Online Appendix

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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 (Ruggles et al. 2020).

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.
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> 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.
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> 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.

Occasionally, we also make use of state aggregate population data. Whenever we use such data, it is from the National Historical Geographic Information System (NHGIS) (Manson et al. 2020). We also draw use state FIPS codes from official government sources

United States Department of Agriculture: Natural Resource Conservation Service (2018).

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 (Federal Highway Administration 1990-2017). 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

For gasoline price data used for quantitative analysis in Section 4 and 5, we draw on data Erich Muehlegger shared with us. This data in turn draws on from Energy Information Administration data, which reports nominal tax-inclusive state-level gasoline price data starting in 1983 (Energy Information Administration 1984-2017). For the years 1966-1982, the data are from the *Highway Statistics* annuals (Li, Linn, and Muehlegger 2014; Small and van Dender 2007). The data adjust for taxes, drawing on Bickley (2012) for federal taxes and two sources for state taxes, Office of Highway Policy Information (1966-2016) in earlier years and Tax Policy Center (2020) in more recent years.

For Figure 1, we draw on the series 'Consumer Price Index for All Urban Wage Earners and Clerical Workers: Gasoline' (Bureau of Labor Statistics) and deflate by 'Consumer Price Index for All Urban Consumers: All Items Less Food andEnergy in U.S. City Average' (Bureau of Labor Statistics). The real series is rescaled to $2017.

Driver License Regulation Data

We use data on minimum driver licensing requirements from the *Minimum Driving Age Database* (Severen 2020). The primary sources of that data are (i) the Federal Highway Administration (FHWA) *Driver License Administration Requirements and Fees* booklets and (ii) the Insurance Institute for Highway Safety (IIHS) database on graduated driver license (GDL) programs (Office of Highway Information Management 1967-2000; Insurance Institute for Highway Safety 2018). That data is supplemented with various newspaper articles, academic articles, legal database queries, and inquiries to reference desks at state libraries. The FHWA booklets have been published roughly biannually since the 1960s; the *Minimum Driving Age Database* primarily uses FHWA data between 1967 and 1996. IIHS data cover 1995 to 2017 and report some information starting in 1990. The database combines these data sources and fills in gaps in reporting.

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 (Surveillance, Epidemiology, and End Results Program 2018). 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. To crosswalk coarse NHTS vehicle descriptions with specific EPA efficiency measures, we create our own crosswalk. See Severen and van Benthem (2021) for details.

Unemployment Data

For the mediation analysis, we use state-level annual unemployment rates. The data comes from several sources. For 1976 on, we average seasonally adjusted unemployment rates over each year, from the Bureau of Labor Statistics' Local Area Unemployment Statistics (Bureau of Labor Statistics 1976-2010). From 1965-67, we draw on Table D-4 in the *Manpower Report of the President* (United States Department of Labor 1968), while for 1967-75 we draw from the *Statistical Abstracts of the United States* (United States Census Bureau 1968-1975).

A.2 Detailed Results and Additional Specifications

Event Study

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.[1] Data are limited to a symmetric bandwidth around the treatment year.

Panel A of Appendix Table A.3 presents event-study 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' discontinuity tests omitting 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 Appendix 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

[1] 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.

influenced by graduating from high school during a recession and if income influences vehicle purchasing (see Section 5.1 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 event-study 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 effect for commuters who reside in the 'principal city' of an MSA.[2] 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

[2]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.

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 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 event-study 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 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).[3] 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):

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

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.

[3]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.

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).[4] 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.

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.[5]

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 indi-

[4]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.

[5]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.

cates 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.[6] 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 possibility of getting trapped at a local (non-global) minimum, we follow the routine below to estimate Equation (6):

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

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

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

4. Use the fixed-effects estimates from Step 1 for starting values of the fixed effects, the sum of α from Step 1 and $\tilde{\alpha}$ from Step 3 as the starting value for α, and the values of (β, ω) from Step 3 as the starting values for (β, ω), and minimize Equation (6) along all parameter values jointly.

[6] The use of Mata is 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 at ages $k \leq 14$. 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.

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.

A.5 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.[7]

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 to driver license age in Section 4.[8] The other measure, minimum intermediate license age, captures the minimum age at which drivers can make unaccompanied trips but with some restrictions.[9] Summary information on these measures is shown in Appendix Table A.1. We include both measures of restrictiveness in each specification.

Columns (1)-(4) in Table A.17 show the effects from models with sample year, state, and age fixed effects. Columns (1) and (2) include the minimum full-privilige driving age and the minimum intermediate license age, respectively. Columns (3) and (4) include both ages. Column (4) includes the full sample for the census data, which is otherwise restricted to the *stayers* sample. Column (5) adds the respective demographic controls as in Table 1 and 3. Column (6) adds income controls, while Column (7) adds state-by-sample year fixed effects, and Column (8) includes a quadratic trend in birth year. We find little evidence of a long-run effect of these regulations on later-life driving behavior. Estimates in Table A.17 show the effect of a one-year increase in either measure of minimum driving

[7]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.

[8]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.

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

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.[10]

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.

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

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A.6 Appendix Figures and Tables

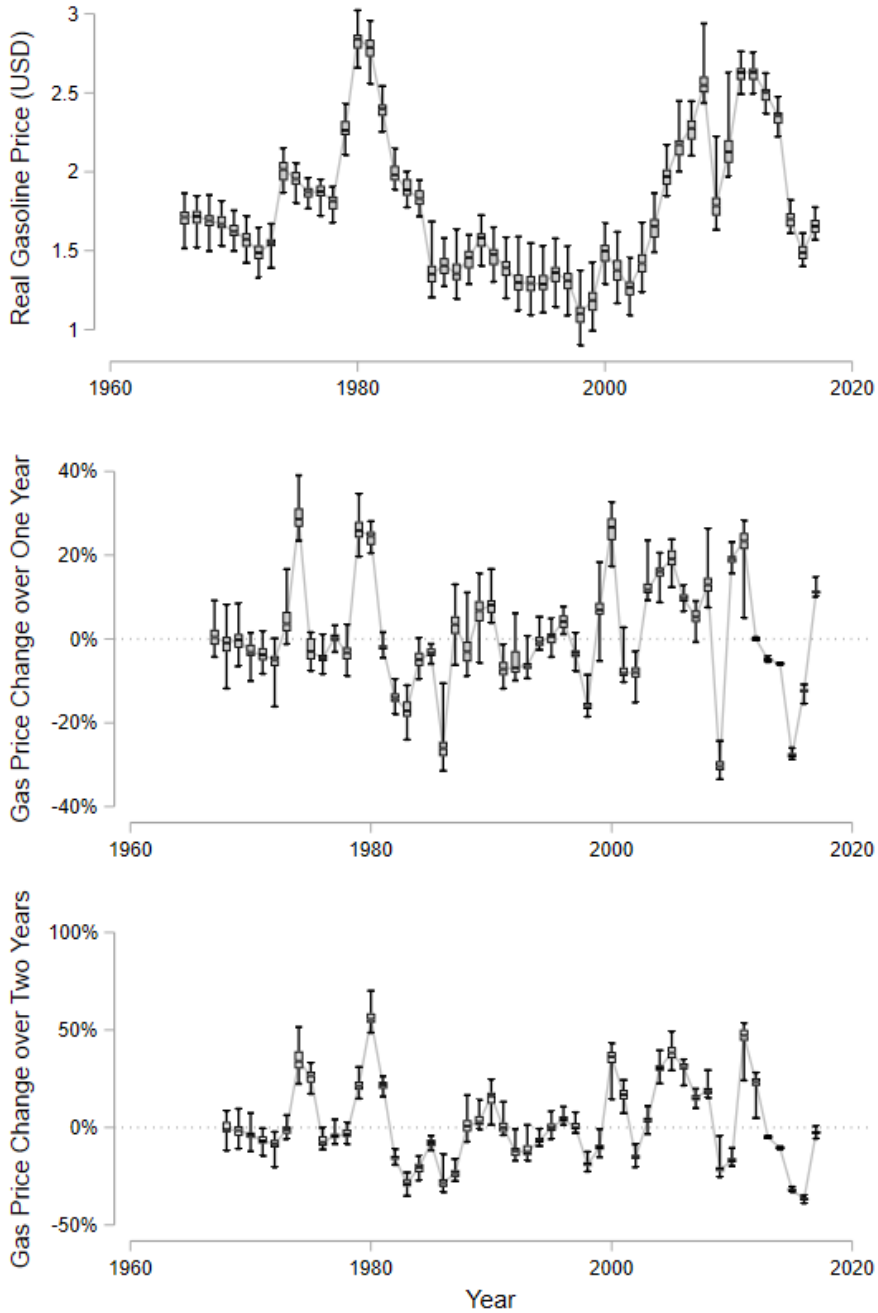

Figure A.1: Box plots of state gas prices and 1- and 2-year percentage changes; minimum, maximum and quartiles.

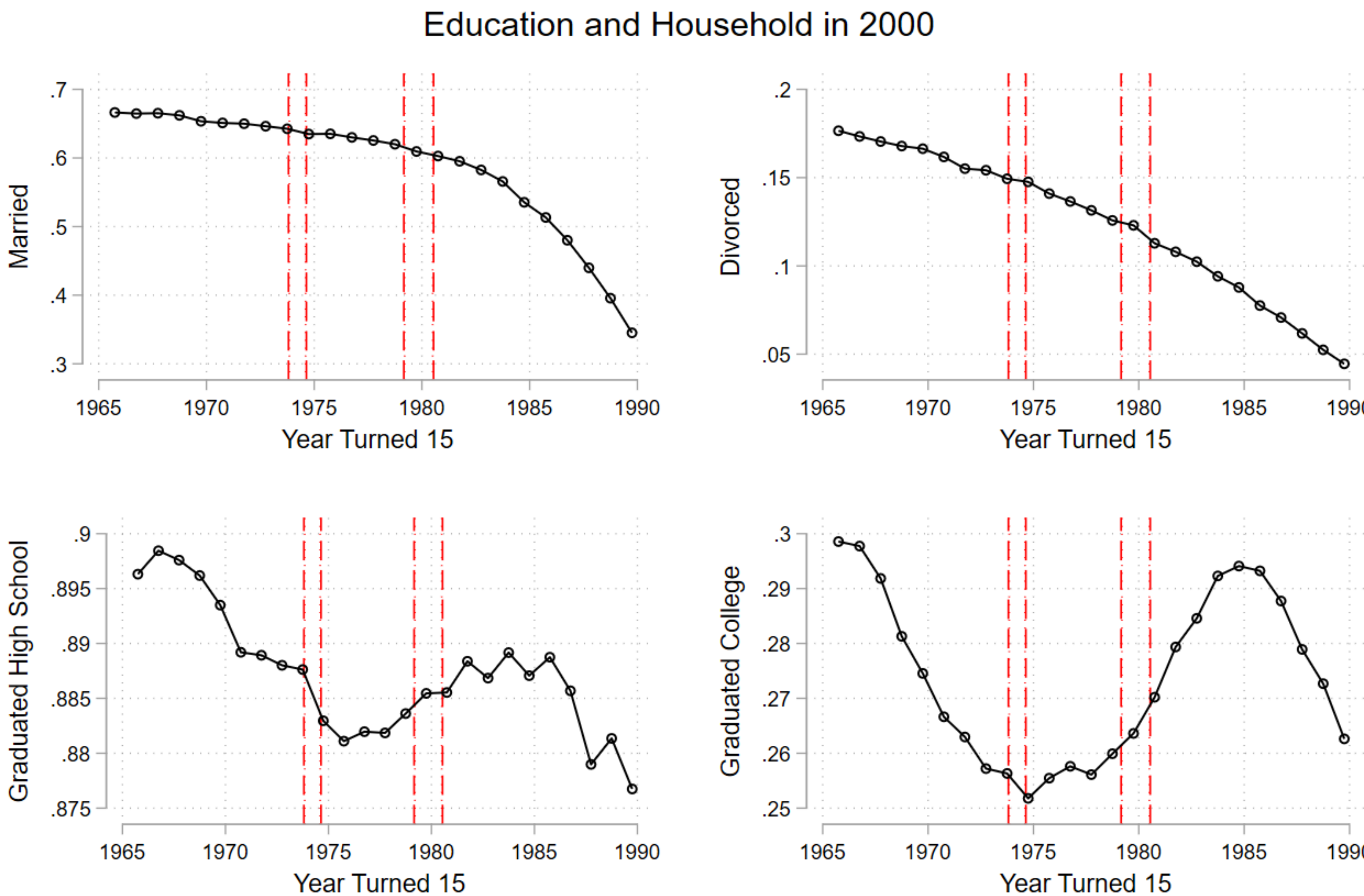

Figure A.2: Demographic characteristics in 2000 by year turned 15.

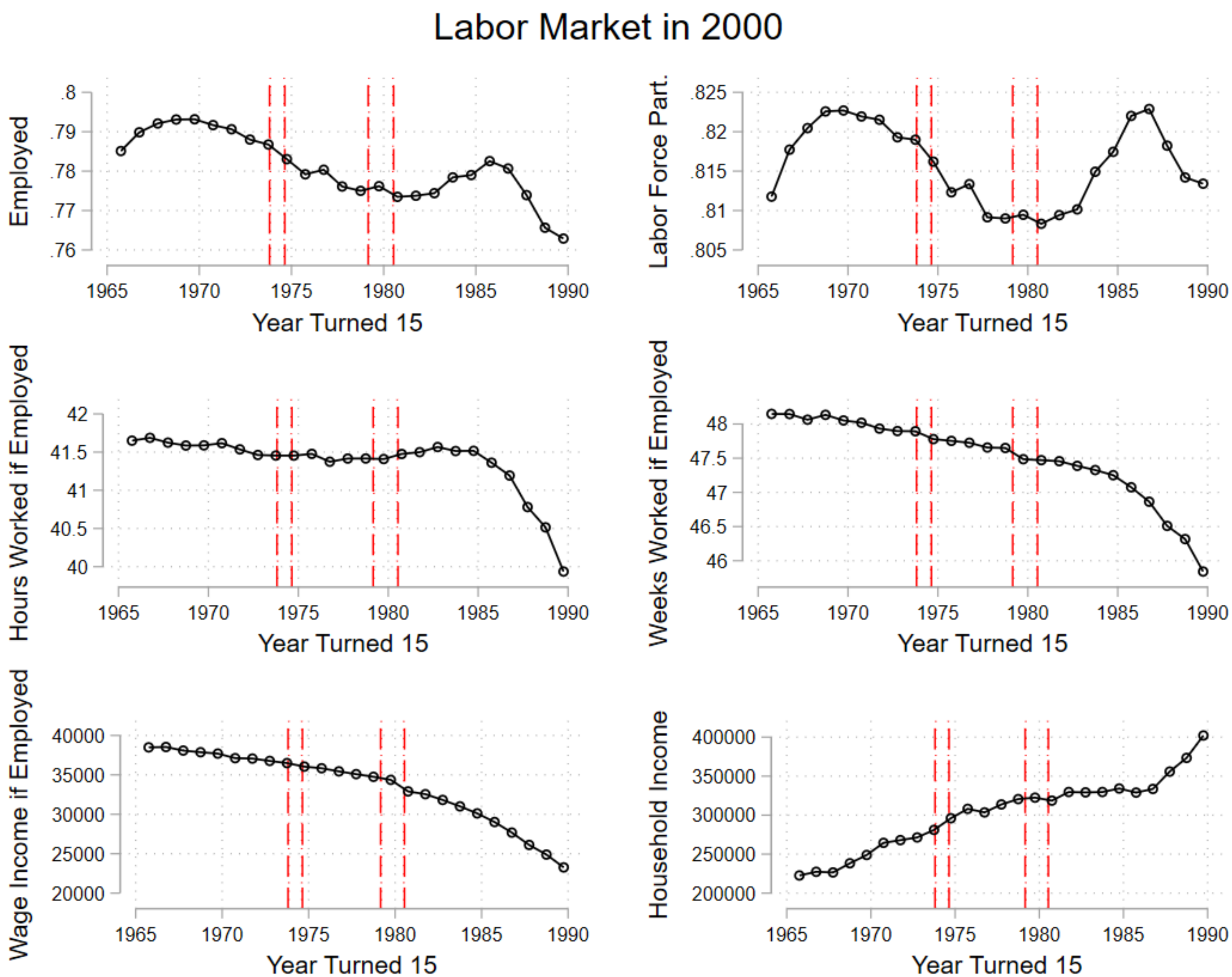

Figure A.3: Labor market characteristics in 2000 by year turned 15.

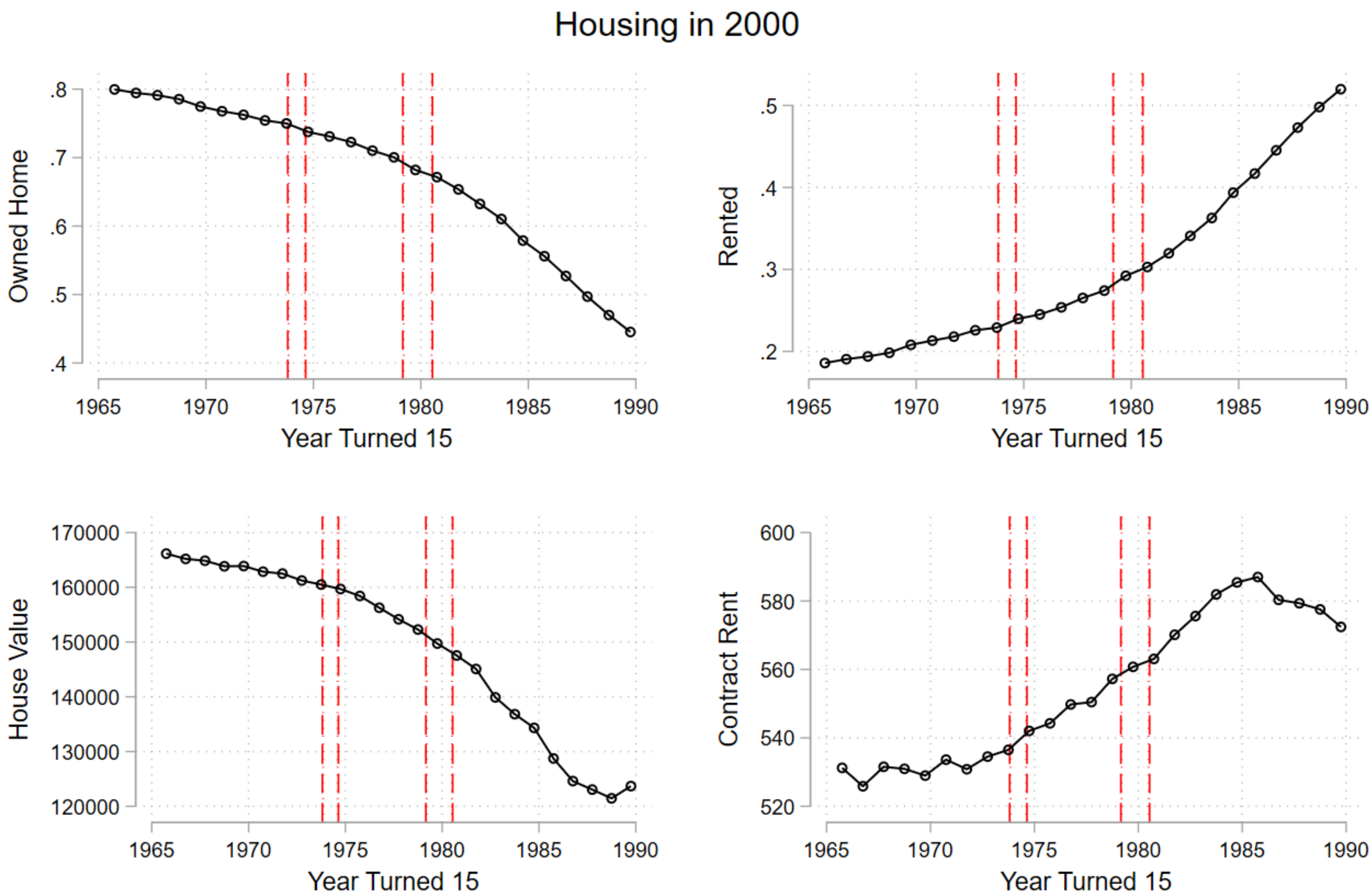

Figure A.4: Housing characteristics in 2000 by year turned 15.

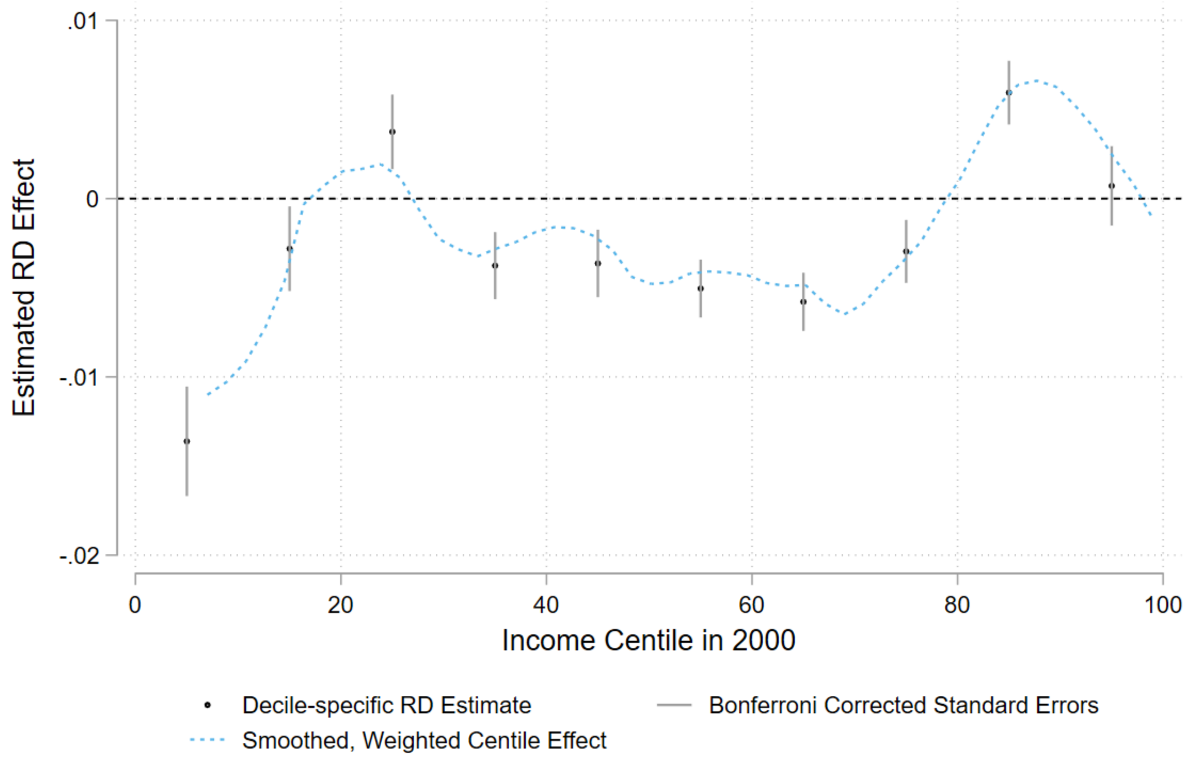

Figure A.5: Event study estimates of the 1979-80 gas price shock on driving in 2000 by income decile and (smoothed) centile. All coefficients estimates using a five-year bandwidth and linear trends. Decile estimates shown as dots with Bonferroni-corrected 95 percent confidence intervals represented by 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 $)	86,157	81,614	87,982
1[in state of birth]	.638	-	-
N	19,052,577	12,201,920	9,330,029
NHTS Sample			
VMT	9,854	-	-
VMT$\vert$(VMT>0)	14,318	-	-
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 in turning 15 after 1979 on commuting behavior in 2000.

Model	Poly. order	Bandwidth (years) 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

Event study 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 1). 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 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

Event study 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 1). $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

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

Model	Poly. order	Bandwidth (years) 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

Event study 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 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

Event study 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
$P_{cs}^{\Delta (m_{cs}+1, m_{cs}-1)}$	0.023	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)
$P_{cs}^{\Delta(18,16)}$	-0.0804** (0.0237)	-0.0846*** (0.0239)	-0.0705** (0.0246)	-0.0716** (0.0249)	-0.0521* (0.0228)
$P_{cs}^{\Delta(18,17)}$	-0.0948* (0.0378)	-0.1014* (0.0384)	-0.0797+ (0.0407)	-0.0777+ (0.0410)	-0.0480 (0.0372)
$P_{cs}^{\Delta(17,16)}$	-0.1123** (0.0406)	-0.1167** (0.0407)	-0.1054* (0.0410)	-0.1093* (0.0408)	-0.0832* (0.0400)
$P_{cs}^{\Delta(16,15)}$	-0.0923* (0.0433)	-0.0969* (0.0420)	-0.0926* (0.0413)	-0.0895* (0.0404)	-0.0734+ (0.0406)
$P_{cs}^{\Delta(m_{cs}+2,m_{cs})}$	-0.0521* (0.0230)	-0.0534* (0.0233)	-0.0400 (0.0244)	-0.0403 (0.0249)	-0.0209 (0.0230)
$P_{cs}^{\Delta(m_{cs}+2,m_{cs}+1)}$	-0.0683+ (0.0348)	-0.0696+ (0.0353)	-0.0568 (0.0396)	-0.0549 (0.0399)	-0.0237 (0.0365)
$P_{cs}^{\Delta(m_{cs}+1,m_{cs})}$	-0.0603+ (0.0347)	-0.0626+ (0.0361)	-0.0447 (0.0354)	-0.0470 (0.0360)	-0.0264 (0.0351)
$P_{cs}^{\Delta(m_{cs},m_{cs}-1)}$	-0.0572 (0.0353)	-0.0707* (0.0341)	-0.0664+ (0.0353)	-0.0658+ (0.0348)	-0.0498 (0.0350)
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				
$P_{cs}^{\Delta(m_{cs}+2,m_{cs})}$	0.0370	0.0440	0.0589+	0.0610+
	(0.0345)	(0.0348)	(0.0334)	(0.0327)
$P_{cs}^{\Delta(m_{cs}+1,m_{cs}-1)}$	0.0512	0.0421	0.0425	0.0410
	(0.0400)	(0.0397)	(0.0373)	(0.0359)
1-year price change				
$P_{cs}^{\Delta(m_{cs}+2,m_{cs}+1)}$	0.0077	0.0157	0.0328	0.0354
	(0.0408)	(0.0414)	(0.0445)	(0.0438)
$P_{cs}^{\Delta(m_{cs}+1,m_{cs})}$	0.0628	0.0664	0.0760	0.0774
	(0.0544)	(0.0550)	(0.0537)	(0.0514)
$P_{cs}^{\Delta(m_{cs},m_{cs}-1)}$	0.0382	0.0175	0.0039	0.0012
	(0.0491)	(0.0456)	(0.0440)	(0.0422)
Levels				
$P_{cs}^{m_{cs}}$	0.0112	0.0032	-0.0052	-0.0109
	(0.0331)	(0.0320)	(0.0326)	(0.0316)
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	Average GPM	GPM	GPM	Any Big	Any Big	1[Big]	1[Big]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$P_{cs}^{\Delta(18,16)}$	-0.0000	-0.0002	-0.0001	-0.0001	-0.0269**	-0.0251*	-0.0197*	-0.0199*
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0096)	(0.0102)	(0.0093)	(0.0098)
$P_{cs}^{\Delta(17,15)}$	0.0000	-0.0002	-0.0002	-0.0003	-0.0213+	-0.0176	-0.0154	-0.0142
	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0112)	(0.0113)	(0.0106)	(0.0103)
$P_{cs}^{\Delta(m_{cs}+2,m_{cs})}$	0.0000	0.0001	-0.0001	-0.0000	-0.0209*	-0.0173*	-0.0146	-0.0113
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0090)	(0.0086)	(0.0095)	(0.0085)
$P_{cs}^{\Delta(m_{cs}+1,m_{cs}-1)}$	-0.0002	-0.0004	-0.0003	-0.0004	-0.0241+	-0.0213+	-0.0195	-0.0182
	(0.0003)	(0.0003)	(0.0003)	(0.0002)	(0.0126)	(0.0124)	(0.0117)	(0.0115)
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))										
$P_{cs}^{\Delta(a,a-1)}$	-0.0553	0.0255	0.0206	-0.0923*	-0.1123**	-0.0948*	-0.0404	0.0082	-0.0249	-0.0191
	(0.0500)	(0.0377)	(0.0402)	(0.0433)	(0.0406)	(0.0378)	(0.0424)	(0.0387)	(0.0417)	(0.0371)
$P_{cs}^{\Delta(m_{cs}+\tau,m_{cs}+\tau-1)}$	-0.0581	-0.0127	-0.0187	-0.0572	-0.0603+	-0.0683+	-0.0614	-0.0070	-0.0211	0.0187
	(0.0381)	(0.0430)	(0.0443)	(0.0353)	(0.0347)	(0.0348)	(0.0404)	(0.0384)	(0.0383)	(0.0412)

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))										
$P_{cs}^{\Delta a,(a-2)}$	-0.0242	-0.0156	0.0215	-0.0277	-0.0776**	-0.0804**	-0.0531*	-0.0126	-0.0086	-0.0180
	(0.0315)	(0.0217)	(0.0265)	(0.0262)	(0.0267)	(0.0237)	(0.0216)	(0.0278)	(0.0235)	(0.0236)
$P_{cs}^{\Delta(m_{cs}+\tau,m_{cs}+\tau-2)}$	-0.0312	-0.0299	-0.0092	-0.0288	-0.0483*	-0.0521*	-0.0524*	-0.0270	-0.0142	0.0008
	(0.0235)	(0.0204)	(0.0279)	(0.0257)	(0.0194)	(0.0230)	(0.0227)	(0.0261)	(0.0239)	(0.0231)

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**	-0.0057***	-0.0869*	-0.0527
	(0.0018)	(0.0014)	(0.0431)	(0.0419)
1[35-44]	-0.0001	0.0002	-0.0527	-0.0325
	(0.0014)	(0.0013)	(0.0580)	(0.0525)
1[45-54]	-0.0050***	-0.0059***	-0.0928+	-0.1114*
	(0.0014)	(0.0014)	(0.0516)	(0.0497)
$P_{cs}^{\Delta(m_{cs}+1, m_{cs}-1)} \times$				
1[25-34]	-0.0031*	-0.0040*	-0.0431	-0.0245
	(0.0015)	(0.0014)	(0.0336)	(0.0319)
1[35-44]	-0.0038*	-0.0021	-0.0579	-0.0564
	(0.0019)	(0.0015)	(0.0478)	(0.0474)
1[45-54]	-0.0056**	-0.0069**	-0.0449	-0.0409
	(0.0019)	(0.0021)	(0.0427)	(0.0426)
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.

Table A.17: Do youth driving restrictions affect later driving behavior?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Extensive margin (1[drive])								
Minimum Full-Privilege Age	0.0074		0.0078	0.0048	0.0071	0.0072	0.0082+	0.0092
	(0.0051)		(0.0052)	(0.0040)	(0.0047)	(0.0048)	(0.0048)	(0.0056)
Minimum Intermediate License Age		-0.0079	-0.0107	-0.0088	-0.0091	-0.0097	-0.0137	-0.0124
		(0.0131)	(0.0147)	(0.0122)	(0.0136)	(0.0138)	(0.0127)	(0.0121)
F-test on joint sig.			1.153	0.727	1.197	1.169	1.470	1.365
			[0.324]	[0.489]	[0.310]	[0.319]	[0.240]	[0.265]
Panel B: Intensive margin (ln(person VMT))								
Minimum Full-Privilege Age	-0.0010		0.0011		0.0008	-0.0028	-0.0107	0.0198
	(0.0114)		(0.0128)		(0.0131)	(0.0159)	(0.0180)	(0.0142)
Minimum Intermediate License Age		-0.0258	-0.0268		-0.0235	-0.0266	-0.0004	0.0242
		(0.0618)	(0.0652)		(0.0567)	(0.0594)	(0.0701)	(0.0589)
F-test on joint sig.			0.089		0.095	0.157	0.187	1.151
			[0.915]		[0.910]	[0.856]	[0.830]	[0.325]
Sample year FEs	Y	Y	Y	Y	Y	Y	-	-
State FEs	Y	Y	Y	Y	Y	Y	-	-
Age FEs	Y	Y	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	Stay	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, and the p-value of the F-test of joint signficance is shown in []. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.18: Results excluding oil crisis cohorts.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Extensive margin (1[drive])								
$P_{cs}^{\Delta 17,15}$	-0.0038***		-0.0040***		-0.0039***		-0.0041*	
	(0.0010)		(0.0009)		(0.0009)		(0.0016)	
$P_{cs}^{\Delta(m_{cs}+1,m_{cs}-1)}$		-0.0042***		-0.0040***		-0.0041***		-0.0034*
		(0.0010)		(0.0008)		(0.0008)		(0.0014)
Panel B: Intensive margin (ln(person VMT))								
$P_{cs}^{\Delta 17,15}$	-0.0761**		-0.0706*		-0.0683*		-0.1055*	
	(0.0262)		(0.0265)		(0.0257)		(0.0442)	
$P_{cs}^{\Delta(m_{cs}+1,m_{cs}-1)}$		-0.0495*		-0.0424*		-0.0426*		-0.0378
		(0.0192)		(0.0183)		(0.0181)		(0.0279)
Sample year FEs	Y	Y	Y	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y	Y	Y	Y
Age FEs	Y	Y	Y	Y	Y	Y	Y	Y
Excluded year(s), age 15	1974/75	1974/75	1979/80	1979/80	1974/75 & 1979/80	1974/75 & 1979/80	1973/74–1980/81	1973/74–1980/81

Each panel and column contains the results from a different regression, for sixteen total. Dependent variable in first panel is a binary indicator of whether the respondent drove to work. Dependent variable in second panel is log person VMT. Observations weighted by person sample weights. Excluded year(s) refers to the cohort(s) by the year they turned 16: e.g., the 1979/80 cohort reported age 35 in the 2000 census. Standard errors clustered by state. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table A.19: Multiple gas-price treatments and national-level variation.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Extensive margin (1[drive])						
$P_{cs}^{\Delta 17,15}$	-0.0037***	-0.0041***				
	(0.0011)	(0.0010)				
P_{cs}^{16}	-0.0005	-0.0009				
	(0.0010)	(0.0008)				
$\bar{P}_{c}^{\Delta 17,15}$ (national)			-0.0040***	-0.0045***		
			(0.0010)	(0.0009)		
$\bar{P}_{c}^{16}$ (national)					-0.0008	-0.0011
					(0.0009)	(0.0007)
Panel B: Intensive margin (ln(person VMT))						
$P_{cs}^{\Delta 17,15}$	-0.0841**	-0.0648*				
	(0.0276)	(0.0268)				
P_{cs}^{16}	0.0261*	0.0092				
	(0.0112)	(0.0102)				
$\bar{P}_{c}^{\Delta 17,15}$ (national)			-0.0775**	-0.0591*		
			(0.0260)	(0.0257)		
$\bar{P}_{c}^{16}$ (national)					0.0208+	0.0027
					(0.0109)	(0.0101)
Sample year FEs	Y	-	Y	-	Y	-
State FEs	Y	-	Y	-	Y	-
Age FEs	Y	Y	Y	Y	Y	Y
Dem. controls	-	Y	-	Y	-	Y
Income controls	-	Y	-	Y	-	Y
State-X-year FEs	-	Y	-	Y	-	Y
Quad. birth year	-	Y	-	Y	-	Y

Each panel and column contains the results from a different regression, for twelve 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.

Table A.20: Alternate standard errors.

	(1)	(2)	(3)	(4)
Panel A: Extensive margin (1[drive])				
$P_{cs}^{\Delta 17,15}$	-0.0038	-0.0043		
	(0.0010)***	(0.0009)***		
	[0.0008]***	[0.0008]***		
	{.}	{0.0007}***		
P_{cs}^{16}			-0.0007	-0.0011
			(0.0010)	(0.0008)
			[0.0006]	[0.0006]+
			{.}	{0.0006}+
Panel B: Intensive margin (ln(person VMT))				
$P_{cs}^{\Delta 17,15}$	-0.0776	-0.0613		
	(0.0267)**	(0.0256)*		
	[0.0234]**	[.]		
	{0.0253}**	{0.0192}**		
P_{cs}^{16}			0.0216	0.0034
			(0.0108)+	(0.0096)
			[0.0113]+	[.]
			{0.0106}*	{0.0091}
Sample year FEs	Y	-	Y	-
State FEs	Y	-	Y	-
Age FEs	Y	Y	Y	Y
Dem. controls	-	Y	-	Y
Income controls	-	Y	-	Y
State-X-year FEs	-	Y	-	Y
Quad. birth year	-	Y	-	Y

Each panel and column contains the results from a different regression, for eight 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 in (), clustered by cohort in [], and two-way clustered by both state and cohort in {} (a . indicates that the standard errors could not be computed using `reghdfe` in Stata). + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.