

Formative Experiences and the Price of Gasoline*

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1 Introduction

Behavior is often modeled as an economic choice that depends on preferences and the current economic environment. Observable characteristics of agents and of the economic environment often fail to explain significant variation. In many consumer choice models, remaining variation is cast as idiosyncratic preferences for particular goods or outcomes (e.g., [Berry, Levinsohn, and Pakes 1995](#); [Manski and McFadden 1981](#)). Where do these idiosyncratic tastes come from?

This paper suggests ‘formative experiences’ shape individual behavior toward choices experienced later. In particular, we address whether the economic cost of driving that teenagers encounter during their formative driving years influences later-life driving behavior. We document a striking fact: Commuters in the United States who experience high gasoline prices while coming of age are less likely to commute to work by private automobile later in life. This suggests the price encountered when first interacting with the driving environment affects their transportation choices for decades to come.

We use complementary approaches to show that this relationship is not due to unobserved common experiences of cohorts that come of age during periods of high gasoline

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prices. We analyze the effects of 1979 oil crisis, which led to a dramatic increase in gasoline prices in the following years, within a regression discontinuity framework. Twenty years later (at the time of the 2000 census), workers who experienced these high prices during their formative driving years (around 16 years old) are significantly less likely to drive to work than preceding cohorts: their likelihood of driving alone to work goes down by between 0.21-0.50pp. We find stronger effects in urban settings with plausible alternative transport choices, and for lower-income workers. We also find that the decrease in driving is counteracted by an increase in public transit usage. There is little evidence of other discontinuities, and results are robust to including covariates that could provide standard explanations like contemporaneous income.

We then expand the analysis to study the effects of a long time series of gasoline price movements (not just those due to the 1979 oil crisis). By using repeated cross sections of the U.S. population and variation of gasoline prices across states and time, we are able to control for age, time, state, and even cohort confounders. This approach also allows us to confirm that nominal gasoline prices experienced at age 16 (rather than earlier) drives the results. We find that an increase in the nominal price of gasoline of \$1 leads to a 0.35-0.40pp reduction in the probability of driving to work later in life, which corresponds to the effects found using the 1979 oil crisis. The effect is about twice as large for age groups below 40 years old and disappears for older drivers, suggesting that formative driving experiences last for about a quarter of a century.

Supplementary analysis of driver license take up by age allows us to speak to whether frictions in skill acquisition or price anchoring are behind our findings.¹ Take up remains relatively constant across age profiles in response to the 1979 oil crisis, suggesting that higher skill acquisition costs are not suppressing learning. Age at first license shows a small increase, which could indicate a slight delay in licensing. Together, these results highlight the relative role price anchoring plays in explaining the later-life effects.

Our results directly complement an emerging literature that connects earlier-in-life experiences on preferences and behavior many years later. Some studies connect recessions to later-life investing and consumption patterns. [Malmendier and Nagel \(2011\)](#) show that those growing up during the depression are more risk averse in terms of asset holdings later in life, and growing up during recessionary a period leads to an increased preference for redistribution ([Giuliano and Spilimbergo 2013](#)). Similarly, exposure to severe

¹By price anchoring, we mean the persistent influence of an earlier information shock. Throughout our analysis, we control for current gasoline prices through fixed effects.

unemployment spells can lead to consumption deviations inconsistent with the permanent income hypothesis (Malmendier and Shen 2018). Related studies have connected risk preferences to earlier military experience and exposure to violence (Callen et al. 2014; Malmendier, Tate, and Yan 2011). We show that persistent effects of formative experiences apply much more broadly, influencing behavior in a very different environment: commuting. Further, formative experiences need not be extreme (such as experiencing severe economic downturns or violence), but can be much more mundane. Everyday interactions with market prices potentially influence later decisions.

An expansive literature in urban and environmental economics seeks to understand the short run relationship between gasoline prices and driving behavior. Much of this literature was, in fact, spawned by volatile oil prices in the 1970s (Espey 1998). The literature then began to study how fleet composition responded to changes in gasoline prices (Puller and Greening 1999), and whether this is consistent with standard models of rational expectations about gasoline prices (Li, Timmins, and Haefen 2009; Allcott and Wozny 2014; Busse, Knittel, and Zettelmeyer 2013). Hughes, Knittel, and Sperling (2008) show that gasoline usage has become more inelastic in recent years; 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 has been little evidence on how changes or heterogeneity in behavior arise.

This paper also suggests a richer story than the dominant narrative that changes in transportation patterns in the United States are driven solely by supply factors. In fact, a long literature in urban economics shows path dependent effects of the supply of transportation infrastructure—it influences the location of cities (Bleakley and Lin 2012; Davis and Weinstein 2008; Michaels and Rauch 2018), urban form (Brooks and Lutz 2018), and regional growth (Donaldson and Hornbeck 2016). Another line of inquiry connects urban form to contemporaneous driving (e.g., Duranton and Turner 2018). Little research has been conducted into longer run determinants of transportation *demand*.² While the determinants of demand may be more liquid than infrastructure, we show that there is a channel for path dependence through formative experience.

The next section describes some relevant information about the research setting; data is discussed in Section 3. We then analyze how the 1979 oil crisis continues to influence

²One notable exception is Anderson et al. (2015), who show that automobile brand preferences are highly correlated across generations.

driving patterns into the 21st century in Section 4. Section 5 uses a broader definition of treatment to more precisely quantify the long run effects of gasoline prices. We then examine contemporaneous changes in the licensing of young drivers in Section 6. We conclude and offer implications and policy suggestions.

2 Context and Motivation: Driving in the United States

The United States is a notably automobile-friendly nation: about 76% of workers commute alone in a private vehicle (85% including carpoolers), compared with 56% (64%) in the UK. Laws regulating driving tend to provide few barriers, and people start driving at younger ages than in most other nations. In 30 states in the United States, it is still possible to obtain an unrestricted driver license under the age of 18, the standard minimum unrestricted age in most of Europe. In 1980, only in five states was the minimum driving age for unrestricted driving greater than 16, and in another 5 it was less than 16. Well over half of those 17 years old have traditionally had driver licenses in the United States.

This age distribution matters: 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). First, young people often have access to vehicles, as well as training and supervision, while living at home. It is the norm in most non-urban and many urban communities to learn to drive during their 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.

3 Data

We draw our primary data from several sources, and discuss each in turn below:

Commuting Behavior and Vehicle Ownership. The U.S. Census asks questions about commuting mode and time. These ‘Journey to Work’ questions appear in the 1980, 1990, and 2000 Censuses, and more recently in the American Community Survey (ACS). We use data from these three Censuses, as well as the 2006/10 and 2011/15 ACS. Key variables of interest are: (i) the primary mode of commute for workers, and (ii) whether a household keeps a vehicle at home for use by members of the household. We also use a number of other variables, primarily demographic variables, employment status, and income.

Age plays a central role in our analysis, but interpretation of age from Census data requires qualification. Census microdata report both age and birth year (from which we define cohorts). 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 36 years of age in the 2000 Census was born in 1963, whereas someone born in March reporting an age of 36 in 2000 was born in 1964. We use birth year to define cohorts at age 16, recognizing that there is some spillover across years. The ACS is conducted on a rolling basis, meaning that there is no constant reference day. Although the sampling year is reported in the multi-year ACS data, it is difficult to precisely recover the birth year because the sampling date is not reported. However, in both Census and ACS, errors should be consistent from year to year.

In general, we restrict our analysis to adults aged 25 to 65 who are not living on a farm and were born in the United States. Labor force attachment, and therefore commuting, varies greatly for younger and older workers. We expect transportation behavior of farmers to be quite different from standard commuting behavior, and immigrants may have exposed to different gasoline price environments. We also exclude workers that did not report a mode of transportation (typically because they were not actively working, though employed).

Gasoline Prices. We investigate several measures of gasoline prices; most analyses use nominal price. The Energy Information Administration reports nominal tax-inclusive state-level gasoline price data starting in 1983. For the years 1966-1982, we use the *Highway Statistics Annuals*.³ We convert to real prices using the Consumer Price Index from the Bureau of Labor Statistics.

Driver Licensing. The Federal Highway Administration (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 driver licenses held by people of each age from 16 to 24 in each year. We also draw supplementary material on driver licensing requirements from the FHWA *Driver License Administration Requirements and Fees* booklet, which has been published roughly biannually since the 1960s.

Age-Specific Population. To estimate rates of driver license adoption, we require age-specific estimates of population. We construct this data from the National Cancer Insti-

³We thank Erich Muehlegger for sharing these data with us. See [Small and Van Dender \(2007\)](#) and [Li, Linn, and Muehlegger \(2014\)](#) for more details.

tute'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.

4 The 1979 Oil Crisis and Later-Life Driving

The price of gasoline in the United States was relatively stable over the 1950s and 1960s. Beginning in the 1970s, the global oil market entered a phase of increased price volatility. The United States experienced a small shock to gasoline prices in late 1973 and 1974, in response to oil production limits by OPEC. However, a much larger price shock was in store. Iranian oil production fell 94% in late 1978 during the Iranian Revolution, and the United States began to experience gasoline shortages and rising prices in early 1979. Prices continued to rise until the early 1980, at which point they briefly leveled off (in nominal terms). The Iran-Iraq war and removal of U.S. price controls by the Reagan administration in late 1980 and early 1981 led to another increase in prices. Nominal prices stayed at this unprecedented level through 1986, though high inflation devalued the real price.⁴ Figure 1 highlights the period of dramatically increasing real and nominal gasoline prices.

The high gasoline prices of the early 1980s are notable for three reasons. First, the increase was sudden. The nominal price doubled over the course of a year. This increase was unexpected, and was exacerbated by unpredictable demand-side responses.⁵ 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 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 there was an additional time expenditure required to obtain gasoline. These queues could be quite substantial: [Deacon and Sonstelie \(1989\)](#) and [Frech III and Lee \(1987\)](#) highlight the negative consequences of time wasted by queuing; [Deacon and Sonstelie \(1985\)](#) use this event

⁴See [Hamilton \(1985\)](#) for a discussion of the proximate causes of oil shocks.

⁵While the traditional explanation for the Second Oil Crisis was a supply-side shock, a recent economic literature mainly attributes the price spike to two types of demand shocks—increased inventory demand in anticipation of future shortages triggered by widespread panic about the political unrest in Iran, and ‘traditional’ demand shocks reflecting a strong global economy ([Baumeister and Kilian 2016](#)). Such demand shocks are unpredictable by nature.

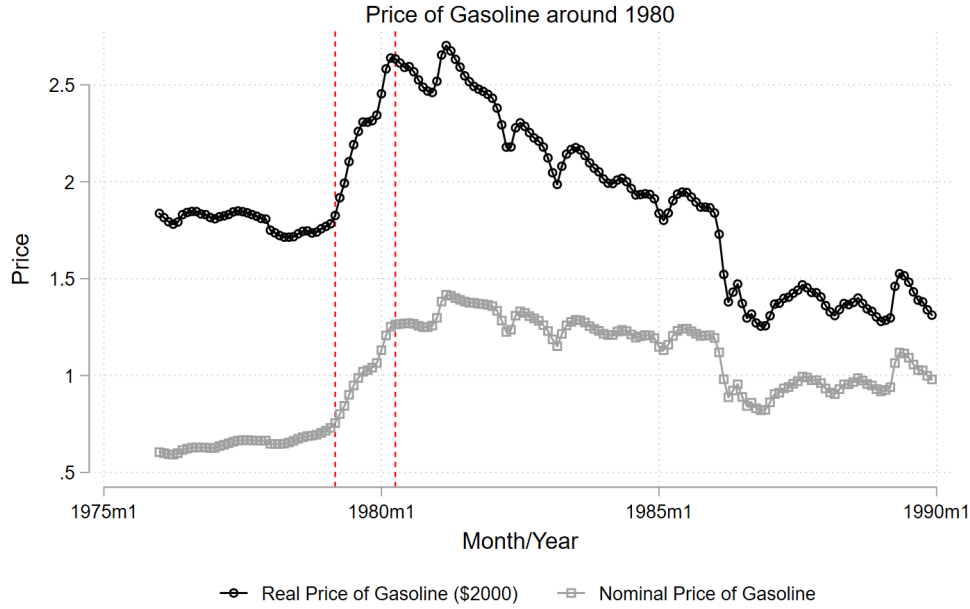


Figure 1: Gasoline prices from 1975 to 1990.

to estimate drivers' value of time. High prices led to a reduction in driving during the early 1980s. Total vehicle miles traveled (VMT) had been increasing by 4-4.5% annually from 1976 to 1979, but then declined over the next two years before slowly recovering.

The price shock was likely most consequential for those coming of driving age around the Iranian Revolution—i.e., those who were around 16 years old in 1980. There are two consequences of this price episode that may be related to long-run driving behavior: (i) The perceived price of driving may have increased, and (ii) learning to drive may have become more expensive. Both factors could plausibly lead to a reduction in driving later in life. If particular cohorts perceive prices as higher (even though everyone faces the same contemporaneous prices)—for example, because gasoline prices are particularly salient when deciding to take driving lessons and learning how to drive—they will be less likely to drive. Given that most drivers in the U.S. learn to drive before the age of 18, those who fail to learn to drive early in life may face more difficulties learning to drive later in life and even forego driving altogether.

4.1 Later-Life Driving

Figure 2 plots commuting mode and vehicle ownership more than two decades after the 1979 oil crisis, as reported in the 2000 Census. Commuters are matched to cohorts at age 16 (displayed along the horizontal axis). For example, the behavior of those born in 1964 in 2000 (at age 36) is indexed to the year 1980 (when they turned 16) in Figure 2, while the behavior of those born in 1968 in 2000 (at age 32) is indexed to cohort year 1984. Vertical bars bound the period of rapidly increasing gasoline prices shown in Figure 1.

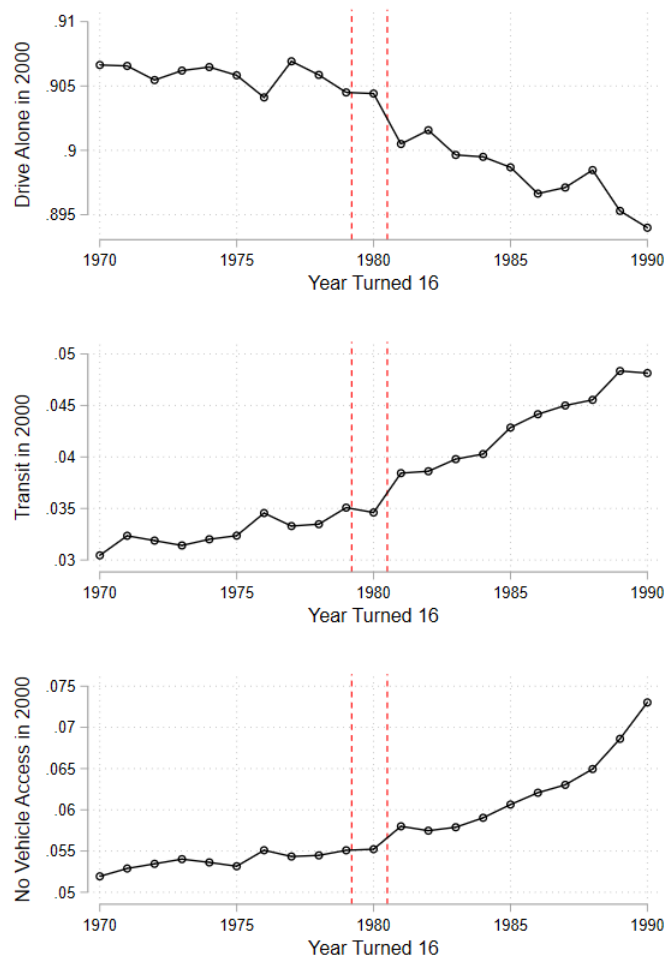


Figure 2: Commuting behavior and vehicle ownership by cohort in 2000.

There is a change in driving behavior for those who came of age after 1980. The probability that a commuter drives to work alone in a private automobile decreases and the

probability that a commuter takes mass transit to work increases. The decrease appears to be slightly less than one-half a percentage point, and marks a jump in behavior from cohorts turning 16 in 1980 and before, and those coming of age later. Furthermore, the bottom panel illustrates that individuals (not just commuters) are less likely to have access to a vehicle.

Treatment is not precise: gasoline prices increased over the course of 1979-80, and then bumped up further in 1981. We do not have a strong prior *ex ante* for the precise age when gasoline prices become salient. Nonetheless, it is clear from Figure 2 that a break, if it exists, most likely occurs between the 1980 and 1981 cohorts. We therefore turn to a regression discontinuity (RD) framework to quantify the size of this break. We discuss under what conditions these RD estimates have a causal interpretation below, and adopt a more general approach in Section 5.

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

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

where Y_i is an outcome of interest for individual i in the 2000 Census, S_i is the year that i turned 16, and X_i are other characteristics of i . Treatment is the binary variable D_i , which is equal to one if i turned 16 after 1980. The function $g(\cdot)$ captures trends in driving behavior; we experiment with linear and quadratic functions that are allowed different slopes before and after 1980.⁶ Data are limited to a symmetric bandwidth around the treatment year.⁷ We restrict the sample to non-farm workers born in the United States between the ages 25-65.

Panel A of Table 1 present 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 alone to work of 0.21pp to 0.50pp that persists roughly twenty years after turning 16. The quadratic results are less precise, but less prone to bias because it accommodates more curvature of the running variable. Point estimates are relatively consistent across both linear and quadratic estimates.

⁶We 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.

⁷We define the treatment time as just after 1980. Thus, a bandwidth of two includes cohorts that turn 16 in 1979, 1980, 1981, and 1982.

To assign a causal link between the gasoline price jump in 1979-80 and reduced later-life driving behavior of cohorts coming of age after 1980, we need to perform two related tasks. First, we have to establish that there are no other discontinuous breaks in characteristics that occur at that time. That is, can we show that the change in driving is due to turning 16 after 1980, rather than some other factor? Second, we need to ensure that there are no other competing explanations: Does this effect truly represent a response to increased gasoline prices, or something else that happened to these cohorts?

We show graphs of covariate smoothness in the Appendix across a range of demographic, employment, and housing characteristics. There are no obvious 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 in the Appendix and alleviate measurement error due to the gradual change of prices throughout the years 1979-80. Results are similar in magnitude, though slightly less significant.

Table 1: Discontinuity in turning 16 after 1980 on commuting behavior in 2000.

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

Regression discontinuity estimates of the effect of turning 16 after 1980 or later on a binary indicator of whether the respondent drove to work as reported in the 2000 Census. Bandwidth is symmetric around 1980.5. The 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. ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

We cannot explain the effect solely through observable characteristics. Panels B through D of Table 1 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).⁸ Panel C adds state of birth fixed effects to control for differential commuting behavior in different places. We include state of birth, rather than residence, because it is exogenous with respect to later life commuting decisions. Panel D adds contemporaneous income, but we recognize it may not be an appropriate control if later-life income is both influenced by graduating into a recession and income influences vehicle purchasing.⁹

The covariates decrease point estimates, 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.¹⁰ This suggests that there are persistent effects of gasoline prices while coming of age that cannot be explained by earnings.

It is apparent from the preceding analysis that post-1980 cohorts exhibit different driving behavior later in life. This cannot be due to gasoline prices immediately leading up to 2000: Everyone faced the same price profile in the preceding years. Analysis centers on adults 36 years old, so life cycle trends have smoothed out for the most part. Further, there is a logical link between the high gas prices and reduced driving. However, though highly suggestive, this research design ultimately does not let us conclusively confirm that the effect is due to high gasoline prices. We therefore approach this question with a different research design in Section 5, but first, we examine the responses of different subgroups to the 1979 oil crisis. The intuitive responses we find add some credibility to the RD results presented above.

4.2 Other Outcomes: Transit and Vehicle Accessibility

The negative effect on driving is largely compensated by an increase in transit, as shown by Panel A of Table 2. Those coming of age just after 1980 are 0.2-0.4pp more likely to transit than their counterparts coming of age just a bit earlier. The magnitude of the effect

⁸The price of gasoline is associated with business cycles, which can affect both educational attainment and income (Stuart et al. 2017).

⁹For example, Oreopoulos, Von Wachter, and Heisz (2012) find short run and persistent (ten year) wage penalties for graduating into a recession, and Stuart et al. (2017) finds evidence that wage effects still persist from the 1980-82 recessions.

¹⁰In fact, income is a plausible mechanism for the effect in Panel A.

is ~50%-100% in absolute of the effect size in Panel A of Table 1, suggesting that transit is the primary substitute from driving (relative to working at home, carpooling, or self-powered means).

Consistent with the effects on driving to work and public transit, those coming of age after 1980 are also less likely to have access to a vehicle. Panel B shows RD estimates of Equation (1) on vehicle access for all non-farm natives (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 effect, roughly 0.3pp, is generally in line with the transit results. Those coming of age after 1980 are less likely to drive along to work, more likely to take transit, and less likely to have access to a private vehicle.

Table 2: Discontinuity in turning 16 after 1980 on transit usage and vehicle access in 2000.

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

Regression discontinuity estimates of the effect of turning 16 after 1980 or later on a binary indicator of FIX. Bandwidth is symmetric around 1980.5. The 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). + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

4.3 Variation by Location and Income

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.¹¹ The choice of location is potentially endogenous, however, so we present subgroup analysis on location as suggestive evidence. Estimates, shown in Table 3, are large (from -0.9pp to -1.9pp) and are largely robust to bandwidth and trend specification. For urban dwellers, someone 36 or under is roughly 1pp less likely to drive to work than someone 37 or older in 2000.

Conversely, there is little effect on workers who live outside of metropolitan areas, as Panel B reveals. Point estimates are small, variable, and insignificant. Taken with the results in Tables 1 and 2, these results suggest that the persistent effect of the 1980 oil price shock is largely concentrated in cities where viable transportation alternatives are available.

Panels C and D of Table 3 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 in Panel B 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 1.

¹¹There are several MSAs for which principal city status may violate disclosure rules and therefore not reported in the 2000 Census. This is why sample sizes are significantly lower than in Table 1.

Table 3: Discontinuity in turning 16 after 1980 on commuting behavior in 2000 – Subgroup analysis.

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

Regression discontinuity estimates of the effect of turning 16 after 1980 or later on a binary indicator of whether the respondent drove to work as reported in the 2000 Census. Bandwidth is symmetric around 1980.5. The 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). + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

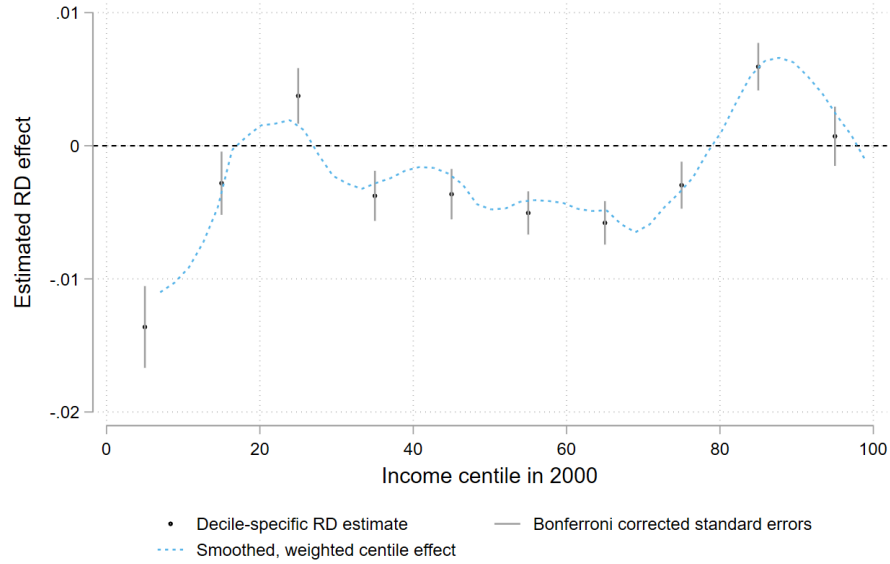


Figure 3: Regression discontinuity estimates of the 1979-80 gas price shock on driving in 2000 by income decile and (smoothed) centile.

Finally, Figure 3 examines the effect of being in a post-1980 cohort on driving across the income distribution. We divide the population of commuters both into centile and into decile bins, and then run the RD estimator using the linear specification with a bandwidth of five years within each bin. Decile estimates are shown as dots with Bonferroni-corrected 95% confidence intervals represented by the vertical bars.¹² Estimates for the lowest decile are negative (about -1.4pp) 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.

Taken together, these results suggest that the post-1980 cohorts are different in ways that support a difference in transportation patterns: lower income workers, and workers in cities, show stronger effects. Further, transit use compensates the loss in driving.

¹²Corrected for ten tests across the deciles.

5 Long-Run Driving Effects of Gasoline Prices

We have established differences in later-life driving behaviors between cohorts that come of age after 1980 and those that come of age earlier using only a single shock to the price of gasoline. To more directly link long-run behavior with early-life exposure, we use all available variation in gasoline prices across time and states with a fixed effects research design. We tie the price of gasoline that someone experiences at age 16 in their state of birth to later life driving. Timing is somewhat different in this section: we suggest 16 as the age when the formative experience occurs; sensitivity tests follow later. Our approach allows controlling for age, time, and state confounders. In the most stringent specification, we also include cohort fixed effects to compare individuals coming of age at the same time in slightly different price environments. Placebo tests show that timing matters: the price of gasoline around age 16 matters, while the price of gasoline when younger does not.

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

$$Y_{icst} = \theta P_{s_i, t-(A_i-16)} + h(A_i) + \kappa_s + \delta_t + X_i' \lambda + \varepsilon_{icst} \quad (2)$$

where A_i is the age of person i at the time of the sample. The price of gasoline in state s at year t is P_{st} , so $P_{s_i, t-(A_i-16)}$ is the price of gasoline faced by person i born in state s_i experienced in state s_i 16 years after they were born (i.e. in year $t-(A_i-16)$). We model the effect of age using either a quadratic specification or individual age fixed effects in $h(A_i)$ to control for life cycle effects. Different states may have different behavior on average due to different provision of infrastructure, social norms, etc.; state of birth fixed effects, κ_s , capture these differences. Finally, sample year fixed effects, δ_t , control for current gas prices, business cycle trends in employment, etc. We limit the sample to non-farm workers born in the United States aged 25 to 65 (inclusive) in the 1980, 1990, or 2000 Census or the 2006/10 or 2011/15 ACS.¹³ In most specifications, we only include those who still reside in the state of their birth.¹⁴ Gasoline price data begins in 1966, so that is

¹³Excluding Alaska, Hawaii, and the District of Columbia, because they do not have a complete series of gasoline prices.

¹⁴We are concerned about incorrectly matching people to gasoline prices experienced earlier in life. In our data, there is no way to track people through time and space. However, we do observe state of birth and current state of residence. Given the life cycle of migration decisions, we believe this to be reasonable (Kaplan and Schulhofer-Wohl 2017). One concern is that those who leave their birth state to attend college may move back, but face a different gas price environment during early adulthood. While we cannot rule

the earliest cohort we track.

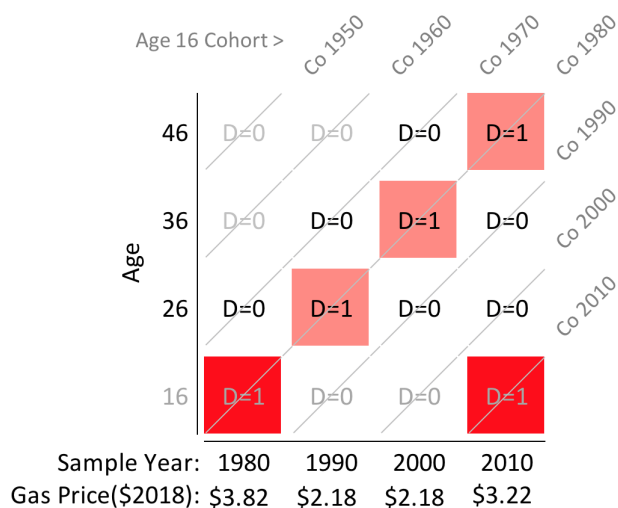


Figure 4: Identification in cohort designs.

This specification is identified from variation in driving behavior across the life cycle of cohorts facing different prices. To gain intuition, consider Figure 4. Columns represent sample years, whereas rows represents ages at time of sample. Cohorts are tracked by following anti-diagonals. The price of gasoline that each cohort experiences at age 16 is shown along the bottom line. For ease of explanation, consider a binary treatment equal to one if the real price of gasoline at age 16 was greater than \$3/gallon; this is shown in red. Our model omits those younger than 25 (their driving behavior is very noisy), and so the ‘treated’ 1980 cohort advances and appears in the sample in 1990. They remain treated as they progress through the life cycle. The estimator in Equation (2) includes sample year and age fixed effects, and is thus conceptually similar to a difference-in-differences estimator where treatment progresses through the life cycle. Cohort membership determines treatment status.¹⁵ Instead of the dichotomous treatment shown in Figure 4, we use a continuous measure of gasoline price.

Estimates of θ from baseline specifications of Equation (2) on the sample born in the

this out, even students applying to selective schools are significantly more likely to attend if they attended high school in the same state (Bostwick 2016; Griffith and Rothstein 2009), limiting concern about return migration.

¹⁵One adjustment that needs to be made is clustering by cohort, rather than by ‘age’, because of the serial correlation that results from belonging to the same cohort. We cluster by state, which nests cohorts within each state.

current state of residence are shown in Table 4. Most columns use nominal gasoline prices, while columns (4) and (5) use real gasoline prices (deflated by the CPI). All coefficients are negative. The coefficients on nominal price are significant, while those on real price are insignificant. An increase of the nominal gasoline price by \$1 is associated with a 0.35-0.40pp decrease in the likelihood of driving later in life. The magnitude of these estimates corresponds to the effect measured in Section 4 (reflecting roughly a doubling of prices in between 1979 and 1981). Columns (1) and (4) use a quadratic specification for age, while the other columns use age fixed effects; results are similar in both cases. Column (3) expands the sample to all workers (not just those who reside in their state of birth). Because we do not observe state of residence at age 16, this specification also includes current state of residence fixed effects to mitigate the effects of measurement error in price. The coefficient is smaller, but still significant.

The significance of the nominal prices, rather than the real prices, suggest that that real expenses are not playing a large role. We conjecture that the nominal gasoline price breaking the highly salient threshold value of \$1 played a role. This adds weight to a price shock or anchoring mechanism, rather than cost frictions.

Table 4: The effect of gasoline prices at age 16 on driving to work using the Census/ACS 1980-2015 panel.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|-------------------------------|-----------------------|-----------------------|----------------------|---------------------|---------------------|----------------------|------------------------|------------------------|
| Nominal $P_{s_i, t-(A_i-16)}$ | -0.0040** (0.0012) | -0.0035** (0.0013) | -0.0017+ (0.0009) | | | -0.0033* (0.0013) | -0.0048*** (0.0010) | -0.0104*** (0.0016) |
| Real $P_{s_i, t-(A_i-16)}$ | | | | -0.0013 (0.0012) | -0.0011 (0.0013) | | | |
| Census year FEs | Y | Y | Y | Y | Y | Y | Y | Y |
| State of birth FEs | Y | Y | Y | Y | Y | Y | Y | Y |
| Age (quadratic) | Y | - | - | Y | - | - | - | - |
| Age (FEs) | - | Y | Y | - | Y | Y | Y | Y |
| ln HH income | - | - | - | - | - | Y | Y | Y |
| Demographics | - | - | - | - | - | - | Y | Y |
| Sample | Stayers | Stayers | All | Stayers | Stayers | Stayers | Stayers | Stayers 16>1982 |
| N | 9364k | 9364k | 14763k | 9364k | 9364k | 9326k | 9326k | 3376k |

Dependent variable is 'drive to work alone'. The sample includes all native born persons actively working in the Census, and between the ages of 25-65. Column also includes current state of residence FE. Regressions include person weights. Standard errors clustered by state of birth. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In the last three columns of Table 4, we expand on these results. The inclusion of household income, surprisingly, has little effect. Demographics have a stronger impact on

point estimates, increasing the point estimates to -0.48pp (Column 7), but this is generally in line with previous estimates. To ensure that the estimated effect is not being identified from the 1979 Oil Crisis (the same variation used in Section 4), Column (8) shows the effect for cohorts that came of age in 1983 or later. The effect is larger and significant: a \$1 increase in nominal gasoline prices is associated with a decrease in later life driving by approximately 1.04pp. The larger magnitude likely reflects higher nominal gas prices in later periods.

We can strengthen the above design and exploit variation in gasoline price changes across states. We include *cohort* fixed effects, eliminating concerns that particular cohorts experience common confounding trends. As is apparent from Figure 4, cohorts (along the anti-diagonals) begin to saturate the available variation within a single state. Differential variation across states therefore becomes more important. To this end, we estimate a variant of Equation (2) including cohort fixed effects, ξ_c :

$$Y_{icst} = \theta P_{s_i, t-(A_i-16)} + h(A_i) + \kappa_s + \delta_t + \xi_c + X_i' \lambda + \varepsilon_{icst} \quad (3)$$

Identification in this setting is violated if gasoline price changes within states are correlated with other factors that influence later life driving behavior, conditional on sample year, state of birth, and age.

Results, shown in Columns (1)-(4) of Table 5 support long-run negative effects of gasoline prices increases on driving. All columns are negative and at least marginally significant, despite the stringent specification. Columns (1) and (2) shows results from those residing in their state of birth. Estimates show that a \$1 increase in the price of gasoline leads to a decrease in a later-life driving by -3.4 to -3.7pp. The larger magnitude is notable. The identifying variation comes solely from differences in gasoline price changes across states, rather than also relying on national movements in gasoline prices.¹⁶ Estimates including all workers are larger and more significant (Columns 3-4), though there is some risk in this sample that treatment is contaminated by movers. The largest estimates suggest nearly a 5pp effect.

Columns (5) through (8) report estimates of the effect on transit usage for commuting, and whether or not workers have a car available. Results are positive and significant, suggesting that workers are substituting away from driving alone to using public transit.

¹⁶Nominal prices are higher in more recent periods. This implies greater variation and thus a greater role in identification. Further, this may be amplified or muted if cross-state differentials are increasing and decreasing over time. We hope to investigate this further.

Table 5: The effect of gasoline prices at age 16 on driving to work using the Census/ACS 1980-2015 panel – Including cohort fixed effects.

| Dependent variable | (1) driving to work | (2) driving to work | (3) driving to work | (4) driving to work | (5) transit to work | (6) transit to work | (7) no vehicle | (8) no vehicle |
|-------------------------------|---------------------------|---------------------------|---------------------------|---------------------------|---------------------------|---------------------------|----------------------|----------------------|
| Nominal $P_{s_i, t-(A_i-16)}$ | -0.037+ (0.021) | -0.034+ (0.018) | -0.049* (0.023) | -0.044* (0.019) | 0.032* (0.015) | 0.039* (0.015) | 0.035+ (0.020) | 0.039* (0.018) |
| Census year FEs | Y | Y | Y | Y | Y | Y | Y | Y |
| State of birth FEs | Y | Y | Y | Y | Y | Y | Y | Y |
| Age (FEs) | Y | Y | Y | Y | Y | Y | Y | Y |
| Cohort FEs | Y | Y | Y | Y | Y | Y | Y | Y |
| ln HH income | - | Y | - | Y | Y | Y | Y | Y |
| Demographics | - | Y | - | Y | Y | Y | Y | Y |
| Sample | Stay | Stay | All | All | Stay | All | Stay | All |
| N | 9.4e6 | 9.3e6 | 14.8e6 | 14.7e6 | 9.3e6 | 14.7e6 | 12.3e6 | 19.3e6 |

The sample includes all native born persons actively working in the Census, and between the ages of 25-65. Regressions include person weights. Standard errors clustered by state of birth. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The transit effect makes up roughly 90% of the drive alone effect. The price of gasoline also appears to influence asset ownership: workers experiencing higher gasoline prices during their formative driving years are less likely to have a vehicle at home.

5.1 Effects over the Life Cycle

We test for heterogeneous effects over the life cycle to determine whether the salience of the early-life gasoline price decreases or increases over time. To do this, we index five-year wide age bins by b and estimate the following variant of Equation (2):

$$Y_{icst} = \sum_b \theta_b 1_{[A_i \in b]} P_{s_i, t-(A_i-16)} + h(A_i) + \kappa_s + \delta_t + \varepsilon_{icst} \quad (4)$$

This gives a unique estimate of θ_b for each window. We restrict the sample to ages 25 to 54, and use age fixed effects.¹⁷

Figure 5 shows that the effect varies over the life cycle. For age groups less than 35, the effect is roughly -0.7pp. It is significant and more negative than the corresponding estimates in Table 4. However, the effect disappears for older cohorts, and becomes (insignificantly)

¹⁷We only have state-specific gasoline prices back to 1966, meaning that we only observe driving behavior for those aged 55-65 starting in the 2006/10 ACS. There is thus little identifying variation for these groups.

positive for the oldest cohorts. This suggests a persistent, anchoring effect that lasts for roughly a quarter century before diminishing.

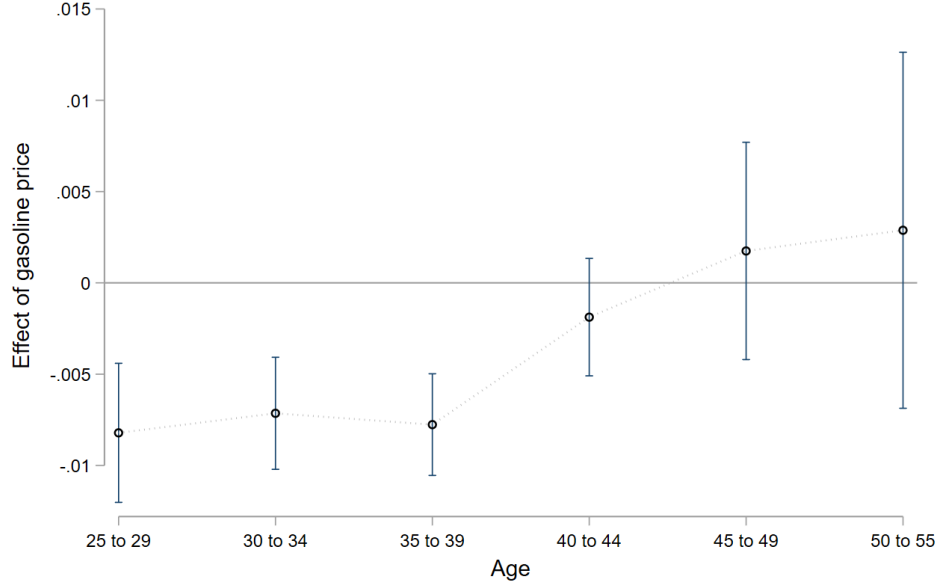


Figure 5: Heterogeneous effects of the gasoline price faced at age 16 on driving to work over the life cycle.

5.2 Placebo Test

To show that the timing of gasoline price changes matter, we show that gasoline prices at younger ages do not affect later-life driving. Columns (1)-(5) compare estimates of θ using $P_{s_i, t-(A_i-K)}$, with $K = 12, \dots, 16$. For this placebo test, we use Equation (2) with a quadratic specification in age and limit to those to whom we can match a gasoline price for each starting at age 12.¹⁸ Estimates show that the price of gasoline before the age of 15 has a small and insignificant effect; the effect becomes significant for age 15 and 16 (Table 6). In fact, the point estimates are small and consistently grow with age.¹⁹ This suggests that timing is playing a role: gas prices before age 15 do not matter.

¹⁸If the specification in Equation (3) is too stringent, it may reject the placebo too easily. Thus, using a less stringent specification is more conservative as a placebo.

¹⁹Column (5) is different from Table 4 Column (1) because a few cohorts are excluded to ensure a constant sample in Table 6.

Columns (6)-(8) expand the analysis to all workers with matched gasoline prices. Gasoline price at age 17 continues to play a role, signifying the broad age range at which drivers first learn to drive (see next Section). Column (8) smooths price exposure by averaging gasoline prices across ages 15 to 17. The coefficient is in line with previous estimates and highly significant.

Table 6: Placebo test of the effect of the gasoline price faced at age 16 on driving to work using Census/ACS 1980-2015 panel.

| At age: | (1) 12 | (2) 13 | (3) 14 | (4) 15 | (5) 16 | (6) 16 | (7) 17 | (8) Ave(15-17) |
|----------------------------------|---------------------|---------------------|---------------------|----------------------|-----------------------|-----------------------|-----------------------|-----------------------|
| Nominal price, state of birth | -0.0001 (0.0015) | -0.0004 (0.0017) | -0.0015 (0.0015) | -0.0027* (0.0013) | -0.0035** (0.0013) | -0.0040** (0.0012) | -0.0043** (0.0013) | -0.0047** (0.0015) |
| Age (quadratic) | Y | Y | Y | Y | Y | Y | Y | Y |
| Census year FEs | Y | Y | Y | Y | Y | Y | Y | Y |
| State of birth FEs | Y | Y | Y | Y | Y | Y | Y | Y |
| Sample | Stayers | Stayers | Stayers | Stayers | Stayers | Stayers | Stayers | Stayers |
| N | 7891k | 7891k | 7891k | 7891k | 7891k | 9364k | 9364k | 9019k |

Dependent variable is 'drive to work alone'. The sample includes all native born persons actively working in the Census, and between the ages of 25-65. Columns 1-5 include only drivers with gasoline prices at each age from age 12 to 16. Columns 6-8 include all drivers with a gas price at the denoted age. Regressions include person weights. Standard errors clustered by state of birth. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

6 Interpretation and Mechanisms

We have shown that nominal gasoline prices have a long run effect on driving behavior, and that this effect lingers until roughly the age of 40. The influence of nominal rather than real price suggests price anchoring effects as opposed to reduced skill acquisition. To provide further evidence of this, we return to the gasoline price shock induced by the 1979 oil crisis and examine the response in driver licensing.

6.1 Evidence from Driver Licensing

We compute two sets of statistics related to driver licensing to reinforce the finding that gasoline prices experienced when coming of age influence long-run driving behavior: (i) the percentage of each cohort that has a license by a certain age, and (ii) the average age at first license. Neither of these statistics are directly observable in the data, but the Federal

Highway Administration (FHWA) does publish annual data on the number of drivers of each age from 15 to 24. We combine supplemental estimates of the age distribution of the population with reasonable assumptions to generate these statistics. In general, we focus on the era before the mid-1990s when states began instituting graduated driver licensing programs.²⁰

There does not appear to be a large change in driver licensing following the 1979 oil crisis, although data is somewhat noisy. Figure 6 shows the percentage of each age (16, 17, 18, 20, and 22) that has a license in each year.²¹ 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 for in 1981 and 1982 but a slight uptick for others.

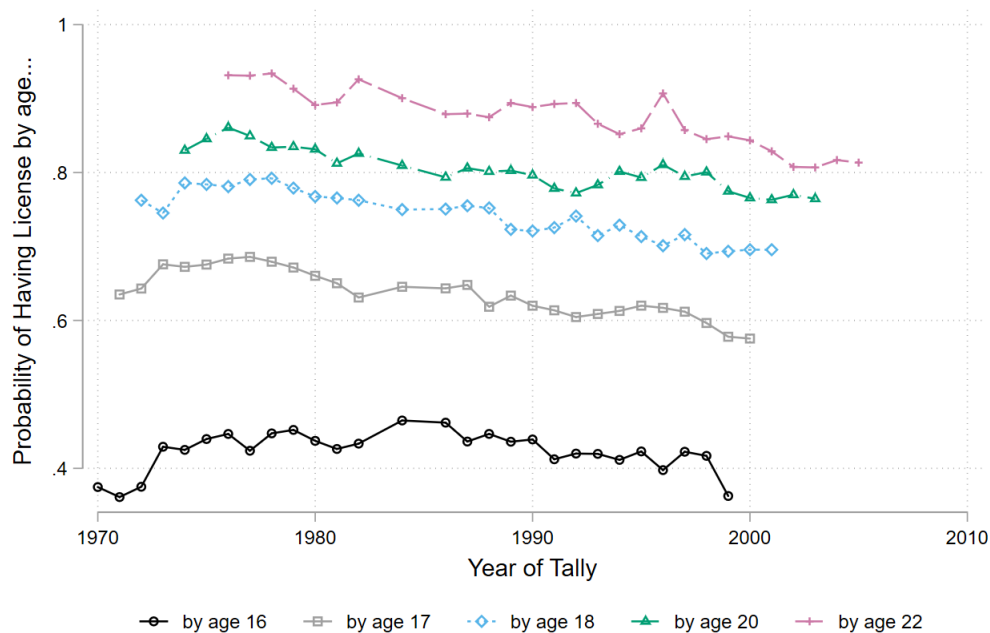


Figure 6: The probability of having a driver license by calendar year and age.

²⁰Graduated driver licensing programs require young drivers to progress through a series of levels with increasing privileges, such as being able to drive at night or with passengers. The requirements for each level is typically function of age, experience, and training.

²¹Figure 6 omits 1983 and 1985 because the driver license counts by age in those years was extrapolated from prior years. This data is noisy, but shows a marked increase in driver licenses of 18 year olds in 1983. Figure 7 shows a consequence of this.

We can also use the data to try to estimate an intensive margin of driver license adoption: timing. We impute the average age at first license from our data. Though we do not observe the precise age at which drivers first receive their license, we see the number of licensed drivers of each age in each year. Following cohorts across years, we estimate the number of new drivers who obtained their license between, e.g., age 16 and 17. The average age at first license is then a weighted average of these ages with weights determined by the implied number of new drivers. Calculate average age at first license for cohort b as:

$$\bar{A}_b^F = \frac{15 \cdot N_{\leq 15,b} + \sum_{a=16}^T a \cdot \max\{0, N_{a,b} - N_{a-1,b}\}}{N_{\leq 15,b} + \sum_{a=16}^T \max\{0, N_{a,b} - N_{a-1,b}\}} \quad (5)$$

where $N_{a,b}$ is count of people of birth cohort b who have a driver license and T is some cutoff age above which few people are first obtaining licenses. We report caps of 18, 20, and 22.²² The difference $N_{a,b} - N_{a-1,b}$ is occasionally negative. This is unlikely (license revocations are rare), so we assume these are due reporting errors and require the difference be non-negative.²³

Results in Figure 7 show a distinct bump in the average age of those obtaining licenses around 1980 for different cutoff ages. This spike varies with cutoff age because Figure 7 plots results by cohort year (unlike Figure 6 which plots results by observation year). This jump would seem to suggest that the 1979 oil crisis increased the average age at first license by about 0.2 years for those immediately impacted relative to preceding or following cohorts. Interestingly, there does not seem to be a long run effect. Note, however, that we cannot rule out that this jump is due to age imputation errors in the data for 1983 and 1985, and omitting these years would render this exercise impossible.

Because Figure 6 does not rely on imputed license data, it is more reliable than Figure 7. It suggests no extensive margin adjustment on driver licensing adoption, which in turn suggests that reduced or delayed skill acquisition played a minor role at most. Figure 7 suggests a small intensive margin adjustment, but this may be due to imputation errors. Given the muted evidence for an effect of the 1979 oil crisis on driver licensing, we conclude that anchoring likely plays a large role in explaining the results shown in Sections 4 and 5.

²²The data can only be computed for cohorts with enough preceding FHWA data books, starting in year $\text{Year} - (T - 15)$.

²³Hence the max statements in Equation (5).

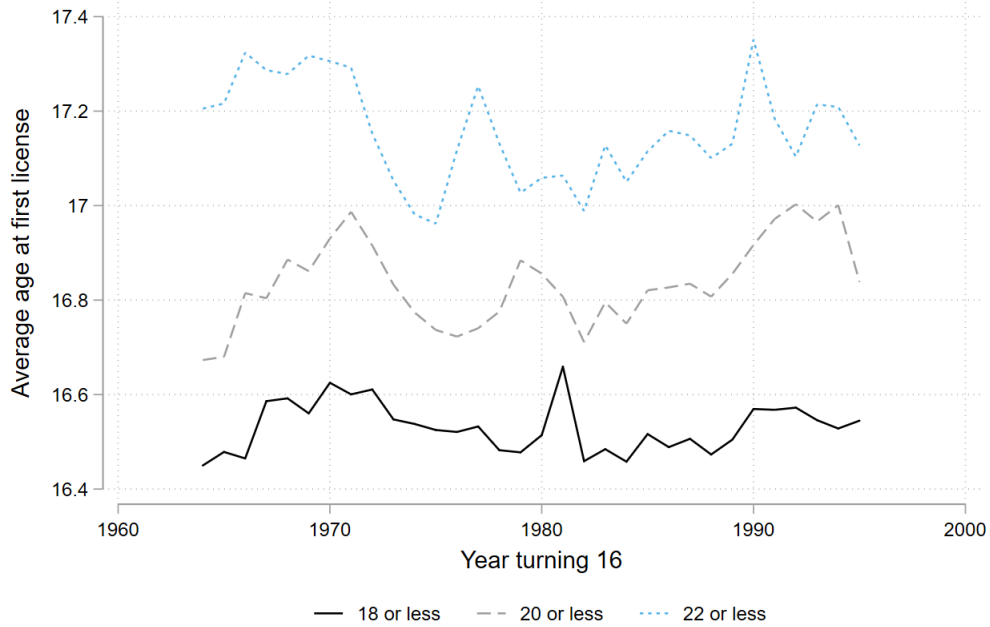


Figure 7: The average age at which drivers receive their first license, for different cutoff ages.

7 Conclusion

Early experiences frame how people perceive different goods and activities. These formative periods can drive later-life behavior expectations and norms. In the case of driving, we find that individuals who experience high prices during their formative driving period behave different than those that experienced lower prices: they drive to work less often, take transit more, and are less likely to have access to a vehicle. We show that these effects are most likely due to a different perception of the driving experience, rather than difficulties acquiring driving skill.

Our results suggest that programs focused on adjusting driving-related norms may be effective, but timing plays an important role. Targeting teenagers during formative driving experiences can have long lasting impacts on behavior.

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Appendix

A Figures and Tables

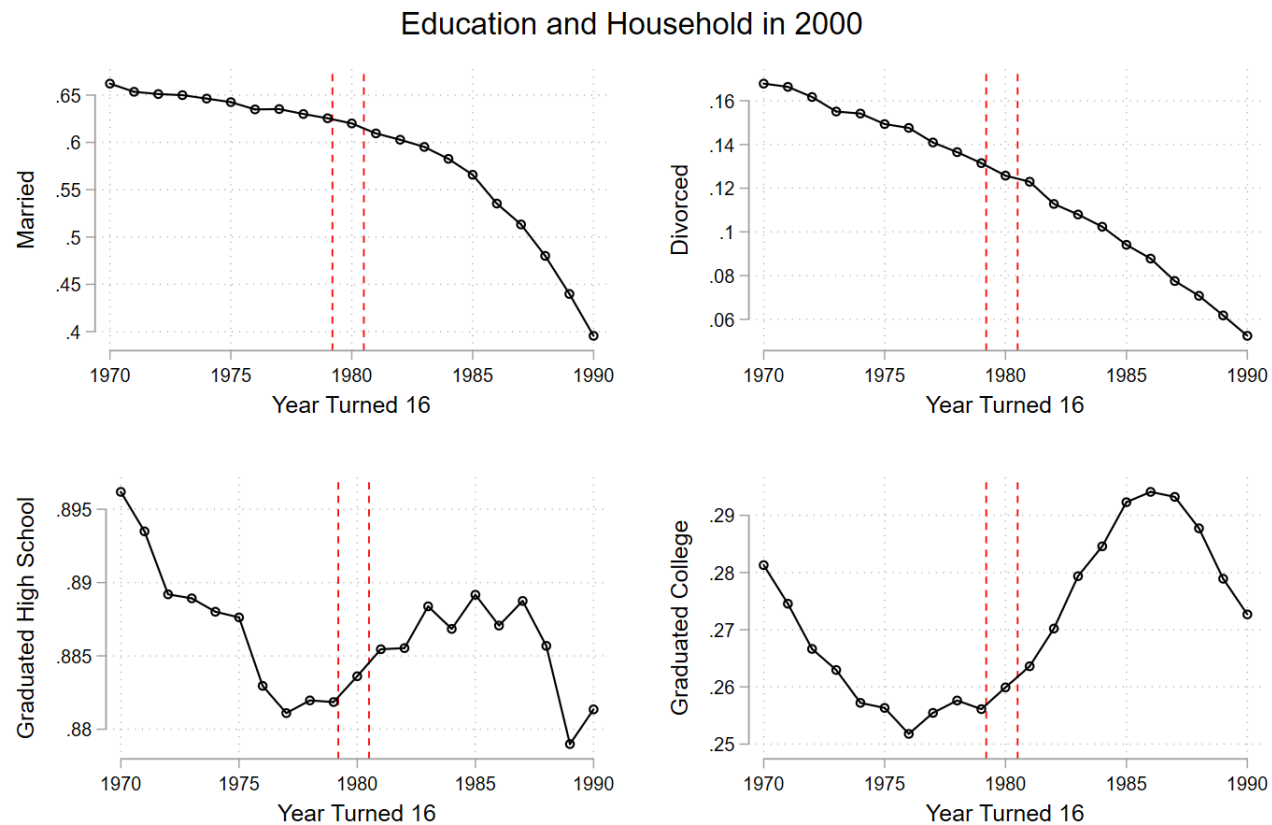


Figure A.1: Demographic characteristics in 2000 by year turned 16.

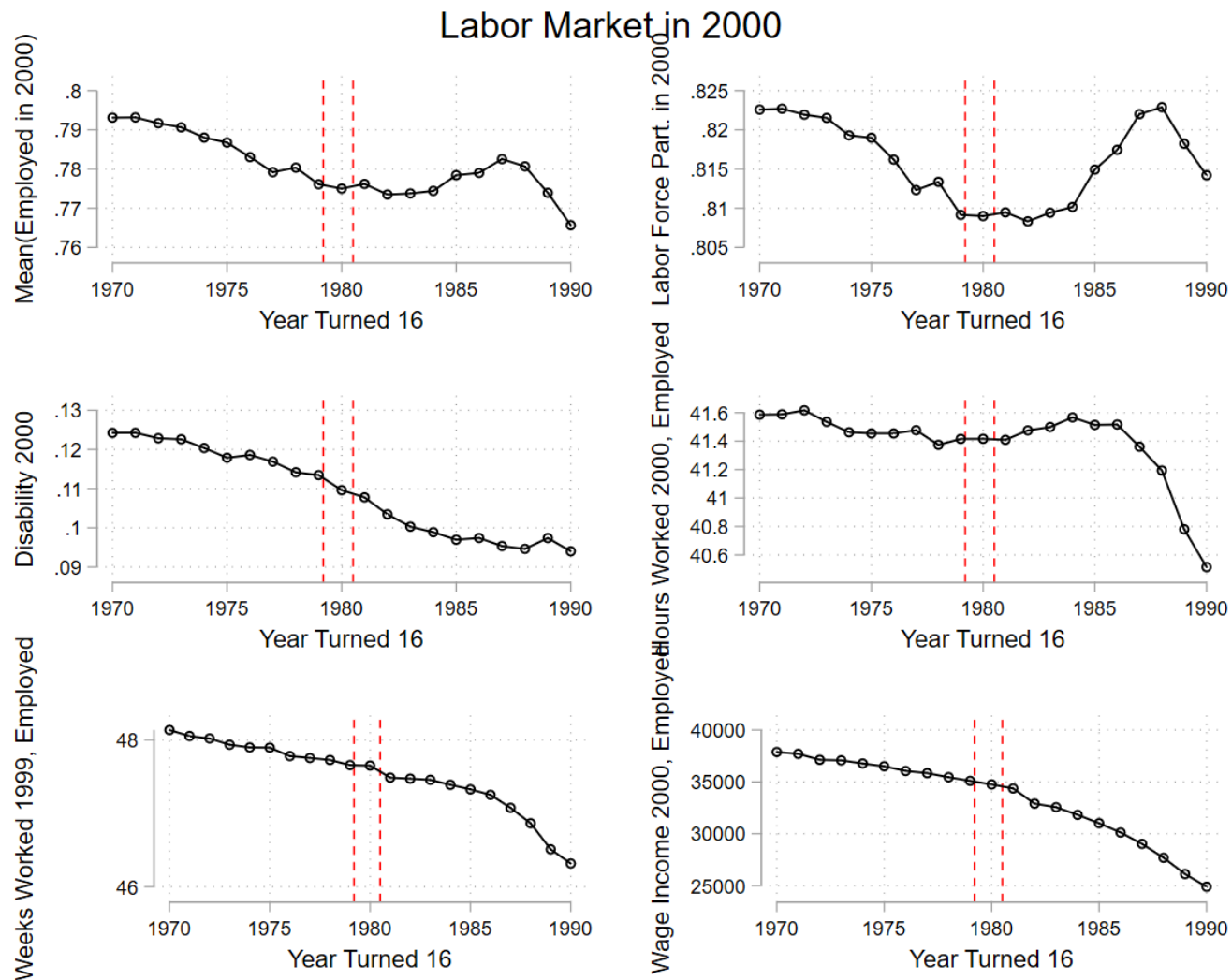


Figure A.2: Labor market characteristics in 2000 by year turned 16.

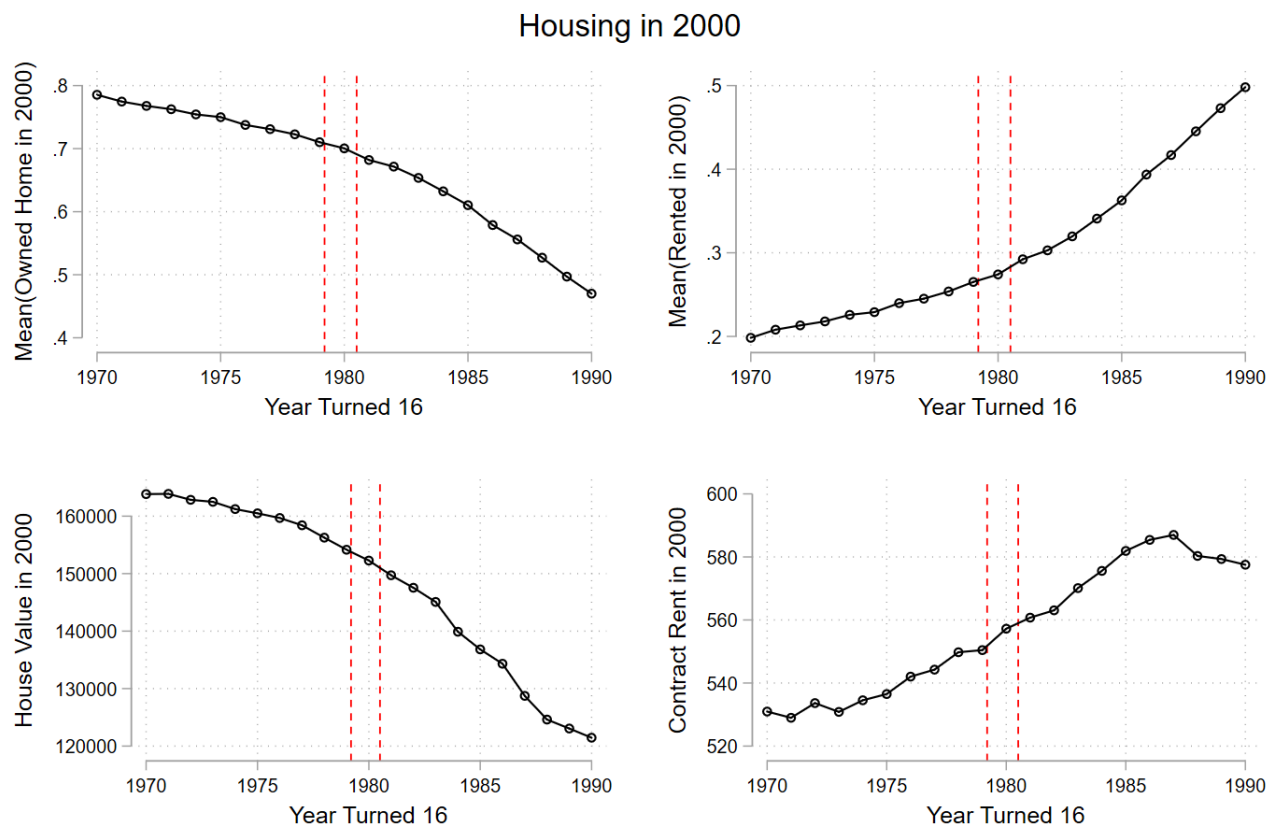


Figure A.3: Housing characteristics in 2000 by year turned 16.

Table A.1: Discontinuity in turning 16 after 1980 on transportation behavior in 2000 – Donut RD omitting those who turn 16 in 1980.

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

Regression discontinuity estimates of the effect of turning 16 after 1980 or later on a binary indicator of whether the respondent drove to work as reported in the 2000 Census. Bandwidth is symmetric around 1980, but excludes 1980 (e.g., a bandwidth of two includes 1978, 1979, 1981, and 1982). The 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. ⁺ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.