models later-life driving behavior as a function of both formative-year gasoline price shocks and either the state-specific unemployment rate at age 18 or contemporaneous income. The coefficient on this second term captures the effect of the mediating factor on driving in general; it is essentially a control. The second equation models age 18 unemployment or contemporaneous income as a function of the formative-year gasoline price shocks to measure the part of the mediating factor driven by the gasoline price shock. Multiplying the latter two of these coefficients gives the indirect (mediated) effect.

Results are reported in Appendix Table A.9 and show that higher age 18 unemployment is indeed positively associated with—and income is negatively associated with—upward gasoline price shocks during formative years. However, the mediation analysis reveals that 0% of the effect can be explained by graduating into a recession, and between

[24]More precisely, when we estimate the same model as in Column (1) of Table 1 but exclude the 1963 to 1965 birth cohorts, the coefficient on $P_{cs}^{\Delta 17,15}$ is -0.0042** (s.e. 0.0015), slightly less precise but larger in magnitude than when these cohorts are included.

2% and 24% of the effect can be explained by later-life income, depending on the measure of income used (the average across all estimates of mediation gives 10%). Reduced income due to the experience of recessions during formative years is not the primary component driving the results in Table 1.

4.1.3 Other Extensive Margins (Transit Use, Vehicle Ownership)

We also investigate if those who experience positive gasoline price shocks during formative years substitute from driving to transit and if they are less likely to have access to a vehicle. Table 2 summarizes the results for the same definitions of treatment as in Table 1. In Columns (1) and (2), the outcome variable is transit usage; in Columns (3) to (6), it is access to a vehicle. We find that about one-half to three-quarters of the extensive-margin effect is accounted for by a shift to transit. There may also be a small, negative effect of increases in gas prices on vehicle ownership (about one-quarter to one-half of the extensive-margin effect). The effect is not extremely robust, although more so in the sample that includes all household members than in the employed sample.

4.2 Intensive Margin

We next perform a similar exercise to study the intensive margin: miles of driving and the fuel economy of the vehicles owned. We merge data on vehicle miles traveled (VMT) from all NHTS waves since 1990 with gasoline prices based on respondent age and current state. There are five waves in this dataset: 1990, 1995, 2001, 2009, and 2017. The NHTS also reports the vehicle's make and model, which we use to identify the type of vehicle (car vs. light-duty truck) and fuel economy. Household income is reported in binned values; we standardize these to correspond to quintiles of the income distribution and interact them with sample year dummies to flexibly capture changes in income patterns.

4.2.1 Miles of Driving

One might expect that drivers who were exposed to large gasoline price increases during their initial driving years perceive driving as more costly throughout their lives and drive fewer miles. Estimates of Equation (2) on the log of miles traveled are reported in Table 3. Column (1) uses NHTS sample year, state of residence, and age fixed effects. Column (2) adds demographic controls for race, urban/rural status, and family size. Column (3) adds the household income bins interacted with observation year. Columns (4) and (5)

Table 2: The effect of formative gasoline prices on other census outcomes.

	Transit usage		Vehicle available			
	1[transit] (1)	1[transit] (2)	1[vehicle] (3)	1[vehicle] (4)	1[vehicle] (5)	1[vehicle] (6)
Exposure defined by age						
$P_{cs}^{\Delta 17,15}$	0.0029***	0.0024**	-0.0014	-0.0009	-0.0019*	-0.0018**
	(0.0007)	(0.0009)	(0.0008)	(0.0006)	(0.0009)	(0.0006)
P_{cs}^{16}	0.0001	0.0004	0.0004	0.0007	-0.0007	-0.0001
	(0.0007)	(0.0005)	(0.0007)	(0.0005)	(0.0009)	(0.0007)
Exposure defined by minimum driver license age						
$P_{cs}^{\Delta(m_{cs}+1,m_{cs})}$	0.0028*	0.0021	-0.0025	-0.0023+	-0.0019	-0.0022
	(0.0012)	(0.0013)	(0.0016)	(0.0013)	(0.0016)	(0.0013)
$P_{cs}^{m_{cs}}$	0.0006	0.0008	0.0001	0.0003	-0.0008	-0.0005
	(0.0007)	(0.0005)	(0.0007)	(0.0005)	(0.0008)	(0.0006)
Census year FEs	Y	-	Y	-	Y	-
State of birth FEs	Y	-	Y	-	Y	-
Age FEs	Y	Y	Y	Y	Y	Y
Demographics	-	Y	-	Y	-	Y
ln HH income	-	Y	-	Y	-	Y
State-X-year FEs	-	Y	-	Y	-	Y
Quad. birth year	-	Y	-	Y	-	Y
Sample	Empl	Empl	Empl	Empl	All	All

Each row and column represents the results from a different regression, for twenty-four total. Dependent variable is either an indicator for transit usage or whether a vehicle is present in the household. Sample includes all native-born persons actively working in the census between the ages of 25-54 and still living in their state of birth, and excludes farm workers and, for transit use, those coded N/A for transportation mode. Demographics include sex, marital status, educational attainment, and race. Observations weighted by person sample weights. Standard errors clustered by state of birth. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

add more flexible state-by-year fixed effects to control for contemporaneous differences in driving conditions (current gas prices, parking restrictions, etc.) Column (5) adds a quadratic birth year trend.

These estimates represent the long-run elasticity of driving behavior to formative gasoline price changes. As with the extensive-margin results, gasoline price changes during early driving years matter while levels do not. Coefficients for specifications using changes are negative and significant whether controls or additional fixed effects are included or not. The effect varies from -3.4 to -8.2 percent—a substantial and meaningful response. A doubling in the price of gasoline between the ages of 15 and 17 reduces later-

Table 3: The effect of formative gasoline prices on log miles traveled (using NHTS).

	ln(VMT) (1)	ln(VMT) (2)	ln(VMT) (3)	ln(VMT) (4)	ln(VMT) (5)
Exposure defined by age					
$P_{cs}^{\Delta 17,15}$	-0.0786**	-0.0822**	-0.0771**	-0.0773**	-0.0624*
	(0.0264)	(0.0260)	(0.0261)	(0.0259)	(0.0255)
P_{cs}^{16}	0.0213+	0.0202+	0.0190+	0.0198+	0.0032
	(0.0109)	(0.0110)	(0.0109)	(0.0111)	(0.0096)
Exposure defined by minimum driver license age					
$P_{cs}^{\Delta(m_{cs}+1,m_{cs}-1)}$	-0.0502*	-0.0567**	-0.0470*	-0.0478*	-0.0344+
	(0.0193)	(0.0197)	(0.0201)	(0.0204)	(0.0196)
$P_{cs}^{m_{cs}}$	0.0147	0.0127	0.0108	0.0108	-0.0027
	(0.0120)	(0.0120)	(0.0117)	(0.0118)	(0.0107)
Sample year FEs	Y	Y	Y	-	-
State FEs	Y	Y	Y	-	-
Age FEs	Y	Y	Y	Y	Y
Controls	-	Y	Y	Y	Y
Income-by-year bin FEs	-	-	Y	Y	Y
State-X-year FEs	-	-	-	Y	Y
Quad. birth year	-	-	-	-	Y

Each row and column represents the results from a different regression, for twenty total. Dependent variable is log person VMT. Sample includes all respondents aged 25-54 with positive person VMT. Demographics include race, urbanization, and family size. Observations weighted by person sample weights. Standard errors clustered by state. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

life miles traveled by 6.2 to 8.2 percent; for the age window one year before to one year after the minimum driving age, the effect of doubling gasoline prices on miles traveled is -3.4 to -5.7 percent.[25] As with the extensive-margin analysis, including controls measured contemporaneously in Columns (2) to (5) could introduce bias in the estimated effect. However, these controls also capture other important differences between drivers. Regardless, the effect size is not greatly impacted by their inclusion. The specification with a quadratic trend in birth year in Column (5) shows a slightly attenuated effect on miles traveled.

Appendix Table A.11 shows the results for a broader range of treatment ages. Results behave similarly for these other treatment definitions, and effects remain significant for

[25]In our sample, respondents drive about 14,080 miles per year on average across the vehicles for which they are described as the main driver.

most alternatives.[26] Compared with the extensive-margin results, the effects are large in magnitude and strongly present for the entire 15 to 18 age range, with the largest treatment effect for $P_{cs}^{17,16}$. Together, these effects point to a persistent impact of experiences with gasoline prices between the ages of 15 and 18 on later-life driving habits—both on the extensive and (particularly) the intensive margin.

4.2.2 Fuel Economy and Vehicle Choice

Besides miles traveled, another margin of response is the fuel efficiency of the type of vehicle chosen by a driver. Appendix Table A.13 reports two additional outcome variables: the fuel-consumption rate in gallons-per-mile (GPM), and whether a driver owns a more fuel-efficient passenger car or a less fuel-efficient light-duty truck (pickup, SUV, or minivan). As the NHTS reports data at both the person and the vehicle level, we use the average fuel-consumption rate per driver (across the vehicles to which the person is assigned as the main driver) as the outcome variable in Columns (1) and (2); Columns (3) and (4) use GPM at the vehicle level. Columns (5) through (8) have a similar structure, now looking at the effect of formative gasoline price changes on large-vehicle ownership. All specifications include a rich set of fixed effects and controls. We also include vehicle age and a quadratic trend in vehicle model year to compare drivers of a vehicle of a certain vintage and age.

We find zero effect on the fuel-consumption rating. This is not entirely surprising given that fuel economy is measured with considerable error.[27] We do find a negative effect on the large-vehicle indicator, suggesting that those who experienced a doubling of gasoline prices during their initial driving years are 1.1-2.7 percentage points less likely to drive a light-duty truck. The results are statistically significant in half of the specifications. All in all, we interpret this as modest suggestive evidence that gasoline price shocks have long-term effects on the types of vehicles that people drive, but more precisely measured fuel-economy data would be required to estimate the effect with a higher degree of confidence.

[26]The effect is also similar when restricting the sample to solely employed or unemployed persons (though with some loss of power), thus compositional differences in travel purpose are not first order.

[27]Fuel-economy ratings vary substantially within make and model, but we are unable to match this given the coarseness of the NHTS data.

4.3 The Formative Window, Cumulative Exposure, and Persistence

To show that the timing of gasoline price shocks matters, and to identify at what ages the formative experiences are most powerful, we merge later-life driving behavior to gasoline price shocks experienced between ages 13 and 25. We similarly merge later-life driving behavior to gasoline prices several years before and after state-level minimum driving ages. We modify Equation (2) to accommodate heterogeneous effects by age at exposure, but otherwise preserve a similar specification (with observation year, state, and age fixed effects) as that used in Column (1) in Tables 1 and 3:

$$Y_{icst} = \sum_{a=13}^{20} \theta_a P_{cs}^{\Delta a,a-1} + \kappa_s + \delta_t + \eta_{t-c} + X_{it}'\lambda + \varepsilon_{icst} \tag{3}$$

Estimates of the long-run effect of early-life exposure to gasoline price shocks are shown for two windows of ages, 14 to 19 and 13 to 20, in Table 4.[28] Results generally show that gasoline price shocks between the ages of 15 and 18 influence later-life driving behaviors, whereas shocks before or after do not matter. The largest coefficients in magnitude are on $P_{cs}^{\Delta 16,15}$. For the extensive margin, the effect of shocks between 16 and 17 is much smaller, and fades away thereafter. Estimates of the long-run intensive-margin effect on miles traveled are reported in Columns (3) and (4). The largest coefficients again are for shocks experienced between age 15 and 16, but shocks between ages 16 and 17 are nearly as large and shocks between 17 and 18 not much smaller. The most responsive age range is generally between 15 and 18 years, or between one year before and two or three years after the minimum full-privilege driving age.

The evidence for a short formative window of early experiences in Table 4 confirms that personal experiences influence behavior in the long run, but in a way that is different from what the extant literature suggests. The sharp time horizon in which the effect is present argues that initial experiences with a good matter more than mental plasticity during the first decade of early adulthood, which instead predicts an effect over a broad range of ages (e.g., (Giuliano and Spilimbergo 2013)). Social-psychology theories of experience-based learning—the process of learning directly through experiences through-

[28]While Table 4 shows estimates of Equation (3) that include gasoline price shocks for several ages of exposure, Appendix Tables A.14 and A.15 show many estimates of Equation (3) each using only the shock at a single age: $P_{cs}^{\Delta a,a-1}$ with ages $a \in \{13, 14, \ldots, 21, 22\}$. These tables also include specifications in which the formative window is defined using state-level minimum driving ages: $P_{cs}^{\Delta(m_{cs}+\tau),(m_{cs}+\tau-1)}$ with years relative to the state-cohort specific ages $\tau \in \{-3, -2, \ldots, 5, 6\}$. Results are generally very similar.

Table 4: The effect of gasoline price changes at different ages.

	Extensive margin		Intensive margin	
	1[drive] (1)	1[drive] (2)	ln(VMT) (3)	ln(VMT) (4)
$P_{cs}^{\Delta 13,12}$		-0.0007		-0.0633
		(0.0018)		(0.0587)
$P_{cs}^{\Delta 14,13}$	-0.0002	-0.0002	0.0009	0.0084
	(0.0015)	(0.0016)	(0.0334)	(0.0415)
$P_{cs}^{\Delta 15,14}$	-0.0002	-0.0003	0.0162	0.0002
	(0.0019)	(0.0022)	(0.0433)	(0.0450)
$P_{cs}^{\Delta 16,15}$	-0.0057**	-0.0057**	-0.1012*	-0.0929+
	(0.0019)	(0.0021)	(0.0480)	(0.0520)
$P_{cs}^{\Delta 17,16}$	-0.0027+	-0.0026	-0.0795+	-0.0960*
	(0.0015)	(0.0017)	(0.0413)	(0.0411)
$P_{cs}^{\Delta 18,17}$	-0.0024	-0.0023	-0.0847*	-0.0658+
	(0.0017)	(0.0019)	(0.0386)	(0.0384)
$P_{cs}^{\Delta 19,18}$	-0.0013	-0.0013	-0.0545	-0.0712
	(0.0017)	(0.0018)	(0.0495)	(0.0465)
$P_{cs}^{\Delta 20,19}$		-0.0006		-0.0143
		(0.0019)		(0.0458)
Sample year FEs	Y	Y	Y	Y
State FEs	Y	Y	Y	Y
Age FEs	Y	Y	Y	Y

Dependent variable in Columns (1) and (2) is a binary indicator of whether the respondent drove to work. Dependent variable in Columns (3) and (4) is log person VMT. Observations weighted by person sample weights. Standard errors clustered by state. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

out one's life—predict an effect over an even broader range of years, likely with more weight given to recent experiences. Nor are economic theories of habit formation consistent with a short, critical formative period early in life. Standard models of habit formation (Pollak 1970; Becker and Murphy 1988) predict that current consumption depends on prior consumption, and so only indirectly on price levels; such predictions translate to a cumulative exposure function that places higher weights on recent periods.

We compare our methodology and results to a common approach taken in the literature. Malmendier and Nagel (2011) develop a single-parameter cumulative exposure function that measures the weighted average of exposure to a treatment variable over some period of time. The parameter determines whether recent experiences matter more or less relative to earlier experiences (the 'shape' of experience). The exposure function restricts the effects of exposure to be weakly monontonic, and does not permit windows during which exposure exclusively matters (formative years). Malmendier and Nagel (2011) find that the shape parameter has a positive value, meaning that recent experiences matter substantially more than early-life experiences.

The cumulative exposure function is denoted by $A_{cst}(\omega, \mathbf{T}_{st})$. Its first argument is the shape parameter ω, which weights recent versus earlier experiences. For $\omega > 0$, recent experiences matter relatively more than earlier experiences, whereas earlier experiences matter more than recent experiences if $\omega < 0$. The second argument is the vector of treatments $\mathbf{T}_{st}$. This vector varies by state s and by year t (it includes enough lagged years to populate the treatment experience of the oldest person in the sample). However, the precise way that A weights treatments depends on the age of the cohort under consideration. Specifically,

$$A_{cst}(\omega, \mathbf{T}_{st}) = \sum_{k=15}^{\text{age}_{ct}-1} w_{ct}(k, \omega) T_{s,t-(\text{age}_{ct}-k)} \tag{4}$$

where the weighting function is given by:

$$w_{ct}(k, \omega) = \frac{(k-14)^{\omega}}{\sum_{k=15}^{\text{age}_{ct}-1}(k-14)^{\omega}} \tag{5}$$

Recall that t is the year of observation, so that age_{ct} is the age of cohort c *in the year they are observed*. Based on the results in Table A.14, we only allow exposure to 'turn on' at age 15 (and so the minimum value of k is 15).[29] Thus, for year t in which we observe a 35 year old, the earliest value of T_{st} used in the cumulative exposure is $t-(35-15) = t-20$.

We include the non-linear cumulative exposure function into an otherwise linear regression model similar to Equation (2):[30]

$$Y_{icst} = \beta A_{cst}(\omega, \mathbf{T}_{st}) + \kappa_s + \delta_t + \eta_{t-c} + \varepsilon_{icst} \tag{6}$$

In order to compare the treatment effect from Equation (2) (denoted θ) with the estimated cumulative impact β, note that

$$\frac{\partial Y_{icst}}{\partial T_{s,t-(\text{age}_{ct}-k)}} = \theta_{[k]} = \beta w_{ct}(k, \omega) \tag{7}$$

where $\theta_{[k]}$ is the effect of a gas price shock at during the formative age k (i.e., one of the

[29]Malmendier and Nagel (2011) instead have exposure 'turn on' at birth.

[30]Because Equation (6) is non-linear in ω, non-linear estimation methods are required. We describe our approach in Appendix A.4.

estimated effects in Table A.14). The cumulative effect β must be down-weighted by the weighting function to be comparable to θ. So, for someone 35 years old in sample year t, the effect of treatment experienced at age 15 (i.e., 20 years ago) is $\theta_{[15]}$ in Equation (2) and $\beta w_{ct}(15, \omega)$ in Equation (6).

We estimate Equation (6) using the cumulative exposure to one-year gasoline price shocks starting between the ages of 15 and 16, and ending with the shock between the year prior to observation and the year of observation (denoted $\mathbf{P}_s^{\Delta 1\text{yr}}$). We model both the extensive and intensive margins. Coefficient estimates for β and ω are presented in Table 5. Estimates of β indicate that positive shocks generally reduce both extensive and intensive margins of driving, while estimate of ω indicate that *earlier* experiences matter substantially more than more recent experience. To give a sense of magnitude, for someone aged 36 when sampled, the effect of a gasoline price shock twenty years earlier is roughly 25.3 times as important as a shock within the prior year for whether or not they drive, and 2.7 times as important for how much they drive.

Table 5: Cumulative exposure function results.

	Extensive margin	Intensive margin
	1[drive] (1)	ln(VMT) (2)
β ($A_{cst}(\omega, \mathbf{P}_s^{\Delta 1\text{yr}})$)	-0.0140**	-0.6796***
	(0.0045)	(0.1809)
ω (shape)	-1.0786***	-0.3294*
	(0.2796)	(0.1617)
Sample year FEs	Y	Y
State FEs	Y	Y
Age FEs	Y	Y

Dependent variable in Column (1) is a binary indicator of whether the respondent drove to work; dependent variable in Column (2) is log person VMT. Sample in Column (1) limited to stayers. Observations weighted by person sample weights. Standard errors clustered by state. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

To facilitate comparison, Figure 4 extends the heterogeneous effects analysis of Table 4 through age 25 and plots them with the marginal effects of the cumulative exposure functions estimated in Table 5 (90% and 95% confidence intervals are shown with vertical spikes). These marginal effects represent the values of $\beta w_{ct}(k, \omega)$ evaluated at each k from

16 to 25 for a 39 year old adult (the average age in our sample).[31] Recall that exposure at $k = 16$ refers to the price shock between ages 15 and 16. Note that we use information from Table A.14 to set the earliest allowable exposure age in $A_{cst}(\omega, \mathbf{T}_{st})$. Without such information, the cumulative exposure function is unlikely to accurately reflect the exposure response and may oversmooth the effect.

Figure 4 reveals that both our approach and the cumulative weighting function suggest that early experience shortly after coming of driving age matter substantially more than more recent experiences. This contrasts with most results in the literature, which typically support experience-based learning in which recent experiences dominate earlier ones. Indeed, the single-year results in Table A.14 reflect a qualitatively similar path of exposure response as the cumulative exposure function. However, conclusions related to a formative window are different. By construction, the cumulative exposure function disallows differentiating eras by effect significance; either exposure matters at every age or it does not matter at all (though the effect of exposure can increase or diminish). Our approach finds a formative window between the ages of 15 and 18, and does not require using the cumulative exposure function and its implicit functional form assumptions.

We also ask whether these effects only last for a few years (say, for people in their early 30s) or whether they are persistent throughout the life cycle. We test this by allowing heterogeneous treatment effects by 10-year age bins in Equation (2). Results (shown in Appendix Table A.16) reveal some differences in persistence across the intensive and extensive margins and specifications of the formative window. Generally, we conclude that the effects are stronger for younger (age 25 to 34) and older (age 45 to 54) drivers, with little discernible effect for those age 35 to 44—possibly due to greater reliance on private vehicles when raising children. On the intensive margin, conclusions are broadly similar, but estimates are less precise.

5 Interpretation and Mechanisms

Empirical results presented so far demonstrate that gasoline price shocks during a relatively short formative period early in life have an effect on driving behavior in the long run. We now discuss if costly skill acquisition is a potential mechanism that can explain our results. If learning to drive has high time, vehicle, and/or fuel costs, increases in

[31] Plotting the marginal effects for an adult of a different age somewhat alters the slope (steeper for younger, flatter for older), but does not qualitatively change the conclusions.

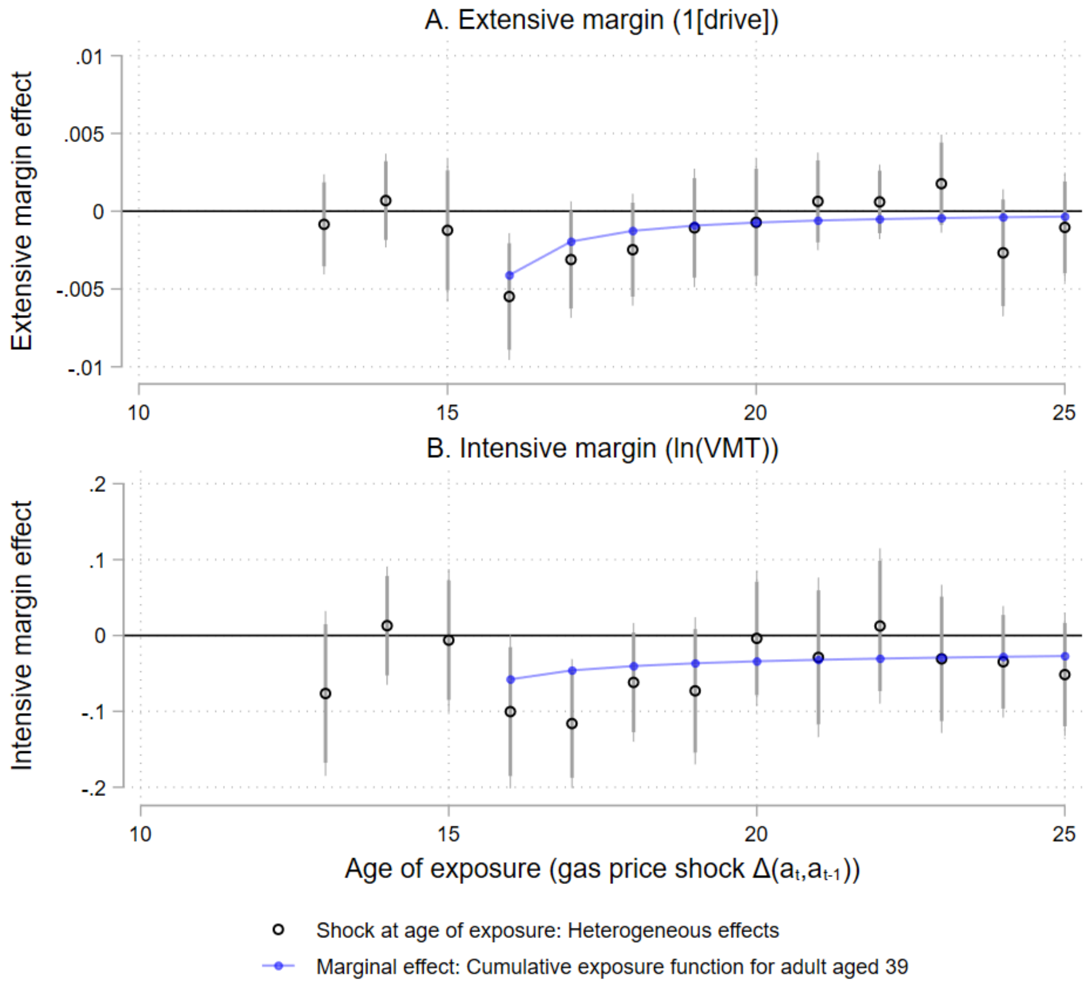

Figure 4: Single year vs. cumulative exposure effects of gasoline price shocks

these costs may have long-run impacts on driving adoption. Given that most drivers in the U.S. have historically learned to drive before they turn 18, parental inputs are also important. With a binding constraint on gas expenses, households may delay or completely avoid teenage driver training in favor of other necessary commuting expenses. Do high gasoline prices keep people from learning to drive in the long run?

We provide several pieces of evidence showing that costly skill acquisition is unlikely to wholly explain the results in Sections 3 and 4. First, it is difficult to assert that the intensive-margin effect for those who own a vehicle is due to a high cost of learning to drive. Hence, the presence of an intensive-margin effect on miles driven is highly suggestive that delayed skill acquisition cannot be the sole or dominant explanation for the long-run effects on driving behavior. Second, there does not appear to be a reduction in the take up of teen driver licenses in response to the 1979 oil crisis. Finally, we show that regulations that explicitly restrict teenage driving through minimum driver licensing age requirements do not have negative effects on later-life driving rates.

Together, these null results suggest a role for formative experiences that shape preferences for the long run. Teen's preferences for driving or perceptions of the costs of driving are impacted by gasoline price shocks, and these differences persist. We cannot distinguish between a shift in a deep preference parameter and how preferences are filtered by updated perceptions of the cost of driving.

5.1 Evidence from Driver Licensing Counts

We return to the gasoline price shock induced by the 1979 oil crisis and examine the response in driver licensing. We compute the percentage of each cohort with a license by a certain age. This statistic is not directly observable in the data, but the FWHA publishes annual data on the number of drivers at each age from 15 to 24. We combine these data with supplemental estimates of the age distribution of the population to generate percentage licensed by age. In general, other factors (such as changes in minimum driver licensing age) were mostly constant during the late 1970s and early 1980s.

There is not a noticeable change in driver licensing following the 1979 oil crisis, although the data are somewhat noisy. Figure 5 shows the percentage of each age (16, 17, 18, 20, and 22) that has a license in each year.[32] Timing for ages is based on calendar years rather than individual birthdays, so the lines should be read as "the percentage of those aged 16 at the end of the year who received a license by the end of the year." Driver saturation is generally smooth and slightly decreasing, though there may be a slight depression in some of the series in 1981 and 1982 but a slight uptick for others. All things considered, we do not find clear evidence for a delay in driver license uptake. This suggests that, at least on the extensive margin, price volatility of the 1979 oil crisis did not lead to differential habit formation for impacted cohorts; this as another piece of evidence against habit formation in this context.

[32] Figure 5 omits 1983 and 1985 because the driver license counts by age in those years was extrapolated from prior years. These data are noisy, but show a marked increase in driver licenses of 18 year olds in 1983.

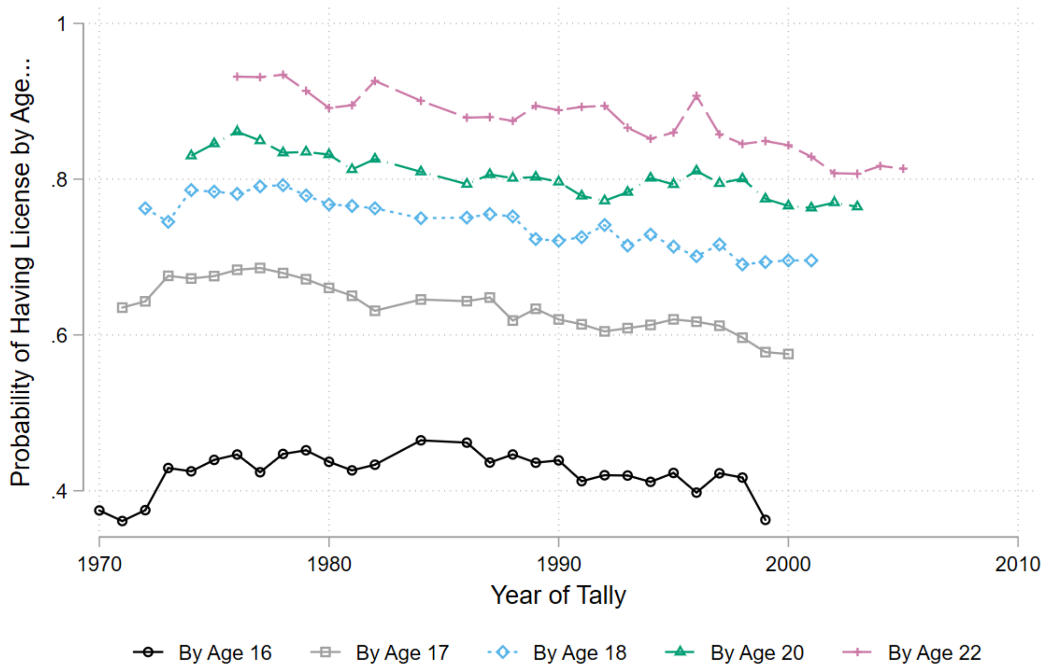

Figure 5: The probability of having a full-privilege driver license by calendar year and age.

5.2 Evidence from Driver License Minimum Age Requirements

Legislative restrictions provide another avenue that potentially limit driver training. If high gas prices delay driving skill acquisition and reduce later-life driving because of this delay, it is likely that directly delaying driver skill acquisition through driving age restrictions will also reduce later-life driving. We combine data from several sources to develop a panel of teenage driver license requirements covering 1967 to 2017 to test this channel.[33]

We test for extensive- and intensive-margin effects of changing minimum driving age restrictions. We first construct two measures of minimum driving age. The first measure, minimum full-privilege age, gives the minimum age at which a suitably trained teenager can obtain an unrestricted license (i.e., with no restrictions on time of use, purpose, destination, or passengers). This is very similar to the age used to merge gas price relative

[33]Our two primary sources are the FHWA's *Driver License Administration Requirements and Fees* report and a database of graduated driver license (GDL) adoptions from the Insurance Institute for Highway Safety.

to driver license age in Section 4.[34] The other measure, minimum intermediate license age, captures the minimum age at which drivers can make unaccompanied trips but with some restrictions.[35] Summary information on these measures is shown in Appendix Table A.1. We include both measures of restrictiveness in each specification.

Table 6: Do youth driving restrictions affect later driving behavior?

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Extensive margin (1[drive])						
Minimum Full-Privilege Age	0.0078	0.0048	0.0071	0.0072	0.0082+	0.0092
	(0.0052)	(0.0040)	(0.0047)	(0.0048)	(0.0048)	(0.0056)
Minimum Intermediate License Age	-0.0107	-0.0088	-0.0091	-0.0097	-0.0137	-0.0124
	(0.0147)	(0.0122)	(0.0136)	(0.0138)	(0.0127)	(0.0121)
Panel B: Intensive margin (ln(person VMT))						
Minimum Full-Privilege Age	0.0012		0.0010	-0.0030	-0.0108	0.0196
	(0.0129)		(0.0132)	(0.0159)	(0.0182)	(0.0143)
Minimum Intermediate License Age	-0.0269		-0.0239	-0.0270	-0.0007	0.0239
	(0.0651)		(0.0565)	(0.0592)	(0.0699)	(0.0588)
Sample year FEs	Y	Y	Y	Y	-	-
State FEs	Y	Y	Y	Y	-	-
Age FEs	Y	Y	Y	Y	Y	Y
Dem. controls	-	-	Y	Y	Y	Y
Income controls	-	-	-	Y	Y	Y
State-X-year FEs	-	-	-	-	Y	Y
Quad. birth year	-	-	-	-	-	Y
Sample	Stay	All	Stay	Stay	Stay	Stay

Each panel and column contains the results from a different regression, for eleven total. Dependent variable in first panel is a binary indicator of whether the respondent drove to work; demographics include sex, marital status, educational attainment, and race; and income is log household income. Dependent variable in second panel is log person VMT; demographics include race, urbanization, and family size; and income is income bins interacted with sample year. Observations weighted by person sample weights. Standard errors clustered by state. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Columns (1) and (2) in Table 6 show the effects from models with sample year, state, and age fixed effects. Column (2) includes the full sample for the census data, which is otherwise restricted to the *stayers* sample. Column (3) adds the respective demographic controls as in Table 1 and 3. Column (4) adds income controls, while Column (5) adds state-by-sample year fixed effects, and Column (6) includes a quadratic trend in birth

[34]We use minimum age in years and months to define treatment here, whereas in Section 4 this variable is rounded to the nearest year to facilitate matching with coarser age data.

[35]That is, they need not be accompanied by a parent or older driver (as for a learner's permit).

year. We find little evidence of a long-run effect of these regulations on later-life driving behavior. Estimates in Table 6 show the effect of a one-year increase in either measure of minimum driving age, and are never significant in the expected direction. Similarly, Gilpin (2019) finds that although increasing the minimum driving age reduces fatalities by reducing teen driving, these regulations do not improve driving behaviors over longer horizons.[36]

We suggest some caution in interpretation: magnitudes cannot be directly compared to results from analysis in the prior sections because the treatments are fundamentally different (and measured in incompatible units). Our primary analysis studies the effects of gasoline price changes, while the analysis here studies the long-run impacts of driving age restrictions. However, with these caveats in mind, this analysis suggests that age restrictions for teenagers on learning to drive do not inhibit the long-run adoption of driving. As high gasoline prices are less extreme than legal restrictions on driving, it is reasonable to conjecture that gasoline price shocks do not impact driver license uptake either.

6 Conclusion

Early experiences frame how people perceive different goods and activities. These formative periods can drive later-life behaviors, expectations, and norms. In the case of driving, we find that individuals who experience large price increases during their formative driving years behave differently than those who do not experience such shocks: they drive to work less often, take transit more, are less likely to have access to a vehicle, and drive fewer miles if they own a vehicle. Early-life experiences are thus one source of path dependence in transportation demand.

Our results build on a growing literature that documents how personal experiences matter for preference formation, but suggest a different story. Economic theory (habit formation) and theories from social psychology (experience-based learning and mental plasticity during impressionable years) cannot explain our findings. Rather than cumulative experiences with prices over long time periods, we find that formative experiences during a narrow time window when people first interact with the price of gasoline is a stronger predictor of their later-in-life driving behavior. We also find that price shocks

[36] Bostwick and Severen (2020) show that these regulations are quantitatively important for teen education and labor market outcomes.

matter more than price levels, consistent with recent work by Haushofer and Fehr (2019).

These results highlight that macroeconomic price shocks can give rise to long-lived preference heterogeneity. Combining the intensive-margin (miles traveled) and extensive-margin (vehicle ownership) effects of a doubling of gasoline prices, we find a combined long-run, path-dependent driving reduction of 3.6-8.7 percent. The literature has reported short- and medium-run estimates that are generally in a wide range of -2 to -39 percent and decreasing in magnitude over time (Small and van Dender 2007; Li, Linn, and Muehlegger 2014). Our long-run effect is smaller but in the same order of magnitude of the more recent short-run miles-traveled elasticities in the literature, although it operates only on cohorts that are exposed to large gasoline price movements during teenage driving years. When viewed with the results in Knittel and Tanaka (2019) (who find that prices in the last few days matter more than prices a few months ago), our results suggest the cumulative exposure is bimodal: Formative and very recent experiences matter at the expense of other periods.

We show that these long-run effects are most likely due to the formation of preferences for the driving experience and its perceived associated costs, rather than due to long-run income effects or a reduction in the number of people who end up learning to drive. These results show that formative experiences that determine later-in-life behavior need not be 'extreme'; everyday mundane experiences with market prices can have long-lasting impacts on preferences and behavior. On the policy front, a large carbon or gasoline tax could have an 'imprintation' effect that persists into the more distant future.

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Appendix: For Online Publication

A.1 Data Notes

Census Data
We draw data on individual commuting behavior in part from the United States Census and from the American Community Survey (ACS). In particular, we collect the 5% state samples for 1980 and 1990, the 5% sample for 2000, the 2006-10 5-year ACS, the 2011-15 5-year ACS, and the 2016 and 2017 1-year ACS data abstracts from the IPUMS website. We focus on 'Journey to Work' variables, but also draw on a variety of demographic and economic characteristics. These variables have been harmonized by IPUMS.

The primary outcome variables from the census/ACS that we use are derived from the following variables (along with IPUMS descriptions):

> TRANWORK is asked in a similar manner from 1980 on, and "reports the respondent's primary means of transportation to work ... over the course of the previous week ... The primary means of transportation was that used on the most days or to cover the greatest distance." This variable varies by person, and is only available for employed persons who are currently working.
>
> VEHICLES is available from 1990 on, and "reports the number of cars, vans, and trucks of one-ton capacity or less kept at home for use by household members," including "company cars regularly kept at home and used for non-business purposes." This variable is available for households.
>
> AUTOS is available in 1980, and "reports the number of automobiles owned or used regularly by any household member. It includes company cars kept at home and available for personal use." This variable is available for households.
>
> TRUCKS is available in 1980, and "reports the number of trucks and vans regularly kept at home for use by members of the household, including company vehicles. It excludes trucks with more than one-ton capacity, those permanently out of working order, and those used only for business purposes." This variable is available for households.

We then define our primary outcome variables from these as follows:

1[drive] is equal to 1 if TRANWORK takes codes 10-15 (for auto, truck, or van conveyance), and 0 otherwise.

1[transit] is equal to 1 if TRANWORK takes codes 30-34 or 36 (public transit conveyance excluding taxis), and 0 otherwise.

1[vehicle] is equal to 1 if VEHICLES is greater than or equal to 1 (1990 on) or the sum of AUTOS and TRUCKS is greater than or equal to 1 (1980), and 0 otherwise.

A couple of other variables play key roles in our analysis, as we use them to merge census/ACS data with measures of gasoline price variation (i.e., treatment):

AGE is asked in all years, and is used to create a variable BIRTHYR by subtraction from the survey year. Ostensibly, AGE is meant to be relative to census day (in early April) for census samples and relative to the day of survey for ACS samples. Thus, BIRTHYR is not necessarily an accurate measure of the year of birth. For example, someone born on March 15, 1964, would respond to the 2000 census that their AGE is 36, and BIRTHYR would be recorded as 1964. However, someone born on April 15, 1964, would respond that their AGE is 35, and BIRTHYR would be recorded as 1965. Hence, there is some measurement error in birth year, and results should be interpreted with this caveat in mind. Respondents from ACS years also suffer from some measurement error, but it should be zero on average.

BPL reports the respondent's state or country of birth, and STATEFIP gives a respondent's current state of residence. We use these variables to merge respondents to gasoline prices in their formative years in their likely state of residence at that time. We merge on BPL, on STATEFIP, and on both for respondents currently residing in their state of birth. 63.8% of the whole sample and 63.3% of the commuting sample currently reside in their state of birth.

We also use a variety of other variables as controls, for sample selection, or in robustness exercises. These include sex, marital status, educational attainment (separate indicators for high school and college completion), race and ethnicity (indicators for African American and Hispanic), household income (inflation adjusted using CPI), employment and labor force participation, wage, housing tenure, rent and house value, and measures of travel time for commuting.

NHTS Data

We use five waves of the National Household Travel Survey and its predecessor, the Nationwide Personal Transportation Survey, (collectively NHTS) from 1990, 1995, 2001, 2009, and 2017. These data document details at the household, person, vehicle, and trip level. Our analysis focuses on the person level. We use the following variables to generate our primary outcomes:

> ANNMILES is a self-reported annualized miles estimate given per vehicle.
>
> WHOMAIN describes which person in the household drives each vehicle the most.

We then use these to create a person-specific measure of total vehicle miles traveled (VMT) by adding together all ANNMILES across vehicles for which WHOMAIN is the primary driver. We top code this value at 115,000 miles annually (alternative top codes at 50,000 or 200,000 miles make little difference).

We use the variable R_AGE to determine a respondent's birth year and to perform merges to gasoline prices. Information on when (generally the month) the interview was conducted is also used. The variable HHSTATE captures the household's current state of residence; no historical detail on location of nativity or migration is provided.

For some specifications, we use other details associated with particular vehicles. MAKE, MODEL, VEHYEAR, HYBRID, and FUELTYPE (or near variants) give make, model, vehicle year, hybrid status, and gas/diesel/electric information about vehicles. The make and model information is relatively coarse, and codes roughly align with those used by the National Highway Traffic Safety Administration.

We also derive a number of other control variables including race (an indicator for white), urbanization (an indicator for urban residential environment), family size, and bins of household income (we harmonize binned measures across years and adjust for inflation, resulting in five bins, which are then interacted with year).

Gasoline Price Data

The Energy Information Administration reports nominal tax-inclusive state-level gasoline price data starting in 1983. For the years 1966-1982, data are from the *Highway Statistics* annuals (Li, Linn, and Muehlegger 2014; Small and van Dender 2007).

Driver License Regulation Data

We develop a data set of driver licensing requirements from several sources. Our primary sources are (i) the Federal Highway Administration (FHWA) *Driver License Administration Requirements and Fees* booklet and (ii) the Insurance Institute for Highway Safety (IIHS) database on graduated driver license (GDL) programs. We supplement these materials with various newspaper articles, legal database queries, and inquiries to reference desks at state libraries.

The FHWA booklet has been published roughly biannually since the 1960s. We found and use years 1967, 1972, 1980, 1982, 1984, 1986, 1988, 1994, and 1996. IIHS data cover 1995 to 2017 and report some information starting in 1990. In general, if we do not observe a change between two periods, we assume no legislative change. If we do observe a change, we perform additional inquiries to determine the date of change.

We define two measures of driver license minimum age. Our primary measure is the minimum age at which a teenager can obtain a full-privilege driver license. Our definition allows for teenagers to have taken driver education classes and be enrolled in school (often requirements for receiving a license before the age of 17 or 18). We exclude hardship rules, farm licenses, and other types of specialty licenses (e.g., motorcycle). The second measure captures the minimum age at which a teenager can obtain an intermediate license. These licenses permit unaccompanied driving, but place some restrictions on when a license holder may drive alone (e.g., daytime only) or who they may drive with (e.g., one non-family member).

Driver Licensing Counts

The FHWA publishes data on driver licensing. We use Table DL-220 "Licensed Drivers, by Sex and Age Group, 1963-2016" from *Highway Statistics* (2016), which lists the number of driver licenses held by people of each age from 16 to 24 in each year. The FHWA did not require states to report counts by age in 1983 and 1985, and instead extrapolated these data. We exclude these years.

To estimate rates of driver license adoption, we require age-specific estimates of population. We construct these data from the National Cancer Institute's SEER Population data, which provides population estimates by age from 1969 to 2017. We sum county-level population estimates across all counties for each age and year.

Fuel-Efficiency Data

We use EPA fuel-economy data from Allcott and Knittel (2019). These data report fuel-economy data by make, model, year, trim, fuel type, and engine size. In the NHTS data, we only observe make, model, year, and fuel type. We therefore create an average fuel efficiency by make, model, year, and fuel type class, measured in gallons per mile (GPM), and use this as our measure of vehicle efficiency.

A.2 Detailed Results and Additional Specifications

Event Study/Regression Discontinuity in Time Details

We quantify the break in the year 2000 driving behavior of those who came of driving age before and after the 1979 oil crisis by estimating variants of the following equation:

$$Y_i = \alpha + g(S_i) + \tau D_i + X_i' \lambda + \varepsilon_i$$

where Y_i is an outcome of interest for individual i in the 2000 census, S_i is the year that i turned 15, and X_i are other characteristics of i. Treatment is the binary variable D_i, which is equal to one if i turned 15 after 1979. The function $g(\cdot)$ captures trends in driving behavior; we experiment with linear and quadratic functions that are allowed different slopes before 1980 and after.[37] Data are limited to a symmetric bandwidth around the treatment year.

Panel A of Appendix Table A.3 presents RD estimates of τ using linear and quadratic trends. Estimates with linear trends are shown over a bandwidth of two to ten years, while those with quadratic trends are shown over a bandwidth of five to ten years. Results indicate a sharp decrease in the likelihood of driving to work of 0.2 to 0.5 percentage points that persists roughly twenty years after turning 15. The quadratic results are less precise, but also less prone to bias by accommodating more response curvature along the running variable. Point estimates are relatively similar across both linear and quadratic specifications.

This relationship can be assigned a causal interpretation if no confounding factors experience a discontinuous break at the same point in time. We demonstrate that the observable covariates are smooth in Appendix Figures A.2, A.3, and A.4 across a range of demographic, employment, and housing characteristics. There are no obvious discontinuities in these graphs. Results from the 'donut' regression discontinuity tests omitting

[37] We report heteroskedasticity-robust standard errors throughout this section. Kolesár and Rothe (2018) caution against clustering standard errors by the running variable, and present simulation evidence that shows the heteroskedasticity-robust standard errors outperform standard errors clustered by the running variable with small or moderate window widths. Furthermore, clustering on the annual running variable here would lead to a few-clusters problem.

the 1980 cohort are shown in Appendix Table A.5. These results, which alleviate concerns of measurement error due to the gradual change of prices throughout the years 1979-80, are similar in magnitude though slightly less significant.

The effect cannot be explained away by controlling for observable, contemporaneous characteristics. Panels B through D of Table A.3 progressively add more controls to the specification in Equation (1). Panel B adds demographic controls we take as exogenous (sex and race), as well as educational attainment (which could be endogenous). Panel C adds state of birth fixed effects to control for differential commuting behavior in different places. We include state of birth, rather than state of residence, because it is exogenous with respect to later-life commuting decisions. Panel D adds contemporaneous income, but we recognize this may not be an appropriate control if later-life income is influenced by graduating from high school during a recession and if income influences vehicle purchasing (see Section 4.1.2 and Appendix A.3 for more discussion).

These covariates decrease point estimates by about a quarter, but do not completely explain behavior. State of birth plays an important role, but estimates are still significant after accounting for differences across locations. Contemporaneous income also influences estimates, but the effect is still present in many specifications. This suggests that there are persistent effects of gasoline prices while coming of age that cannot be explained by earnings.

The negative effect on driving is largely compensated by an increase in transit use (Appendix Table A.4 shows estimates on alternative outcomes). Those coming of age in 1980 are 0.2-0.4 percentage points more likely to take transit to work than their counterparts coming of age a bit earlier. The absolute magnitude of this effect is between 50 and 100 percent of the effect size in Panel A of Appendix Table A.3, suggesting that transit is the primary substitute for driving. Consistent with the effects on driving to work and public transit, those coming of age in 1980 are also less likely to have access to a vehicle. Panel B of Appendix Table A.4 shows RD estimates of Equation (1) on vehicle access for all prime-age adults (not just workers). Linear results are dubious at larger bandwidths, as Panel C of Figure 2 shows greater curvature in vehicle access for cohorts coming of age in the late 1980s. The estimates from the quadratic specifications, 0.2-0.3 percentage points, are generally in line with the transit results. Those coming of age after 1979 are less likely to drive to work, more likely to take transit, and less likely to have access to a private vehicle.

Whether or not commuters are able to substitute away from the automobile depends on the choices available to them. Therefore, we expect effects to be stronger in urban settings where there are plausible alternatives to driving (public transit, walking, etc.). We first examine the RD effect for commuters who reside in the 'principal city' of an MSA.[38] The choice of location is potentially endogenous, however, so we interpret subgroup analysis on location as suggestive. Estimates (Appendix Table A.6) are larger than the effect in the whole population and are largely robust to bandwidth and trend specification. For urban dwellers, someone turning 35 in 2000 is 0.6 to 1.9 percentage points less likely to drive to work than someone (slightly older) turning 36. Conversely, there is little effect on workers who live outside of metropolitan areas, as Panel B reveals. Point estimates are small, mostly positive, and insignificant. Taken together with prior results, this suggests that the persistent effect of the 1979 oil price shock is largely concentrated in cities where viable transportation alternatives are available.

Panels C and D of Appendix Table A.6 report RD estimates for two other groups. Panel C limits the sample to black workers and shows evidence of significant and negative effects. The linear specification loses significance at higher bandwidths; this is likely due to greater curvature in the running variable. Panel D limits the sample to workers without a college education. Results are smaller in magnitude and significance, and point estimates are generally smaller than those reported in Table A.3.

Finally, Appendix Figure A.5 examines the effect of being in a 1980 or later cohort on driving across the income distribution. We divide the population of commuters into both centile and decile bins, and then run the RD estimator using the linear specification with a bandwidth of five years within each bin. Estimates for the lowest decile are negative (about -1.4 percentage points) and significant. The third decile is, unexpectedly, positive, but otherwise the first eight deciles are negative and significant. There is a positive or no effect for the two highest deciles. Estimates for each centile are smoothed and shown with a dotted line, and generally conform to the decile estimates.

Cohort Fixed Effects

The use of cohort fixed effects absorbs most of the variation in gasoline prices, as gasoline prices vary much more over time than space (see Appendix Figure A.1). This should signal caution to taking estimates from models with cohort fixed effects too seriously. Though θ in Equation (2) is still identified when we control for cohort (birth year) fixed

[38]There are several MSAs for which principal city status may violate disclosure rules and therefore is not reported in the 2000 census. This is why sample sizes are lower than in Table A.3.

effects, the source of identifying variation changes. The variation that remains after conditioning on cohort fixed effects (in addition to the state fixed effects) is due to differential changes in T_{cs} across states, e.g., a larger increase in Georgia than in Alabama in a given year. These movements are only a small piece of the observable variation facing agents.

We present results with cohort fixed effects in Appendix Table A.10 (outcome: driving) and Appendix Table A.12 (outcome: miles traveled). In general, estimates from these models are statistically significant only for a subset of specifications because of a loss of power due to much less variation. Using $P_{cs}^{\Delta(m_{cs}+2,m_{cs})}$ as the treatment definition, we find that estimates of the extensive-margin effect are very similar in magnitude to the results in Table 1. Additional identifying variation in this case is coming from within state changes in driver license age requirements over time. The estimated effect is now concentrated in the 16 to 18 age range. Intensive-margin results on miles traveled are noisy and almost always lack significance when cohort fixed effects are included.

A.3 Mediation Analysis

We perform mediation analysis to explicitly account for the *indirect* effect that gasoline price shocks experienced during formative years could have on later-life driving through the general experience of coming of age during a recession or an income effect. If a gasoline price shock during this formative window impacts labor market outcomes (through wages, cohort effects, labor market entry timing, educational attainment, etc.), and these effects influence transportation behavior, the indirect portion of the effect is not due to a shift in preferences.

We build a simple mediation model (Baron and Kenny 1986; MacKinnon 2012).[39] We mostly retain notation from Section 4: Later-life driving, Y, is modeled as a function of the gasoline price shock experienced during formative driving years, T, and a mediator, M. However, the gasoline price shock may also have an effect on M, and therefore mediate later-life driving indirectly through M. The mediation model can be expressed by the stacked equation (suppressing subscripts for exposition):

$$\begin{pmatrix} Y \\ M \end{pmatrix} = \begin{pmatrix} \theta^Y \\ \theta^M \end{pmatrix} T + \begin{pmatrix} \gamma \\ 0 \end{pmatrix} M + \begin{pmatrix} \delta^Y \\ \delta^M \end{pmatrix} X + \begin{pmatrix} \epsilon^Y \\ \epsilon^M \end{pmatrix}. \tag{A.1}$$

[39] Mediators are a class of what are denoted as 'bad controls' in Angrist and Pischke (2008). They are 'bad' in the sense that they can confound estimation of average treatment effects. Recent literature has begun to explicitly explore these estimators (e.g., Dippel et al. 2017; Heckman and Pinto 2015). In particular, Heckman and Pinto (2015) review early econometric mediation analysis.

In Equation (A.1), θ^Y is the effect of T on Y, while γ is the effect of M on Y. T is permitted to have its own effect on M via θ^M. The direct effect of formative gasoline price shocks on later-life driving is captured by θ^Y, the indirect mediated effect is the product $\gamma\theta^M$, and the total effect sums these two together: $\theta^Y + \gamma\theta^M$.

We consider two classes of mediators: early adult unemployment and contemporaneous income. Early adult unemployment, implemented as the unemployment rate in the state of birth at age 18, captures the general experience of coming of age during a recession (as indicated by a soft labor market). The contemporaneous income measures (household, wage, and personal income in the year of survey) capture the income channel.

We implement Equation (A.1) in a similar manner to Equation (2), and include in δ age, state of birth, sample year fixed effects, and exogenous demographics (sex and race). The fixed effects and demographic covariates are allowed to vary across outcomes (hence δ^Y and δ^M). We cluster standard errors by state of birth across outcomes. Finally, we assume that T and M are exogenous conditional on the fixed effects and covariates we include as well as autonomy (that is, γ does not vary with T).[40] Alternatively, mediation analysis could proceed using estimates of γ and θ^M drawn from the literature (e.g., the literature on graduating during a recession provides proxies for θ^M).

Appendix Table A.9 presents several specifications for different combinations of treatment and mediators. Columns (1), (3), (5), and (7) report effects using the absolute calendar age measure of treatment $P_{cs}^{\Delta 17,15}$, while Columns (2), (4), (6), and (8) use the measure based on minimum full-privilege driver license age $P_{cs}^{\Delta(m_{cs}+1),(m_{cs}-1)}$. Columns (1) and (2) are mediated by age 18 unemployment rate, Columns (3) and (4) by household income, Columns (5) and (6) by wage income, and Columns (7) and (8) by personal income.

Estimates of θ^Y (the effect of the gas price shock on later-life driving conditional on the mediator) are similar to those in the main text. Estimates of γ indicate a relationship between the contemporaneous income and later-life driving (Columns 3-6), but not between age 18 unemployment and later life driving. Contemporaneous income has a positive relationship with driving a private vehicle to work; estimates of γ indicate that a 10% increase in income is associated with roughly a 0.2 percentage point increase in driving to work.

[40] One set of assumptions under which this model is identified is labeled sequential exogeneity: (i) $(Y, M) \perp\!\!\!\perp T|X$, (ii) $Y \perp\!\!\!\perp M|T, X$ and (iii) common support (Imai, Keele, and Yamamoto 2010; Imai et al. 2011). Heckman and Pinto (2015) argue that such conditions may be strong, and Dippel et al. (2017) provide an approach to identification with endogenous variation.

Gasoline price shocks during formative years have a positive relationship with age 18 unemployment and a negative relationship with income, though the strength of this relationship varies with the definition of income used. Estimates of θ^M indicate that experiencing a doubling of gasoline prices during formative years is associated with up to a 1.03 percentage points higher unemployment rate at age 18 and up to a 4.9 percent decrease in later-life income.[41]

These results show that most of the gasoline price shock effect does not come through indirect recession or income channels. The ratio of the direct effect to the total effect indicates that between 76 and 100 percent of the total effect cannot be explained by income. This analysis suggests that recession- or income-based explanations are, for the most part, unimportant in understanding the long-run relationship between gasoline price shocks during formative years and later-life driving.

A.4 Cumulative Exposure

Because Equation (6) is non-linear in ω, non-linear estimation methods are required. In our setting, this is complicated by the fixed effects for state, year of observation, and age in our standard specifications. Such fixed effects increase the dimensionality of the minimization problem and can cause the performance of standard minimizers to degrade.

We use Stata's non-linear estimation tool. We write an evaluator function that calls a Mata routine.[42] This routine calculates the exposure function conditional on the parameter ω. The evaluator function can ostensibly accommodate a moderate length vector of fixed effects, but experiments reveal that it only performs well finding the global minimum if fed reasonable starting values.

To limit the likelihood that we get trapped a local (non-global) minimum, we fit Equation (6) according to the following steps:

1. Run linear regression on fixed effects ($Y_{icst} = \kappa_s + \delta_t + \eta_a + \varepsilon_{icst}$) to obtain estimates of the residuals $\hat{e}_{icst}$ and estimates of the fixed effects.

[41] This suggests, for example, that the 1979 oil crisis is associated with income loss of about 2.5 percent later in life. This measure is smaller than in Kahn (2010) and Stuart (2017), reflecting the fact that gasoline price shocks and recessions are not always correlated.

[42] The use of Mata was dictated in part by how we dealt with a peculiar feature of the exposure function: People of different ages require different length vectors of past treatments. To deal with this, we effectively assign a weight of 0 for exposure years $k \leq 14$. However, because these weights are exponentiated by ω, there is a discontinuity at 0 as $\omega \to 0$ for 0^ω. A simple logic correction in Mata overcomes this and returns a smooth function that treats 0^0 as 0.

2. Perform a grid search in (β, ω) and record the residual sum of squares of $\hat{e}_{icst} - (\alpha + \beta A_{cst}(\omega, \mathbf{T}_{st}))$ as the minimizing criterion.

3. Use Stata's non-linear solve to minimize $\hat{e}_{icst} = \alpha + \beta A_{cst}(\omega, \mathbf{T}_{st})$, using the results from Step 2 as starting values.

4. Using the fixed-effects estimates from Step 1 and values of (β, ω) from Step 3 as starting values, minimize Equation (6).

We estimate Equation (6) on the cumulative exposure to one-year gasoline price shocks starting between the ages of 15 and 16, and ending with the shock between the year prior to observation and the year of observation. Coefficient estimates for β and ω are presented in Table 5. Intermediate results and code is available on request.

Appendix References

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A.5 Appendix Figures and Tables

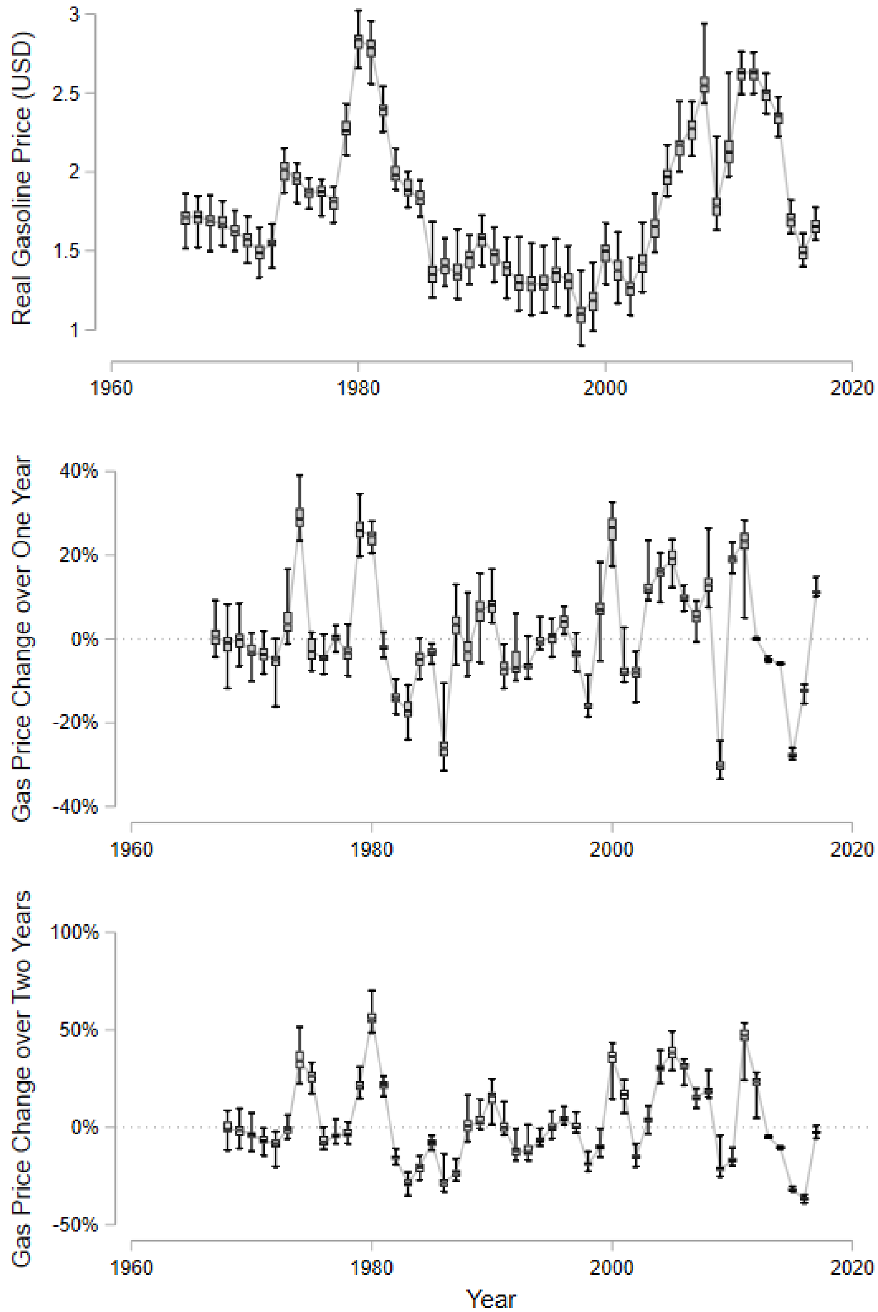

Figure A.1: Box plots of state gas prices and 1- and 2-year percentage changes; minimum, maximum and quartiles.

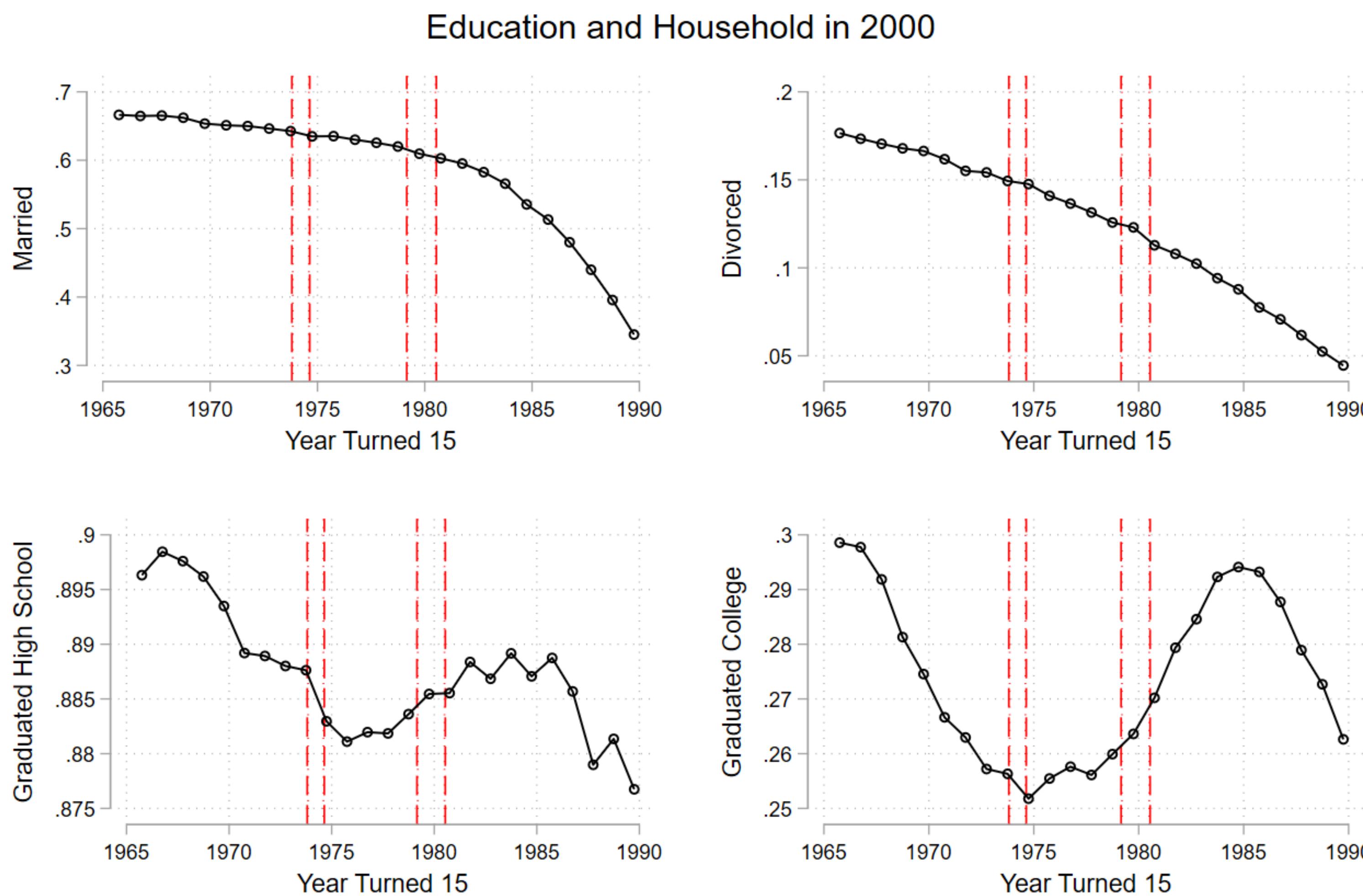

Figure A.2: Demographic characteristics in 2000 by year turned 15.

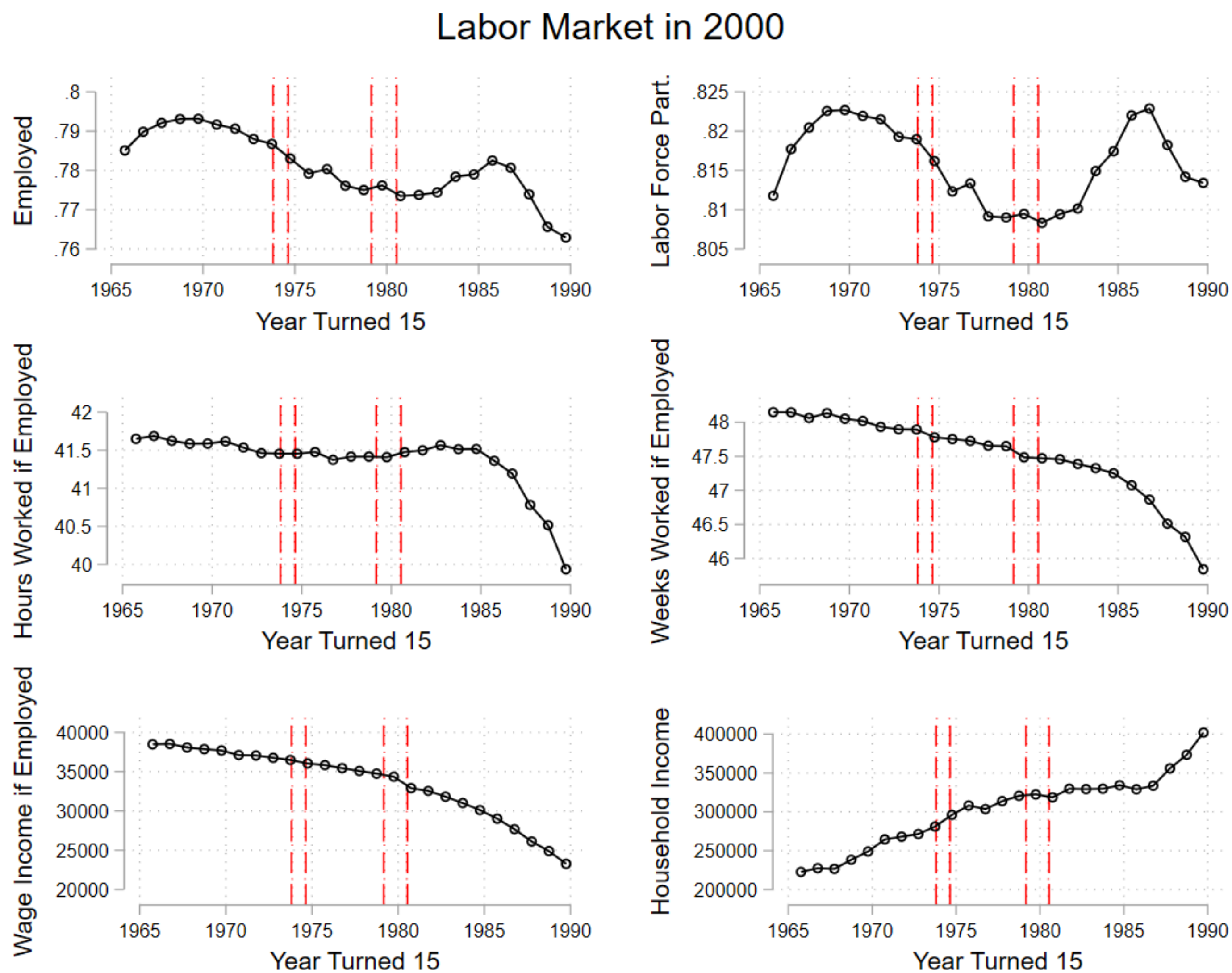

Figure A.3: Labor market characteristics in 2000 by year turned 15.

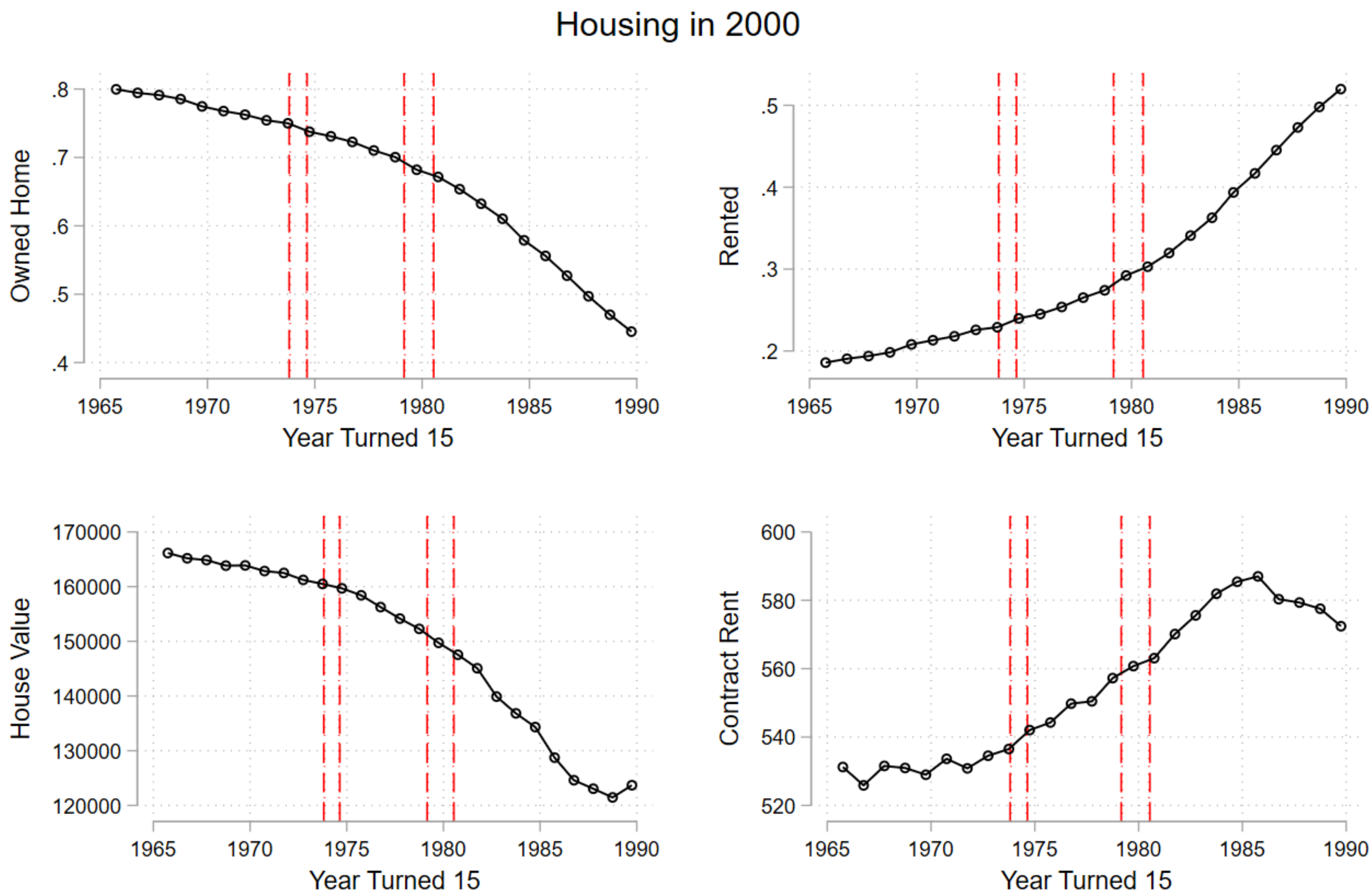

Figure A.4: Housing characteristics in 2000 by year turned 15.

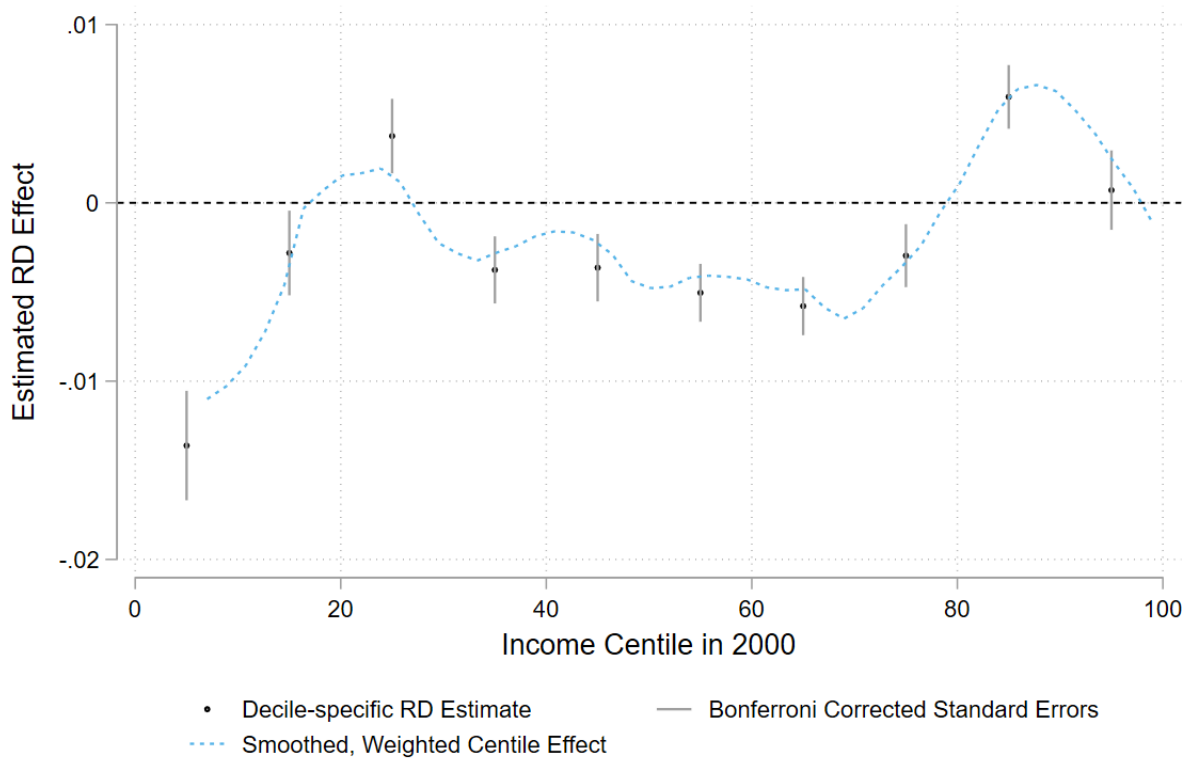

Figure A.5: Event study (regression discontinuity in time) estimates of the 1979-80 gas price shock on driving in 2000 by income decile and (smoothed) centile. All coefficients estimates using a five-year bandwidth and linear trends. Decile estimates shown as dots with Bonferroni-corrected 95 percent confidence intervals represented by the vertical bars (corrected for ten tests).

Table A.1: Minimum driver licensing ages across states.

Year	[14,14.5)	[14.5,15.5)	[15.5,16.5)	[16.5,17.5)	[17.5,18]	Average minimum age
Panel A: Minimum full privilege license age						
1970	1	5	38	4	3	16.37
1980	0	5	39	5	2	16.29
1990	0	5	39	5	2	16.27
2000	0	2	24	18	7	16.83
2010	0	0	4	32	15	17.23
Panel B: Minimum provisional license age						
1970	2	7	39	3	0	16.00
1980	2	7	40	2	0	15.97
1990	1	7	41	2	0	15.98
2000	1	4	41	5	0	16.05
2010	1	2	39	9	0	16.10
Panel C: Learner's permit minimum age						
1972	8	18	24	1	0	
1980	8	21	22	0	0	
1988	7	22	22	0	0	
1994	6	24	21	0	0	
2010	6	25	20	0	0	

Frequency of states (and DC) with minimum driver age in each bin for the years listed. Provisional licenses allow unaccompanied driving, but limit time of use or number of passengers. Average minimum age is weighted by state population. Learner Permit Minimum Age is less accurately recorded and reported in FHWA data, and states vary widely in the privileges it accords. Source: see description in text and Appendix.

Table A.2: Summary statistics for samples.

	(1) All obs.	(2) Only those obs. in state of birth	(3) Only those obs. in state of birth & employed
Census Sample			
1[drive]	.883		.905
1[transit]	.042		.034
1[vehicle]	.946	.948	.966
1[employed]	.776	.765	-
Age	38.01	37.71	37.72
1[female]	.503	.507	.480
1[married]	.535	.569	.607
1[at least HS education]	.899	.873	.912
1[at least college education]	.308	.244	.282
1[black]	.138	.125	.103
1[hispanic]	.084	.074	.069
Household Income (in 2017 $)	86,157	81,614	87,982
1[in state of birth]	.638	-	-
N	19,052,577	12,201,920	9,330,029
NHTS Sample			
VMT	9,852	-	-
VMT|(VMT>0)	14,315	-	-
Age	37.49	-	-
Gallons per Mile (across vehicles)	.051	-	-
Any big vehicle	.468	-	-
N	292,358	-	-

Average characteristics of the samples. In the census sample, Column 1 includes all non-farm native-born persons in the census between the ages of 25-54. Column 2 retains those living in their state of birth when surveyed. Column 3 further retains only those actively working. In the NHTS sample, all observations between the ages of 25-54 are included. Observations weighted by person sample weights.

Table A.3: Event study (discontinuity) in turning 15 after 1979 on commuting behavior in 2000.

		Bandwidth (years)								
Model	Poly. order	2	3	4	5	6	7	8	9	10
Panel A: Effect on driving, no controls										
	1	-0.0050*	-0.0029+	-0.0026+	-0.0032**	-0.0026*	-0.0027**	-0.0032**	-0.0032**	-0.0029**
		(0.0022)	(0.0016)	(0.0014)	(0.0012)	(0.0011)	(0.0010)	(0.0009)	(0.0009)	(0.0008)
	2				-0.0033	-0.0039*	-0.0032+	-0.0021	-0.0027+	-0.0032*
					(0.0022)	(0.0019)	(0.0016)	(0.0015)	(0.0014)	(0.0013)
Panel B: Effect on driving, controls:										
+ demographics	1	-0.0046*	-0.0025	-0.0023+	-0.0029*	-0.0025*	-0.0024*	-0.0028**	-0.0026**	-0.0021*
		(0.0022)	(0.0016)	(0.0014)	(0.0012)	(0.0011)	(0.0010)	(0.0009)	(0.0009)	(0.0008)
	2				-0.0028	-0.0035+	-0.0030+	-0.0020	-0.0026+	-0.0034**
					(0.0022)	(0.0018)	(0.0016)	(0.0015)	(0.0014)	(0.0013)
Panel C: Effect on driving, controls:										
+ demographics, state of birth FEs	1	-0.0046*	-0.0023	-0.0019	-0.0025*	-0.0020+	-0.0019+	-0.0022*	-0.0020*	-0.0014+
		(0.0022)	(0.0016)	(0.0013)	(0.0012)	(0.0011)	(0.0010)	(0.0009)	(0.0009)	(0.0008)
	2				-0.0027	-0.0031+	-0.0027+	-0.0019	-0.0024+	-0.0030*
					(0.0021)	(0.0018)	(0.0016)	(0.0015)	(0.0014)	(0.0013)
Panel D: Effect on driving, controls:										
+ demographics, state of birth FEs	1	-0.0046*	-0.0022	-0.0018	-0.0024*	-0.0019+	-0.0017+	-0.0021*	-0.0019*	-0.0013
+ $\ln(income)$		(0.0022)	(0.0016)	(0.0013)	(0.0012)	(0.0011)	(0.0010)	(0.0009)	(0.0009)	(0.0008)
	2				-0.0027	-0.0030+	-0.0026	-0.0018	-0.0023	-0.0029*
					(0.0021)	(0.0018)	(0.0016)	(0.0015)	(0.0014)	(0.0013)
N		545k	811k	1075k	1343k	1614k	1888k	2148k	2398k	2642k

Regression discontinuity estimates of the effect of turning 15 after 1979 on a binary indicator of whether the respondent drove to work, as reported in the 2000 census. Bandwidth is symmetric around 1979.5. Sample includes all native-born persons actively working in the census, and excludes farm workers and those coded N/A for transportation mode. Demographic controls include sex, race, and educational attainment. Observations weighted by person sample weights. Standard errors are robust to heteroskedasticity (see footnote 37). Sample sizes are 1-2% smaller in panels B through D. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table A.4: Event study (discontinuity) in turning 15 after 1979 on transit usage and vehicle access in 2000.

	Bandwidth (years)								
Poly. order	2	3	4	5	6	7	8	9	10
Panel A: Transit usage									
1	0.0036*	0.0027*	0.0027**	0.0023**	0.0017*	0.0016*	0.0016**	0.0015**	0.0018**
	(0.0015)	(0.0011)	(0.0009)	(0.0008)	(0.0007)	(0.0007)	(0.0006)	(0.0006)	(0.0005)
2				0.0038**	0.0037**	0.0030**	0.0023*	0.0024**	0.0018*
				(0.0014)	(0.0012)	(0.0011)	(0.0010)	(0.0009)	(0.0009)
N	545k	811k	1075k	1343k	1614k	1888k	2148k	2398k	2642k
Panel B: No vehicle access									
1	0.0033*	0.0026*	0.0020*	0.0016+	0.0009	0.0007	0.0005	-0.0002	-0.0012*
	(0.0016)	(0.0011)	(0.0010)	(0.0008)	(0.0008)	(0.0007)	(0.0007)	(0.0006)	(0.0006)
2				0.0037*	0.0034**	0.0027*	0.0023*	0.0028**	0.0034**
				(0.0015)	(0.0013)	(0.0012)	(0.0011)	(0.0010)	(0.0009)
N	698k	1038k	1376k	1717k	2061k	2409k	2739k	3058k	3370k

Regression discontinuity estimates of the effect of turning 15 after 1979 on a binary indicator of transit usage or vehicle access as reported in the 2000 census. No controls included, as in Panel A of Appendix Table A.3. Bandwidth is symmetric around 1979.5. Panel A includes all native-born persons actively working in the census, and excludes farm workers and those coded N/A for transportation mode. Panel B includes non-workers. Observations weighted by person sample weights. Standard errors are robust to heteroskedasticity (see footnote 37). $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table A.5: Event study (discontinuity) in turning 15 after 1979 on transportation behavior in 2000 – Donut RD omitting those who turn 15 in 1979.

		Bandwidth (years)								
Model	Poly. order	2	3	4	5	6	7	8	9	10
Panel A: Effect on driving, no controls										
	1	-0.0037	-0.0020	-0.0039*	-0.0036**	-0.0028*	-0.0029**	-0.0037**	-0.0034**	-0.0031**
		(0.0029)	(0.0020)	(0.0016)	(0.0013)	(0.0012)	(0.0011)	(0.0010)	(0.0009)	(0.0009)
	2				-0.0030	-0.0045+	-0.0035+	-0.0021	-0.0031+	-0.0037*
					(0.0028)	(0.0023)	(0.0020)	(0.0018)	(0.0016)	(0.0015)
Panel B: Effect on driving, controls:										
+ demographics	1	-0.0037	-0.0018	-0.0037*	-0.0034*	-0.0025*	-0.0025*	-0.0031**	-0.0027**	-0.0022*
		(0.0028)	(0.0019)	(0.0016)	(0.0013)	(0.0012)	(0.0011)	(0.0010)	(0.0009)	(0.0009)
	2				-0.0028	-0.0043+	-0.0036+	-0.0023	-0.0033*	-0.0039*
					(0.0027)	(0.0023)	(0.0020)	(0.0018)	(0.0016)	(0.0015)
Panel C: Effect on driving, controls:										
+ demographics, state of birth FEs	1	-0.0035	-0.0015	-0.0032*	-0.0028*	-0.0021+	-0.0020+	-0.0025*	-0.0021*	-0.0016+
		(0.0028)	(0.0019)	(0.0015)	(0.0013)	(0.0012)	(0.0011)	(0.0010)	(0.0009)	(0.0009)
	2				-0.0026	-0.0037+	-0.0031	-0.0019	-0.0028+	-0.0033*
					(0.0027)	(0.0023)	(0.0020)	(0.0018)	(0.0016)	(0.0015)
Panel D: Effect on driving, controls:										
+ demographics, state of birth FEs	1	-0.0035	-0.0015	-0.0031*	-0.0026*	-0.0020+	-0.0019+	-0.0024*	-0.0020*	-0.0015+
+ $\ln(income)$		(0.0028)	(0.0019)	(0.0015)	(0.0013)	(0.0012)	(0.0011)	(0.0010)	(0.0009)	(0.0009)
	2				-0.0027	-0.0036	-0.0031	-0.0018	-0.0027+	-0.0032*
					(0.0027)	(0.0023)	(0.0020)	(0.0018)	(0.0016)	(0.0015)
N		550k	818k	1085k	1349k	1622k	1892k	1250k	2401k	2642k

Regression discontinuity estimates of the effect of turning 15 after 1979 on a binary indicator of whether the respondent drove to work, as reported in the 2000 census. Bandwidth is symmetric around 1979, but excludes 1979 (e.g., a bandwidth of two includes 1977, 1978, 1980, and 1981). Sample includes all native-born persons actively working in the census, and excludes farm workers and those coded N/A for transportation mode. Demographic controls include sex, race, and educational attainment. Observations weighted by person sample weights. Standard errors are robust to heteroskedasticity (see text). Sample sizes are 1-2% smaller in panels B through D. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table A.6: Event study (discontinuity) in turning 15 after 1979 on commuting behavior in 2000 – Subgroup analysis.

		Bandwidth (years)								
Model	Poly. order	2	3	4	5	6	7	8	9	10
Panel A: Effect on driving										
Sample: Principal city	1	-0.0185*	-0.0120+	-0.0108*	-0.0124**	-0.0092*	-0.0061	-0.0090*	-0.0096**	-0.0094**
		(0.0089)	(0.0065)	(0.0054)	(0.0047)	(0.0043)	(0.0039)	(0.0037)	(0.0035)	(0.0033)
	2				-0.0157+	-0.0167*	-0.0163*	-0.0087	-0.0085	-0.0096+
					(0.0085)	(0.0073)	(0.0065)	(0.0059)	(0.0055)	(0.0051)
	N	62k	92k	122k	154k	187k	220k	252k	283k	313k
Panel B: Effect on driving										
Sample: Not in metro	1	-0.0030	0.0004	0.0000	0.0013	0.0008	0.0014	0.0002	0.0003	0.0006
		(0.0042)	(0.0030)	(0.0025)	(0.0022)	(0.0020)	(0.0019)	(0.0017)	(0.0017)	(0.0016)
	2				-0.0016	0.0003	-0.0002	0.0022	0.0013	0.0006
					(0.0041)	(0.0035)	(0.0031)	(0.0028)	(0.0026)	(0.0024)
	N	114k	170k	225k	280k	336k	393k	447k	500k	552k
Panel C: Effect on driving										
Sample: Black	1	-0.0168*	-0.0099	-0.0107*	-0.0107*	-0.0067+	-0.0052	-0.0048	-0.0019	0.0002
		(0.0083)	(0.0061)	(0.0050)	(0.0045)	(0.0040)	(0.0037)	(0.0035)	(0.0033)	(0.0031)
	2				-0.0145+	-0.0176*	-0.0144*	-0.0118*	-0.0135**	-0.0136**
					(0.0080)	(0.0068)	(0.0061)	(0.0056)	(0.0052)	(0.0048)
	N	57k	84k	111k	139k	166k	193k	220k	245k	270k
Panel D: Effect on driving										
Sample: No college	1	-0.0037	-0.0017	-0.0022	-0.0027*	-0.0020+	-0.0023*	-0.0028**	-0.0023*	-0.0016+
		(0.0025)	(0.0018)	(0.0015)	(0.0014)	(0.0012)	(0.0011)	(0.0011)	(0.0010)	(0.0009)
	2				-0.0021	-0.0033	-0.0022	-0.0016	-0.0027+	-0.0036*
					(0.0025)	(0.0021)	(0.0019)	(0.0017)	(0.0016)	(0.0015)
	N	394k	585k	774k	965k	1157k	1350k	1534k	1711k	1883k

Regression discontinuity estimates of the effect of turning 15 after 1979 on a binary indicator of whether the respondent drove to work, as reported in the 2000 census. Bandwidth is symmetric around 1979.5. Sample includes all native-born persons actively working in the census, and excludes farm workers and those coded N/A for transportation mode. Observations weighted by person sample weights. Standard errors are robust to heteroskedasticity (see text).$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table A.7: Summary statistics of treatment variables in the sample.

	Mean	SD	Min	Max
P_{cs}^{16} (in 2017 $)	1.75	0.44	0.90	3.02
$P_{cs}^{\Delta 16,15}$	0.011	0.127	-0.315	0.391
$P_{cs}^{\Delta(m_{cs},m_{cs}-1)}$	0.011	0.128	-0.335	0.391
$P_{cs}^{\Delta 17,15}$	0.024	0.205	-0.351	0.700
$P_{cs}^{\Delta(m_{cs}+1,m_{cs}-1)}$	0.022	0.206	-0.351	0.700

Treatment statistics for the employed, same-state census sample, weighted by person sample weights.

Table A.8: The effect of formative gasoline price on driving to work using the census/ACS 1980-2017, one year price changes, various other definitions of treatment.

	1[drive] (1)	1[drive] (2)	1[drive] (3)	1[drive] (4)	1[drive] (5)	1[drive] (6)	1[drive] (7)
$P_{cs}^{\Delta(18,16)}$	-0.0027*	-0.0030***	-0.0030***	-0.0024*	-0.0024*	-0.0023*	-0.0027**
	(0.0011)	(0.0008)	(0.0008)	(0.0010)	(0.0010)	(0.0010)	(0.0009)
$P_{cs}^{\Delta(18,17)}$	-0.0024	-0.0038**	-0.0041***	-0.0017	-0.0017	-0.0016	-0.0020
	(0.0016)	(0.0011)	(0.0011)	(0.0015)	(0.0015)	(0.0015)	(0.0014)
$P_{cs}^{\Delta(17,16)}$	-0.0038**	-0.0030*	-0.0036**	-0.0036*	-0.0037**	-0.0037**	-0.0041**
	(0.0014)	(0.0012)	(0.0012)	(0.0013)	(0.0013)	(0.0013)	(0.0012)
$P_{cs}^{\Delta(16,15)}$	-0.0054**	-0.0039**	-0.0046**	-0.0053**	-0.0056***	-0.0056**	-0.0061***
	(0.0016)	(0.0013)	(0.0014)	(0.0016)	(0.0016)	(0.0017)	(0.0015)
$P_{cs}^{\Delta(m_{cs}+2,m_{cs})}$	-0.0034**	-0.0043***	-0.0041***	-0.0031**	-0.0032**	-0.0032**	-0.0037**
	(0.0012)	(0.0009)	(0.0010)	(0.0012)	(0.0011)	(0.0012)	(0.0011)
$P_{cs}^{\Delta(m_{cs}+2,m_{cs}+1)}$	-0.0036*	-0.0049**	-0.0050**	-0.0030+	-0.0033*	-0.0029+	-0.0035*
	(0.0017)	(0.0014)	(0.0014)	(0.0016)	(0.0016)	(0.0016)	(0.0015)
$P_{cs}^{\Delta(m_{cs}+1,m_{cs})}$	-0.0044*	-0.0051***	-0.0054***	-0.0041*	-0.0042*	-0.0044*	-0.0048**
	(0.0018)	(0.0014)	(0.0015)	(0.0018)	(0.0018)	(0.0018)	(0.0017)
$P_{cs}^{\Delta(m_{cs},m_{cs}-1)}$	-0.0048***	-0.0038**	-0.0046***	-0.0048**	-0.0047**	-0.0049**	-0.0052***
	(0.0013)	(0.0013)	(0.0012)	(0.0014)	(0.0014)	(0.0014)	(0.0013)
Census year FEs	Y	Y	Y	Y	Y	-	-
State of birth FEs	Y	Y	Y	Y	Y	-	-
Age FEs	Y	Y	Y	Y	Y	Y	Y
Demographics	-	-	-	Y	Y	Y	Y
ln HH income	-	-	-	-	Y	Y	Y
State-X-year FEs	-	-	-	-	-	Y	Y
Quad. birth year	-	-	-	-	-	-	Y
Price in state of	Birth	Birth	Res	Birth	Birth	Birth	Birth
Sample	Stay	All	All	Stay	Stay	Stay	Stay

Each row and column represents the results from a different regression, for fifty-six total. Dependent variable is a binary indicator of whether the respondent drove to work, as reported in the census. Sample includes all native-born persons actively working in the census between the ages of 25-54, and excludes farm workers and those coded N/A for transportation mode. Demographics include sex, marital status, educational attainment, and race. Observations weighted by person sample weights. Standard errors clustered by state of birth. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.9: Mediation analysis of indirect effects of recession and income channels M.

Mediator (M):	Unempl. Rate at 18		Household income		Wage income		Personal income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effects of T and M on Y	1[drive]	1[drive]	1[drive]	1[drive]	1[drive]	1[drive]	1[drive]	1[drive]
θ^Y	-0.0042***	-0.0044***	-0.0038***	-0.0041***	-0.0032**	-0.0037**	-0.0031**	-0.0037**
	(0.0011)	(0.0010)	(0.0010)	(0.0011)	(0.0009)	(0.0010)	(0.0011)	(0.0012)
γ	0.0001	0.0000	0.0223***	0.0223***	0.0170***	0.0170***	0.0216***	0.0216***
	(0.0002)	(0.0002)	(0.0024)	(0.0024)	(0.0045)	(0.0045)	(0.0044)	(0.0045)
Effect of T on M	M	M	$\ln(M)$	$\ln(M)$	$\ln(M)$	$\ln(M)$	$\ln(M)$	$\ln(M)$
θ^M	1.0286***	0.0451	-0.0053	-0.0062+	-0.0488***	-0.0371***	-0.0460***	-0.0335***
	(0.2875)	(0.3481)	(0.0034)	(0.0036)	(0.0034)	(0.0034)	(0.0035)	(0.0033)
Direct effect (θ^Y)	-0.0042***	-0.0044***	-0.0038***	-0.0041***	-0.0032**	-0.0037**	-0.0031**	-0.0037**
	(0.0011)	(0.0010)	(0.0010)	(0.0011)	(0.0009)	(0.0010)	(0.0011)	(0.0012)
Indirect effect ($\gamma\theta^M$)	0.0001	0.0000	-0.0001	-0.0001	-0.0008**	-0.0006**	-0.0010***	-0.0007***
	(0.0002)	(0.0000)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Total effect ($\theta^Y + \gamma\theta^M$)	-0.0041***	-0.0044***	-0.0040***	-0.0042***	-0.0040***	-0.0043***	-0.0041***	-0.0044***
	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0008)	(0.0043)	(0.0010)	(0.0010)
Treatment definition (T)	$P_{cs}^{\Delta 17,15}$	$P_{cs}^{\Delta(m_{cs}\pm 1)}$	$P_{cs}^{\Delta 17,15}$	$P_{cs}^{\Delta(m_{cs}\pm 1)}$	$P_{cs}^{\Delta 17,15}$	$P_{cs}^{\Delta(m_{cs}\pm 1)}$	$P_{cs}^{\Delta 17,15}$	$P_{cs}^{\Delta(m_{cs}\pm 1)}$

See Appendix A.3 for details. Dependent variable is a binary indicator of whether the respondent drove to work, as reported in the census. Sample includes all native-born persons actively working in the census between the ages of 25-54, and excludes farm workers and those coded N/A for transportation mode. All models include age, state of birth, and sample year fixed effects. Demographics include sex and race. Observations weighted by person sample weights. Standard errors clustered by state of birth. Income is modeled in logs. $P_{cs}^{\Delta(m_{cs}\pm 1)}$ is equivalent to $P_{cs}^{\Delta(m_{cs}+1, m_{cs}-1)}$. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.10: The effect of formative gasoline price on driving to work using the census/ACS 1980-2017, with cohort FEs.

	1[drive] (1)	1[drive] (2)	1[drive] (3)	1[drive] (4)
2-year price change				
$P_{cs}^{\Delta(m_{cs}+2,m_{cs})}$	-0.0041+	-0.0039+	-0.0038+	-0.0037+
	(0.0023)	(0.0021)	(0.0021)	(0.0020)
$P_{cs}^{\Delta(m_{cs}+1,m_{cs}-1)}$	-0.0016	-0.0016	-0.0012	-0.0017
	(0.0019)	(0.0019)	(0.0019)	(0.0019)
1-year price change				
$P_{cs}^{\Delta(m_{cs}+2,m_{cs}+1)}$	-0.0057*	-0.0053*	-0.0054*	-0.0048*
	(0.0024)	(0.0022)	(0.0021)	(0.0021)
$P_{cs}^{\Delta(m_{cs}+1,m_{cs})}$	-0.0019	-0.0018	-0.0016	-0.0019
	(0.0025)	(0.0025)	(0.0025)	(0.0025)
$P_{cs}^{\Delta(m_{cs},m_{cs}-1)}$	-0.0009	-0.0009	-0.0004	-0.0008
	(0.0024)	(0.0023)	(0.0024)	(0.0024)
Levels				
$P_{cs}^{m_{cs}}$	-0.0013	-0.0015	-0.0020	-0.0022
	(0.0026)	(0.0024)	(0.0024)	(0.0019)
Census year FEs	Y	Y	Y	Y
State of birth FEs	Y	Y	Y	Y
Age FEs	Y	Y	Y	Y
Birth year FEs	Y	Y	Y	Y
Demographics	-	Y	Y	Y
ln HH income	-	-	Y	Y
State-X-year FEs	-	-	-	Y

Each row and column represents the results from a different regression, for twenty-four total. Dependent variable is a binary indicator of whether the respondent drove to work, as reported in the census. Sample includes all native-born persons actively working in the census between the ages of 25-54, and excludes farm workers and those coded N/A for transportation mode. Demographics include sex, marital status, educational attainment, and race. Observations weighted by person sample weights. Standard errors clustered by state of birth. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.11: The effect of formative gasoline price on log miles traveled using NHTS 1990-2017, one year price changes, various other definitions of treatment.

	ln(VMT) (1)	ln(VMT) (2)	ln(VMT) (3)	ln(VMT) (4)	ln(VMT) (5)
$P_{cs}^{\Delta(18,16)}$	-0.0807** (0.0236)	-0.0851*** (0.0237)	-0.0709** (0.0246)	-0.0719** (0.0249)	-0.0524* (0.0226)
$P_{cs}^{\Delta(18,17)}$	-0.0954* (0.0374)	-0.1023** (0.0380)	-0.0802+ (0.0404)	-0.0781+ (0.0406)	-0.0484 (0.0366)
$P_{cs}^{\Delta(17,16)}$	-0.1125** (0.0401)	-0.1171** (0.0403)	-0.1060* (0.0409)	-0.1097** (0.0407)	-0.0837* (0.0398)
$P_{cs}^{\Delta(16,15)}$	-0.0949* (0.0428)	-0.0995* (0.0415)	-0.0953* (0.0409)	-0.0920* (0.0401)	-0.0760+ (0.0402)
$P_{cs}^{\Delta(m_{cs}+2,m_{cs})}$	-0.0525* (0.0229)	-0.0540* (0.0231)	-0.0403 (0.0244)	-0.0405 (0.0248)	-0.0212 (0.0228)
$P_{cs}^{\Delta(m_{cs}+2,m_{cs}+1)}$	-0.0678+ (0.0346)	-0.0693+ (0.0350)	-0.0561 (0.0393)	-0.0541 (0.0396)	-0.0229 (0.0361)
$P_{cs}^{\Delta(m_{cs}+1,m_{cs})}$	-0.0618+ (0.0343)	-0.0644+ (0.0357)	-0.0462 (0.0353)	-0.0484 (0.0358)	-0.0279 (0.0347)
$P_{cs}^{\Delta(m_{cs},m_{cs}-1)}$	-0.0606+ (0.0350)	-0.0743* (0.0338)	-0.0699+ (0.0352)	-0.0690+ (0.0347)	-0.0531 (0.0349)
Sample year FEs	Y	Y	Y	-	-
State FEs	Y	Y	Y	-	-
Age FEs	Y	Y	Y	Y	Y
Controls	-	Y	Y	Y	Y
Income-by-year bin FEs	-	-	Y	Y	Y
State-X-year FEs	-	-	-	Y	Y
Quad. birth year	-	-	-	-	Y

Each row and column represents the results from a different regression, for twenty total. Dependent variable is log person VMT. Sample includes all respondents aged 25-54 with positive person VMT. Demographics include race, urbanization, and family size. Observations weighted by person sample weights. Standard errors clustered by state. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.12: The effect of formative gasoline price on log miles traveled using NHTS 1990-2017, with cohort FEs.

	ln(VMT) (1)	ln(VMT) (2)	ln(VMT) (3)	ln(VMT) (4)
2-year price change				
$P_{cs}^{\Delta(m_{cs}+2,m_{cs})}$	0.0363	0.0432	0.0588+	0.0608+
	(0.0341)	(0.0344)	(0.0330)	(0.0323)
$P_{cs}^{\Delta(m_{cs}+1,m_{cs}-1)}$	0.0475	0.0383	0.0394	0.0377
	(0.0396)	(0.0393)	(0.0369)	(0.0355)
1-year price change				
$P_{cs}^{\Delta(m_{cs}+2,m_{cs}+1)}$	0.0088	0.0167	0.0343	0.0369
	(0.0401)	(0.0406)	(0.0438)	(0.0432)
$P_{cs}^{\Delta(m_{cs}+1,m_{cs})}$	0.0604	0.0639	0.0741	0.0752
	(0.0533)	(0.0539)	(0.0530)	(0.0508)
$P_{cs}^{\Delta(m_{cs},m_{cs}-1)}$	0.0338	0.0129	-0.0003	-0.0028
	(0.0487)	(0.0453)	(0.0436)	(0.0418)
Levels				
$P_{cs}^{m_{cs}}$	0.0091	0.0012	-0.0076	-0.0129
	(0.0326)	(0.0316)	(0.0323)	(0.0313)
Sample year FEs	Y	Y	Y	-
State FEs	Y	Y	Y	-
Age FEs	Y	Y	Y	Y
Birth year FEs	Y	Y	Y	Y
Controls	-	Y	Y	Y
Income-by-year bin FEs	-	-	Y	Y
State-X-year FEs	-	-	-	Y

Each row and column represents the results from a different regression, for twenty total. Dependent variable is log person VMT. Sample includes all respondents aged 25-54 with positive person VMT. Demographics include race, urbanization, and family size. Observations weighted by person sample weights. Standard errors clustered by state. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.13: The effect of formative gasoline price on vehicle efficiency and type.

	Gallons per mile				Truck, SUV, etc.			
	Average GPM (1)	Average GPM (2)	GPM (3)	GPM (4)	Any Big (5)	Any Big (6)	1[Big] (7)	1[Big] (8)
$P_{cs}^{\Delta(18,16)}$	-0.0000	-0.0001	-0.0001	-0.0001	-0.0265**	-0.0245*	-0.0193*	-0.0194+
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0095)	(0.0101)	(0.0092)	(0.0097)
$P_{cs}^{\Delta(17,15)}$	0.0000	-0.0002	-0.0002	-0.0003	-0.0213+	-0.0173	-0.0155	-0.0141
	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0111)	(0.0112)	(0.0106)	(0.0104)
$P_{cs}^{\Delta(m_{cs}+2,m_{cs})}$	0.0001	0.0001	-0.0001	-0.0000	-0.0203*	-0.0169+	-0.0141	-0.0110
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0090)	(0.0085)	(0.0094)	(0.0085)
$P_{cs}^{\Delta(m_{cs}+1,m_{cs}-1)}$	-0.0002	-0.0003	-0.0003	-0.0004	-0.0238+	-0.0209	-0.0193	-0.0179
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0126)	(0.0125)	(0.0117)	(0.0116)
Sample year FEs	Y	-	Y	-	Y	-	Y	-
State FEs	Y	-	Y	-	Y	-	Y	-
Age FEs	Y	Y	Y	Y	Y	Y	Y	Y
Demographics	-	Y	-	Y	-	Y	-	Y
Income-by-year bin FEs	-	Y	-	Y	-	Y	-	Y
State-X-year FEs	-	Y	-	Y	-	Y	-	Y
Vehicle age	-	-	Y	Y	-	-	Y	Y
Quad. vehicle year	-	-	Y	Y	-	-	Y	Y
Sample	Person	Person	Vehicle	Vehicle	Person	Person	Vehicle	Vehicle
Mean of dep. var.	0.0508	0.0508	0.0509	0.0509	0.4681	0.4681	0.4422	0.4422

Each row and column represents the results from a different regression, for thirty-two total. Dependent variable in Columns (1) to (4) is a measure of fuel economy in gallons per mile, and in Columns (5) to (8) is an indicator for a large vehicle (larger than a station wagon). Columns (1), (2), (5) and (6) treat people as the level of observation; other columns treat vehicles as the level of observation. Demographics include race, urbanization, and family size. Observations weighted by person sample weights. Standard errors clustered by state. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.14: The effect of gasoline price changes at different ages.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$a =$	13	14	15	16	17	18	19	20	21	22
$\tau =$	-3	-2	-1	0	1	2	3	4	5	6
Panel A: Extensive margin (1[drive])										
$P_{cs}^{\Delta(a,a-1)}$	-0.0005	0.0012	-0.0001	-0.0054**	-0.0036**	-0.0023	-0.0009	0.0001	0.0005	0.0022*
	(0.0016)	(0.0012)	(0.0015)	(0.0016)	(0.0014)	(0.0016)	(0.0014)	(0.0017)	(0.0013)	(0.0011)
$P_{cs}^{\Delta(m_{cs}+\tau,m_{cs}+\tau-1)}$	0.0009	-0.0015	-0.0029	-0.0048***	-0.0044*	-0.0036*	0.0004	0.0012	0.0002	-0.0011
	(0.0012)	(0.0013)	(0.0019)	(0.0013)	(0.0018)	(0.0017)	(0.0020)	(0.0013)	(0.0014)	(0.0019)
Panel B: Intensive margin (ln(VMT))										
$P_{cs}^{\Delta(a,a-1)}$	-0.0567	0.0263	0.0211	-0.0949*	-0.1125**	-0.0954*	-0.0395	0.0080	-0.0253	-0.0169
	(0.0498)	(0.0374)	(0.0403)	(0.0428)	(0.0401)	(0.0374)	(0.0422)	(0.0378)	(0.0412)	(0.0366)
$P_{cs}^{\Delta(m_{cs}+\tau,m_{cs}+\tau-1)}$	-0.0571	-0.0120	-0.0204	-0.0606+	-0.0618+	-0.0678+	-0.0583	-0.0077	-0.0213	0.0198
	(0.0379)	(0.0428)	(0.0445)	(0.0350)	(0.0343)	(0.0346)	(0.0399)	(0.0376)	(0.0379)	(0.0406)

Each row and column represents the results from a different regression, for forty total. Dependent variable is a binary indicator of whether the respondent drove to work in the census data and log person VMT in the NHTS sample. Regressions include state (or state of birth), sample year, and age fixed effects. Observations weighted by person sample weights. Standard errors clustered by state of birth. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.15: The effect of gasoline prices changes at different ages (two-year difference).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$a =$	13	14	15	16	17	18	19	20	21	22
$\tau =$	-3	-2	-1	0	1	2	3	4	5	6
Panel A: Extensive margin (1[drive])										
$P_{cs}^{\Delta a,(a-2)}$	-0.0018*	0.0004	0.0004	-0.0022*	-0.0038***	-0.0026*	-0.0016	-0.0003	0.0002	0.0014+
	(0.0009)	(0.0010)	(0.0008)	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0011)	(0.0008)	(0.0008)
$P_{cs}^{\Delta(m_{cs}+\tau,m_{cs}+\tau-2)}$	-0.0005	-0.0002	-0.0020+	-0.0030*	-0.0041***	-0.0034**	-0.0014	0.0008	0.0006	-0.0004
	(0.0007)	(0.0007)	(0.0010)	(0.0012)	(0.0010)	(0.0012)	(0.0014)	(0.0010)	(0.0009)	(0.0011)
Panel B: Intensive margin (ln(person VMT))										
$P_{cs}^{\Delta a,(a-2)}$	-0.0253	-0.0156	0.0222	-0.0287	-0.0786**	-0.0807**	-0.0532*	-0.0124	-0.0087	-0.0174
	(0.0316)	(0.0215)	(0.0264)	(0.0260)	(0.0264)	(0.0236)	(0.0218)	(0.0272)	(0.0227)	(0.0234)
$P_{cs}^{\Delta(m_{cs}+\tau,m_{cs}+\tau-2)}$	-0.0309	-0.0289	-0.0096	-0.0309	-0.0502*	-0.0525*	-0.0511*	-0.0261	-0.0146	0.0011
	(0.0232)	(0.0205)	(0.0278)	(0.0256)	(0.0193)	(0.0229)	(0.0228)	(0.0255)	(0.0233)	(0.0228)

Each row and column represents the results from a different regression, for forty total. Dependent variable is a binary indicator of whether the respondent drove to work in the census data and log person VMT in the NHTS sample. Regressions include state (or state of birth), sample year, and age fixed effects. Observations weighted by person sample weights. Standard errors clustered by state of birth. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.16: Persistence in the effect of formative gasoline prices on driving.

	Extensive margin		Intensive margin	
	1[drive]	1[drive]	ln(VMT)	ln(VMT)
	(1)	(2)	(3)	(4)
$P_{cs}^{\Delta 17,15} \times$				
1[25-34]	-0.0050**	-0.0054***	-0.0890*	-0.0552
	(0.0018)	(0.0013)	(0.0433)	(0.0425)
1[35-44]	-0.0001	0.0006	-0.0529	-0.0328
	(0.0014)	(0.0014)	(0.0578)	(0.0524)
1[45-54]	-0.0050***	-0.0054***	-0.0925+	-0.1111*
	(0.0014)	(0.0013)	(0.0516)	(0.0497)
$P_{cs}^{\Delta(m_{cs}+1, m_{cs}-1)} \times$				
1[25-34]	-0.0031*	-0.0039*	-0.0464	-0.0279
	(0.0015)	(0.0015)	(0.0341)	(0.0323)
1[35-44]	-0.0038*	-0.0019	-0.0595	-0.0581
	(0.0019)	(0.0014)	(0.0479)	(0.0474)
1[45-54]	-0.0056**	-0.0069**	-0.0445	-0.0406
	(0.0019)	(0.0020)	(0.0427)	(0.0425)
Sample year FEs	Y	Y	Y	Y
State FEs	Y	Y	Y	Y
Age FEs	Y	Y	Y	Y
Demographics	-	Y	-	Y
Income	-	Y	-	Y
State-X-year FEs	-	Y	-	Y
Quad. birth year	-	Y	-	Y

Dependent variable in Columns (1) and (2) is a binary indicator of whether the respondent drove to work; demographics include sex, marital status, educational attainment, and race; and income is log household income. Dependent variable in Columns (3) and (4) is log person VMT; demographics include race, urbanization, and family size; and income is income bins interacted with sample year. Observations weighted by person sample weights. Standard errors clustered by state. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.