Stimulating Auto Markets*

David Berger
Duke and NBER
david.berger@duke.edu†

Geoffrey Gee
U.S. Treasury
Geoffrey.Gee@treasury.gov

Nick Turner
Federal Reserve Board
nick.turner@frb.gov

Eric Zwick
Chicago Booth and NBER
ezwick@chicagobooth.edu

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Abstract

This paper evaluates how unprecedented pandemic fiscal stimulus affected household car purchases and inflation. Using administrative data on vehicle registrations and nearly $900 billion in stimulus, we exploit the precise timing of payments and geographic variation in exposure to identify the effect on car purchases. We find stimulus payments increased purchases by approximately 4%, amounting to 5 million additional vehicles sold during 2020–2022 relative to a no-stimulus counterfactual. Despite this aggregate effect, fiscal transfers alone explain only modest increases in car prices. Using a general equilibrium model with new and used car markets, we show the importance of secondary markets, where fiscal transfers simultaneously boosted demand and supply: stimulus recipients often purchased new vehicles and traded in their old ones, expanding the supply of used cars and dampening price increases. These results remain robust under temporary new car supply constraints and substantial household savings from other pandemic programs. An interaction between temporary supply constraints and shifting consumer preferences toward goods from services is necessary to generate auto price dynamics that match the data.

Preliminary; Comments welcome

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†Corresponding author.

The fiscal stimulus enacted by U.S. policymakers during the COVID-19 pandemic was unprecedented in both its nature and scale, totaling roughly $5 trillion in response to widespread job losses, rapid economic decline, and significant uncertainty about the virus's progression. Starting in early 2020, these expansive countercyclical measures included historic levels of support aimed at households ($1.8 trillion), businesses ($1.7 trillion), and state and local governments ($745 billion). At the heart of household support were three rounds of economic impact payments (EIP), or "stimulus checks," which represented largely unconditional direct transfers to individuals. Understanding how these payments affected household consumption patterns and aggregate inflation dynamics is essential, both for evaluating immediate policy outcomes and informing future fiscal policy design.[1]

In this paper, we analyze two questions. First, what was the impact of pandemic-era fiscal stimulus on consumption? Addressing this question is essential not only for evaluating policy effectiveness but also, more broadly, for informing the optimal design of household-targeted fiscal policy. Specifically, it helps policymakers determine the appropriate size of stimulus measures and assess the efficacy of unconditional versus conditional transfers, such as more targeted tax credits, sector-specific subsidies, or temporary scrappage programs.

Second, we ask what role these transfers played in driving inflation, particularly in sectors that disproportionately contributed to the price surge as the economy recovered. Understanding this relationship is crucial because, if transfers primarily fuel inflation rather than consumption growth, they effectively shift from being stimulative to being redistributive. Given limited existing knowledge about inflation dynamics, clarifying this issue is critical for evaluating the usefulness of expansionary fiscal policy.

We take a granular approach to answering these questions and focus on the automobile market (Figure 1, Panel A). This market offers a particularly relevant setting because of its disproportionate role in recent inflation. Car prices experienced some of the highest inflation rates during the pandemic, with prices rising approximately 50 percent—significantly outpacing overall inflation (Figure 1, Panel B). Autos alone substantially contributed to aggregate inflation, boosting overall price increases far in excess of their weight in the consumption basket. Another reason to study auto markets is its high sensitivity to fiscal and monetary policy.

[1] Armantier, Goldman, Koşar, Lu, Pomerantz, Van der Klaauw et al. (2020), Chetty, Friedman and Stepner (2021), Coibion, Gorodnichenko and Weber (2020), Cox, Jacoby and Marr (2022), Baker, Farrokhnia, Meyer, Pagel and Yannelis (2023), and Parker, Schild, Erhard and Johnson (2022) find evidence that the initial round of pandemic-era stimulus checks boosted spending significantly, particularly among lower-income households and those experiencing income losses. Survey data indicate most spending targeted essential and non-durable goods (Armantier, Goldman, Koşar and Van der Klaauw, 2021), while impacts on durable goods purchases remain less clear (Parker, Schild, Erhard and Johnson, 2022).

For example, Orchard, Ramey and Wieland (2025) highlight that the marginal propensity to consume (MPC) from fiscal transfers is heavily driven by auto-related spending. Thus, an in-depth analysis of the auto sector allows for a detailed exploration of consumption and inflation dynamics in response to stimulus payments.

We start by quantifying the causal relationship between stimulus payments and durable goods purchases during the pandemic. This exercise presents unique challenges, as it requires both high-quality consumption data and random variation in household transfers large enough to influence durable goods purchases. The pandemic further complicates this analysis due to the distinctive characteristics of a health-induced economic slowdown, including mandated lockdowns and widespread fear of virus transmission. To address these challenges, we leverage administrative data on automobile sales derived from state vehicle registrations, coupled with administrative data on household transfers totaling nearly $900 billion, providing $10,000 or more to many households. Our primary research design exploits cross-sectional differences across narrow geographic areas based on pre-pandemic exposure to these household transfers by ZIP code and month. This strategy and our data enable us to analyze the high-frequency dynamics of car purchases around each round of transfer payments.

Our main finding is that stimulus transfers increased used auto sales by just over 4 percent compared to pre-pandemic levels, amounting to approximately 5 million additional units sold between 2020 and 2022. These sales responses occurred quickly after payments, remaining elevated for several months without subsequent reversals, suggesting either pulled-forward demand or purchases that otherwise would not have occurred. We emphasize these aggregate calculations do not immediately deliver a macroeconomic counterfactual because our research design absorbs other macroeconomic shocks in time fixed effects. We tackle the challenge of aggregate market dynamics in our quantitative model.

Combining our aggregate quantity estimates with total stimulus payments, our aggregate estimate corresponds to $135 billion of spending. Compared to the total value of transfer payments, we estimate a 3-year MPX on used autos of 0.13. The analogous calculation for new autos gives an MPX of 0.09, and the total auto MPX is 0.22. Following the approach in Laibson, Maxted and Moll (2022), the MPC for all consumption implied by these MPXs is 0.38.

We address potential concerns about confounding factors in several ways. First, we include CBSA-by-month fixed effects, ensuring our identification arises from differences across ZIP codes within the same CBSA-month combination. These fixed effects account for pandemic-related factors common within each CBSA-month, such as COVID caseloads, vaccine rollouts, and local lockdowns. Second, we directly control for time-varying COVID-related outcomes,

including cases, deaths, and vaccinations, as well as county-level GPS data capturing mobility patterns, such as time spent away from home and at work. Third, we demonstrate that confounding factors—COVID outcomes, mobility patterns, and local unemployment rates—are largely uncorrelated with our ex-ante exposure measure and show similar evolution patterns across high- and low-exposure areas. Lastly, our results hold for transfers made during later pandemic stages, when approximately 70 percent of transfers occurred and health concerns were less likely to influence consumer behavior.

We then turn to understanding the impact of fiscal stimulus on auto prices. A cross-sectional empirical approach to prices is problematic because cars are traded nationally, meaning local stimulus payments would not necessarily generate distinct local price variation. While registrations clearly indicate where a vehicle is purchased and registered, the car itself can originate from any region, weakening potential local price responses. Therefore, we use multiple models to investigate price dynamics. First, we employ a simple partial equilibrium framework that explicitly maps our empirical differences-in-differences estimates into implied price effects. This model emphasizes the critical roles of supply and demand elasticities, suggesting that, even under conservative assumptions such as an extremely low supply elasticity and a reasonable elasticity of demand, auto price increases consistent our estimated quantity effect remain modest. Across a range of assumptions, we can explain less than 10 percent of the price runup.

We then model auto market dynamics more seriously to assess whether this intuition remains true. We adopt a small open economy (SOE) version of the model in Gavazza and Lanteri (2021), which integrates new and used car markets within a general equilibrium framework. In our baseline scenario, we calibrate a stimulus-like income shock and assume perfectly elastic new car supply, which is necessary to match the empirically observed 4% increase in used auto sales we observe in the data. Interestingly, under this scenario, we find that used auto prices actually decline. The intuition is driven by the introduction of a secondary market: fiscal stimulus simultaneously increases demand for used vehicles and expands their supply. Households receiving transfers often purchase new cars, trading in their existing vehicles and thus raising the inventory of used cars. This dual demand-and-supply response significantly dampens stimulus-induced price pressure.

We explore the robustness of these findings by relaxing our baseline assumption regarding supply elasticity. During the pandemic, supply constraints—particularly silicon chip shortages and shipping channels—likely rendered new car supply highly inelastic, potentially amplifying the price response in used car markets as demand spilled over from the new car market. How-

ever, we find that simulations incorporating an initially highly inelastic new car supply yield only minimal price increases (around 1%), far below the price surge observed in the data. Even introducing a substantial wealth shock to capture excess pandemic savings only increases used car prices by 4%. We conclude that fiscal transfers alone are insufficient to be the first-order driver of the price surge.

If not fiscal policy, what combination of shocks can produce the substantial inflation we observe during the pandemic? We next introduce additional pandemic-related shocks that might influence the observed price dynamics in auto markets. First, we model a temporary relaxation of borrowing constraints, reflecting historically low interest rates and exceptionally easy credit conditions. Second, we consider a temporary shift in consumer expenditure shares toward durable goods—especially automobiles—and away from services, consistent with consumption patterns documented by Cox, Ganong, Noel, Vavra, Wong, Farrell and Greig (2020).

These additional shocks significantly amplify auto price responses. New and used auto prices rise by nearly 20 percent under these conditions, with used cars experiencing slightly larger gains. Temporary supply constraints play a crucial role as well—without these constraints, used car prices would have declined as households shift to new cars to satisfy the preference shift. Thus, our analysis suggests that the observed auto price inflation during the pandemic can be largely explained by the combined effects of relaxed credit conditions, shifts in consumer spending towards durable goods, and temporary supply disruptions. Pandemic-era fiscal transfers alone add to the inflationary pressure for sure, but do not appear central in explaining car price inflation.

Related Literature. Our paper relates to three distinct strands of literature. First, we engage with a growing body of research on the effects of fiscal stimulus on durable goods markets, particularly automobiles and housing. Numerous studies have examined the "Cash for Clunkers" program, a targeted vehicle trade-in initiative. For instance, Mian and Sufi (2012) find that the program primarily pulled forward automobile purchases rather than generating sustained additional demand, thereby limiting its stimulative potential. Consistent with this, Hoekstra, Puller and West (2017), along with Green, Melzer, Parker and Rojas (2020), identify only modest long-term impacts of such policies on overall car sales. In comparison, research examining housing subsidies, such as the First-time Homebuyer Tax Credit, indicates greater effectiveness in stabilizing housing markets and increasing sales (Best and Kleven, 2017; Berger, Turner and Zwick, 2020). At a theoretical level, macroeconomic models by Kaplan and Violante (2014), McKay and Wieland (2021), Beraja and Zorzi (2024), and Berger, Cui, Turner

and Zwick (2024) provide frameworks to understand how pandemic-era stimulus programs influence durable goods consumption.

Second, a related area of research investigates the determinants of marginal propensities to consume (MPCs) and marginal propensities to spend (MPXs). Empirical studies indicate that pandemic-era fiscal transfers generated high MPCs, particularly from unemployment insurance (UI) payments (Ganong, Greig, Noel, Sullivan and Vavra, 2024). The literature consistently finds that large, one-time transfers strongly induce purchases of durable goods, a result highlighted by Adams, Einav and Levin (2009), Parker, Souleles, Johnson and McClelland (2013), and Fuster, Kaplan and Zafar (2021). Moreover, liquidity constraints significantly influence MPC magnitudes, with less-liquid households typically displaying higher consumption responses to fiscal transfers. Laibson, Maxted and Moll (2022) contribute further nuance by differentiating between the MPC and MPX for durable goods, a framework we use to interpret our results.

Finally, our paper contributes to the literature examining the inflationary consequences of fiscal transfers. Jordà, Liu, Nechio and Rivera-Reyes (2022) offer macro-empirical evidence indicating that fiscal transfers contributed roughly three percentage points to core inflation in late 2021, demonstrating how substantial fiscal interventions interact with supply disruptions and heightened demand pressures. Havlin (2023) analyzes the role of auto dealers in shaping new car prices, highlighting the importance of supply-side constraints and dealer markups in driving inflationary pressures. Our study extends this literature by offering direct quasi-experimental evidence of the impact of fiscal transfers on auto spending, analyzed through a microfounded model of the auto market.

1 Policy Background

During the COVID pandemic and early recovery, policymakers enacted stimulus via three rounds of economic impact payments (EIP) that totaled over $800 billion.[2] These payments were the largest unconditional transfer to individuals in U.S. history. By comparison, policymakers enacted one round of stimulus payments during the Great Recession that totaled just over $180 billion in 2020 dollars.

[2]Because they were unconditional transfers provided to support a broad segment of the population, we use the term "stimulus" to refer to these programs, consistent with the terminology in other work (see, e.g., Meyer, Han and Sullivan (2024)). This design distinguishes them from transfers that were intended to serve as income replacement, such as unemployment insurance payments and the Paycheck Protection Program (PPP), and from programs that are only available to a smaller segment, such as SNAP payments and student loan moratoria.

The first set of payments was enacted in the CARES Act, passed on March 25, 2020, with payments sent in late March and early April. These payments provided $1,200 per adult and $500 per child. Married (single) households with adjusted gross income (AGI) less than $150,000 ($75,000) received the full value of the payment, accounting for 80–90% of U.S. households.[3] Higher income households were subject to a 5-percent phaseout rate per additional $1,000 dollars of AGI beyond the thresholds.

The second round of payments was provided by the Consolidated Appropriations Act of 2021, with payments sent in January 2021. These payments provided up to $600 per adult and $600 per child but were subject to a slightly lower income threshold, as the fully phased out for married (single) filers with AGI more than $150,000 ($75,000).

The American Rescue Plan, passed March 10, 2021, enacted the most generous of the payments, up to $1,400 per adult and $1,400 per child sent to households in March and April of 2021. These payments were subject to the same income thresholds as the second version. Households with dependent children also qualified for advanced payments of the expanded Child Tax Credit (CTC). This expansion increased the value of the credit per child up to $3,600, compared to $2,000 previously, and made the credit fully refundable so that lower-income households could benefit from the full CTC.[4] The advanced payments provided 50-percent of the value of the credit, spread across monthly payments in the second half of 2021 (up to $300 per child each month). These payments totaled roughly $90 billion in aggregate.

Because our research design exploits differences in pre-pandemic family structure across regions, we combine the three rounds of stimulus with the advanced CTC payments in our aggregate calculations. Altogether, the stimulus totaled nearly $900 billion disbursed over less than two years (Figure 2, Panel A).

Figure 2, Panel B shows the generosity of these payments for a married household with two young children at different AGI levels. For those with incomes below $150,000, such a household was eligible for $15,000 cumulatively across these programs. Benefit generosity varied substantially by family structure (Figure 2, Panel C). A single parent household with two children that was fully eligible received over $10,000. A non-married, household with no

[3]Based on tabulations reported in IRS Tax Stats. Alternatively, eligibility for the first round EIP for tax filers was based on 2019 tax return information, or if a 2019 return was not filed, information from a 2018 return. Individuals who did not file a return in either of these years but received certain benefits, including Social Security benefits, and VA pensions, received the first EIP without having to file a return. In addition, individuals who did not file in either 2019 or 2018 could claim the first EIP using an online portal, or through other simplified IRS procedures. The total number of first round EIPs was 164 million and the number of filers in 2019 was 158 million. Including non-filers brings coverage to approximately 80-90% of U.S. households.

[4]The maximum value of the CTC was also a function of child age, with $3,600 available to children 5 and younger and $3,000 for children ages 6 to 17.

children received less than $4,000.

Our main research design takes advantage of how the policy depends on family structures, specifically the frequency of dependent children among eligible households. For such a household, an additional child increased their cumulative payments by as much as $4,300 per household ($500 from EIP 1, $600 from EIP 2, $1,400 from EIP 3, and $1,800 in total from the advanced CTC).

We focus on the stimulus checks and advanced CTC because these programs align with our core research question: what is the effect of unconditional cash stimulus on durable goods markets? However, policymakers also enacted other household transfers during the pandemic, which we account for in various ways. These include food support via SNAP, unemployment insurance (UI) benefits, as well as business support programs like the PPP. There was also a student debt moratorium that exempted borrowers from having to make loan payments during the pandemic.

The UI expansions were the largest individual transfers, which amounted to approximately $700 billion across three main programs: federal pandemic unemployment compensation (FPUC), pandemic emergency compensation (PEUC), and pandemic unemployment assistance (PUA). Figure 2, Panel A shows that, when combining expansions with baseline UI benefits, cumulative UI payments amounted to approximately $900 billion during the stimulus. These programs provided support both to unemployed workers typically covered by the UI system and to "gig" workers such as ride-share drivers who typically are not eligible for benefits. While the timing of these payments overlaps with the transfer payments we study, we show our exposure measure is not strongly correlated with either ex ante unemployment rates or unemployment rates over the course of the pandemic. Further, in some specifications, we control for both ex ante unemployment rates as well as for ZIP-by-month unemployment rate estimates from the Bureau of Labor Statistics to guard against the possibility that the spending response reflects UI related benefits.

The student loan moratoria and SNAP expansions were also sizable, but on the order of $75 billion per year for the former and $50 billion total for the latter. The PPP program provided $800 billion in total support to businesses, which was intended to help firms retain workers. Estimates of the economic incidence of the program tend to find more of the benefits went to owners (Autor, Cho, Crane, Goldar, Lutz, Montes, Peterman, Ratner and Villar, 2022; Granja, Makridis, Yannelis and Zwick, 2022). We find limited correlation between our exposure measure and markers of these programs as well as that the estimates do not change when we add related controls. In our aggregation exercises, we provide back-of-the envelope estimates for

these program's contribution to our quantity and price results.

2 Data

In this section, we describe the underlying data sources used in our analysis, define key variables and our analysis sample, and present summary statistics

2.1 Data Sources and Variable Definitions

We define program exposure as the average Child Tax Credit (CTC) value per household in 2018. We use ZIP-level data from the Internal Revenue Service (IRS) Statistics of Income (SOI) to measure exposure, where the number of tax returns in the ZIP serves as a proxy for the number of families. Children eligible for the CTC in 2018 would be eligible for the transfer payments we study, so all else equal a ZIP code with higher average CTC benefits in 2018 should receive higher transfer payments during the pandemic. We use 2018 because it is the earliest year that both predates the pandemic and includes CTC expansions enacted in the Tax Cut and Jobs Act (TCJA) of 2017.[5]

For our main outcome variables, auto sales data comes from Experian, which sources vehicle registrations from state-level Department of Motor Vehicle databases. These data cover all 50 U.S. states and nearly all Core-Based Statistical Areas (CBSAs). The data include information on the type of purchase (new versus used), fuel type (gas, diesel, EV, etc.), as well as if the sale was cash- or loan-financed. Most important, these registrations allow us to measure the quantity of transactions independent of the price paid, which distinguishes our setting from many studies that rely on consumption expenditures to study stimulus responses. Relative to credit-card-derived consumption data, another advantage of the auto data is the geographic marker refers to the registration address and not the location of the transaction, which allows us to map transfer payments to the correct geographies.

Figure 1, Panel A plots for each month total auto sales as well as used and new sales, along with bars showing aggregate stimulus. Aggregate sales in our sample represent 75% of national sales based on data from Bureau of Transportation Statistics (BTS). Similar to the BTS data, there are three used car sales for every new car sale in our sample.

[5]The TCJA replaced personal exemptions for dependent children with an expansion of the CTC that increased the amount of the credit to $2,000 per child (from $1,000). Unmarried (married) households with AGI less than $200,000 ($400,000) are eligible for the full credit, with higher income households subject to a 5% phaseout rate. The TCJA also increased the value of the refundable portion of the credit to $1,400 (up from $1,000), so that families that otherwise had no tax liability could receive a larger benefit.

The auto sales data are reported monthly based on when Experian posts the data in the file, not necessarily when the state issues the title, with possible lags up to 2 to 3 weeks that may vary across region. For this reason, while we report both monthly estimates and pooled annual estimates, we use the annual estimates in our aggregate calculations. Appendix Figure B.1 shows that these data track other indicators of auto sales drawn from the Bureau of Transportation Statistics, consistent with the interpretation that lags in reporting do not generally have a large effect on count data.

We measure actual household payments using administrative population-level tax data from the IRS, which we aggregate to the ZIP level to match the aggregation of our main outcomes. We use these payments to measure the first stage mapping exposure to stimulus received and in our aggregation calculations.

Additional control variables and covariates come from several sources. From the 2018 IRS SOI file, we draw ZIP-level data on the married share of tax returns, the natural log of AGI per tax return, and the natural log of the number of tax returns as a proxy for population aligned with our outcome and exposure measures. We also measure population growth as the number of tax returns between 2015 and 2018 using IRS SOI statistics. Pre-pandemic values (2016–19) of the unemployment rate come from the American Community Survey.

To proxy for another source of finance for auto purchases, we control for mortgage refinances at the ZIP-by-month level using data from McDash. From these data we create counts of both cash-out and no-cash-out refinances as well as mortgage originations, which we include as controls in our baseline specification. We also control for Paycheck Protection Program (PPP) loans with data from the Small Business Administration, from which we take the number and average amount of loans. For mortages and PPP loans, we scale the loan data by the number of tax returns in 2018. Our baseline regressions include average cash-out refinance and PPP loan amounts, but we also report robustness that includes the additional mortgage and PPP controls.

County-level controls for COVID-related variables, including cases, deaths, and vaccinations at the monthly frequency, come from Center for Disease Control and Prevention (CDC) COVID tracker. County-by-month controls for local area unemployment come from the Bureau of Labor Statistics. We use GPS data from the Opportunity Insights economic tracker for time spent at work, away from home, and at retail establishments. These data are benchmarked to values for early 2020, and we assign the pre-period values this same benchmarked value. We include 2016 presidential election vote shares at the county-level from the MIT Election Lab.

As a supplement to the Experian data, we also measure auto transactions with restricted

access credit report data from the FRBNY/Equifax Consumer Credit Panel (CCP). These data are a 5-percent random sample of individuals with credit reports. Compared to the auto registration data, these data are a smaller sample by construction and include only loan-financed purchases. They also do not indicate whether the purchase was a new or used vehicle. However, key benefits of these data include detailed borrower information such as age and credit score, and the ability to track loan payments and status over time. We use these data in a companion paper (Berger, Cui, Turner and Zwick, 2024) to study the characteristics of stimulus recipients most likely to benefit from transfers. Here, we use them to validate our quantity estimates in a different data set. Appendix Figure B.1 shows that, after adjusting for sample coverage, our loan-financed sales counts match with a slight lag those reported in the CCP.

2.2 Analysis Sample

The 2018 SOI data serves as the starting point for our analysis sample, which contains information for most ZIPs in the U.S. We merge these data to ZIP-by-month level auto registrations from Experian for the years 2018–23. From the Experian data we construct outcome measures with categorizations for new, used, cash, and loan financed, scaled by the number of tax units in 2018 in thousands. We merge these data with our additional ZIP and county-level controls.

We make a few sample refinements. First, we omit ZIPs for which we do not have COVID, GPS, or unemployment data. Second, we drop all observations from Iowa and Louisiana because we do not observe auto registrations for independent dealerships in these states. The resulting analysis sample contains 12,568 ZIP codes in 362 metro CBSAs spanning 72 months (2018–2023). The sample covers approximately 75% of both tax returns in the 2018 SOI tax file and of 2018–19 auto sales in the Experian data.

Table 1 presents summary statistics for our key outcomes, exposure measures, and other covariates. Monthly auto sales per 1,000 tax returns average 29 with a standard deviation of 11. New and used sales per 1,000 tax returns average 7 and 22, respectively. Half of all transactions are cash-financed, including 62% of used sales and 16% of new sales. Thus, the registrations data provides an important window into the full market, which cannot be captured in credit record data.

Mean exposure is $950 per tax return with a P10 to P90 range of $566 to $1,360, indicating a large amount of variation in the support of the instrument. To put this in perspective, the full value of the CTC in 2018 was $2,000 per eligible child.

3 Research Design

3.1 Empirical Strategy

Our reduced-form analysis exploits cross-sectional variation in ex ante exposure to the stimulus payments to measure the causal effect on auto sales. Our exposure measure uses pre-pandemic tax data to construct a proxy for expected transfer payments. Specifically, we define exposure as the amount of Child Tax Credit (CTC) dollars received per tax return in each ZIP code in 2018. The exposure measure should be correlated with transfer payments because exposure is based on the CTC that imposes AGI limits and requires the presence of one or more dependent children. These factors closely correspond to eligibility and formulas used to generate stimulus checks during the pandemic response.

We conduct our analysis at the ZIP code level, the most granular level available in both our exposure and outcome data. The key underlying assumption in this design is that low-exposure regions are a valid counterfactual for how auto sales would have evolved in high-exposure regions absent the transfers. We interpret differences in auto sales in the months after payments are made as resulting from differences in the amount of stimulus received. Others have applied this regional approach to study the effects of fiscal policies in other contexts, including the Cash-for-Clunkers program (Mian and Sufi, 2012), transfers to state governments (Chodorow-Reich, Feiveson, Liscow and Woolston, 2012), and the First-Time Homebuyer Credit (Berger, Turner and Zwick, 2020).

In the second part of our analysis, we use our identified estimate for quantities to calculate an aggregate marginal propensity to spend (MPX) on autos and an implied marginal propensity to consume (MPC) for all goods out of the income shock. Unlike prior work that uses high-freqency timing variation in stimulus receipt, our cross-sectional design allows us to measure the cumulative MPX and MPC over a longer horizon while avoiding comparison to later-treated units (Orchard, Ramey and Wieland, 2025, 2023). We benchmark our estimated MPCs to those from the literature as a validation of our aggregate quantity results.

With our quantity estimates in hand, we then evaluate the contribution of stimulus payments to aggregate price increases in the new and used auto markets. Throughout this analysis, we treat each market as national. Shipping costs across regions within the U.S. are sufficiently small relative to observed price fluctuations to ignore in our setting.

We present a simple partial equilibrium model that focuses on the used auto market and calibrates unobserved parameters, namely, the elasticities of demand and supply in that market. This model allows a transparent quantification of the share of the price increase we can explain

with our quantity estimate.

We then calibrate a general equilibrium model that features both used and new car markets and forward-looking consumers. The model matches our reduced-form quantity estimate and reinforces our conclusion from the partial equilibrium framework. We then use the model to conduct counterfactuals that layer non-fiscal shocks onto the environment. These exercises shed light on the types of shocks that are necessary to account for the dramatic relative price increases in the car market during the pandemic.

3.2 First Stage Validation

Figure 4 plots colored maps of variation at the ZIP-level within Chicago, San Francisco, and the Washington DC metro areas. Darker areas indicate higher exposure and tend to concentrate in certain suburbs. The average standard deviation in exposure is over $300 within metro-area CBSAs in our sample, suggesting we have substantial power to detect differences in stimulus payments received across neighborhoods within cities.

Figure 3, Panels A and B show how program exposure correlates with demographic variables, pandemic variables, and other fiscal transfers. Panel A plots monthly unemployment for the top and bottom quintiles of exposure. The unemployment rates are nearly identical across the two groups initially, and they remain extremely similar over the course of the pandemic. Thus, differences in UI benefits across exposure areas, the largest individual transfer during the pandemic other than the stimulus payments, are unlikely to explain differences in auto sales.

Panel B plots correlates of program exposure, estimated via regressions of standard-deviation-normalized exposure versus various controls. We divide variables into categories that reflect the program formula, general demographics, COVID-specific factors, and other fiscal transfers.

The top panel uses variables from the 2018 SOI ZIP-level file. We see exposure is negatively correlated with total income (due to income limits) and strongly positively correlated with the number of children per tax return. Exposure is not materially correlated with the married share.

The second panel uses a variety of data sources to characterize ex ante conditions and shows that exposure is positively correlated with Republican party vote share in the 2016 presidential election, ex ante (2016–19) population and unemployment, and uncorrelated with population growth prior to the pandemic. These relationships are much weaker than the correlation with the number of children or total income. Exposure is not materially correlated with COVID cases, deaths or vaccinations, as shown in the third panel. However, higher exposure areas had somewhat different patterns in time at retail, work, and away from home over the course

of the pandemic, reflecting a tilt toward less dense urban areas where people were less likely to limit their mobility.[6] Such differences could be attributable to risk preferences or from the nature of work and school in these places. Lastly, as shown by the bottom panel, exposure has a generally weak correlation with a variety of other factors that household may have used to finance auto sales, including other transfers. The association between exposure and the pandemic unemployment rate is weak, suggesting that differences in UI benefits across exposure areas were minimal. Similarly, the correlation between exposure and mortgage refinances is low. Higher exposure areas received smaller loans from the PPP on average, but the correlations are not particularly striking. Our robustness checks show our results do not change when accounting for these factors via time-varying controls (either interactions of predetermined characteristics with time dummies or inclusion of monthly time-varying controls).

Figure 5, Panel A plots mean exposure by quintile, which varies from $600 per tax return in the first quintile to more than $1400 in the fifth quintile. Panel B plots a binscatter with 50 bins of ZIP-level stimulus transfers per tax return against exposure. We first residualize the exposure and transfer variables by CBSA fixed effects to demonstrate the first-stage relationship that corresponds to our within-CBSA research design.

The figure shows a strong positive correlation, indicating that our exposure measure is a good proxy for actual transfer payments. The slope is 1.98 (s.e.=0.08), indicating that a $1,000 increase in exposure is associated with a $1,980 increase in stimulus payments. The variation in transfer payments spanned by the exposure measure is several thousand dollars, considerably larger than the typical transfer used in prior work to study consumption responses to stimulus.

3.3 Reduced-Form Specification

To quantify the effect of the transfers on auto sales, we estimate regressions of the form:

$$\text{Auto Sales}_{zt} = \delta_{\text{CBSA}_t} + \delta_{zm(t)} + \sum_t \beta_t \text{Exposure}_z + \sum_t \lambda_t X_z + W_{zt} + \epsilon_{zt} \tag{1}$$

where z and t index ZIP codes and month, respectively. The vector X contains ex ante ZIP code characteristics. W is a vector of ZIP- and county-level time-varying characteristics. We interact both the exposure measure and the ex ante characteristics with indicators for each month beginning in 2019. The key parameters of interest are contained in the vector β_t that

[6]The GPS data are updated only through October 2022. We use this last value for the remainder of the sample period.

represents the differential effect of an additional $1,000 of exposure on monthly sales relative to 2018.

The outcome is the level of auto sales per 1,000 households (number of tax returns in 2018). The high-frequency outcome data allows us to exploit not only geographic differences in program exposure but also timing of the transfer payments. We include ZIP-by-calendar-month fixed effects to seasonally adjust the data.

We conduct our analysis with CBSA-by-month fixed effects, effectively running a series of experiments comparing ZIP codes within each CBSA. This approach isolates pandemic-related factors like local lockdowns and virus spread, place-specific shocks at the state and larger regional level, as well as seasonal variation in auto sales that varies across larger regions. We cluster the standard errors at the CBSA-year level, but report less conservative standard errors in robustness exercises.

Eligibility for stimulus depends on both household income and household structure, with the latter being a stronger correlate with our exposure measure. To remove concerns about confounding income-specific shocks, we control for the log of average adjusted gross income (AGI) in our baseline specifications. Thus, our identifying variation comes from the cross-sectional difference in family structures.

To isolate the effects of the policy from potential confounding factors, we also include a rich set of controls in our baseline specification. First, we interact additional ex ante characteristics with month indicators. These include controls from the 2018 IRS data (married share of tax returns, natural log of wages per tax return, the natural log of the number of tax returns), the 2016–2019 average unemployment rate from the ACS, and the 2016 presidential election vote share. Second, to account for differences in pandemic lifestyle behavior, we include COVID factors (cases, deaths, and vaccinations) and GPS tracking data (time away from home, time at work, and time spent in retail establishments) at the county-by-time level. To account for other sources of income that households could use to finance auto sales, we control for the frequency of mortgage refinances (both cash-out and no-cash-out), as well as PPP loans (count and amounts per tax return) at the ZIP-by-month level. While we cannot directly control for the additional UI payments received, we control for the unemployment rate at the monthly frequency using the local area estimates from the BLS. We report robustness specifications that show our quantitative conclusions do not require these controls.

These controls help isolate the effect of pandemic transfers from other shocks that might be affecting high-exposure regions differently. One might still worry that households with more children were particularly exposed to pandemic shocks, for instance, due to changes in school-

ing or childcare. Assuaging this concern, we find our results are not correlated with severity of lockdowns across states during the 2020 period, and we find similar implied MPXs for the 2020 and 2021 payments, though the economy had reopened substantially by the second half of 2021. One might also worry that households with more children exhibit different elasticities with respect to income shocks. We show in the appendix that, under CES preferences, preferences for autos need not be homogeneous for the income elasticity to be the same across groups. We provide simulations under more general preferences suggesting the bias from assuming a response that doesn't vary with household size is likely small.

4 The Effect on Auto Sales

4.1 Main Results

Before turning to our regression results, we present simple plots of unadjusted data that highlight our main findings. Figure 6 shows average auto sales within the top and bottom exposure quintiles, indexed to the level in 2018, for new sales (Panel A), used sales (Panel B) and total sales (Panel C). We highlight five points. First, pre-pandemic outcomes trend similarly for each exposure group. Second, auto sales drop precipitously in the early stages of the pandemic in both high and low exposure areas for all types. Third, higher exposure areas recover more quickly after these rapid declines and remain elevated through 2020. Fourth, the gap between high and low exposure areas expands further in the months in 2021 following the second and third stimulus payments. Sales remain relatively higher in the top quintile when advanced CTC payments are distributed in the second half of 2021. Fifth, the gap in sales across exposure areas closes without turning negative by the end of 2023. Overall, these patterns suggest that higher exposure regions generally had differentially elevated sales relative to lower exposure regions following the transfer payments.

Our regression estimates condition on CBSA-by-time fixed effects to implement a within-city analysis and add other controls to isolate the impact of the stimulus from other region-specific shocks. Figure 7 plots the monthly coefficients estimated in equation (1) (red circles) along with estimates and standard errors for pooled yearly estimates for each year from 2020–23 (blue solid and dashed lines). Panels A, B, and C plot results for new sales, used sales, and total sales, respectively.

The monthly and annual coefficients for used, new, and total sales are substantively small and indistinguishable from zero in 2019, consistent with the absence of pre-trends. Total sales increase in 2020 on net, driven by increases in used car sales, with the additional sales in the

second half of the year more than offsetting relative declines in the early part of the year.[7] Auto sales in 2021 increased further and remained elevated in 2022, driven especially by used autos. Larger effects in 2021 coincide with the larger stimulus payments during this time, as the average payment for rounds 2 and 3 was nearly double that of the first round of stimulus checks. By 2023 when the payments ended, used sales return to baseline. The estimate for new sales remains slightly elevated, perhaps consistent with supply disruptions pushing sales into future months. Taken together, the regression estimates support the broad takeaways from the raw data.

Table 2 reports regression estimates for each year from 2019 to 2023 (2018 is the omitted benchmark year). The first three columns plot show estimates for all transactions in each market, and the remaining columns split the outcomes into cash and loan transactions.

The point estimate for total sales of 1.74 (s.e.=0.18) suggests an average increase per $1,000 in exposure of 1.74 cars per household, a 5.7% increase relative to a pre-pandemic mean of 30.5. Three-quarters of this response comes from used car sales. Analogous estimates for used and new sales suggest increases in 2021 of 5.7% and 6.2%, respectively, relative to their 2018 means. The estimates for 2020 and 2022 are approximately half the size of the 2021 effect, with a larger relative share coming from new cars in 2022.

In terms of financing, both cash and loan sales contribute to the overall increase, suggesting one mechanism through which the transfers boosted car sales is by helping consumers with down payments. Cash sales increase in 2020, whereas loan-financed sales first pick up in 2021, which may reflect differences in financing conditions over the course of the pandemic. Cash sales are particularly important for used car transactions, as they account for all of the response in 2020, 70% of the response in 2021, and half the response in 2022.

Appendix Tables C.1–C.3 shows our findings are robust to alternative specifications. We consider weighted regressions by either 2018 tax units or 2018 auto sales. We expand the analysis to include all CBSAs, rather than just metros. We split the sample based on state-level travel restrictions during the pandemic to see if interactions with lockdowns and other COVID-precautionary behavior affect the results. We consider an omitted period that includes both 2017 and 2018. We also use a version of the exposure measure that only includes the number

[7]The negative coefficients in April and May 2020 may reflect the differential effects of local lockdowns and school closures on economic activity (Parker, Schild, Erhard and Johnson, 2022) or issues with Experian's processing of auto registrations during this time. In Appendix Figure B.2, we estimate our model using auto loan data from the FRBNY/Equifax Consumer Credit Panel (CCP), which identifies a loan the month it is attached to an individual's credit record. We find nearly identical estimates for this outcome as for the loans outcome in the Experian data, albeit with larger standard errors due to the CCP's 5% sample. However, the CCP data do not show the same negative coefficients. We therefore interpret these dips as artifacts of the Experian registration data collection process.

of kids per tax return, rather than CTC amount.

Appendix Table C.4 shows the sales response is concentrated in traditional fuel types (gas, diesel, flex fuel) rather than hybrid or electric sales. We find limited evidence that the cars transacted during this time are newer or older than typical, or that they transact with more or less mileage than typical.

4.2 Aggregate Estimates

We use regression estimates from each month starting with the initial stimulus payments in April 2020 and running through the end of 2022. These monthly estimates reflect the average sales effect (per 1,000 households) per $1,000 of exposure in each ZIP code. Using sample averages of the number of households per ZIP code (9,159) and exposure ($950), as well as the number of zip codes (12,576) we aggregate these regression results to obtain in-sample estimates of total auto sales. Finally, because our sample contains roughly 75 percent of all households, we scale our estimates to make them nationally representative.

This exercise implies the stimulus payments increased total auto sales by 2.4 million per year during this period (1.6 million per year from used sales and 0.8 million per year from new sales). This represents a 4.1 percent boost to sales overall (3.9 percent for used, 4.7 percent for new) relative to 2019 levels. We use this aggregate impact estimate to calibrate our partial and general equilibrium models.

Note we aggregate against a zero-payment baseline, assuming the marginal effects we estimate are applicable to all ZIP codes across the exposure distribution. Because other aggregate factors are absorbed in the fixed effects, this calculation should not be confused with the macroeconomic aggregate impact. In particular, if the demand shock from stimulus causes auto prices to increase, an offsetting effect on demand will reduce the macroeconomic aggregate relative to the cross-sectional estimate we report here.

4.3 Marginal Propensity to Spend and Consume

We can also use our cross-sectional estimates to calculate the marginal propensity to spend (MPX) and to consume (MPC) out of the household transfers. As long as we convert our quantity estimates to expenditures using appropriate auto prices, we can compare implied expenditures to the total value of transfer payments received.

To determine the valuation of auto sales during the policy period, we start with the average 2019 price level published by Bureau of Transportation Services. We then adjust this price for

inflation using the monthly CPI series for new and used autos, respectively, to create an average auto price paid during the policy period. We account for trade-ins at the time of purchase, assuming 40 (20) percent of buyers have trade-ins worth $15,000 ($10,000).[8]

Define the marginal propensity to spend (MPX) as the expenditure on auto purchases, net of trade-ins and including money borrowed to finance a purchase, divided by the value of transfer payments received. Our aggregate estimates of 4.8 million used sales over 2020–2022 corresponds to $135 billion of spending. Compared to the total value of transfer payments, we estimate a 3-year MPX on used autos of 0.13. The analogous calculation for new autos gives an MPX of 0.09, and the total auto MPX is 0.22.

To estimate the effects on consumption more generally, we follow Laibson, Maxted and Moll (2022) and map the MPX into the MPC using the expression:

$$\text{MPC}_t = \left(\frac{r+\delta}{s}\right)\left[\sum_{i=1}^{t}(1-\delta)^{t-i}\text{MPX}_i\right] \tag{2}$$

where t indexes months, r is the interest rate, s is the durable goods share of household consumption, and δ is the depreciation rate.[9] Conceptually, this method models consumption across all goods from the household payments as proportional to the propensity to spend on a given good—in our case autos—accounting for the durable good service flow and user cost including the time value of money and depreciation. We estimate the auto-sales share of consumption (4.9 percent new, 4.4 percent used, 9.3 percent total) using data from the Consumer Expenditure Survey (CEX) in 2022, following Aguiar and Bils (2015). We assume an annual interest rate of 2 percent and annual depreciation rates of 10 percent for used autos, 15 percent for new autos, and 11.25 percent for total auto sales. We convert these rates to monthly rates in our calculations.

Using this conversion, the implied 3-year MPC estimates are 0.34, 0.21, and 0.58 using the MPX for total auto sales, new auto sales, and used auto sales, respectively. Given the sensitivity of these estimates to the CEX consumption shares, our preferred estimate is the pooled average of these three of 0.38.

We can also calculate a running monthly MPX and implied running MPC. Such estimates are useful for calibrating and testing macroeconomic models of fiscal policy. Past work has relied on short-run responses because longer-run MPCs are difficult to estimate using staggered stimulus check research designs.

[8]These shares are consistent with reports from Edmunds here and from the Edmunds Used Vehicle Report.

[9]Using the expression for the lagged MPC, this expression can be simplified to: $\text{MPC}_t = (1-\delta)\text{MPC}_{t-1} + \left(\frac{r+\delta}{s}\right)[\text{MPX}_t]$.

We start with our month-by-month estimates for sales quantities from April 2020 through December 2022 and the imputed price net of trade-in to construct a monthly estimate of the total valuation of induced sales. We add these estimates each month to create a running sum and divide the cumulative sum by the total value of transfer payments made. We split the sample into three periods to account for the repeated payments, with each period aggregating the valuation and sales and the transfer payments from the prior period. We demarcate these periods because the running MPX jumps discretely when a new transfer occurs.

Figure 8, Panel A, shows the cumulative MPX for used, new, and total auto sales. The MPX in early 2020 includes only sales through that date and the first stimulus payments (made in April and May 2020). The estimates in 2021 (January through June) include all sales through that date and all three stimulus payments (the second and third payments were made in January and February 2021). Estimates for the second half of 2021 and 2022 include the three stimulus payments and the monthly CTC payments made in the second half of 2021. Figure 8, Panel B, shows the running MPC estimates. Unlike the MPX, which are for specific categories of consumption, the MPC here provides independent estimates of the total consumption response based on the responses of new, used, or total auto sales spending. The last data points in December 2022, which includes all auto sales from April 2020 through the end of 2022, correspond to the 3-year MPXs and MPCs reported above.

As noted above, the estimates drop mechanically moving across the first dashed line, reflecting the additional payments from the second and third stimulus checks (an increase of $520 billion in payments). Likewise, the estimates decline moving across the second dashed line from the advanced CTC payments (an additional $110 billion in total).

The figures show how adjusting for the durability of autos affects the dynamic impulse response to stimulus checks. The MPX estimates accelerate after payments are received and then grow more slowly in subsequent years. In contrast, the MPC estimates start lower and grow steadily over time, as buyers consume durable goods service flows.

Our estimates of the MPC should be interpreted carefully for at least two reasons. First, the MPC is based solely on the auto purchase share, which is a relatively small component of overall household spending. We also assume in these calculations that the relative auto share is unchanged over time.[10] Second, auto sales are levered consumption for many buyers, so the MPXs we report might overstate the implied effects of the transfer payments for other types of consumption. Nevertheless, the MPCs we estimate imply that households consumed a relatively large fraction of the transfer payments over the 2020–22 period and are within

[10]Consistent with this assumption, using the CEX we find that these shares are roughly similar in pre-pandemic years.

the estimates that prior work has found in response to similar transfer payments and in other contexts during the pandemic (see Appendix Figure B.3).

5 Partial Equilibrium in the Used Auto Market

Consider a simple model of used car quantities and prices in the presence of a stimulus-induced demand shock (Figure 9). There are N locations and two time periods $t \in \{0, 1\}$. Demand in location i is given by log-linear form:

$$q_{dit} = \bar{\alpha}_i - \frac{\varepsilon_d}{N} p_t + \gamma X_{it} + \nu_{dit}, \tag{3}$$

with a location-specific intercept $\bar{\alpha}_i$, a common slope $\frac{\varepsilon_d}{N}$, a common sensitivity of car purchases to the transfer X_{it}, and (mean-zero) disturbance ν_{dit}.

The market for used cars is national. Accordingly, each location faces a common log-linear supply curve:

$$q_{st} = \kappa + \varepsilon_s p_t + \nu_{st}, \tag{4}$$

with a national intercept κ, slope ε_s, and (mean-zero) disturbance ν_{st}.

Demand and supply changes following the stimulus, which features a common shock $\Delta\alpha/N$ to demand and location-specific stimulus given by ΔX_i. Aggregating across locations for the first difference in demand gives:

$$\Delta q_d = \sum_{i=1}^{N} \Delta q_{di} = \Delta\alpha - \varepsilon_d \Delta p + \sum_{i=1}^{N} \gamma \Delta X_i + \sum_{i=1}^{N} \Delta \nu_{di}. \tag{5}$$

The first difference in supply is $\Delta q_s = \varepsilon_s \Delta p + \Delta \nu_s$. In equilibrium, the change in quantity demanded equals the change in quantity supplied ($\Delta q_d = \Delta q_s$). Solving for Δp gives:

$$\Delta p = \left(\frac{1}{\varepsilon_d + \varepsilon_s}\right)\left(\Delta\alpha + \sum_{i=1}^{N} \gamma \Delta X_i + \sum_{i=1}^{N} \Delta \nu_{di} - \Delta \nu_s\right). \tag{6}$$

Plugging in for the quantity response using the definiton of Δq_s gives:

$$\Delta q = \left(\frac{\varepsilon_s}{\varepsilon_d + \varepsilon_s}\right)\left(\Delta\alpha + \sum_{i=1}^{N} \gamma \Delta X_i + \sum_{i=1}^{N} \Delta \nu_{di}\right) + \left(\frac{\varepsilon_d}{\varepsilon_d + \varepsilon_s}\right)\Delta \nu_s. \tag{7}$$

Assume the supply disturbance is zero and the demand disturbances are independent of

the stimulus. The difference-in-differences (DD) estimator between any two locations $i = 1$ and $i = 2$ is given by:[11]

$$\mathbb{E}[\Delta q_{d1} - \Delta q_{d2}] = \hat{\gamma}\,(\Delta X_1 - \Delta X_2)\,. \tag{8}$$

The aggregate DD estimator summing across all locations is:

$$\hat{q} = \hat{\gamma}\left(\sum_{i=1}^{N} \Delta X_i\right). \tag{9}$$

We can use this expression to relate the DD estimator to the aggregate change in quantity.

$$\Delta q = \left(\frac{\varepsilon_s}{\varepsilon_d + \varepsilon_s}\right)\Delta\alpha + \left(\frac{\varepsilon_s}{\varepsilon_d + \varepsilon_s}\right)\hat{q}. \tag{10}$$

The market impact predicted $\hat{\gamma}$ is attenuated by crowding out through the price response, which is captured by the elasticity of supply. In addition, the aggregate demand shock contributes to the aggregate change in quantities but cannot be estimated using the DD estimator. In general, both factors contribute to the aggregate change in quantities and prices. The change in price can similarly be written using the DD estimator:

$$\Delta p = \left(\frac{1}{\varepsilon_d + \varepsilon_s}\right)\Delta\alpha + \left(\frac{1}{\varepsilon_d + \varepsilon_s}\right)\hat{q}. \tag{11}$$

We use this expression and calibrate the elasticities of demand and supply to quantify the share of aggregate price changes implied by our estimate of $\hat{q}$. Define this share ζ as the ratio of the price change due to the stimulus to the total price change, given by $(\hat{q}/(\varepsilon_d + \varepsilon_s))/\Delta p$. We calibrate ε_d to 0.6 and ε_s to alternative values between 0.5 and 3. These are conservative values, since recent work from new capital goods markets estimates or calibrates elasticities greater than 5 (House and Shapiro, 2008; Orchard, Ramey and Wieland, 2025). We take Δp from the maximum price change in the data in Figure 1, Panel B, which is 50%.

The share of the price change due to the stimulus is plotted in Figure 9, Panel B. We plot impacts using the central estimate of $\hat{q}$ of 4% and a 95% confidence interval estimate of 5.2%. Under these assumptions and with our central estimate, ζ ranges from 2% to 7%. The relative price change for used cars was approximately 30% compared to aggregate prices. Using this figure as the baseline would deliver ζ of 4% and 12%, respectively. Thus, the stimulus-induced demand shock accounts for a meaningful share of the price change. However, most of the

[11] The coefficient γ is the same as identified in our panel regression, which is based on deviations relative to place-specific means and time-specific means. In particular, $q_{dit} - \bar{q}_{di} - \bar{q}_{dt} + \bar{q}$ identifies γ with the right hand side being $\gamma(X_{it} - \bar{X}_i - \bar{X}_t + \bar{X})$.

increase appears driven by other factors.

While supportive of our central conclusion on prices, our partial equilibrium analysis has at least three limitations. First, we treat the supply curve as exogenous and in reduced form. Without modeling the supply of used cars, it is difficult to know whether the elasticities we calibrate are reasonable. Second, we neglect the general equilibrium interaction between the new car and used car markets. On one hand, supply constraints in the new car market may propagate into the used car market, causing both demand to increase and supply to fall. This force would tend to raise prices. On the other hand, when consumers adjust their purchases of cars, they trade in their old cars, increasing supply and lowering price pressure. Finally, the model does not account for the dynamic nature of durable goods demand, in which consumers may adjust the timing of purchase behavior if prices shift up and are expected to subsequently revert. We address these limitations in the next section.

6 Equilibrium Model with Used and New Auto Markets

In this section, we use a small open economy (SOE) version of the model of new and used auto markets from Gavazza and Lanteri (2021) to quantitatively examine how various shocks, including fiscal stimulus, affect prices and quantities. The model is general equilibrium in the sense that the interaction between new and used car markets is fully endogenous. However, we do not allow the interest rate to adjust to shocks. We abstract from interest rate movements because, during our policy period, rates remained roughly constant, and because these models are known to be extremely sensitive to interest rates. Thus, we believe the SOE version both best reflects reality and allows us to focus on the fiscal shocks that are central to our paper.

The key lesson of this analysis is that, once realistic secondary markets are introduced, it is even *more* difficult to generate large increases in used car prices in response to a fiscal stimulus. The key reason is that while fiscal stimulus increases demand for used cars, it also increases the supply of used cars because households who purchase new or nicer used cars often resell their old car in the secondary market.

6.1 Model Setup

Households. The economy consists of a continuum of households indexed by i, who derive utility from consuming a non-durable good, c_{it}, and durable services, d_{it}, provided by cars of varying quality. Households face uninsurable idiosyncratic income risk and a borrowing constraint plus a fixed interest rate. Households have ex-ante heterogeneity in their preference

for cars indexed by θ. Households maximize lifetime utility subject to these constraints and choose whether or not they want to own a car and of what type:

$$E_0 \sum_{t=0}^{\infty} \beta^t \frac{(c_{it}^{\alpha} d_{it}^{1-\alpha})^{1-\gamma}}{1-\gamma} \tag{12}$$

Households can borrow and save by trading one-period non-contingent bonds $b_{i,t+1}$ at a price p_b, subject to a borrowing constraint:

$$b_{i,t+1} \geq \phi, \tag{13}$$

where $\phi \leq 0$ is the debt limit.

Durable Goods. Cars are indivisible and vertically differentiated by quality, gradually moving down a discrete quality ladder due to stochastic depreciation. Used cars are traded in quality-specific secondary markets at equilibrium prices, with sufficiently low-quality cars endogenously scrapped, while new cars can be purchased at market prices. There are 4 cars of decreasing qualities, $\{q_1 = \text{new}, q_2, q_3, q_4 = \text{junk}\}$. A car of quality q_n transitions to quality q_{n+1} with probability π_n until it reaches q_4, at which point it is scrapped. The supply of q_1 has elasticity parameter c_1.

Auto prices are determined as follows. We assume that new cars are endogenous with the marginal cost of a new car at time t equal to $p_{1,t} = c_0 + c_1(x_t - \bar{x})$ where c_0 and c_1 are positive coefficients, x_t is aggregate production of quality q_1 cars at time t, and $\bar{x}$ is the level of production in stationary equilibrium. A higher c_1 indicates that new cars are more inelastically supplied and the case of exogenous prices (perfectly elastically new car supply) occurs when $c_1 = 0$ Households trade used cars in secondary markets, where equilibrium prices for p_2 and p_3 are determined, and p_4 is the exogenous scrap value. Households that sell used cars face transaction costs equal to $\lambda(p_n) = \lambda_0 + \lambda_1 p_n$.

Government. The government issues a constant level of non-contingent bonds b_G and imposes lump-sum taxes τ on all households to finance interest payments on its debt:

$$b_G(1 - p_b) = \tau.$$

Recursive formulation of the household problem. The household's value function $V(b, w, n; \theta)$ can be written recursively. Households choose how much to consume in non-durable con-

sumption c, durable expenditures $p_{\tilde{n}}$ on car $\tilde{n}$, bond purchases $p_b b'$, and face lump-sum taxes τ subject to a budget and borrowing constraint. $V(b, w, n; \theta)$ satifies the following Bellman equation:

$$V(b, w, n; \theta) = \max_{c, b', n'} \Big[u(c, d(n', \theta)) + \beta E\Big[V(b', w', n'; \theta) | n', w \Big] \Big], \tag{14}$$

subject to:

$$c + p_{n'} + p_b b' + \tau = w + p_n - \lambda(p_n) I(n' \neq n) + b, \tag{15}$$

where $I(n' \neq n)$ is an indicator function equal to one if the household trades cars and zero otherwise.

We define a stationary equillbrium in this economy as one where (i) $V(b, w, n; \theta)$ satifies the above Bellman equation; ii) the stationary distribution is consistent with the type distribution $F_\theta(\theta)$, the exogenous income and car depreciation shocks, and the household policy functions $g_b(b, w, n; \theta)$ and $g_n(b, w, n; \theta)$; and ii) all auto markets clear.

6.2 Calibration

The calibration of the small open economy model follows exactly the baseline model from Gavazza and Lanteri (2021). In brief, we adopt the same functional forms and parameter values for preferences, the income process, the credit market, car production, and trading costs. Preferences are modeled using a per-period utility function following Berger and Vavra (2015), with parameters chosen to match vehicle expenditure shares and risk aversion estimates from the literature. The income process follows an AR(1) specification, calibrated using estimates from the Panel Study of Income Dynamics (PSID). The bond market is calibrated to match liquid assets relative to GDP and the fraction of constrained households, following Guerrieri and Lorenzoni (2017) and Kaplan and Violante (2014), respectively. The car market structure is simplified to four quality levels to maintain computational tractability, with depreciation rates and price dynamics chosen to match empirical estimates from Jacobsen and van Benthem (2015). Finally, transaction costs include both fixed and proportional components, calibrated using data from the National Automobile Dealers Association (NADA) to reflect observed price differentials between retail and trade-in markets.

Since we use the model to examine the response of prices and expenditures to fiscal stimulus, it is crucial that the model accurately captures the spending behavior on both non-durable goods and automobiles. To ensure this, we conduct an out-of-sample test by analyzing the marginal propensity to consume (MPC) and the marginal propensity to expend on automobiles (MPX) in response to a $500 fiscal transfer.

Overall, we find that the model generates MPCs and MPXs close to the empirical estimates reported in Figure 8. This alignment suggests that the model provides a reasonable approximation of household spending responses and serves as a potentially useful laboratory for studying the effects of fiscal stimulus on consumption patterns and market prices.

7 Model Results

In this section, we study the effects of unexpected aggregate shocks, such as a fiscal stimulus, on prices and quantities in the new and used car markets.[12] To analyze these effects, we compute the transitional dynamics of our model economy, which begins from the previously characterized steady state.

Along the transition path, we assume that households have perfect foresight regarding aggregate variables. When the economy deviates from the steady state, the value function, the distribution of households over individual states, and the equilibrium prices for bonds and cars evolve over time. Thus, we solve for the sequences of value and policy functions and the price paths for $t = 0, ..., T$, ensuring consistency with household optimization and market clearing. Here, $t = 0$ represents the period when the shocks occur and households become aware of them, while T denotes the period when the economy reaches its new steady state.

7.1 Baseline Results

Figure 10 presents our baseline results in response to an income shock designed to replicate the fiscal transfers observed in 2021–2022. Specifically, the shock is a temporary increase in household income, amounting to 3.5% of annual income, and persists for two years. To calibrate the response, we select the elasticity of new car supply (c_1) to match the reduced form estimate for the increase in used car sales. We find that setting $c_1 = 0$—implying perfectly elastic new car supply—successfully achieves this target.

During the policy period, used car sales increase by approximately 5%, consistent with our reduced form estimates. Similarly, new car sales rise by about 3%, a magnitude close to empirical estimates. However, when we turn to prices, a different pattern emerges. In this simulation, used car prices actually *decline* during the policy period, while new car prices remain unchanged by assumption.

[12] We follow several recent papers that assume households did not foresee the aggregate shock, e.g., Huo and Ríos-Rull (2016) and Guerrieri and Lorenzoni (2017).

This seemingly counterintuitive price response is driven by the fundamental economics of secondary markets. When households receive an income transfer, they adjust their durable goods consumption based on their financial situation—some purchase new cars, while others buy used cars. This adjustment creates an increase in demand for both new and used autos. However, households that purchase new cars frequently finance part of the purchase by selling their existing used vehicles, leading to a simultaneous increase in the supply of used cars. This countervailing supply effect dampens the impact of fiscal stimulus on used car prices, significantly limiting the ability of income transfers to generate substantial price increases in the secondary market.

An unrealistic assumption in the previous exercise is that new car supply is perfectly elastic. However, during the pandemic, it was likely highly inelastic due to the silicon chip shortage and other supply chain factors. This inelasticity could have contributed to a larger price response for used cars because, when a fiscal transfer increases demand, new car prices rise, shifting some of that demand toward used cars. This shift amplifies the demand shock for used cars while also mitigating the increase in supply, as fewer new car purchases mean fewer trade-ins entering the secondary market.

To explore this mechanism, we assume that $c_1 = 30$ for two periods before returning to $c_1 = 0$. This assumption implies that new car supply is initially highly inelastic—essentially fixed—before reverting to perfectly elastic. The results, shown in Figure 10, Panel B, indicate that even under these extreme assumptions, the price response remains small, rising only 1%—nowhere near the over 30% relative price increase observed in the data. The assumption also causes the observed quantity response to go to zero initially because of the illiquidity in the trade-in market.

7.2 Explaining Auto Prices during the Pandemic

Figure 11, Panels A and B consider an additional scenario where households enter the period with 20% excess pandemic savings, roughly the amount attributable to the fiscal transfers to households and firms excluding the stimulus checks and the CTC.[13] We implement this scenario as an unexpected wealth shock alongside the income shock. Even in this extreme case and

[13] Abdelrahman, Oliveira and Shapiro (2024) shows that household liquid wealth increased from $12 trillion to $16 trillion during the first year of the pandemic. The sources of this increase include reduced consumption, stimulus checks and childcare tax credits ($900 billion), PPP loans ($800 billion), UI payments ($900 billion), and SNAP extensions ($150 billion). Therefore, an upper bound on the share of excess wealth attributable to transfers excluding stimulus checks and the advanced CTC is approximately:

$$\frac{1850}{2750} \times \frac{16-12}{12} \approx 0.22\%$$

inelastic new car supply, used car prices increase by only 4%. With or without the additional wealth, we find that pandemic-related fiscal transfers alone cannot account for the substantial surge in used car prices observed in 2021–2022.

What can explain the large run-up in auto prices? We introduce two additional shocks that could have contributed to the increase in auto sales during the 2021–2022 period. First, we model a temporary relaxation of borrowing constraints, reflecting historically low interest rates and easy credit conditions. Specifically, we assume a there was a large relaxation of the households borrowing constraint from -0.4 to -1. Second, we incorporate a temporary shift in expenditure shares toward goods, including autos, and away from services, consistent with the patterns documented by Cox, Ganong, Noel, Vavra, Wong, Farrell and Greig (2020). We implement this as an increase in the expenditure share of autos from 5% to 6% because there was a temporary, 20 percent increase in real spending on new and used cars during the pandemic (The Conference Board, 2023). These shocks are introduced on top of our existing wealth, income, and temporary supply shocks.

The results, shown in Figure 11, Panels C and D, indicate that both additional shocks contribute to amplifying price increases for goods. New and used auto prices rise by nearly 20%, with used car prices experiencing slightly larger gains. Temporary supply constraints also appear crucial—without them, used car prices would actually decline as increased household preference for autos induces a tilt toward new cars.

Overall, the model suggests that a combination of shocks can account for most of the observed price dynamics in the data. The primary drivers appear to be the relaxation of credit constraints and the temporary shift in consumption towards goods during the pandemic recovery, coupled with temporary supply constraints.

8 Conclusion

We analyze the effects of key household transfers enacted during the pandemic on auto sales. These stimulus payments provided households with benefits that totaled nearly $900B in aggregate. We exploit ex ante differences in exposure across narrow geographic areas combined with the sharp timing of the payments to estimate their effects on auto sales. Our results suggest that the programs stimulated durable goods purchases by increasing auto sales by 5% relative to a no-stimulus counterfactual. We use our quantity estimates to calculate a 3-year MPX on autos between 0.1 and 0.2 and an implied MPC of approximately 0.35. The fact that we do

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not observe a reversal in sales after payments ended implies that these additional sales were either pulled from further in the future, or that many transactions would not have otherwise occurred absent the transfers.

We use our quantity estimates to ask how much of the increase in car prices can be attributed to the stimulus. We find that the stimulus accounts for a relatively small share of the overall price increase. This conclusion holds in both a simple partial equilibrium model and a richer dynamic model that incorporates the interactions between the used and new car markets. In contrast, a combination of a preference shock for goods and away from services on top of a supply constraint in the new car market is more likely to account for a substantial share of price growth.

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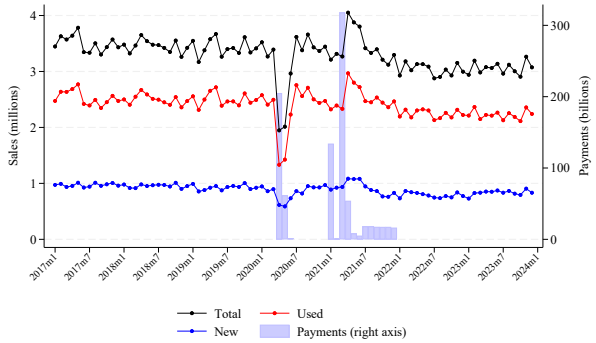
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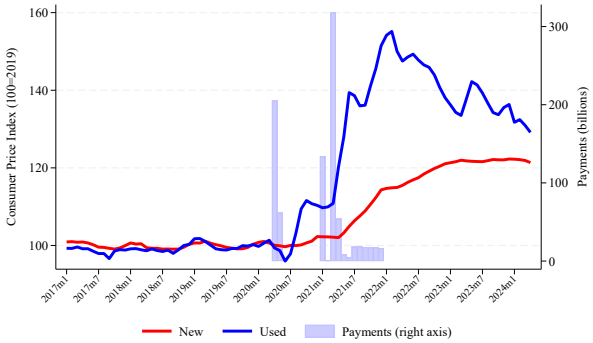
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Figure 1: The State of the Auto Market

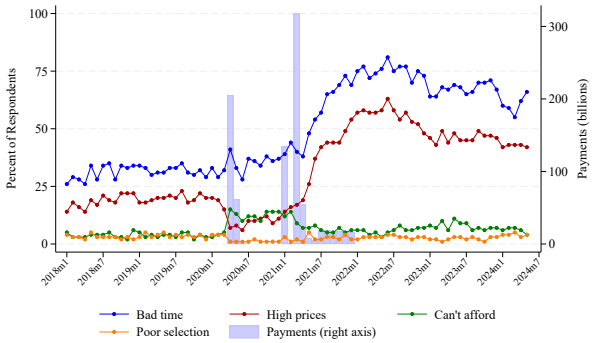
Panel A. Aggregate Car Sales



Panel B. Aggregate Car Prices



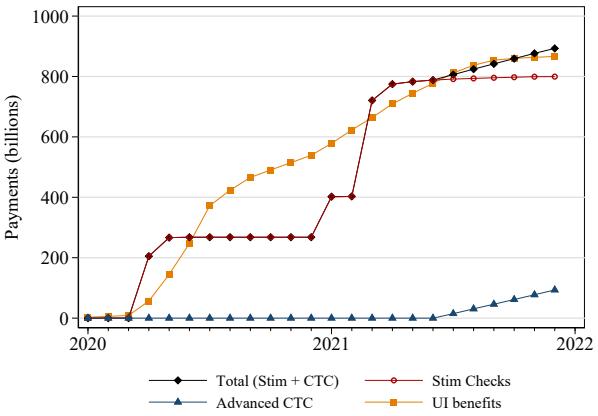
Panel C. Car Buyer Sentiment



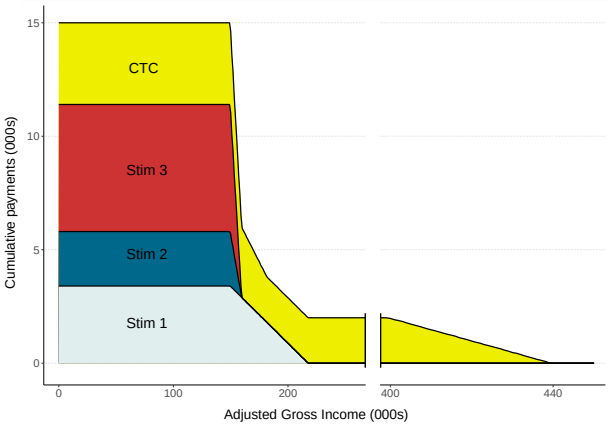
Notes: Panels A and B show auto sales and CPI price index components, respectively, (left axis) and stimulus disbursements (right axis). We report separate series for used and new cars. Panel C shows a sharp, persistent increase in surveyed consumers' beliefs that the car buying environment is bad. They attribute this sentiment to high prices rather than being unable to afford or find a suitable car.

Figure 2: Aggregate Stimulus and Policy Design

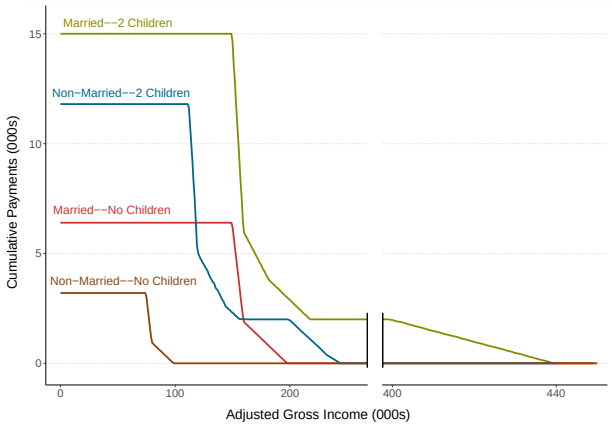
Panel A. Cumulative Aggregate Transfers



Panel B. Cumulative Benefits, Married Parents with 2 Children



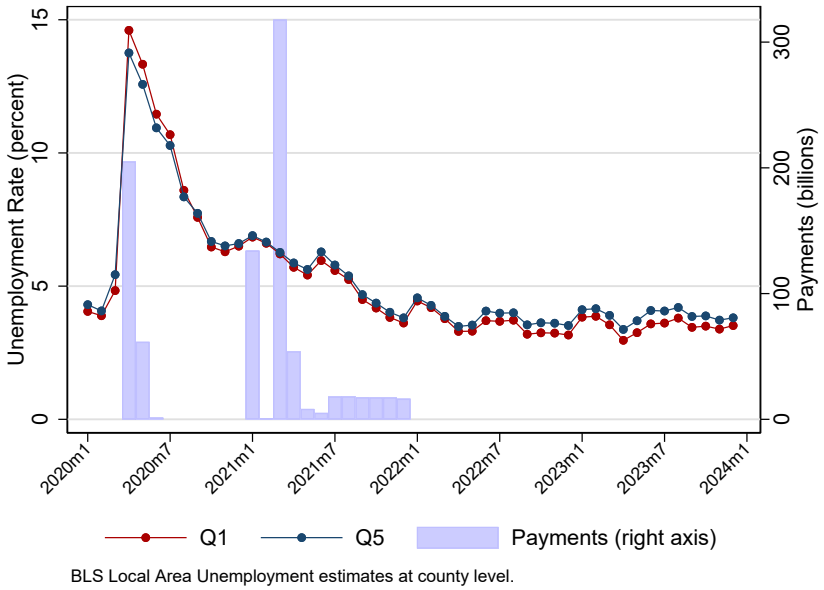
Panel C. Cumulative Benefits, Family Structure Variation



Notes: Panel A shows cumulative aggregate payments over time for the economic impact payments, CTC, as will as unemployment insurance (UI) benefits. Panel B shows the cumulative value of pandemic-related transfer payments by adjusted gross income (AGI) for a married family with three children. We separate the three rounds of economic impact payments and the advanced Child Tax Credit (CTC). Benefits phase out past an AGI of $400,000. Panel C shows how the cumulative payment amounts vary with family structure for some cases.

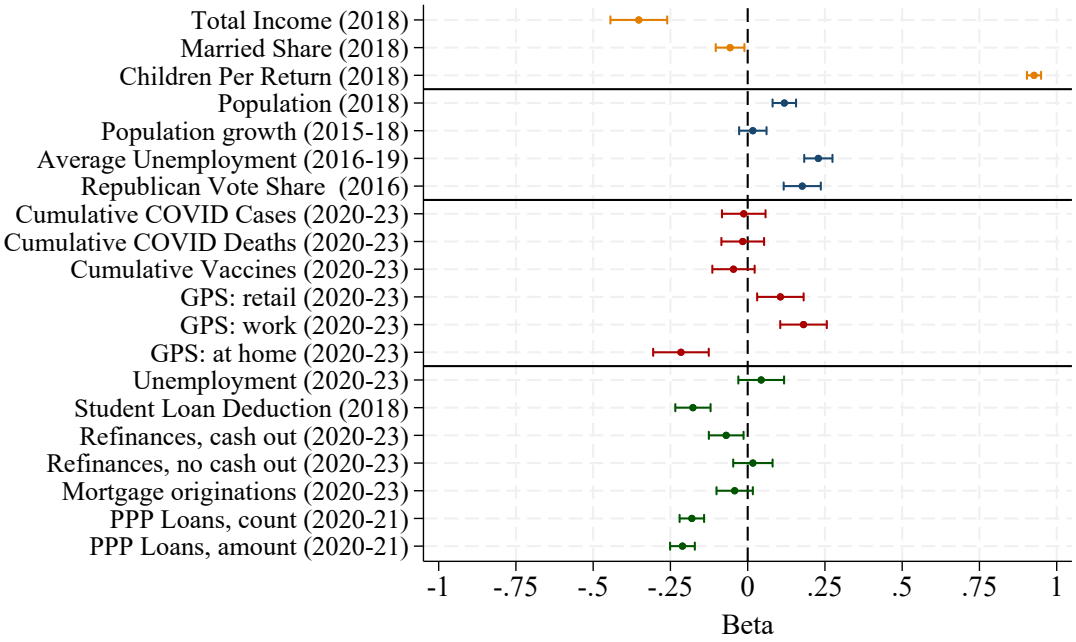
Figure 3: Correlates of Pandemic Stimulus

Panel A. Unemployment
High vs. Low Exposure



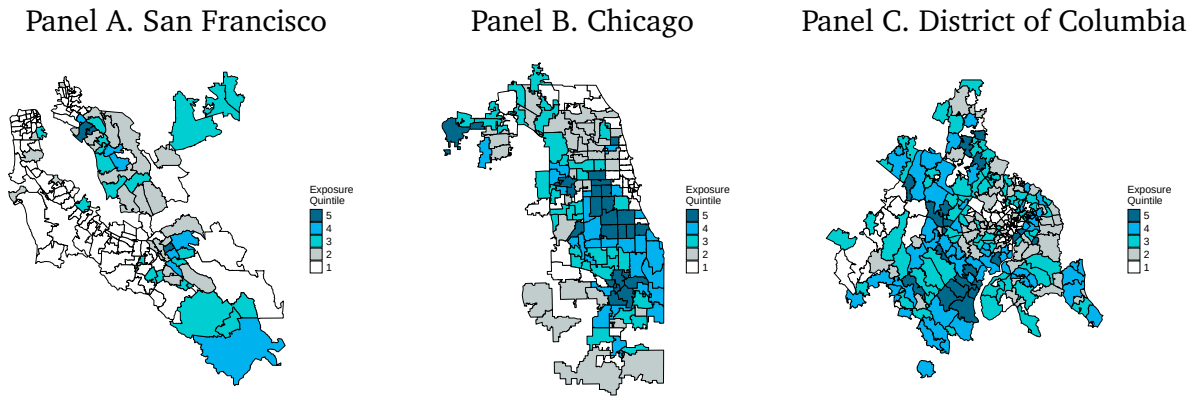
BLS Local Area Unemployment estimates at county level.

Panel B. Correlates of Exposure



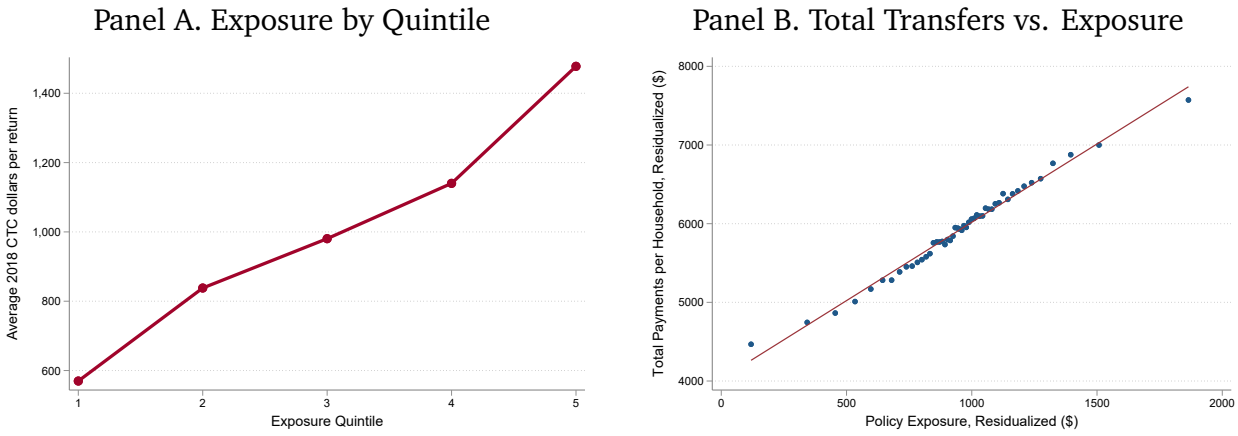
Notes: Panel A shows similar unemployment rates for high and low exposure counties, indicating UI benefits are unlikely to explain differences in pandemic stimulus across counties. Panel B shows coefficients from a regression of the exposure measure on ZIP- or county-level demographic and economic covariates, indicating that the measure is negatively correlated with total income and positively correlated with number of children. Importantly, it is largely uncorrelated with other economic and pandemic-related outcomes.

Figure 4: Geographic Variation in Stimulus Exposure



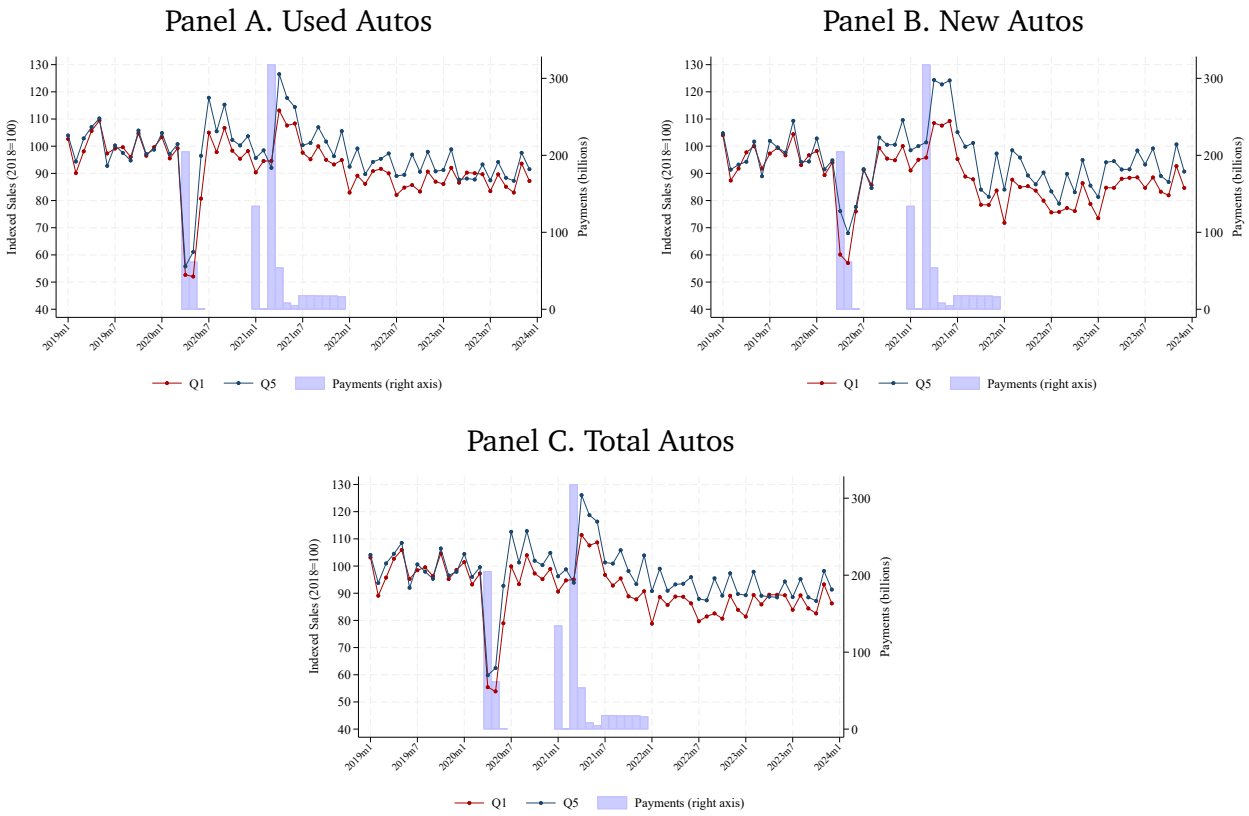
Notes: We show geographic variation in exposure to pandemic stimulus for three major metro regions, using the 2018 county-level average CTC value per tax return as the exposure measure.

Figure 5: Program Exposure and Transfer Payments



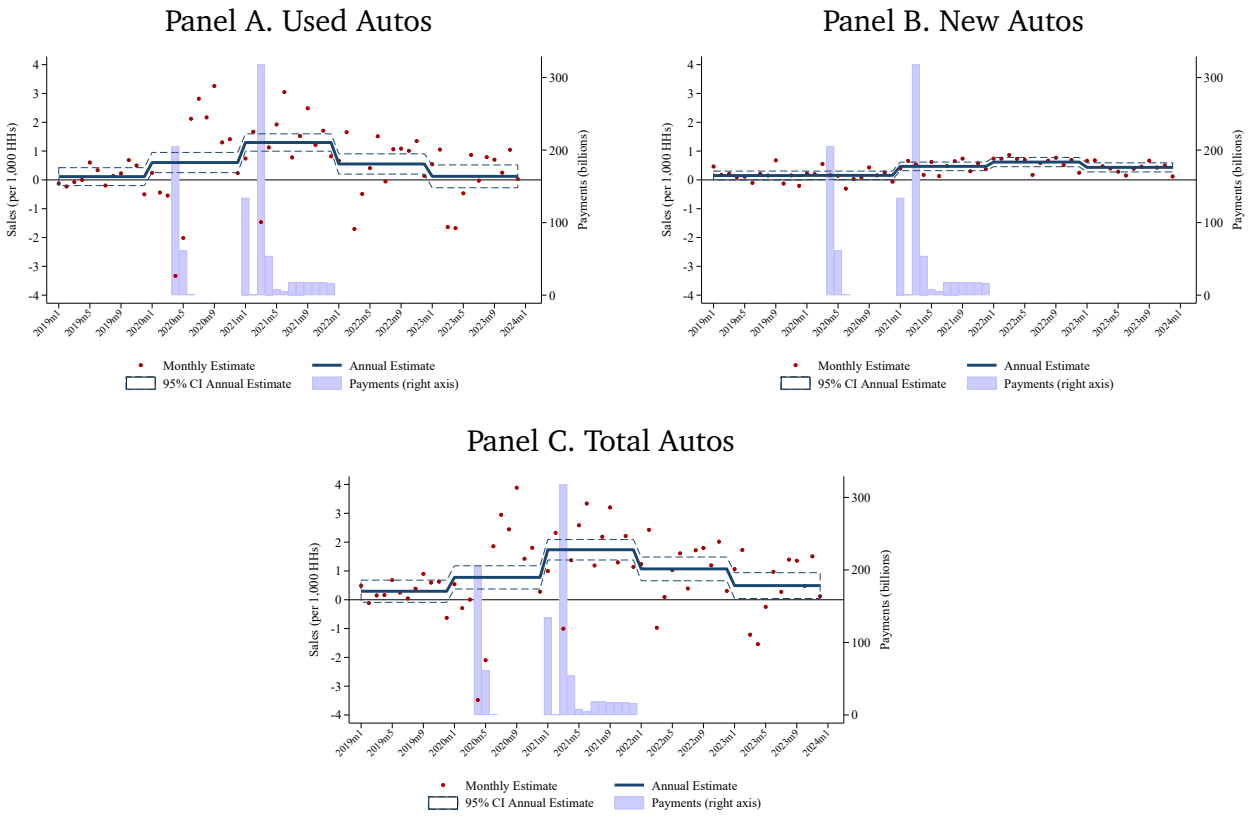
Notes: Panel A plots our exposure measure by quintile. Exposure is defined as 2018 CTC payments per tax return at the ZIP level. Panel B plots a binscatter (50 bins) of ZIP-level stimulus transfers scaled by the number of tax returns versus program exposure. We first residualize the exposure and transfer variables by CBSA fixed effects to demonstrate the first-stage relationship that corresponds to our within-CBSA research design.

Figure 6: The Effect of Transfers on Auto Sales for High versus Low Exposure Areas



Notes: This figure plots monthly auto sales at the ZIP level for the top and bottom quintiles of pandemic transfer exposure, indexed relative to the mean level in 2018. Panels A, B, and C plot series for used autos, new autos, and total auto sales, respectively. The figures show similar trends for high versus low exposure areas prior to the pandemic, with relatively larger increases for both types of sales in higher exposure areas as households receive the transfers. Aggregate monthly payment amounts are presented in blue bars.

Figure 7: The Effect of Transfers on Auto Sales (Within-CBSA Estimates)



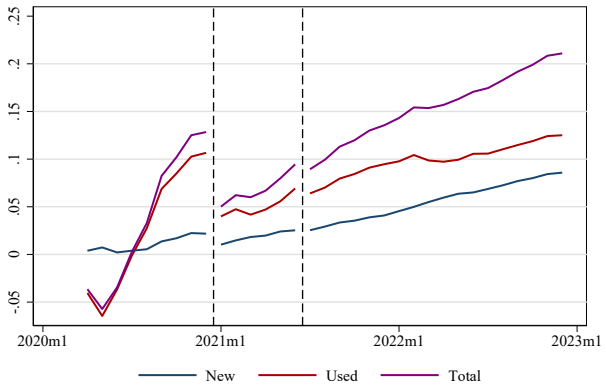
Notes: This figure plots monthly effects of pandemic transfers on auto sales at the ZIP level. We estimate coefficients from a panel regression with controls of the form:

$$\frac{\text{Sales}_{z,t}}{\text{Household}_{z,2018}} = \alpha_z + \sum_t \beta_t \text{Exposure}_z + \delta_{zm(t)} + \delta_{CBSA,t} + \sum_t \gamma_t X_z + \xi W_{z,t} + \epsilon_{z,t},$$

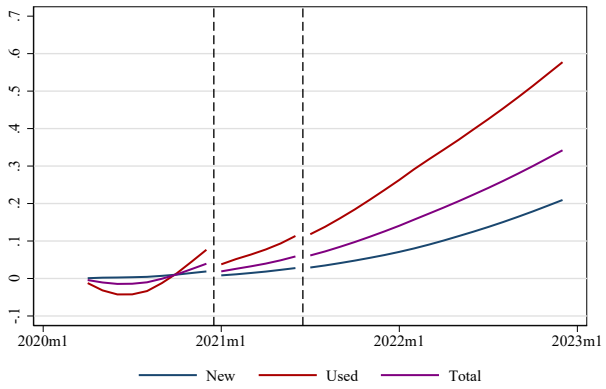
where α_z is a ZIP code fixed effect, β_t is a time fixed effect, γ_t is a month fixed effect, and $\delta_{CBSA,t}$ is a CBSA-month fixed effect. X_z is a vector of time-invariant ZIP-level controls from 2018, including tax return-derived variables for the married household share, the log of average income, and the log of population. $W_{z,t}$ is a set of time-varying controls, including cumulative COVID cases, deaths, and vaccinations at the county level; Google GPS data on workplace, retail, and at-home traffic relative to January/February 2020 at the county level; local area unemployment at the county level; cash-out and no-cash out refinances, as well as mortgage originations per household at the ZIP level; and PPP loans and loan amounts per household at the ZIP level. We cluster standard errors at the CBSA level. The dependent variable is the respective measure of auto sales scaled by initial population. Exposure is CTC payments per household in the ZIP code in 2018 and is normalized by its cross-sectional standard deviation. We exclude 2018 from β_t summation and therefore estimate impacts relative to that omitted period. Panels A, B, and C plot series for used autos, new autos, and total auto sales, respectively. Aggregate monthly payment amounts are presented in blue bars.

Figure 8: Cumulative Marginal Propensity to Spend (MPX) and Consume (MPC)

Panel A. MPX Estimates

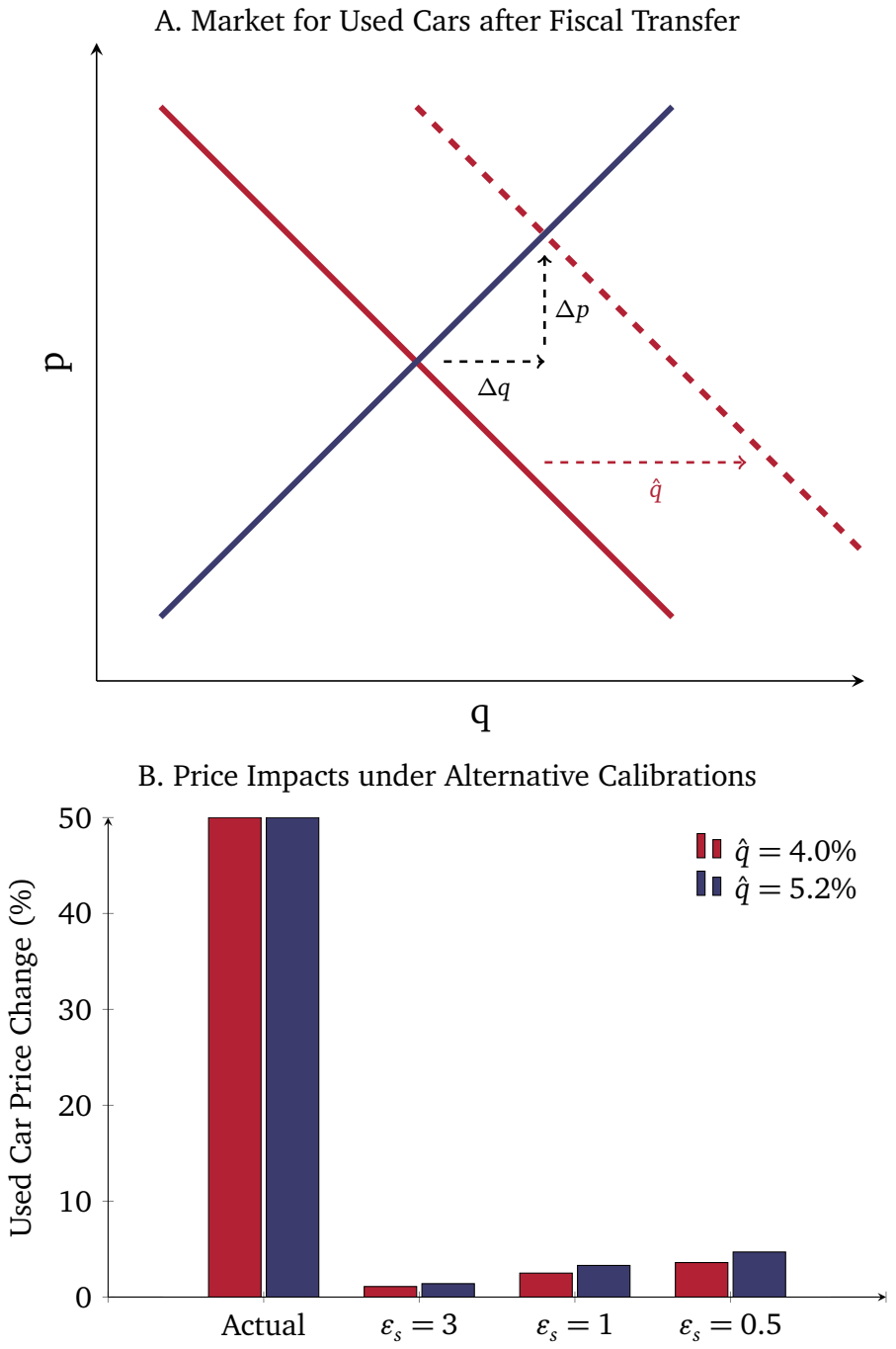


Panel B. MPC Estimates



Notes: Panels A and B present estimates of the marginal propensity to spend (MPX) and consume (MPC), respectively, out of the household transfers. The MPC is estimated following the approach from Laibson, Maxted and Moll (2022). The MPC estimates are based on the auto-sales share of consumption (4.9 percent new, 4.4 percent used, 9.3 percent total) using data from the Consumer Expenditure Survey in 2022. We assume an annual interest rate of 2 percent and annual depreciation rates of 10 percent for used autos, 15 percent for new autos, and 11.25 percent for total auto sales. Dashed lines demarcate regimes with different cumulative transfer payments in the denominator, reflecting the timing of each program.

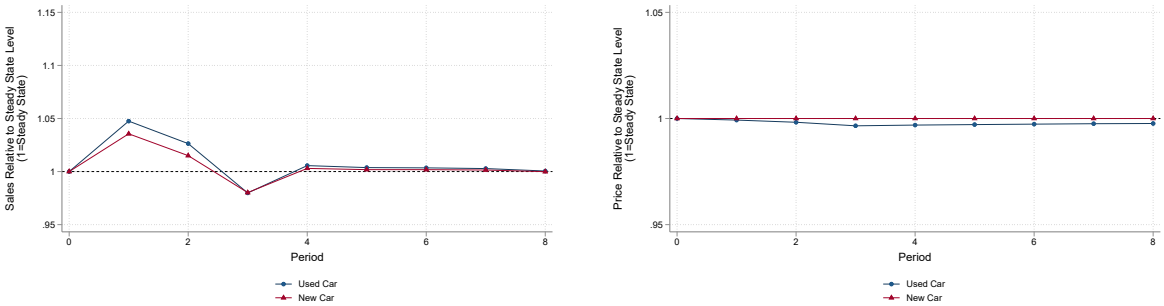
Figure 9: Partial Equilibrium Price Effects of Fiscal Transfer



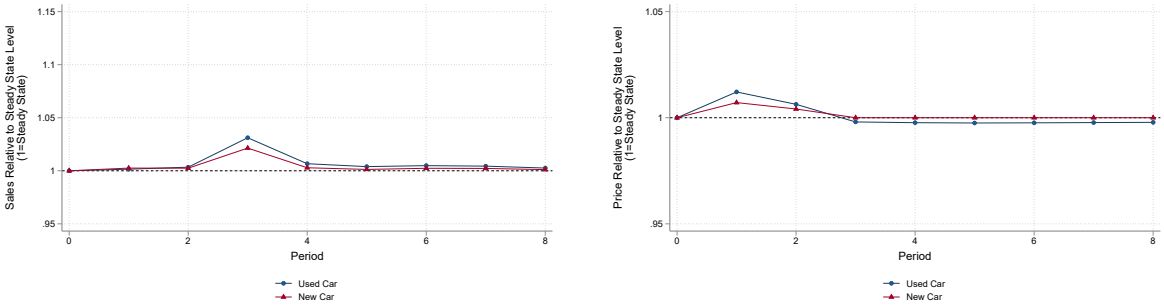
Notes: The figure presents a stylized illustration of the market for used cars following a fiscal transfer. Panel A shows how price and quantity changes relate to the partial equilibrium aggregate impact estimate from our reduced form analysis ('$\hat{q}$). Panel B calculates the share of the price change due to the fiscal transfer, θ, under a calibrated demand elasticity of 0.6 and alternative calibrations of the supply elasticity ε_s. The red bars show the share of the price change under the central estimate of $\hat{q}$, while the blue bars show the share of the price change under the 95% confidence interval estimate. The actual price change from the CPI used car index component is 50%.

Figure 10: Equilibrium Model Results for a Stimulus Shock

Panel A. Elastic New Car Supply



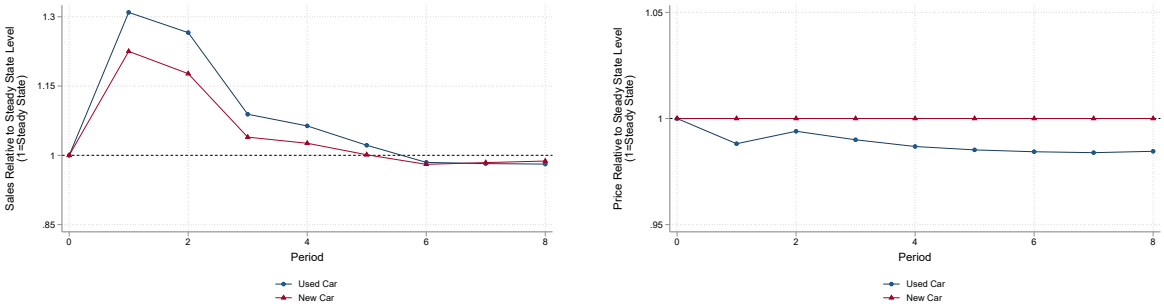
Panel B. Inelastic New Car Supply for Two Periods



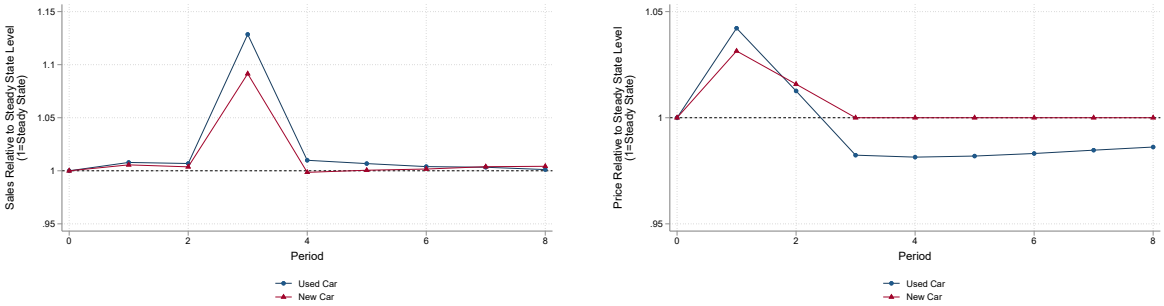
Notes: The figure presents impulses responses from our quantitative model of the new and used car markets in response to a fiscal transfer in period one calibrated to match the relative size of pandemic stimulus to household income. Panel A shows the case where new car supply is elastic, while Panel B shows the case where new car supply is temporarily inelastic for two periods. The left column plots the equilibrium quantity responses, while the right column plots equilibrium prices.

Figure 11: Equilibrium Model Results for Additional Shocks

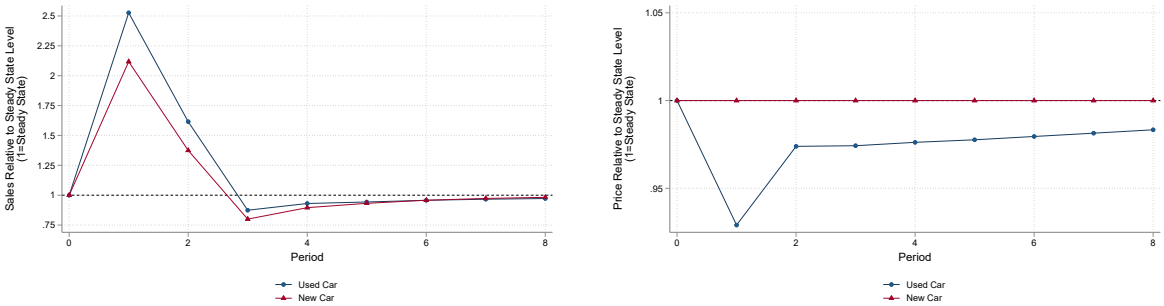
Panel A. Stimulus + 20% Wealth, Elastic Supply



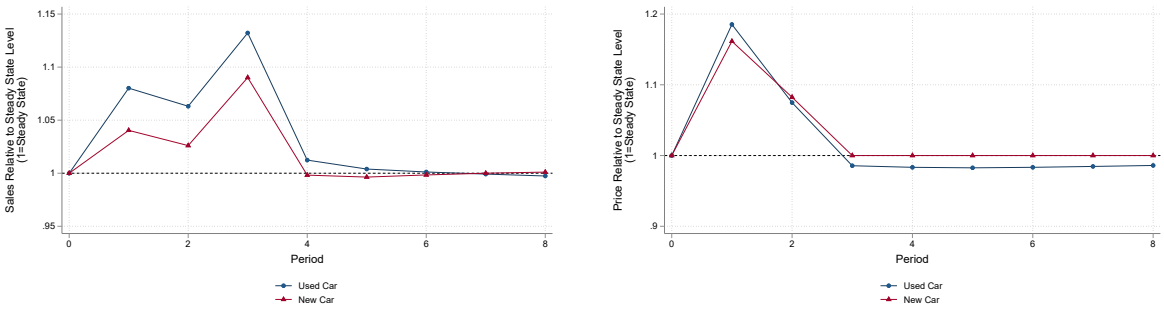
Panel B. Stimulus + 20% Wealth, Inelastic Supply



Panel C. Stimulus + 20% Wealth + Preference + Borrowing, Elastic Supply



Panel D. Stimulus + 20% Wealth + Preference + Borrowing, Inelastic Supply



Notes: The figure is analogous to Figure 10, but now includes additional shocks to household wealth, preferences for autos, and borrowing constraints.

Table 1: Summary Statistics for Auto Sales

Auto Sales (per 1000 households)	Mean	10P	Median	90P	St. Dev.
Sales, Total	28.67	17.50	27.33	41.53	10.72
Sales, Used	21.69	10.46	20.20	34.59	3.75
Sales, New	6.97	2.99	6.44	11.56	3.75
Loans, Total	14.15	7.73	13.81	21.00	5.81
Loans, Used	8.33	3.27	7.79	13.95	4.45
Loans, New	5.81	2.25	5.37	9.89	3.33
Cash, Total	14.51	7.08	12.93	23.86	7.82
Cash, Used	13.36	5.88	11.84	22.58	7.64
Cash, New	1.15	0.00	0.81	2.50	1.42
Stimulus Exposure					
Exposure	950	566	925	1,360	341
Household Payments	5,735	4,122	5,861	7,109	1,196
ZIP Cross Sectional Characteristics					
Married (share of households, 2018)	0.37	0.22	0.39	0.5	0.11
Average income per household ($000s, 2018)	82.91	40.24	64.57	133.89	80.89
Number of households (2018)	9,159	660	6,950	20,740	8,337
Household growth (%, 2015-2018)	2.62	-3.21	1.82	9.31	6.72
Number of children per household (2018)	0.59	0.40	0.58	0.79	0.17
Student loan interest deduction (share of households, 2018)	0.06	0.00	0.07	0.11	0.04
Ex ante unemployment (ACS 2016-19)	5.24	2.1	4.59	9.11	3.47
2016 Republican vote (county share)	0.46	0.22	0.45	0.67	0.16
ZIP & CO Time Varying Characteristics (2020-22)					
Cumulative COVID-19 outcomes (2020 and later)					
Cases	179,457	952	49,520	395,380	426,060
Deaths	2,127	20	675	4,832	4,600
Vaccinations	408,073	0	45,387	1,013,821	1,052,989
Google-based GPS Tracking (indexed relative to Jan3-Feb5 2020)					
GPS time at retail	-0.10	-0.25	-0.10	0.05	0.14
GPS time at home	0.06	0.01	0.05	0.11	0.40
GPS time at work	-0.23	-0.35	-0.24	-0.10	0.10
Local Area Unemployment (BLS)	4.70	2.60	3.91	7.61	2.70
Paycheck Protection Program (2020-2021)					
Number of loans per household	1.07	0.00	0.00	1.83	5.95
Total loan amount ($000s per household)	85.75	0.00	0.00	37.59	1214.00
Mortgage Originations and Refinances					
Originations	14.52	0.00	4.00	39.00	30.22
Refinance, no cash out	0.59	0.00	0.32	1.52	0.83
Refinance, cash out	0.87	0.00	0.17	2.63	1.62
Number of observations (ZIP-by-month)					904,141
Number of zip codes					12,568
Number of CBSAs					362

Notes: All statistics are reported for the full sample period (2018–2023) unless otherwise stated. Data sources include Experian/Velocity (auto sales), Internal Revenue Service public use ZIP-level data 2018 (exposure, married share of households, average income per household, number of households, growth in number of households), Internal Revenue Service Compliance Data Warehouse (household payments), American Community Survey 2016-2019 (ex ante unemployment), MIT Election Lab (2016 Republican party vote share in presidential election), U.S. COVID tracker (cases, deaths, vaccinations), Opportunity Insights Economic Tracker (GPS tracking data), Bureau of Labor Statistics (local area unemployment estimate), U.S. Small Business Administration (PPP loans and amounts), McDash (mortgage originations and refinances).

Table 2: Yearly Regression Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All sales			Total		New		Used	
	Total	New	Used	Cash	Loans	Cash	Loans	Cash	Loans
2019	0.295	0.151	0.114	0.142	0.150	0.041	0.111	0.086	0.027
	[0.196]	[0.079]	[0.159]	[0.128]	[0.114]	[0.227]	[0.070]	[0.125]	[0.071]
2020	0.776	0.157	0.603	0.683	0.059	0.067	0.086	0.598	-0.011
	[0.206]	[0.075]	[0.179]	[0.150]	[0.098]	[0.025]	[0.067]	[0.144]	[0.070]
2021	1.735	0.470	1.298	0.967	0.726	0.048	0.435	0.925	0.368
	[0.182]	[0.076]	[0.154]	[0.127]	[0.094]	[0.028]	[0.066]	[0.122]	[0.069]
2022	1.071	0.619	0.553	0.419	0.653	0.154	0.485	0.265	0.285
	[0.210]	[0.082]	[0.180]	[0.146]	[0.117]	[0.026]	[0.072]	[0.140]	[0.079]
2023	0.492	0.435	0.123	0.239	0.235	0.103	0.370	0.144	-0.021
	[0.229]	[0.080]	[0.202]	[0.160]	[0.112]	[0.029]	[0.070]	[0.156]	[0.083]
2018 Mean	30.52	7.63	22.90	15.36	15.16	1.11	6.51	14.25	8.65
R^2	0.83	0.75	0.87	0.83	0.78	0.68	0.75	0.75	0.84
N									905,472
Number ZIPs									12,576
Number CBSAs									362

Notes: All outcomes are monthly sales per 1,000 households. Sample size, number of ZIP codes, and number of CBSAs is the same in all specifications. Standard errors clustered at the CBSA-by-year level are in brackets. All specifications include the full control set as described in the text, including ZIP-by-month and CBSA-by-time fixed effects.

For Online Publication

A Appendix

A.1 Auto Consumption Share Calculations

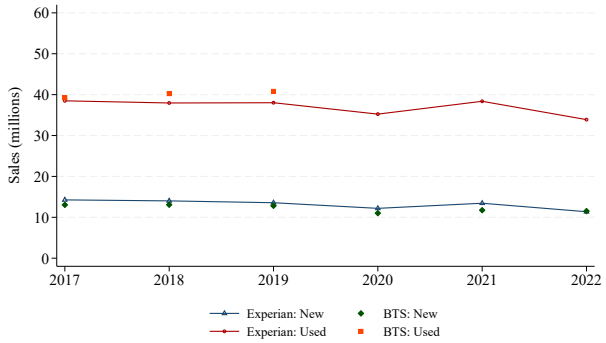
To compute the new and used car purchase shares, we follow CEX's methodology to compute average annual new (*ECARTKN*) and used (*ECARTKU*) car outlays and scale them by average annual total expenditures (*TOTEXP*). The averages are calculated as the ratio of two constructed measures: (1) aggregate annual expenditures at the category level, and (2) sample population. (1) is calculated as the sample-weighted average of quarterly expenditures at the category level, and (2) is calculated by applying within-year adjustment factors to the CEX sample.

We follow Aguiar and Bils (2015) when constructing total expenditures by netting out nondiscretionary expenditures including alimony payments, life insurance, financial fees, and social security contributions. Our average annual category-level expenditures are broadly in line with those reported by the Bureau of Labor Statistics. These expenditure estimates give us a used vehicle share of 0.032 and a new vehicle share of 0.031.

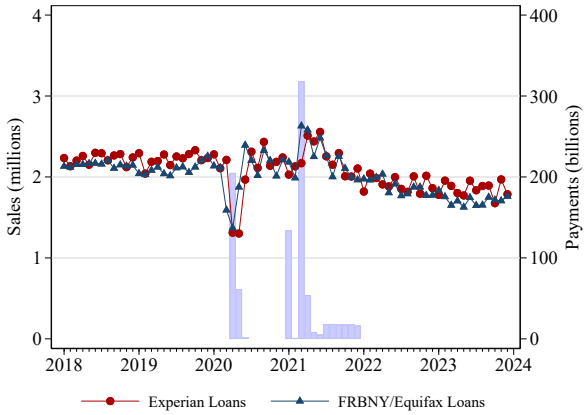
B Appendix Figures

Figure B.1: Benchmarking Experian Data

Panel A. Auto Sales Benchmark Comparison



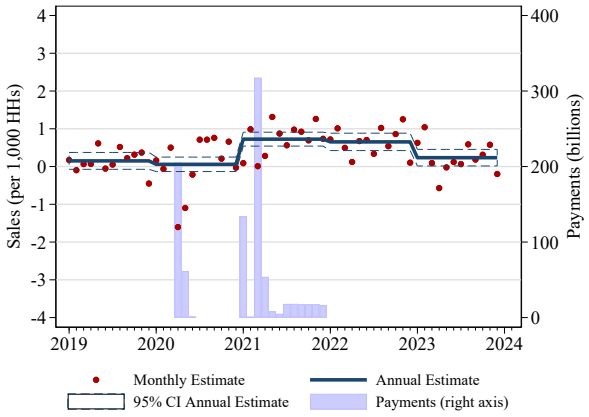
Panel B. Auto Loan Benchmark Comparison



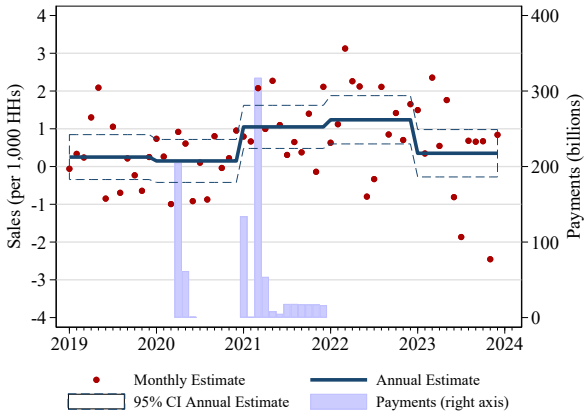
Notes: Panel A compares our auto sales aggregates from Experian with those reported by the Bureau of Transportation Services and find a strong relationship in level and trend. Experian data only cover 75% of the population, so we scale by that amount. Otherwise the totals align very closely. Panel B compares our auto loans aggregates from Experian with those reported by the Federal Reserve Bank of New York and Equifax's Consumer Credit Panel.

Figure B.2: Auto Loan Robustness

Panel A. Auto Loans, Experian

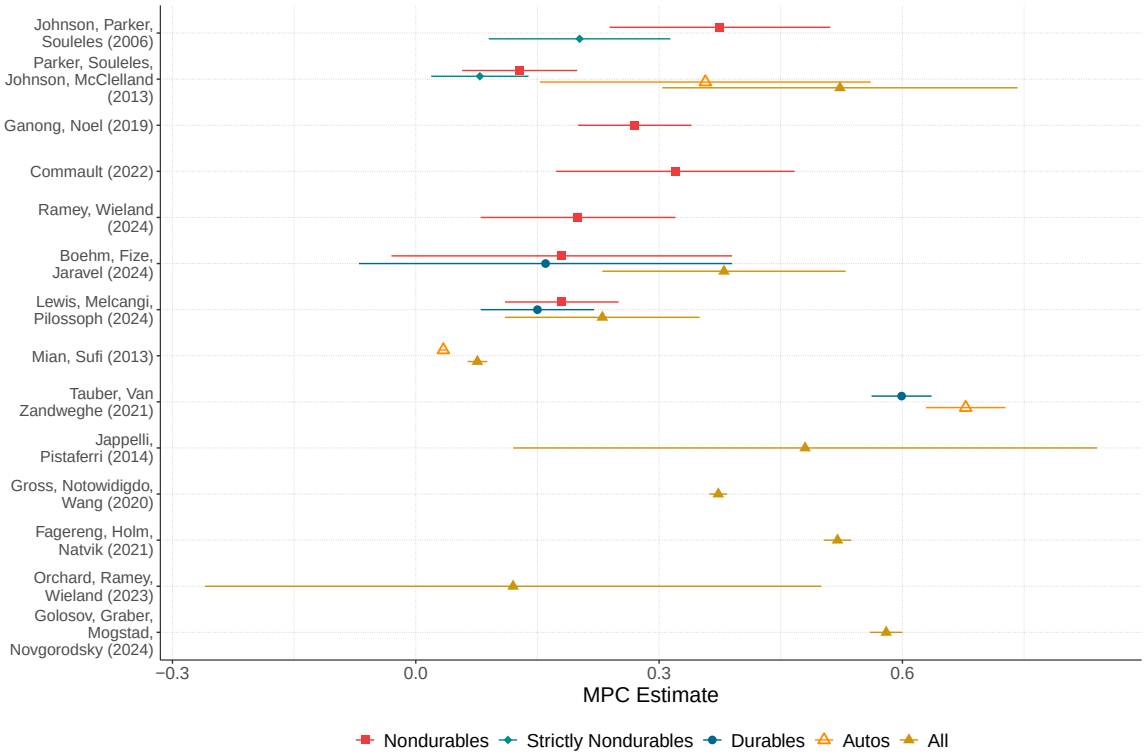


Panel B. Auto Loans, FRBNY/Equifax



Notes: We plot monthly regressions of auto sales on auto loans at the ZIP level.

Figure B.3: MPC Estimates



Notes: We report prominent MPC estimates and ranges from the literature across several categories of consumer expenditures.

C Appendix Tables

Table C.1: Used Sales Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Baseline	Population Weighted	Sales Weighted	All CBSA	No state Level COVID travel Restrictions	Had state Level COVID travel Restrictions	2017-18 as Omitted period	Fixed Effects only	Exposure: Number of children per	Keep Areas with more children	Additional Controls return
2019	0.114	0.041	0.107	0.130	0.181	0.042	0.206	0.021	0.184	0.233	0.099
	[0.159]	[0.126]	[0.181]	[0.156]	[0.269]	[0.194]	[0.124]	[0.151]	[0.245]	[0.198]	[0.158]
2020	0.603	0.148	0.691	0.625	0.917	0.321	0.707	0.040	1.287	0.039	0.639
	[0.179]	[0.208]	[0.201]	[0.170]	[0.302]	[0.214]	[0.150]	[0.199]	[0.286]	[0.233]	[0.175]
2021	1.298	0.966	1.421	1.228	1.614	1.025	1.403	1.005	2.209	0.844	1.276
	[0.154]	[0.133]	[0.176]	[0.152]	[0.261]	[0.185]	[0.125]	[0.163]	[0.235]	[0.205]	[0.152]
2022	0.553	0.404	0.615	0.481	0.718	0.447	0.6647	-0.296	0.582	-0.245	0.657
	[0.180]	[0.167]	[0.196]	[0.170]	[0.291]	[0.228]	[0.160]	[0.188]	[0.275]	[0.207]	[0.178]
2023	0.123	-0.053	0.164	0.0.021	0.231	0.064	0.217	-0.843	-0.078	-0.737	0.197
	[0.202]	[0.222]	[0.222]	[0.191]	[0.324]	[0.257]	[0.190]	[0.233]	[0.317]	[0.232]	[0.203]
N	904,141	904,141	904,141	1,064,586	431,292	472,849	1,054,864	905,031	904,141	681,317	904,141
R^2	0.863	0.948	0.851	0.844	0.866	0.862	0.856	0.859	0.863	0.854	0.865
Weights	None	2018 Population	2017 Total Sales	None	None	None	None	None	None	None	None
Geography Restrictions	Metro CBSAs	Metro CBSAs	Metro CBSAs	All CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs
Other Restrictions	None	None	None	None	Removes states with COVID-related	Includes states with COVID-related	Omitted period includes 2017-18	Zip5-by-month and CBSA-by-time fixed effects only	None	None	None

Notes: All outcomes are monthly sales per 1,000 households. Standard errors clustered at the CBSA-by-year level are in brackets. All specifications include the full control set as described in the text, including ZIP-by-month and CBSA-by-time fixed effects except column (8) that includes only the fixed effects. Weights in column (2) use the counts of tax returns filed by zip code from SOI 2018 tax data and in column (3) are based on average monthly sales at the zip code level from 2017. States with COVID travel restrictions (27 plus D.C.) are based on info from Ballotpedia, see Appendix for details. Exposure interactions in column (9) is defined as the average number of children on tax returns at the zip code level from 2018 using SOI 2018 tax data so that the estimates in column (9) represent the effect of one additional child, on average, per tax return on monthly sales.

Table C.2: New Sales Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Baseline	Population Weighted	Sales Weighted	All CBSA	No state Level COVID travel Restrictions	Had state Level COVID travel Restrictions	2017-18 as Omitted period	Fixed Effects only	Exposure: Number of children per	Keep Areas with more children	Additional Controls return
2019	0.151	0.164	0.142	0.160	0.137	0.160	0.265	0.137	0.272	0.210	0.141
	[0.079]	[0.073]	[0.086]	[0.075]	[0.132]	[0.101]	[0.061]	[0.081]	[0.157]	[0.119]	[0.075]
2020	0.157	0.258	0.147	0.167	0.264	0.069	0.267	0.353	0.087	0.123	0.194
	[0.075]	[0.070]	[0.085]	[0.071]	[0.134]	[0.089]	[0.061]	[0.076]	[0.140]	[0.111]	[0.073]
2021	0.470	0.581	0.444	0.486	0.620	0.349	0.582	0.424	0.659	0.405	0.506
	[0.076]	[0.069]	[0.085]	[0.072]	[0.133]	[0.092]	[0.063]	[0.075]	[0.138]	[0.114]	[0.074]
2022	0.619	0.787	0.614	0.600	0.725	0.544	0.729	0.547	1.014	0.536	0.648
	[0.082]	[0.073]	[0.091]	[0.078]	[0.134]	[0.108]	[0.071]	[0.082]	[0.153]	[0.123]	[0.079]
2023	0.435	0.657	0.386	0.428	0.555	0.339	0.547	0.325	0.904	0.266	0.429
	[0.080]	[0.070]	[0.091]	[0.077]	[0.143]	[0.098]	[0.071]	[0.081]	[0.148]	[0.118]	[0.079]
N	904,685	904,141	904,141	1,064,586	431,292	472,849	1,054,864	905,031	904,141	681,317	904,141
R^2	0.75	0.91	0.72	0.71	0.73	0.76	0.74	0.74	0.75	0.73	0.75
Weights	None	2018 Population	2017 Total Sales	None	None	None	None	None	None	None	None
Geography Restrictions	Metro CBSAs	Metro CBSAs	Metro CBSAs	All CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs
Other Restrictions	None	None	None	None	Removes states with COVID-related	Includes states with COVID-related	Omitted period includes 2017-18	Zip5-by-month and CBSA-by-time fixed effects only	None	None	None

Notes: All outcomes are monthly sales per 1,000 households. Standard errors clustered at the CBSA-by-year level are in brackets. All specifications include the full control set as described in the text, including ZIP-by-month and CBSA-by-time fixed effects except column (8) that includes only the fixed effects. Weights in column (2) use the counts of tax returns filed by zip code from SOI 2018 tax data and in column (3) are based on average monthly sales at the zip code level from 2017. States with COVID travel restrictions (27 plus D.C.) are based on info from Ballotpedia, see Appendix for details. Exposure interactions in column (9) is defined as the average number of children on tax returns at the zip code level from 2018 using SOI 2018 tax data so that the estimates in column (9) represent the effect of one additional child, on average, per tax return on monthly sales.

Table C.3: Total Sales Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Baseline	Population Weighted	Sales Weighted	All CBSA	No state Level COVID travel Restrictions	Had state Level COVID travel Restrictions	2017-18 as Omitted period	Fixed Effects only	Exposure: Number of children per	Keep Areas with more children	Additional Controls return
2019	0.295	0.190	0.283	0.307	0.293	0.265	0.495	0.173	0.497	0.477	0.269
	[0.196]	[0.149]	[0.215]	[0.191]	[0.309]	[0.249]	[0.155]	[0.181]	[0.350]	[0.246]	[0.193]
2020	0.776	0.359	0.845	0.803	1.131	0.441	0.987	0.484	1.323	0.189	0.847
	[0.206]	[0.219]	[0.228]	[0.197]	[0.343]	[0.247]	[0.171]	[0.226]	[0.348]	[0.274]	[0.200]
2021	1.735	1.457	1.856	1.703	2.158	1.358	2.953	1.429	2.816	1.174	1.744
	[0.182]	[0.154]	[0.208]	[0.176]	[0.306]	[0.212]	[0.149]	[0.187]	[0.298]	[0.245]	[0.180]
2022	1.071	1.046	1.114	0.990	1.346	0.904	1.275	0.277	1.357	0.205	1.212
	[0.210]	[0.182]	[0.229]	[0.200]	[0.329]	[0.272]	[0.187]	[0.202]	[0.351]	[0.251]	[0.207]
2023	0.492	0.438	0.510	0.399	0.717	0.352	0.697	-0.496	0.633	-0.520	0.568
	[0.229]	[0.238]	[0.256]	[0.219]	[0.375]	[0.285]	[0.214]	[0.253]	[0.368]	[0.270]	[0.231]
N	904,141	904,141	904,141	1,064,586	431,292	472,849	1,054,864	905,031	904,141	681,317	904,141
R^2	0.83	0.94	0.82	0.82	0.84	0.83	0.83	0.83	0.83	0.83	0.83
Weights	None	2018 Population	2017 Total Sales	None	None	None	None	None	None	None	None
Geography Restrictions	Metro CBSAs	Metro CBSAs	Metro CBSAs	All CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs	Metro CBSAs
Other Restrictions	None	None	None	None	Removes states with COVID-related	Includes states with COVID-related	Omitted period includes 2017-18	Zip5-by-month and CBSA-by-time fixed effects only	None	None	None

Notes: All outcomes are monthly sales per 1,000 households. Standard errors clustered at the CBSA-by-year level are in brackets. All specifications include the full control set as described in the text, including ZIP-by-month and CBSA-by-time fixed effects except column (8) that includes only the fixed effects. Weights in column (2) use the counts of tax returns filed by zip code from SOI 2018 tax data and in column (3) are based on average monthly sales at the zip code level from 2017. States with COVID travel restrictions (27 plus D.C.) are based on info from Ballotpedia, see Appendix for details. Exposure interactions in column (9) is defined as the average number of children on tax returns at the zip code level from 2018 using SOI 2018 tax data so that the estimates in column (9) represent the effect of one additional child, on average, per tax return on monthly sales.

Table C.4: Fuel-Type Regressions

	(1)	(2)	(3)	(4)	(5)	(6)
	Total		New		Used	
	Gas/Diesel	Hybrid/Electric	Gas/Diesel	Hybrid/Electric	Gas/Diesel	Hybrid/Electric
2019	0.256	0.009	0.144	-0.010	0.090	0.016
	[0.195]	[0.026]	[0.080]	[0.023]	[0.153]	[0.007]
2020	0.656	0.039	0.140	0.004	0.519	0.031
	[0.195]	[0.028]	[0.073]	[0.023]	[0.171]	[0.008]
2021	1.616	0.076	0.424	0.044	1.238	0.041
	[0.173]	[0.025]	[0.076]	[0.022]	[0.147]	[0.008]
2022	0.891	0.142	0.519	0.094	0.494	0.053
	[0.206]	[0.026]	[0.083]	[0.022]	[0.172]	[0.007]
2023	0.296	0.123	0.334	0.072	0.067	0.051
	[0.218]	[0.040]	[0.082]	[0.034]	[0.193]	[0.011]
2018 mean value	29.49	0.62	7.20	0.30	22.30	0.32
R^2	0.840	0.707	0.743	0.709	0.866	0.506
N						904,141
Number ZIP codes						12,568
Number CBSAs						362

Notes: All outcomes are monthly sales per 1,000 households. Gas/diesel category also includes flex fuel vehicles. Hybrid/electric includes gas/electric hybrids, plug-in hybrids, and electric vehicles. Sample size, number of ZIP codes and number of CBSAs is the same in all specifications. Standard errors clustered at the CBSA-by-year level are in brackets. All specifications include the full control set as described in the text, including ZIP-by-month and CBSA-by-time fixed effects.

Table C.5: Family Vehicle Regressions

	(1)	(2)	(3)	(4)	(5)	(6)
	Total		New		Used	
	Family	Non-Family	Family	Non-Family	Family	Non-Family
2019	0.168	0.117	0.084	0.045	0.065	0.052
	[0.137]	[0.097]	[0.054]	[0.041]	[0.116]	[0.075]
2020	0.430	0.309	0.084	0.048	0.323	0.285
	[0.143]	[0.090]	[0.053]	[0.035]	[0.126]	[0.078]
2021	1.187	0.556	0.347	0.089	0.844	0.485
	[0.132]	[0.083]	[0.054]	[0.035]	[0.112]	[0.070]
2022	0.707	0.305	0.394	0.200	0.325	0.197
	[0.144]	[0.094]	[0.056]	[0.038]	[0.127]	[0.078]
2023	0.223	0.177	0.230	0.169	-0.008	0.077
	[0.165]	[0.095]	[0.059]	[0.040]	[0.141]	[0.085]
2018 mean value	18.51	12.01	4.90	2.73	13.61	9.28
R^2	0.833	0.693	0.656	0.743	0.852	0.710
N						904,141
Number ZIP codes						12,568
Number CBSAs						362

Notes: All outcomes are monthly sales per 1,000 households. Family cars include all non-sport, non-luxury, and non-compact models. Sample size, number of ZIP codes and number of CBSAs is the same in all specifications. Standard errors clustered at the CBSA-by-year level are in brackets. All specifications include the full control set as described in the text, including ZIP-by-month and CBSA-by-time fixed effects.

Table C.6: Used Car Quality Results

	(1)	(2)	(3)
	Age of Car	Mileage	Quality Index
2019	0.019	854.20	-0.055
	[0.028]	[679.7]	[0.388]
2020	0.048	-21.50	-0.319
	[0.029]	[623.10]	[0.384]
2021	-0.052	-1,372.00	0.924
	[0.027]	[620.70]	[0.367]
2022	-0.143	-2,503.00	2.430
	[0.027]	[663.10]	[0.420]
2023	-0.080	-1,601.00	1.610
	[0.030]	[694.00]	[0.454]
2018 Mean	9.32	137,891	100
R^2	0.76	0.75	0.34
N			905,472
Weights	None	Metro CBSAs	Metro CBSAs
Geography	None	Metro CBSAs	Metro CBSAs
Other Restrictions	None	None	None

Notes: Outcome in column (1) is age of car in years and in column (2) is the odometer reading in miles for used car sales. The outcome in (3) is a quality index based on a mapping of make, model, and age into average price in 2019 (indexed to 2018=100) using data from the National Automobile Dealer Association and JD Power that is extrapolted to all used car observations. Sample size, number of ZIP codes and number of CBSAs is the same in all specifications. Standard errors clustered at the CBSA-by-year level are in brackets. All specifications include the full control set as described in the text, including ZIP-by-month and CBSA-by-time fixed effects.