Stimulating Auto Markets*

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September 2025

Abstract

How does fiscal stimulus affect durable goods sales and to what extent does stimulus drive inflation? We study this question in the context of how the unprecedented pandemic fiscal stimulus affected household car purchases and auto prices. Using administrative data on vehicle registrations, we exploit the timing of nearly $900 billion in stimulus payments and geographic differences in program exposure to identify causal effects on sales. We find the stimulus increased purchases by 5.6 million vehicles (3.2%) during 2020–2022, implying a medium-run (3-year) marginal propensity to spend (MPX) on autos of 0.17 and a total marginal propensity to consume (MPC) of 0.43. Despite this substantial demand response, fiscal transfers account for less than 25% of price increases. We use a general equilibrium model to show show how secondary markets dampen the price effects of the stimulus. The stimulus simultaneously boosts demand and supply as recipients purchase new vehicles while trading in old ones, thereby expanding used car supply and easing price pressure. Non-fiscal factors, including supply constraints, relaxed credit conditions, and preference shifts, can explain the majority of the observed inflation.

*We thank Jediphi Cabal, Gabriel Chodorow-Reich, Sebastian Dyrda, Francois Gourio, Anil Kashyap, Ethan Ilzetzki, Andrea Lanteri, Guido Lorenzoni, Peter Maxted, David Mericle, Emi Nakamura, Pascal Noel, Matt Notowidigdo, Jon Steinsson, Chad Syverson, Gianluca Violante, Nicolas Werquin, and seminar and conference participants for comments, ideas, and help with data. We thank Paul Goldsmith-Pinkham for sharing asset-backed security data. This research was conducted while Gee was an employee at the U.S. Department of the Treasury. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors and do not necessarily reflect the views or the official positions of the U.S. Department of the Treasury or the Federal Reserve Board. Any taxpayer data used in this research was kept in a secured IRS data repository, and all results have been reviewed to ensure that no confidential information is disclosed. The data from confidential individual tax returns and from Experian and cannot be shared with those without proper clearance. Zwick gratefully acknowledges financial support from the Peter G. Peterson Foundation, the Booth School of Business at the University of Chicago, and the Fama Miller Center at Booth.

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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 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," and the Advanced Child Tax Credit (CTC), providing nearly $900 billion in largely unconditional direct transfers to individuals. A prominent narrative emerged that these stimulus measures were "too big," primarily driving inflation rather than supporting recovery. Understanding how these payments affected household consumption patterns and aggregate inflation dynamics is essential for evaluating this claim and informing future fiscal policy design.[1]

We investigate two central questions about these transfers. First, what was the impact of fiscal stimulus on consumption? Second, what role did transfers play in driving inflation, particularly in sectors that experienced the largest price increases during recovery? These questions directly test whether stimulus was indeed "too big" and primarily inflationary rather than stimulative. The distinction matters crucially: 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 relationship is critical for evaluating expansionary fiscal policy and informing models of inflation that feature realistic consumer goods markets.

Our results challenge the view that expansionary fiscal policy drove inflation during the later stages of the pandemic and the recovery. We document substantial real effects but find the observed boost to demand from these policies can only explain a small share of the rise in prices. Our results suggest fiscal transfers increased auto sales by 5.6 million vehicles (3.3%) between 2020–2022. These effects persist without reversal, unlike temporary scrappage programs such as "Cash for Clunkers," highlighting the importance of policy design. We also find that the stimulus accounts for less than 25% of auto price growth. Instead, we find that supply constraints, monetary accommodation, and preference shifts explain the majority of price

[1] Armantier, Goldman, Koşar, Lu, Pomerantz, Van der Klaauw et al. (2020), Chetty, Friedman and Stepner (2021), Chetty, Friedman, Stepner and Team (2023), 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).

increases. Crucially, secondary markets help explain this limited price response: while fiscal transfers increase demand for cars, they simultaneously expand the supply of used vehicles as stimulus recipients purchase new cars and trade in their existing ones, dampening overall price pressure.

Our focus is the automobile market (Figure 1, Panel A). Both new and used car prices experienced some of the highest inflation rates during the pandemic, with used car prices rising approximately 50 percent—significantly outpacing overall inflation (Figure 1, Panel B). Rising auto prices substantially contributed to aggregate inflation, boosting overall price increases in excess of their weight in the consumption basket (Figure 1, Panel C). This inflation, rather than affordability concerns, was the primary reason that consumers in the Michigan Survey reported it being a "bad time" to buy a car (Figure 1, Panel D).

Another reason to study auto markets is their sensitivity to fiscal and monetary policy. 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, ultimately testing whether fiscal policy can boost demand without excessive inflation even in constrained environments. This analysis also speaks to the optimal design of household-targeted fiscal policy by providing moments to discipline its study. Macroeconomic models are increasingly deployed to answer questions around policy design in durable goods contexts, such as the appropriate size of transfers and the relative efficiency of unconditional versus conditional transfers, such as more targeted tax credits, sector-specific subsidies, or temporary scrappage programs. Such questions remain largely unanswered.

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 3 percent per year compared to pre-pandemic (2019) levels.[2] In aggregate, this amounts to around 5.6 million additional units sold between 2020 and 2022. These sales occurred quickly after payments, remained elevated for several months without subsequent reversals, suggesting either pulled-forward demand from a few years in the future, 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 roughly $150 billion of spending (net of trade in values). Compared to the total value of transfer payments, we estimate a 3-year MPX on used autos of 0.09. The analogous calculation for new autos gives an MPX of 0.07, and the total auto MPX is 0.17. Following the approach in Laibson, Maxted and Moll (2022), the MPC for all consumption implied by these MPXs is 0.43.

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. Thus, including these as controls has little effect on our estimates. 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 regis-

[2] The 95% confidence interval ranges from an increase of 2.3% to 4.1%.

trations 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 with our estimated quantity effect remain modest. Across a range of assumptions, we can explain less than 20 percent of the price runup.

We then model auto market dynamics more explicitly 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. This secondary market mechanism is crucial to understanding why fiscal policy can stimulate demand without proportional price increases.

In our baseline scenario, we calibrate a stimulus-like income shock and assume perfectly elastic new car supply. We show that the stimulus shock generates a DD estimate in line with our 3.2% reduced-form response, as well as a reasonable path of MPCs over time. 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. We calibrate a supply shock that yields an aggregate quantity response matching our empirical DD estimate in the absence of other shocks.

We find that simulations incorporating this initially highly inelastic new car supply yield only minimal price increases (around 1%), far below the price surge observed in the data. Even layering on top of the stimulus a substantial wealth shock to capture excess pandemic savings only increases car prices by 6%. In the face of temporary price increases, forward-looking consumers substitute their purchases intertemporally, which further mitigates aggregate price impacts. We conclude that fiscal transfers alone are insufficient to be the first-order driver of the price surge, challenging claims that pandemic stimulus was the primary driver of inflation.

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 in the literature (Tauber and Van Zandweghe, 2021; The Conference Board, 2023).

These additional shocks significantly amplify auto price responses. New and used auto prices rise by nearly 30 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. 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 surely add to the inflationary pressure, but do not appear central in explaining car price inflation. Thus, fiscal policy can boost consumption without excessive inflation if other macroeconomic conditions are amenable, even when supply constraints are present.

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. 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. Dunbar, Kurz, Li and Tito (2024) use a staggered payment design to study the effect of the EIP payments on car sales in survey data and find consistent results with ours. 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 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). This 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).[3] Prior work also finds less-liquid households have higher consumption responses to fiscal transfers (Broda and Parker, 2014) and (Johnson, Parker and Souleles, 2006), a result that numerous papers find in response to the first round of pandemic stimulus payments including Chetty, Friedman, Stepner and Team (2023), Baker, Farrokhnia, Meyer, Pagel and Yannelis (2023), Parker, Schild, Erhard and Johnson (2022), Cox, Ganong, Noel, Vavra, Wong, Farrell and Greig (2020), and Coibion, Gorodnichenko and Weber (2020).[4] Notably, while these papers document increased spending overall in response to the payments, they find mixed results for durable goods purchases.[5] We contribute by directly focusing on durable goods spending using administrative data on auto transactions during a period when households received sizable stimulus payments. We estimate both an MPX for autos and an MPC that reflects spending more generally by building on the work of Laibson, Maxted and Moll (2022) that provides a translation between the MPX and the MPC and differentiates between the two measures for durable goods. Intuitively, differentiating the MPC and MPX for a durable good is important because consumption of the good happens over a relatively long horizon while the spending (generally) occurs quickly. This contrasts with non-durable goods that have roughly simultaneous spending and consumption.

[3]Several prior studies find strong spending responses (MPC of 0.4 to 0.9) (Johnson, Parker and Souleles, 2006; Parker, Souleles, Johnson and McClelland, 2013) in response to earlier tax rebates/stimulus payments. More recent work from Borusyak, Jaravel and Spiess (2024) and Orchard, Ramey and Wieland (2025) that accounts for treatment effect heterogeneity suggest that the MPCs from these tax rebates is roughly half as large, with consumers spending about 25-35 percent over the first quarter following payments. Boehm, Fize and Jaravel (2025) use a random control experiment to study the effect of relatively smaller payments (€300) and find an increase in both durable and non-durable goods, ranging from roughly 20 percent to 60 percent depending on the treatment, over the first month after payments.

[4]Misra, Singh and Zhang (2022) also find increased spending in higher cost of living areas, for both the first round of pandemic stimulus payments and earlier tax rebates. In addition, several studies find relatively high MPCs in response to other pandemic-era transfers including later stimulus payments (Chetty, Friedman and Stepner, 2021); (Karger and Rajan, 2020) and from unemployment insurance benefits (Ganong, Greig, Noel, Sullivan and Vavra, 2024).

[5]For example, Chetty, Friedman, Stepner and Team (2023) use data from credit card and debit card transactions and report spending on durable goods rose more than spending on services. In contrast, using data from Fintech Baker, Farrokhnia, Meyer, Pagel and Yannelis (2023) report little spending increases on durable goods. However, while the administrative data used in these studies offers numerous advantages, including high frequency, low measurement error, and large sample sizes that allow it to accurately characterize non-durable spending, it may not capture all durable goods purchases such as auto sales that are the focus of our study.

Our paper also 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. Lin (2024) uses regional and regression kink designs to study the impact of EIP payments on the housing market, finding that the payments boosted housing consumption for low-income households and house prices. Using a hetergenous agent New Kenynesian model Bardóczy, Sim and Tischbirek (2025) find that spending out of excess savings built up during the pandemic drive about one-third of the trough-to-peak inflation, but that the fiscal transfers account for only a small fraction of the rise in prices. 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, interpreted through a micro-founded general equilibrium model of the auto market.

Finally, a central question in macroeconomics concerns whether inflation is primarily driven by supply or demand factors. Recent empirical work has increasingly emphasized the importance of supply-side constraints. Shapiro (2024) provides evidence that supply-driven factors were dominant in recent inflation episodes, while Comin, Johnson and Jones (2023) demonstrate that pandemic-era supply chain disruptions were a key driver of price increases through manufacturing bottlenecks and shipping delays. Cuba-Borda and L'Huillier (2025) offer a theoretical framework arguing that inflation occurs primarily when supply deficiencies prevent production from expanding to meet demand, rather than from demand shocks alone. This perspective is supported by empirical evidence showing that demand shocks have muted price effects when capacity constraints are not binding (Boehm and Pandalai-Nayar, 2022), and that the Phillips curve exhibits a remarkably flat slope (Hazell, Herreño, Nakamura and Steinsson, 2022). Coibion and Gorodnichenko (2015) further document the "missing disinflation" during the Great Recession, showing that even large negative demand shocks do not necessarily translate to proportional price declines when supply adjusts endogenously. Our study contributes to this literature by providing micro-level evidence on how fiscal transfers affect both demand and supply in durable goods markets, with a particular focus on how secondary markets create endogenous supply responses that dampen inflationary pressures.

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.[6] 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.

The first set of payments was enacted in the CARES Act, passed March 25, 2020, with payments sent in late March and early April. These payments provided up to $1,200 per adult and $500 per qualifying 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.[7] 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, passed December 21, 2020, with payments sent in January 2021. These payments provided up to $600 per adult and $600 per qualifying child. Married (single) households with AGI less than $150,000 ($75,000) still received the maximum credit, but the credits were fully phased out at $160,000 ($80,000).

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 dependent sent to households in March and April of 2021.[8] These payments were subject to the same income limits and phase-out ranges as the second version.

Households with dependent children also qualified for advanced payments of the expanded Child Tax Credit (CTC). This expansion increased the maximum value of the credit per child to $3,600, compared to $2,000 previously, and made the credit fully refundable so that lower-

[6]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 distinguishes the stimulus 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.

[7]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.

[8]The first two stimulus payments included additional amounts for dependent children under age 17. The third version expanded eligibility for the additional amounts to dependents generally.

income households could benefit from the full CTC.[9] 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, the payments totaled $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 children received less than $4,000.[10]

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 child ($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 Paycheck Protection Program (PPP). There were also student debt moratoria 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 pandemic. These

[9]In 2021, 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 age 17 qualified for the credit in 2021). Married (non-married) households with AGI less than $150,000 ($112,000) were eligible for the full amount of the Child Tax Credit expansion, with the increased benefits phasing out at a 5-percent rate for higher incomes.

[10]To illustrate the inflow of payments to households this figure includes the advanced portion of the Child Tax Credit. These advanced payments were a net-increase in the amount of the credit for households that benefited from the full amount of the expansion, but were only a re-timing of benefits for households that did not benefit from the CTC expansion.

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 our baseline specifications, we control for both ex ante zip code level unemployment rates as well as for county-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 PPP tend to find most 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 programs' contributions to our 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.[11]

[11] 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

For our main outcomes, 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. In addition, larger durable goods purchases like autos may not appear in debit or credit card transactions.

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 roughly 90% of national sales based on data from the Bureau of Transportation Statistics (BTS). Similar to the BTS data, there are three used car sales for every new car sale in our sample.

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.[12]

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 change in 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 refi-

$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.

[12] 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 by year.

nances at the ZIP-by-month level using data from Intercontinental Exchange (ICE) McDash. From these data we create counts of both cash-out and no-cash-out refinances as well as mortgage originations.[13] We also control for PPP loans with data from the Small Business Administration, from which we take the number and average amount of loans. For mortgages 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 per tax return, 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 anonymized random sample of individuals. 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.[14]

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.

[13] Appendix Figure B.8 shows aggregate mortgage originations and refinances by cash-out status. We discuss the additional auto demand boost from refinancing activity in Section 7.2.

[14] 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.

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 22,054 ZIP codes spanning 72 months (2018–2023). The sample covers approximately 90% 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 just over 30 with a standard deviation of 12. New and used sales per 1,000 tax returns average 6.5 and 24, respectively. Over half of all transactions are cash-financed, including 67% of used sales and 19% of new sales. Thus, the registrations data provides an important window into the full market, which cannot be captured in credit record data that includes only loan sales.

Mean exposure is $962 per tax return with a P10 to P90 range of $586 to $1,339, 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. This regional approach has been used 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.[15]

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 3 plots colored maps of variation at the ZIP level within San Francisco, Chicago, and the Washington DC metro areas. Darker areas indicate higher exposure and tend to concentrate in family-friendly 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 4, Panels A and B show how program exposure correlates with demographic vari-

[15] Appendix A.1 discusses this assumption in more detail. We use data from asset-backed securities to show that geographic factors account for relatively little of the price dispersion across autos within bins defined by make, model, and model-year.

ables, 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 AGI (due to income limits on the CTC) and strongly positively correlated with the number of children per tax return. Exposure is not materially correlated with the married share of households.

The second panel uses a variety of data sources to characterize ex ante conditions and shows that exposure is not meaningfully correlated with ex ante population level or growth, or with demographic characteristics including white or elderly (65+) populations. In addition, exposure is not correlated with ex ante vehicle miles traveled per household, which may proxy for transportation preferences. The exposure measure is only weakly correlated with ex ante unemployment rates, and with Republican party vote share in the 2016 presidential election. 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 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.[16] 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 households 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. Student loan payments were suspended during much of the pandemic, so cash flow likely improved for many borrowers. However, the correlation between student loans and exposure is weak and negative. Similarly, the correlation between exposure and mortgage refinances is minimal. 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 (either interactions of predetermined characteristics with time dummies or inclusion of

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

monthly time-varying controls).

Figure 5 shows a strong positive correlation between our exposure measure and the transfer payments we study. This figure plots a binscatter with 50 bins of ZIP-level stimulus transfers per tax return against exposure and a fitted regression line. 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 slope of the line is 1.90 (s.e.=0.14), indicating that a $1,000 increase in exposure is associated with a $1,900 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 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 level.

Eligibility for stimulus depends on both household income and household structure, with the latter being a strong correlate with our exposure measure. To remove concerns about confounding income-specific shocks, we control for the log of average ex ante (2018) adjusted gross income (AGI) in our baseline specifications.[17] Thus, our identifying variation comes largely 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, 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 (cash-out), as well as PPP loans (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 are not sensitive to our choice of 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 schooling or childcare, or from differential risk assessment of the virus. Assuaging this concern, we find our results are not driven solely by areas under travel restrictions, and we find the sales effect is largest in 2021 when nearly all restrictions had been lifted (and vaccines were widespread). Still, one might also worry that households with more children exhibit different elasticities with respect to income shocks. We show in Appendix A.4 that, under CES preferences, preferences for autos need not be homogeneous for the income elasticity to be the same across groups. We provide simulations suggesting the bias from assuming a response to aggregate prices that doesn't vary with household size is likely small.

[17] Chetty, Friedman, Stepner and Team (2023) find that lower income households suffered more severe and persistent income losses during the pandemic. Given the negative correlation between income and stimulus, allowing identifying variation from differences in income would lead us to estimate even smaller consumption responses.

4 The Effect on Auto Sales

4.1 Main Results

Before turning to our regression results, we present simple plots of sales 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 used sales (Panel A), new 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 of auto sales. Third, higher exposure areas recover more quickly after these rapid declines and remain elevated in mid-2020 before converging later in the year. Fourth, the gap between high and low exposure areas expands again in early 2021 following the second and third stimulus payments, and 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 used sales, new 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 differential 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.[18] 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 (blue bars). By 2023 when the payments ended, used sales return to

[18] The negative coefficients in April and May 2020 may reflect anticipation effects, as there was a relatively longer lag between the proposed stimulus and when households recieved the payments in the first round of payments compared to later stimulus payments. In addition to longer processing times for IRS to set-up the payment systems, the first round of payments also included a relatively higher share of payments made via paper check (22%) compared to the later payments (18% round 2, 14% round 3).

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 highest point estimate for total sales occurs in 2021, with an estimated increase of 1.40 (s.e.=0.17). This estimate implies a boost to monthly sales of 1.4 per 1,000 households, a 4.4% increase relative to the mean value in 2018. Analogous estimates for used and new sales suggest increases in 2021 of 4.0% and 6.1%, respectively, relative to their 2018 means. The estimates for 2020 and 2022 are approximately half the size of the 2021 effect, with a relatively larger share coming from used cars in 2020 and from new cars in 2022.

As shown in Table 3, 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 increased in 2020 (and remained elevated through 2022), whereas loan-financed sales first picked up in 2021 (and remained elevated in 2022), 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 around 80% of overall sales in 2020, about 50% of the response in 2021, and just over 25% in 2022.

Robustness. Appendix Tables C.3–C.5 show our findings are robust to alternative specifications and subsamples.

First, we consider weighted regressions by either 2018 tax units or 2018 auto sales. These lead to quite similar results to our baseline.

Second, we consider sample splits based on state-level travel restrictions during the pandemic, These results suggests stronger effects in states with fewer restrictions, consistent with the idea that car purchases typically require in-person visits to dealerships.

Third, we consider specifications with different exposure measures and control sets. We use alternative versions of the exposure measure, one based on 2017 SOI data instead of 2018, and the other based on only the average number of dependent children per tax return in 2018 rather than CTC amount. These measures deliver estimates that are quantitatively comparable to our baseline (after accounting for different units). Variants that include no controls other than the ZIP-by-month and CBSA-by-time fixed effects show smaller effects, consistent with the idea that balancing the research design by income neutralizes the influence of pandemic-specific shocks to lower income households Chetty, Friedman, Stepner and Team (2023). Variants that include additional controls for mobility, PPP loans, housing finance activity, and exposure to the student loan moratorium give nearly identical results as our baseline.

Fourth, we show the sales response is concentrated in traditional fuel types (gas, diesel, flex fuel) rather than hybrid or electric sales and.

Last, we specify the outcome variable as the natural log of sales rather than sales per 1,000 tax returns. This approach gives a quantitative estimate that is quite close to the baseline.

The appendix tables also shows that sales of family cars (excluding sport and luxury models) exhibit a stronger increase. In Appendix Table C.2, we find evidence that the used autos sold during this time are only very slightly newer (model year) and have only a bit less miles (odometer reading) than typical.

4.2 Aggregate Estimates

To generate national sales estimates we use regression results 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 (6,393) and exposure ($961), and the number of zip codes (22,054) we aggregate these regression results to obtain in-sample estimates of total auto sales. Finally, because our sample contains roughly 90 percent of all households, we scale our estimates to make them nationally representative.

This exercise implies the stimulus payments increased total auto sales by 1.9 million per year during this period (1.2 million per year from used sales and 0.7 million per year from new sales), or 5.6 million over the three year period.[19] This represents a 3.2 percent boost to sales overall (2.9 percent for used, 4.2 percent for new) relative to 2019 national estimates from the Bureau of Transportation Statistics (BTS).[20]

Table 4 collects these aggregate estimates and the associated 95% confidence intervals. We use these aggregate estimates to calibrate our partial and general equilibrium models.

[19] 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.

[20] See "New and Used Passenger Car and Light Truck Sales and Leases," Table 1-17. https://www.bts.gov/content/new-and-used-passenger-car-sales-and-leases-thousands-vehicles

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. Table 4 reports these estimates and the associated 95% confidence intervals.[21]

Define the 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. Our aggregate estimate of 5.6 million sales (3.5 million used, 2.1 million new) over 2020–2022 corresponds to roughly $150 billion of net spending on autos ($80 billion in used sales, $70 billion in new sales). Compared to the total value of transfer payments, we estimate a 3-year MPX on autos of 0.17. The analogous calculation for new autos gives an MPX of 0.07, and the used auto MPX is 0.09.

We map the MPX into an MPC for all consumption following Laibson, Maxted and Moll (2022). The expression connecting these objects is

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

where t indexes months, r is the interest rate, s is the durable goods share of household consumption, and δ is the depreciation rate. 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. Notably, this method provides multiple estimates of the cumulative MPC for all goods based on the different underlying MPX values, consumption shares, and depreciation rates.

Using this MPX to MPC conversion, the implied 3-year MPC estimates are 0.58, 0.27, and 0.43 used, new, and total auto sales, respectively. Given the sensitivity of these estimates to small CEX consumption shares and because total spending reflects a broader set of auto purchases, our preferred estimate is the total auto MPC of 0.43.

We 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.

[21] Appendix A.2 provides further details on the estimation of the MPX and MPC and the derivation of (2).

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 within set time intervals. 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 drops discretely when a new transfer occurs.[22]

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 provides independent estimates of the total consumption response based on the patterns in new, used, or total auto sales spending. The last data point is December 2022, which includes all auto sales from April 2020 through the end of 2022, corresponding to the 3-year MPXs and MPCs reported above.

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 months. In contrast, the MPC estimates start lower and grow steadily over time, as buyers consume durable goods service flows more smoothly over time.

Our estimates of the MPC should be interpreted carefully for two reasons. First, the MPC is based solely on the auto purchase share, which is a relatively small component of overall household spending (3.1 percent used, 3.2 percent new). We also assume in these calculations that the relative auto share is unchanged over time.[23] 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.[24] Nevertheless, the MPCs we estimate imply that

[22]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).

[23]Consistent with this assumption, Appendix A.3 shows these shares are roughly stable in the pre-pandemic years in the CEX. However, if preferences shifted meaningfully toward autos, a scenario we consider in our general equilibrium model, then the MPCs we estimate would be overstated.

[24]For the MPX calculation, we measure spending based on the imputed value of the auto net of trade in. For

households consumed a relatively large fraction of the transfer payments over the 2020–22 period and are within the range of estimates that prior work has found in response to similar transfer payments both during the pandemic and in other contexts (Appendix Figure B.5).

5 Partial Equilibrium in the Auto Market

We now turn to the price effect of the stimulus payments. We start with a simple model to illustrate our central result on prices.[25]

Setup. Consider a simple model of 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 the log-linear form:

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

with a location-specific intercept $\bar{\alpha}_i$, a common slope ε_d, a common sensitivity of car purchases to income y_{it}, and 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 disturbance ν_{st}.

Stimulus and Pandemic Shocks. Demand and supply changes following the stimulus, which features a pandemic shock $\Delta\nu_d$ to demand and location-specific stimulus given by Δy_i. In equilibrium, the change in quantity demanded equals the change in quantity supplied ($\Delta q_d = \Delta q_s$). Solving for Δp gives:

$$\Delta p = \left(\frac{1}{\varepsilon_d + \varepsilon_s}\right)(\gamma \mathbb{E}[\Delta y_i] + \Delta\nu_d - \Delta\nu_s). \tag{5}$$

The expression for the change in quantity is:

$$\Delta q = \left(\frac{\varepsilon_s}{\varepsilon_d + \varepsilon_s}\right)(\gamma \mathbb{E}[\Delta y_i] + \Delta\nu_d) + \left(\frac{\varepsilon_d}{\varepsilon_d + \varepsilon_s}\right)\Delta\nu_s. \tag{6}$$

loan financed sales and leases buyers will have smaller out of pocket costs at the time of the transaction.

[25] Appendix A.4 presents a microfoundation for demand in the model. Appendix A.5 provides additional details on the model and the calibration.

DD Estimator versus Aggregates. Our identification assumption is that the demand disturbances are independent of the stimulus, conditional on relevant observables. The aggregate DD estimator across all locations is:

$$\hat{q} = \hat{\gamma}\mathbb{E}[\Delta y_i], \tag{7}$$

where $\hat{\gamma}$ is the coefficient on the change in income estimated in our research design. Thus, $\hat{q}$ gives the predicted change in quantity relative to a zero stimulus counterfactual, excluding aggregate shocks and equilibrium feedback. Notice that the DD estimate is independent of the supply elasticity.[26]

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

$$\Delta q = \left(\frac{\varepsilon_s}{\varepsilon_d + \varepsilon_s}\right)\hat{q} + \left(\frac{\varepsilon_s}{\varepsilon_d + \varepsilon_s}\right)\Delta \nu_d + \left(\frac{\varepsilon_d}{\varepsilon_d + \varepsilon_s}\right)\Delta \nu_s. \tag{8}$$

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

$$\Delta p = \left(\frac{1}{\varepsilon_d + \varepsilon_s}\right)\hat{q} + \left(\frac{1}{\varepsilon_d + \varepsilon_s}\right)\Delta \nu_d - \left(\frac{1}{\varepsilon_d + \varepsilon_s}\right)\Delta \nu_s. \tag{9}$$

Contribution of Stimulus to Price Increase. 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_y 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 0.9 based on market-level elasticities for car demand and scrappage elasticities from the literature.[27] We calibrate 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). After accounting for the weights of autos in the aggregate index, we take Δp to be 25%, the relative price change for autos.[28]

[26] Because prices follow a common national path and we absorb CBSA-by-month shocks, the DD estimate $\hat{\gamma}$ loads only on exposure-driven demand, not on supply. Hence $\hat{q} = \hat{\gamma}\,\mathbb{E}[\Delta y]$ is independent of the supply elasticity ε_s; ε_s matters only when mapping $\hat{q}$ into equilibrium price and aggregate-quantity changes.

[27] See Appendix Table C.7 for a list of estimates.

[28] The maximum cumulative price change for used cars in Figure 1, Panel B is 54%. The maximum cumulative price change for new cars is 26%. Aggregate prices rose approximately 18% from March 2020 through the end

The predicted price change due to the stimulus appears in Figure 9, Panel B. We plot impacts using the central estimate of $\hat{q}$ of 3.3% and a 95% confidence interval estimate of 4.3%. Under these assumptions, ζ_y ranges from 3% to 12%. Thus, while the stimulus-induced demand shock accounts for some of the price change, most of the increase appears driven by other factors.

Relative Importance of Supply and Demand Shocks. The system of equations governing equilibrium ((8) and (9)) provides a decomposition for the total price change into relative contributions from stimulus, supply, and demand shocks. The share of the price change due to the demand shock equals:

$$\zeta_d = \frac{(\Delta q - \hat{q})/\Delta p + \varepsilon_d}{\varepsilon_d + \varepsilon_s}. \tag{10}$$

The share of the price change due to the supply shock equals:

$$\zeta_s = \frac{-\Delta q/\Delta p + \varepsilon_s}{\varepsilon_d + \varepsilon_s}. \tag{11}$$

By construction, the three shares (ζ_y, ζ_d, and ζ_s) sum to one.

We calibrate Δq using local projections estimated prior to the pandemic and cumulated through the end of 2022 (Appendix Figure B.7). Using this estimate, the baseline calibration is a 1.5% cumulative increase in aggregate auto sales. To account for uncertainty in the aggregate forecast, we consider one-standard-deviation perturbations to the estimate of Δq. We then plug in our estimates of $\hat{q}$, the elasticities of demand and supply, and the cumulative price change.

Table 5 shows that under our preferred calibration and DD estimate, the relative contributions to the price change from the stimulus, demand shocks, and supply shocks are 3%, 21%, and 75%, respectively. Under a range of assumptions about supply and demand elasticities and permutations of the estimated inputs, the general conclusions hold. Specifically, the stimulus accounts for less than 10% of the total price change. Notably, even accounting for the predicted demand effect of other fiscal programs, including UI expansions, student loan forbearance, and PPP transfers, the role of fiscal policy in driving the inflation appears secondary.[29]

Non-fiscal demand shocks and supply shocks account for more than 90% of price increase, with supply shocks appearing twice as important as demand shocks on average. However, if

of 2022. With weights for used and new cars of 3.2% and 3.1%, respectively, the total change in non-auto prices is 15%. Thus, the relative price change for used and new cars is 39% and 11%, respectively, or 25% when aggregated.

[29] Using our estimated MPX for autos, we estimate these programs contributed an additional 2.7 million purchases to total demand. See Appendix A.6 for details on this calculation.

supply is less elastic initially, then supply and demand shocks can account for equal amounts of the price increases. Alternatively, if supply is quite inelastic initially, then demand shocks other than stimulus can account for up to 60%. We further explore the relative importance of these shocks in our general equilibrium model in the next section.

Limitations. While supportive of our central conclusion on prices, this 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 new and used cars separately, 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 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.

The importance of supply constraints to our results is consistent with Cuba-Borda and L'Huillier (2025), who demonstrate that inflation occurs primarily when supply deficiencies prevent production from expanding to meet demand increases, and Comin, Johnson and Jones (2023), who find that binding supply constraints explain half of the increase in U.S. inflation during 2021-2022. Our finding that secondary markets dampen price pressures through endogenous supply responses provides empirical support for this theoretical framework emphasizing supply-side factors in inflation dynamics.

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 consumption 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. $V(b, w, n; \theta)$ satisfies 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 equilibrium in this economy as one where (i) $V(b, w, n; \theta)$ satisfies 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 as follows: there are four possible 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.

The calibrated model delivers a difference-in-differences (DD) estimate that closely matches our empirical results in Section 4. Specifically, we simulate a temporary two-period income shock of 5.3% for half of the population and 3.3% for the other half, roughly consistent with the variation in empirical exposure to stimulus payments in Table 1. We use these groups to compute $\hat{q}$ in an analogous way to our aggregate estimate (i.e., equation 7). The resulting DD estimate within the simulated data is 3.2%.

Because the purpose of the model is to study how prices and expenditures respond to fiscal transfers, we validate its consumption behavior using this simulated experiment. The model produces a total MPC of about 0.04 in the first year, 0.12 in the second, and 0.20 in the third. These values closely track the dynamic empirical estimates for the total MPC shown in Panel B of Figure 8. Thus, the model captures household spending responses for both non-durables and automobiles in a realistic way, providing a useful laboratory for analyzing the effects of fiscal stimulus on aggregate quantities and market prices.

The new car supply elasticity ε_s is *not* identified by our cross-sectional demand design; it enters only when mapping the demand object $\hat{q}$ into equilibrium prices and aggregate quantities. We therefore discipline ε_s using aggregate evidence. In particular, we choose ε_s to match the observed 3.2% increase in aggregate new car quantities in response to the fiscal stimulus and in the absence of other shocks. This calibration anchors the general equilibrium response of the model to the empirical aggregate benchmark, while leaving the cross-sectional DD estimate $\hat{q}$ unaffected.

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.[30] To analyze these effects, we

[30]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).

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 2020 and 2021. Specifically, the shock is a temporary increase in household income, amounting to 4.3% of annual income on average, and persists for two years. This calibration reflects our empirical estimate that the mean transfer ($6,133) represented 8.5% of mean household income ($71,710) over the two-year policy period.[31]

Panel A shows results under perfectly elastic new car supply ($c_1 = 0$). In this scenario, total auto sales increase by approximately 4% over three years, exceeding our reduced form estimates of 3.2%. More importantly, however, the price response is actually *negative* for used cars. This seemingly counter-intuitive result demonstrates the fundamental economics of secondary markets: while fiscal stimulus increases demand for used cars, it also increases the supply of used cars because households who purchase new cars often trade in their existing vehicles. With perfectly elastic new car supply, prices for new cars remain fixed by definition, but the flood of trade-ins into the used car market more than offsets the increase in used car demand, leading to a net decline in their price.

Panel B introduces supply constraints by setting the new car supply elasticity parameter $c_1 = 21$ to match an aggregate 3.2% quantity response that matches our reduced-form DD response. At the steady state, this calibration corresponds to a supply elasticity of $\frac{1}{c_1} \cdot \frac{p_{ss}}{x_{ss}} = \frac{1}{25} \cdot \frac{0.45}{0.0688} = 0.3$ for new cars, reflecting the reality of pandemic-era supply constraints due to chip shortages and manufacturing disruptions.[32] As a baseline we assume that households

[31] Appendix Tables C.8–C.13 report peak and cumulative impulse response estimates for total, new, and used cars under the various counterfactuals for demand and supply shocks we discuss in this section.

[32] Analyst commentary during the pandemic recovery emphasized the importance of temporary supply shortages interacted with excess durable goods demand. A Goldman Sachs research report from 2021 highlights a semiconductor shortage that sharply curtailed auto production, driving new vehicle inventories to their lowest

believe that new car production will be supply-constrained for four years before returning to elastic supply.[33]

With inelastic new car supply, the general equilibrium price response becomes positive for all auto types but remains minimal, with total auto prices increasing by 1% during the policy period. This represents approximately one-thirtieth of the over 30% increase observed in the data. Even though supply constraints prevent new car production from expanding to meet increased demand, the fundamental economics of secondary markets continues to limit price pressures. The trade-in channel creates an endogenous supply response that partially offsets demand increases, making it difficult for fiscal transfers alone to generate large price increases.

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.

7.2 Explaining Auto Prices during the Pandemic

What forces can explain the large run-up in auto prices? We introduce additional shocks that could have contributed to the increase in auto sales and prices during the 2020–2022 period. Figure 11 shows results under (temporarily) inelastic new car supply since this represents the realistic case that is closer to the data.

Panel A considers a scenario where households enter the period with 10% excess pandemic savings, roughly the maximum amount attributable to fiscal transfers to households and firms excluding the stimulus checks and the CTC.[34] The combination of income transfers and wealth accumulation generates increases of approximately 6% for total auto prices. This represents

levels in decades and spilling over into the used car market (Briggs, 2021). Bottlenecks in global logistics and pandemic-related labor constraints compounded these production issues (Mericle and Nicolae, 2021). By mid-2023, automotive microchip supply normalized, allowing production to rise above pre-pandemic levels, dealer inventories to rebuild, automaker incentives to return, and wholesale auction prices for used cars to partly reverse (Hill, 2023).

[33]To explore robustness to this untestable information assumption, we also report results when households believe the supply constraints will last for 2 periods or be permanent. Our cumulative results are all quantitatively similar; what differs is the timing at which they occur.

[34]Appendix A.7 provides details on our calibrated excess savings shock. Abdelrahman, Oliveira and Shapiro (2024) show that household liquid wealth increased from $12 trillion to $16 trillion during the first year of the pandemic. They forecast liquid wealth in the absence of the pandemic and fiscal response would have grown to $14 trillion. Excluding the contribution from stimulus checks and the advanced CTC, an upper bound on the excess wealth attributable to other transfers is approximately 10%. We also show the distribution of excess wealth is broad-based, but skews toward the middle and top of the wealth distribution. This fact supports using a calibrated uniform wealth shock as a simple approximation, given the limited heterogeneity in our model.

only 25% of the relative price increase observed in the data.

Panel B examines the effects of a temporary relaxation of borrowing constraints, reflecting historically low interest rates and easy credit conditions.[35] We model this as a relaxation of the household borrowing constraint from -0.4 to -1.0. This shock can raise auto prices by almost 7% on its own. Remarkably, when combined with income transfers, the interaction effects are nonlinear, generating price increases of approximately 13%. This amplification occurs because borrowing relaxation matters most to lower-income households who primarily purchase used cars, creating concentrated demand pressure in the used car market.[36]

Panel C incorporates a temporary shift in expenditure shares toward goods, including autos, and away from services, consistent with the aggregate patterns documented in the literature (Tauber and Van Zandweghe, 2021; The Conference Board, 2023). 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. Surprisingly, a pure preference shock toward autos actually generates *negative* price effects even under inelastic new car supply. This result occurs because the preference shift induces households to substitute from purchasing used cars to new cars, which are a normal good, flooding the used car market with trade-ins and more than offsetting the direct demand increase.

Panel D shows results when we combine all shocks: fiscal transfers, wealth accumulation, credit relaxation, and preference shifts toward goods, all under temporary supply constraints. This comprehensive scenario generates substantial price increases: 27% for total cars, 24% for new cars, and 33% for used cars. The aggregate increase in quantities is 16% after accounting for all the shocks, well above the contribution of the transfers alone. As in the data, the relative increase in prices and quantities is also greater for used than new cars. Thus, the model can successfully replicate the overall pattern and magnitude of pandemic-era auto price inflation.

Crucially, supply constraints are essential for these positive price effects. Absent supply constraints, the overall price effects would actually be negative, as demonstrated in Panel A of Figure 10. The combination of increased trade-in supply from new car purchases and the substitution effects from preference shifts would overwhelm the direct demand increases, leading to net price declines. Supply constraints break this mechanism by limiting the ability of households to purchase new cars and trade in their used vehicles, thereby preventing the secondary

[35] Interest rates on auto loans fell meaningfully during the sample period. The dollar weighted average interest rate at finance companies for new (used) auto loans dropped roughly 30% (10%) between September 2019 and September 2021, from 6.4% to 4.5% (14.6% to 13.1%).

[36] Monetary policy likely boosted demand through a housing refinance channel as well. Appendix A.5 estimates that excess refinances provided an additional $275 billion to households, which would translate into an additional 1.7 million auto sales using our baseline MPX estimate.

market supply response that would otherwise dominate.

8 Conclusion

Confronted with widespread job losses, rapid economic decline, and elevated uncertainty about the evolution of COVID, policymakers in the U.S. enacted an historically large amount of fiscal stimulus to support businesses, state and local governments as well as households. The programs we study–the three rounds of stimulus payments and the CTC–were a central component of household support and were designed to support aggregate demand. We provide new evidence that these policies supported household consumption without being a primary driver of the elevated and sustained inflation that plagued the later stages of the pandemic and the subsequent economic recovery.

We provide two main sets of results. First, using administrative data on auto registrations, we exploit ex ante differences in exposure to the payments across narrow geographies to quantify the effect of the payments on auto sales. Our results suggest that the programs stimulated durable goods purchases by increasing auto sales by a bit over 3% relative to a no-stimulus counterfactual. The fact that we do 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. These sales effects imply a 3-year MPX on autos of 0.17 and a general spending MPC of approximately 0.4.

Second, we use our quantity estimates to ask how much of the increase in car prices can be attributed to the stimulus using a series of macro models. We find that fiscal transfers account for less than 25% of the observed auto price increases during the pandemic using both a simple partial equilibrium model and a richer dynamic model that incorporates the interactions between used and new car markets. Instead, our results suggest that non-fiscal factors, particularly supply constraints combined with the relaxation of credit constraints and shifts in household preferences toward goods, drove the majority of price increases.

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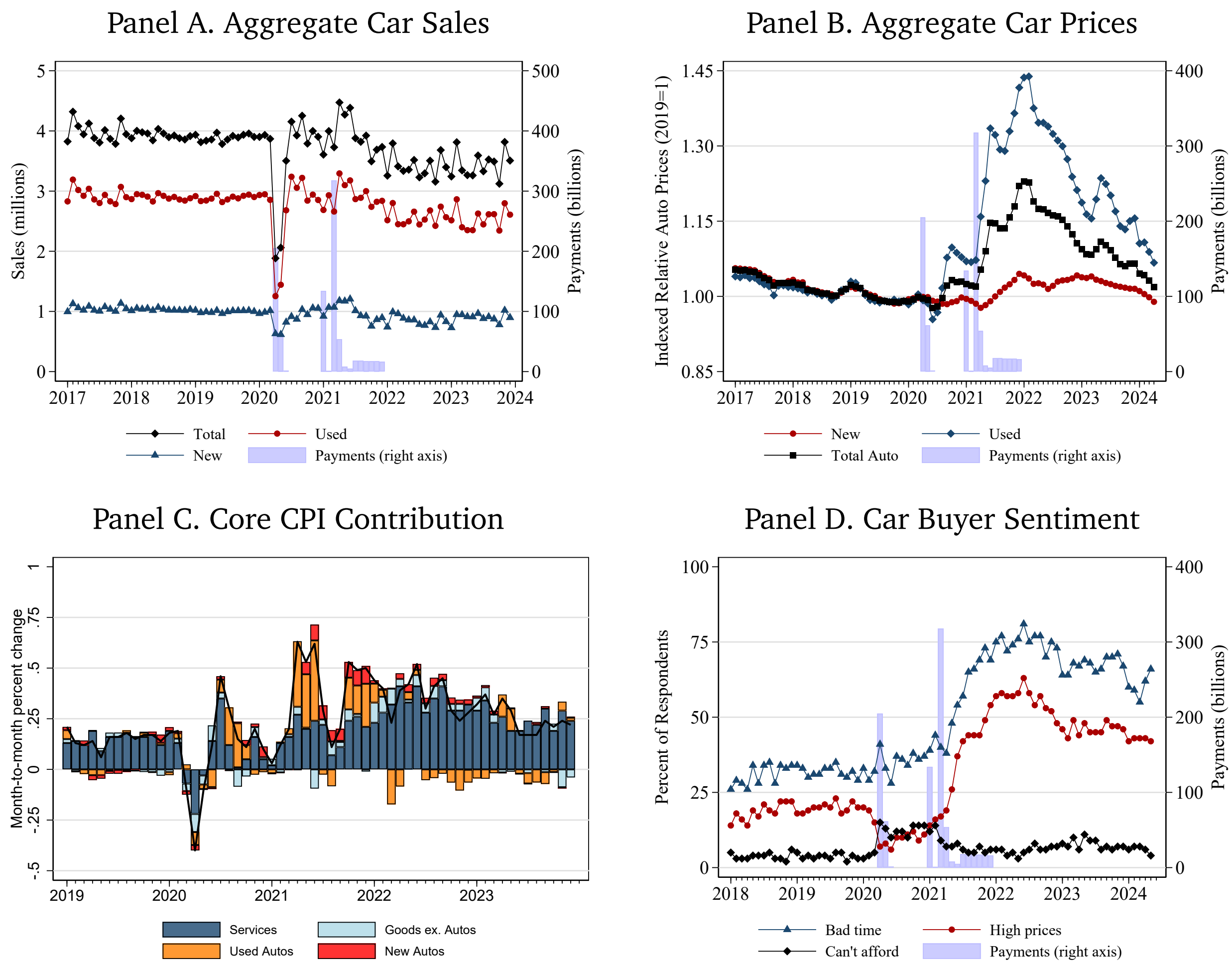
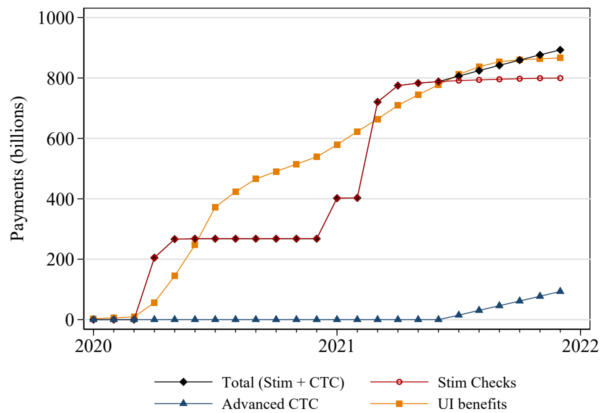
Figure 1: The State of the Auto Market



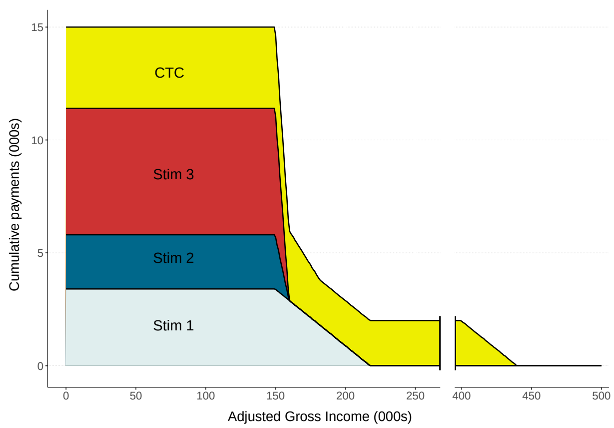
Notes: Panels A, B and C show stimulus disbursements on the right axis. Panel A shows auto sales and (left axis). Panel B shows the relative price increase for used, new, and total (new and used combined) autos relative to the overall index in the CPI (left axis). Panel C shows the month-to-month percent change in core CPI that excludes the effects of food and energy prices (black line) and the stacked bars show the percentage point contribution in each category. Panel D shows a sharp, persistent increase in surveyed consumers' beliefs that the car buying environment is bad (left axis). 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

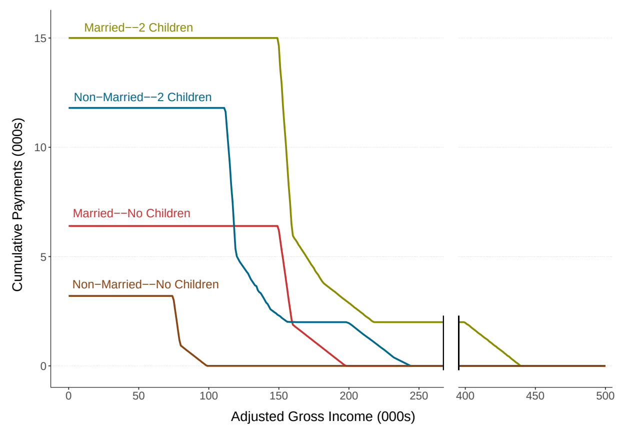
Panel A. Cumulative Aggregate Transfers



Panel B. Cumulative Benefits,
Married Parents with 2 Children

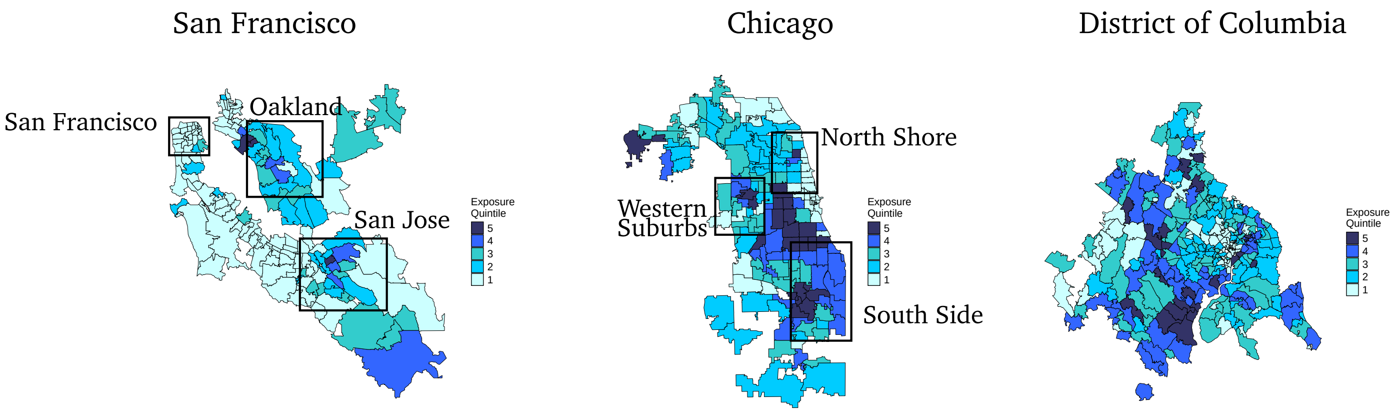


Panel C. Cumulative Benefits,
Family Structure Variation



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.

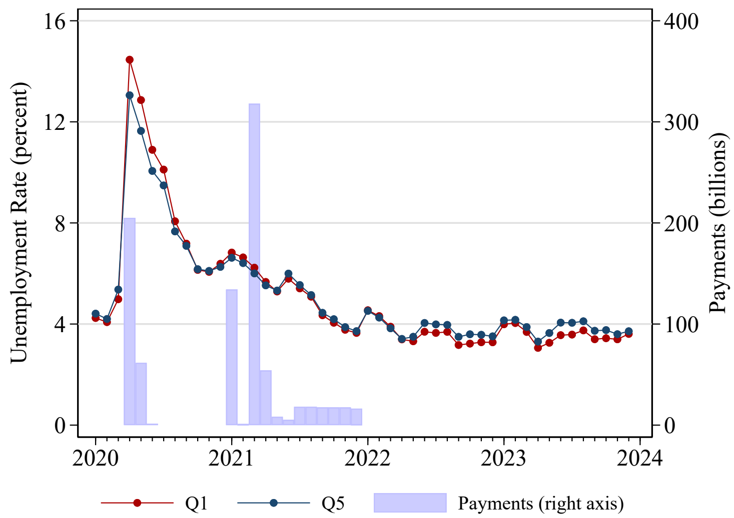
Figure 3: Regional Variation in Stimulus Exposure



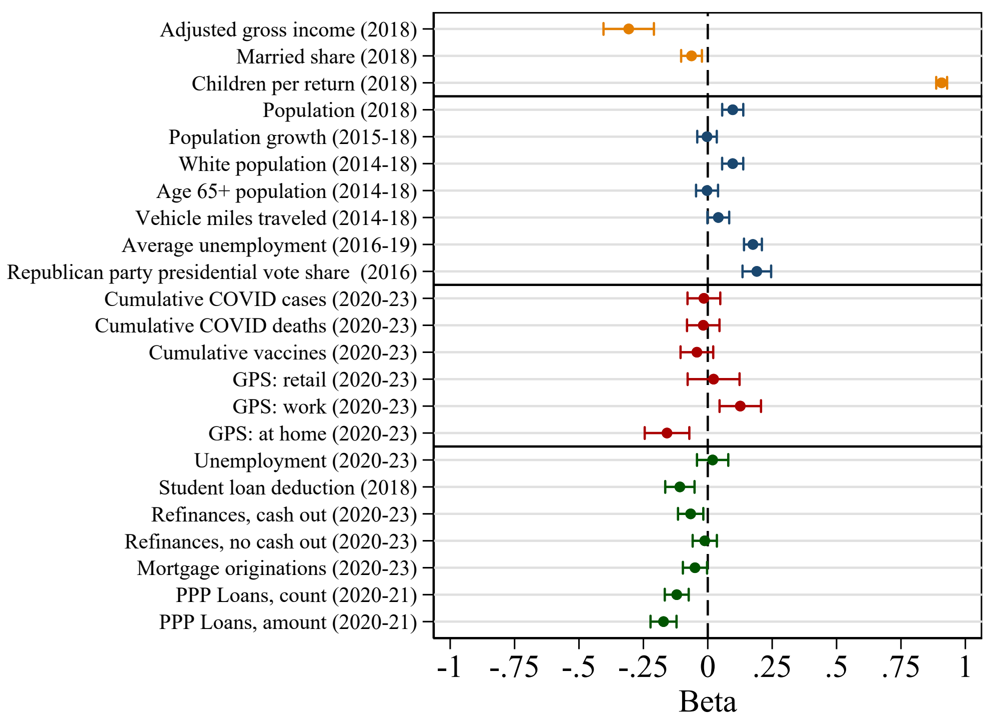
Notes: The figure shows regional variation in exposure to pandemic stimulus for three major metro regions, using the 2018 county-level average CTC value per tax return as the exposure measure.

Figure 4: Correlates of Pandemic Stimulus

Panel A. Unemployment
High vs. Low Exposure

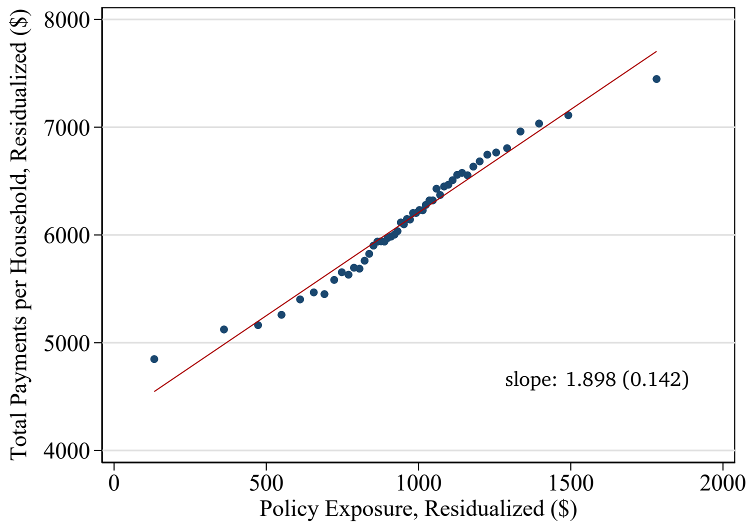


Panel B. Correlates of Exposure



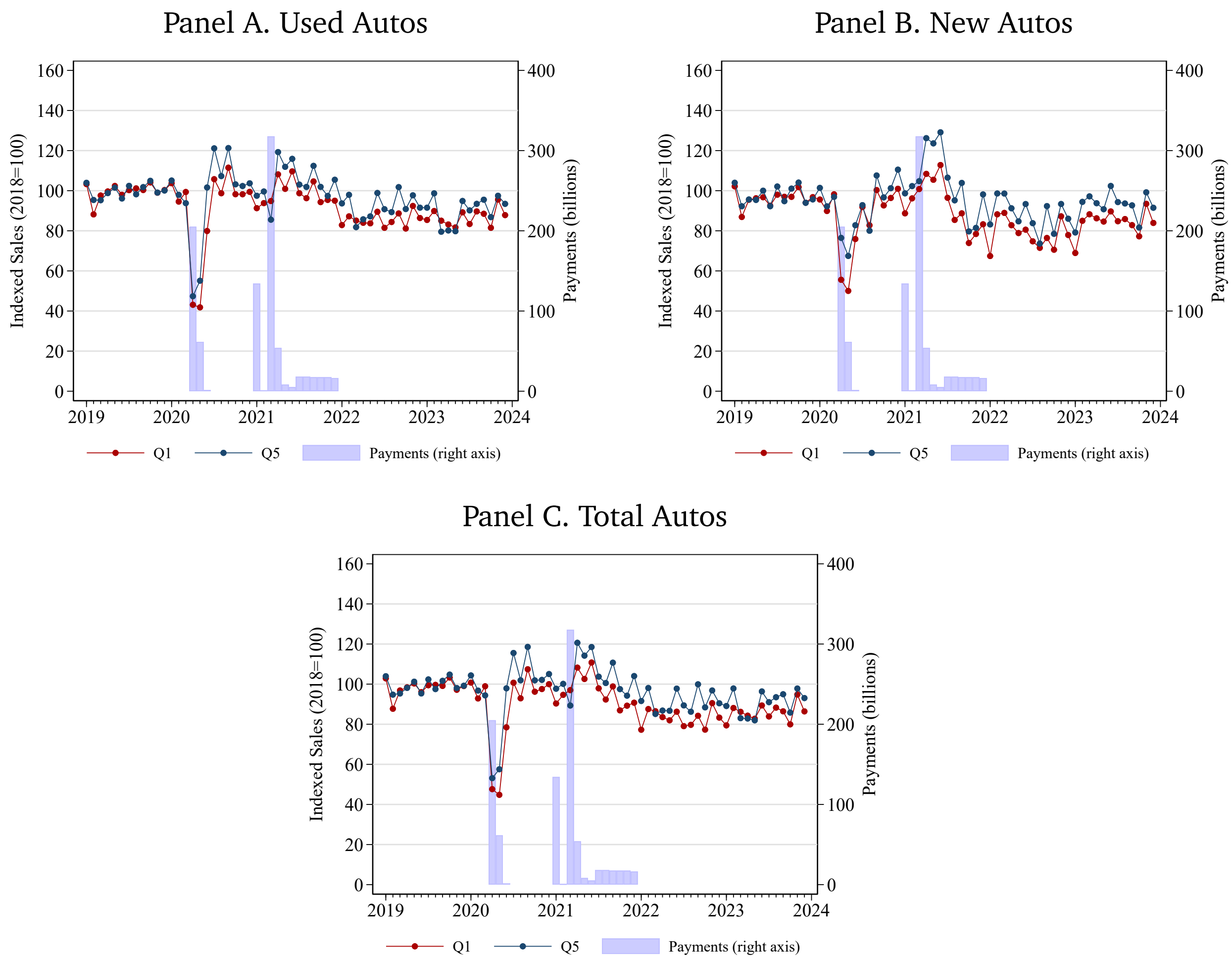
Notes: Panel A shows similar unemployment rates for high and low exposure counties, indicating UI benefits are unlikely to explain differences in pandemic stimulus across counties. Panel B shows coefficients from a regression of the exposure measure z-score on z-scores of ZIP- or county-level demographic 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 5: Program Exposure and Transfer Payments



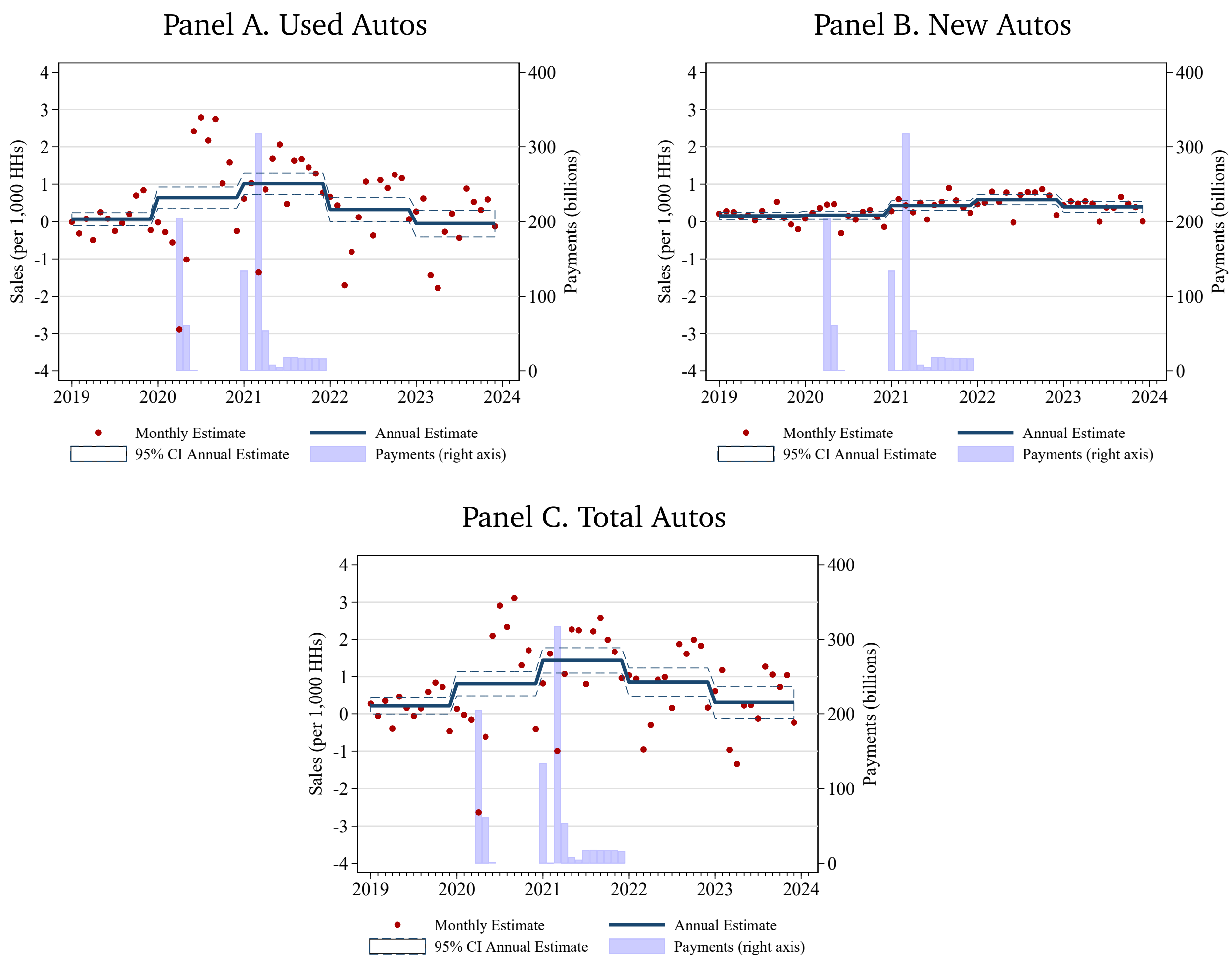
Notes: This figure plots a binscatter (50 bins) of ZIP-level stimulus transfers scaled by the number of tax returns versus program exposure. We first residualize the exposure and transfer variables by CBSA fixed effects to demonstrate the first-stage relationship that corresponds to our within-CBSA research 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)

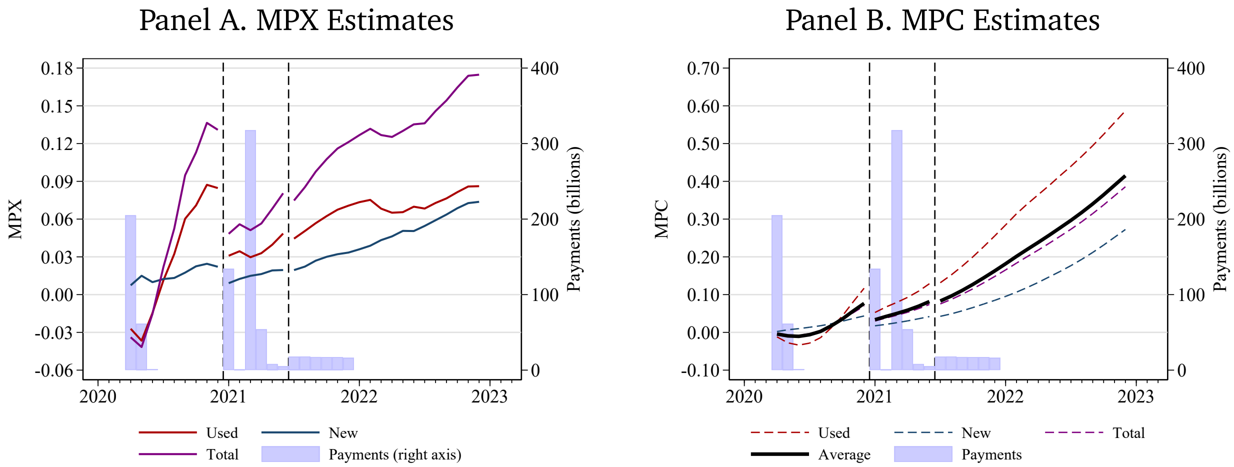


Notes: This figure plots monthly effects of pandemic transfers on auto sales at the ZIP level. We estimate coefficients from a panel regression with controls of the form:

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

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.

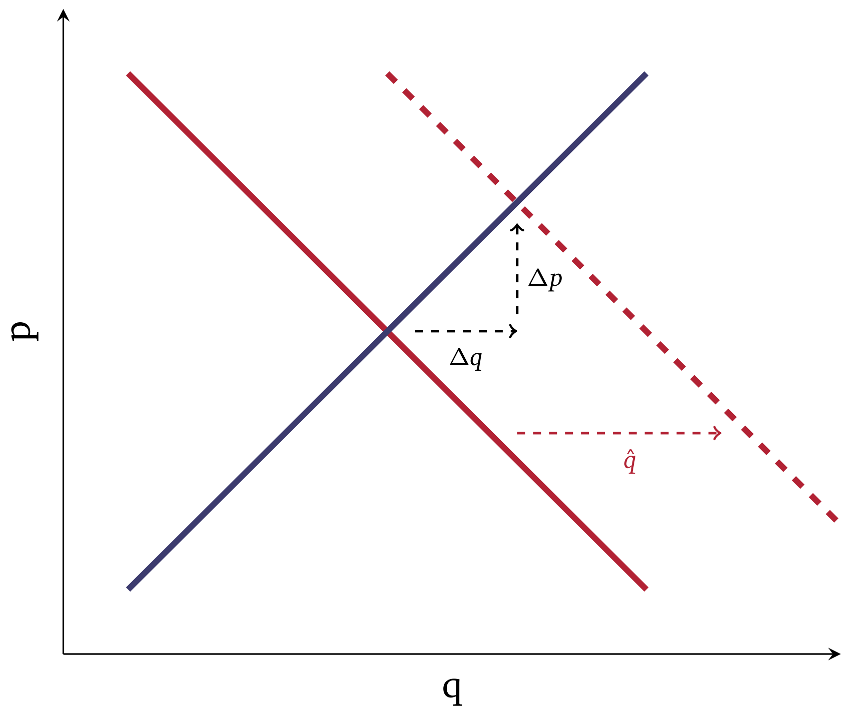
Figure 8: Cumulative Marginal Propensity to Spend (MPX) and Consume (MPC)



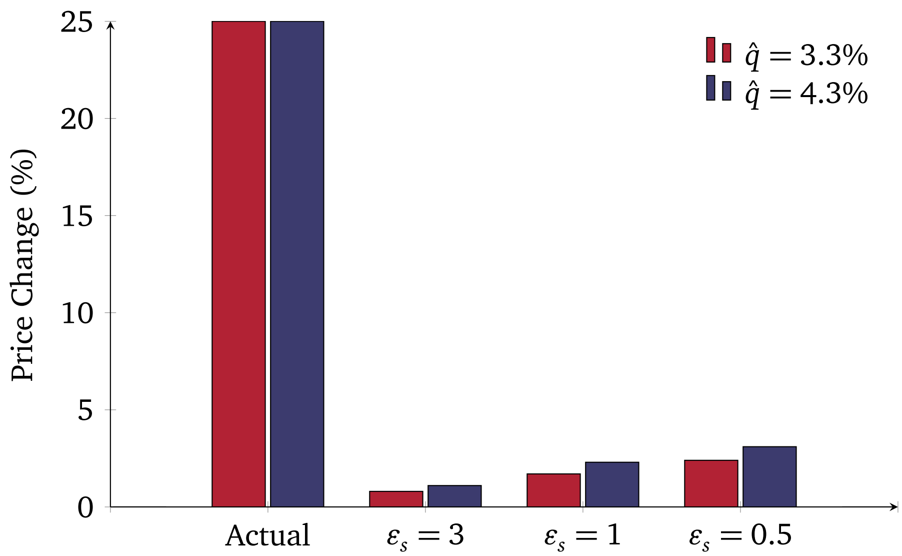
Notes: Panels A and B present estimates of the marginal propensity to spend (MPX) and consume (MPC), respectively, out of the household transfers. The MPC is estimated following the approach from Laibson, Maxted and Moll (2022). The MPC estimates are based on the auto-sales share of consumption (0.031 new, 0.032 used, 0.063 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

A. Market for Cars after Fiscal Transfer



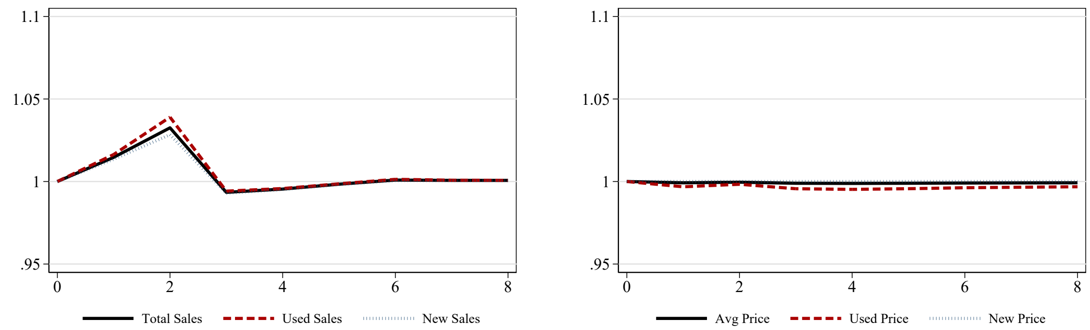
B. Price Impacts of Stimulus under Alternative Calibrations



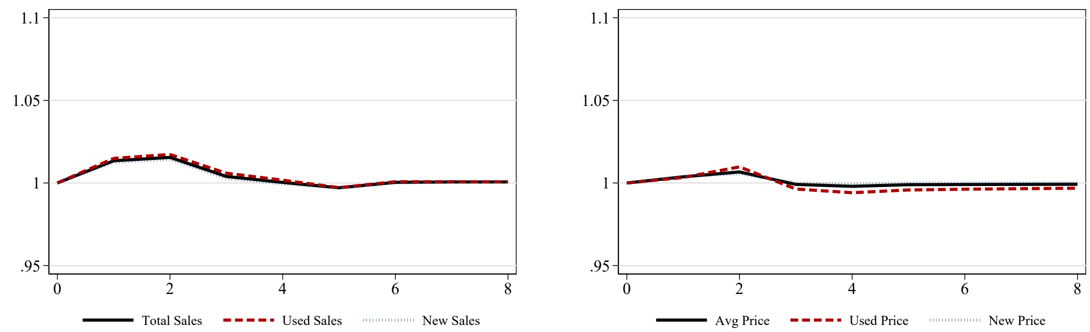
Notes: The figure presents a stylized illustration of the market for cars following a fiscal transfer. Panel A shows how price and quantity changes relate to the partial equilibrium aggregate impact estimate from our reduced form analysis ($\hat{q}$). Panel B calculates the share of the price change due to the fiscal transfer, θ, under a calibrated demand elasticity of 0.9 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. In the CPI, the relative price change for used and new cars is 25%.

Figure 10: Baseline Stimulus Impulse Responses

Panel A. Perfectly Elastic New Car Supply ($c_1 = 0$)



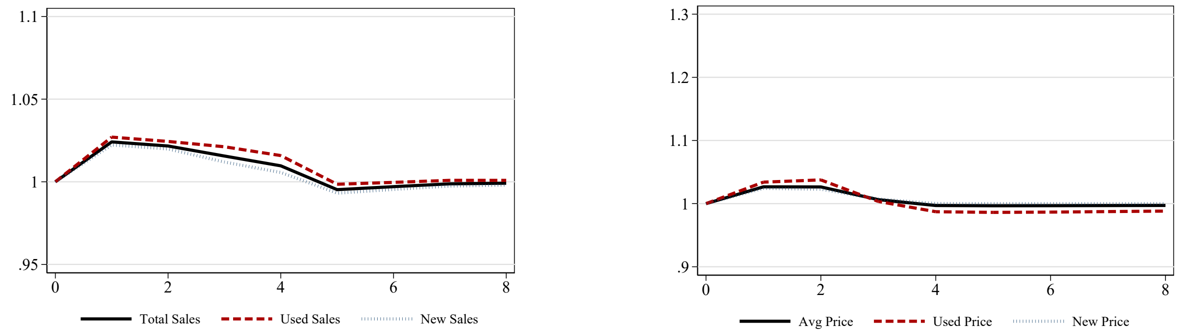
Panel B. (4 periods) Inelastic New Car Supply ($c_1 = 20.795$)



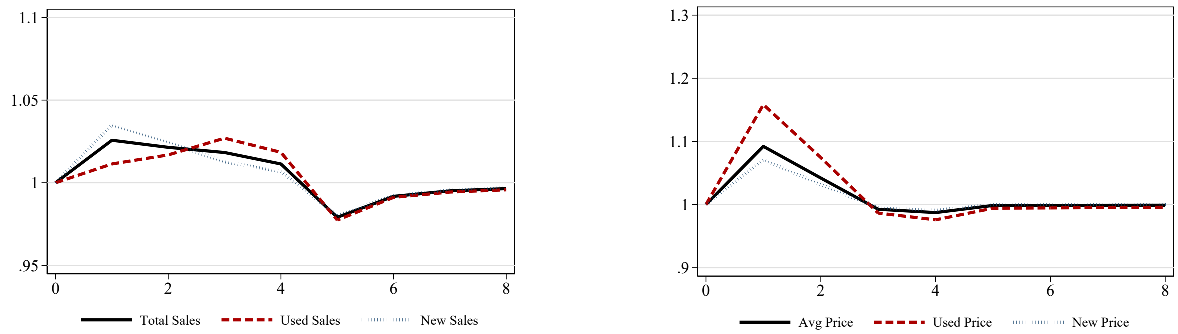
Notes: The figure presents impulses responses from our quantitative model of the new and used car markets in response to a fiscal transfer in period one calibrated to match the relative size of pandemic stimulus to household income. Panel A shows the case where new car supply is elastic, while Panel B shows the case where new car supply is temporarily inelastic for four periods. The left column plots the equilibrium quantity responses, while the right column plots equilibrium prices. The x-axis is time in years. The y-axis is the respective outcome scaled by its steady-state level.

Figure 11: Alternative Shock Impulse Responses

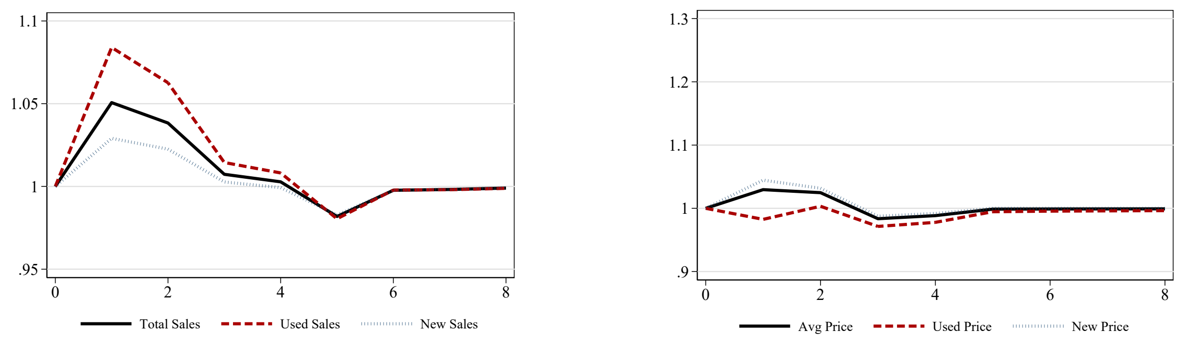
Panel A. Income Shock + Pandemic Wealth Shock (10% excess savings)



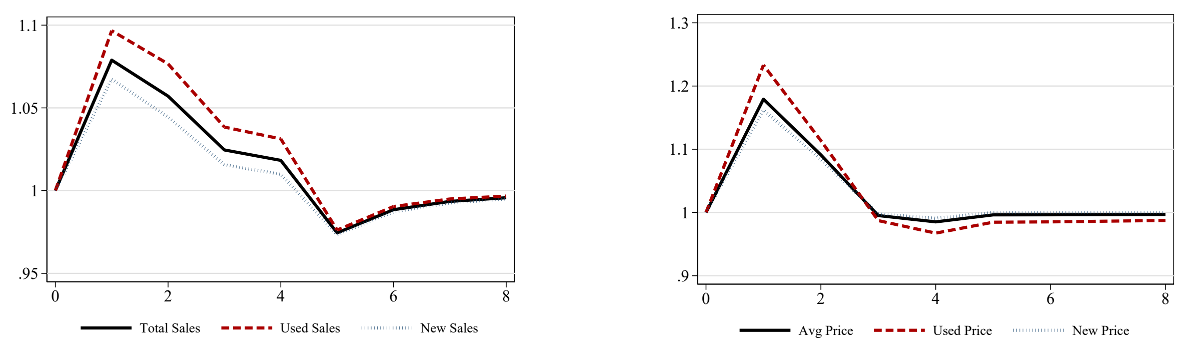
Panel B. Income Shock + Relaxation of Borrowing Constraint (from -0.4 to -1.0)



Panel C. Income Shock + Preference Shift Toward Autos (auto budget share from 6.3% to 7.3%)



Panel D. All Shocks Combined (Income + Wealth + Credit + Preferences)



Notes: The figure is analogous to Figure 10, but now includes additional shocks to household wealth, preferences for autos, and borrowing constraints. Panel A-D are with (four periods) inelastic supply. The x-axis is time in years. The y-axis is the respective outcome scaled by its steady-state level.

Table 1: Summary Statistics for Auto Sales

	Mean	10th	Median	90th	Standard Deviation
Auto Sales (per 1000 households)					
Total	30.38	18.00	28.74	45.05	11.71
New	6.50	2.46	5.96	11.11	3.83
Used	23.88	11.43	22.25	38.24	11.32
Stimulus Exposure (dollars)					
Exposure	962	586	951	1339	324
Household Payments	6,133	4,609	6,256	7,429	1,158
ZIP Cross Sectional Characteristics					
Married (share of households, 2018)	0.40	0.26	0.42	0.5	0.10
Adjusted Gross Income per household ($1000s, 2018)	71.71	41.24	57.53	107.83	63.95
Number of households (2018)	6,385	370	2,950	17,370	7,536
Household growth (percent, 2015-2018)	0.69	-1.35	0.48	2.86	2.22
Population age 65+ (percent, 2015-2018)	0.18	0.10	0.17	0.26	0.07
White population (percent, 2015-18)	0.83	0.54	0.90	0.98	0.20
Number of children per household (2018)	0.59	0.41	0.58	0.78	0.16
Student loan interest deduction (share of households, 2018)	0.07	0.00	0.07	0.11	0.04
Ex ante unemployment (percent, 2016-19)	5.20	1.70	4.50	9.30	3.73
Republican Vote (county level share, 2016 presidential election)	0.53	0.29	0.54	0.74	0.17
ZIP & CO Time Varying Characteristics					
Cumulative COVID-19 outcomes (2020 and later)					
Cases	109,463	250	16,163	248,772	335,142
Deaths	1,306	4	231	2,891	3,940
Vaccinations	675,499	0	61,891	1,702,518	2,026,504
GPS Tracking (2020 and later, indexed relative to Jan3-Feb5 2020)					
Time at work	-0.20	-0.32	-0.20	-0.05	0.11
Time at retail	-0.04	-0.22	-0.05	0.15	0.17
Time at home	0.05	0.01	0.04	0.10	0.04
Local Area Unemployment (percent)	5.01	2.60	4.20	8.37	2.89
Paycheck Protection Program (2020-21)					
Number of Loans	19.71	0	0	49	69.74
Average Loan Amount ($1000s)	47.78	9.96	24.13	107.09	81.16
Mortgage Originations and Refinances					
Originations, purchase	10.82	0.00	3.00	31.00	18.21
Refinance, no cash out	4.37	0.00	1.00	12.00	9.07
Refinance, cash out	7.44	0.00	0.00	21.00	21.43
Number of observations (zip-by-month)					1,587,888
Number of zipcodes					22,054
Number of CBSAs (clusters)					735 (779)

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 code 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), Intercontinental Exchange (ICE) McDash (mortgage originations and refinances).

Table 2: Auto Sales Regression Results

	(1)	(2)	(3)
	Total	New	Used
2019	0.216	0.151	0.066
	[0.113]	[0.049]	[0.088]
2020	0.814	0.170	0.642
	[0.167]	[0.058]	[0.143]
2021	1.435	0.432	1.015
	[0.172]	[0.065]	[0.148]
2022	0.856	0.589	0.323
	[0.192]	[0.070]	[0.167]
2023	0.308	0.397	-0.053
	[0.216]	[0.075]	[0.183]
2018 Mean	32.39	7.07	25.32
R^2	0.769	0.633	0.803
N			1,587,888

Notes: All outcomes are monthly sales per 1,000 households. Sample size, number of zipcodes and number of CBSAs is the same in all specifications. Standard errors clustered at the CBSA 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 3: Auto Sales/Lease Regression Results, by Financing Type

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total Sales		New Sales		New Leases	Used Sales	
	Cash	Loans	Cash	Loans	Loans	Cash	Loans
2019	0.087	0.122	0.020	0.129	0.032	0.059	0.019
	[0.070]	[0.068]	[0.018]	[0.041]	[0.022]	[0.066]	[0.051]
2020	0.678	0.111	0.034	0.132	0.063	0.635	0.007
	[0.116]	[0.084]	[0.021]	[0.050]	[0.032]	[0.111]	[0.062]
2021	0.718	0.700	0.008	0.425	0.136	0.717	0.319
	[0.127]	[0.085]	[0.022]	[0.056]	[0.047]	[0.121]	[0.062]
2022	0.353	0.501	0.118	0.476	0.211	0.235	0.098
	[0.149]	[0.094]	[0.024]	[0.060]	[0.068]	[0.141]	[0.068]
2023	0.155	0.137	0.035	0.407	0.190	0.150	-0.191
	[0.153]	[0.107]	[0.025]	[0.065]	[0.069]	[0.147]	[0.075]
2018 Mean	18.08	14.31	1.16	5.91	1.83	16.92	8.40
R^2	0.761	0.658	0.493	0.626	0.792	0.770	0.673
N							1,587,888

Notes: All outcomes are monthly sales per 1,000 households. Sample size, number of zipcodes and number of CBSAs is the same in all specifications. Standard errors clustered at the CBSA 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 4: Aggregate Estimates

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Sales (millions)	95% CI	Sales (percent)	95% CI	MPX	95% CI	MPC	95% CI
Used	3.5	[2.2, 4.9]	2.9	[1.8, 4.0]	0.097	[0.023, 0.191]	0.647	[0.067, 1.23]
New	2.1	[1.5, 2.7]	4.2	[3.0, 5.3]	0.082	[0.022, 0.144]	0.297	[0.019, 0.576]
Total	5.6	[3.9, 7.2]	3.2	[2.3, 4.1]	0.193	[0.059, 0.331]	0.472	[0.131, 0.814]

Notes: The aggregate effects in column (2) are based on the regression estimates for 2020-2022, the total number of zip codes (22,054), and the sample means of number of households per zip code (6,385) and exposure per zip code ($962), then scaled to be nationally representative (sample covers roughly 90% of households). The confidence intervals in column (3) are calculated from the regression results based on the standard error of the estimates. Columns (4)-(9) use either the estimates in column (2) or (3). Columns (4) and (5) are calculated relative to 2019 levels reported by the Bureau of Transportation Statistics (BTS). The MPX results in column (6) and (7) use the 2019 price level from BTS adjusted by the monthly CPI. The MPC results in columns (8) and (9) use the transformation described in Appendix A.2.

Table 5: Share of Price Change due to Stimulus, Supply Shocks, and Demand Shocks

	Calibrated or Estimated				Implied Shocks (p.p.)		Share Explained (%)		
Scenario	Δp (p.p.)	Δq (p.p.)	$\hat{q}$ (p.p.)	ε_s	Supply	Demand	Supply	Demand	Stimulus
Baseline	25	1.5	3.3	3	74	21	75	21	3
All Fiscal Shocks	25	1.5	4.9	3	74	19	75	20	5
Low ε_s	25	1.5	3.3	1	24	21	49	44	7
Lower ε_s	25	1.5	3.3	0.5	11	21	31	59	9
95% CI Stimulus	25	1.5	4.3	3	74	20	75	20	4
5% CI Stimulus	25	1.5	2.0	3	74	22	75	23	2
Low Δq	25	-1.1	3.3	3	76	18	78	19	3
High Δq	25	4.1	3.3	3	71	23	73	24	3
Mean							67	29	5

Notes: The table reports the share of the total price change that can be attributed to stimulus, supply shocks, and demand shocks. The calculations use the partial equilibrium framework developed in Section 5. All rows use the same calibrated elasticity of demand $\varepsilon_d = 0.9$. Δp is based on the CPI index components for used and new autos. Δq is based on Jorda projections for aggregate auto purchases in the absence of the pandemic cumulated through 2022. $\hat{q}$ is the predicted effect of the stimulus on auto sales relative to a zero-stimulus counterfactual. The *All Fiscal Shocks* scenario adds estimated auto purchases due to fiscal policy that is not captured by our research design, including excess UI benefits (1.4 million extra sales), student loan forbearance (1 million extra sales), and PPP loans (300 thousand extra sales). The *95%* and *5% CI Stimulus* scenarios use the confidence interval for aggregate predicted quantities from the DD estimator relative to a zero-stimulus counterfactual. The *High* and *Low* Δq scenarios use ± one standard deviation from the central prediction.

For Online Publication

A Online Appendix

A.1 Geographic Auto Value Dispersion

In our research design, we assume that the supply of cars occurs in one national market with limited geographic dispersion in prices and elasticity. To support this assumption, we use data from asset-backed securities to evaluate the share of price dispersion across similar autos explained by geographic differences. Hankins, Momeni and Sovich (2025) use these data to study the incidence of tariffs on car loan prices. For new cars, the listed value is the "invoice price" paid by the car dealer, whereas it is the Kelley Blue Book value for used cars.

Pooling data from 2017 to 2022, the mean asset values for new and used cars are about \$34,000 and \$21,000, respectively. The standard deviation within make-model-model-year bins is approximately \$3,700 for new cars and \$4,000 for used cars. Including state fixed effects reduces these standard deviations by around \$100 for each group, suggesting a small role for geographic factors in price determination. Note that just the fact that a national securitization market uses a region-invariant common value for each car supports our assumption.

A.2 MPX and MPC details

Inputs for MPX and MPC calculations. To determine the valuation of auto sales during the policy period, we start with the average 2019 price level published by the BTS (new \$38,000, used \$20,600). 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 for new (used) cars at the time of purchase, assuming 45 (30) percent of buyers have trade-ins worth \$15,000 (\$10,000) for new (used) sales and we assume that the value of trade-ins grows more slowly (roughly 80% of new price growth) compared to retail auto prices.[37]

We estimate the auto-sales share of consumption s (3.1 percent used, 3.2 percent new, 6.3 percent total) using data from the Consumer Expenditure Survey (CEX) in 2022, following Aguiar and Bils (2015) (Appendix A.3, Table A.1). 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.[38] We convert interest rates and depreciation rates to monthly rates in our calculations.

Conversion from MPX to MPC. To estimate the effects on consumption more generally, we follow Laibson, Maxted and Moll (2022) and map the MPX for autos into the MPC for all consumption.

[37] These figures are based on reports from Edmunds here, here, here, and from the Edmunds Used Vehicle Reports for various years.

[38] The depreciation rate for total sales is the sales weighted average of new and used sales.

The discrete-time MPC for all goods over τ periods is defined in their (37) as:

$$\text{MPC}_\tau(x) = \frac{\partial}{\partial b}\mathbb{E}\left[\sum_{t=0}^{\tau-1} c(x_t) \mid x_0 = x\right], \tag{16}$$

where b is liquid wealth, x is the state including both wealth and income, and $c(\cdot)$ is consumption on all goods including the flows from durables.

The durable MPX is derived in their equation (41):

$$MPX_\tau^D(x) = \frac{\delta s}{r+\delta} MPC_{\tau-1}(x) + \frac{s}{r+\delta} \times \frac{\partial}{\partial b}\mathbb{E}[c(x_{\tau-1})|x_0 = x], \tag{17}$$

where r is the interest rate, s is the durable goods share of household consumption, and δ is the depreciation rate. Solving for $MPC_{\tau-1}(x)$ from the durable MPX gives:

$$MPC_{\tau-1}(x) = \left(\frac{r+\delta}{\delta s}\right) MPX_\tau^D(x) - \frac{1}{\delta} \times \frac{\partial}{\partial b}\mathbb{E}[c(x_{\tau-1})|x_0 = x]. \tag{18}$$

Rearranging this and multiplying by δ gives:

$$\delta MPC_{\tau-1}(x) + \frac{\partial}{\partial b}\mathbb{E}[c(x_{\tau-1})|x_0 = x] = \left(\frac{r+\delta}{s}\right) MPX_\tau^D(x). \tag{19}$$

Plugging in the definition of MPC gives:

$$\delta\left(\frac{\partial}{\partial b}\mathbb{E}\left[\sum_{t=0}^{\tau-2} c(x_t)dt \mid x_0 = x\right]\right) + \frac{\partial}{\partial b}\mathbb{E}[c(x_{\tau-1})|x_0 = x] = \left(\frac{r+\delta}{s}\right) MPX_\tau^D(x). \tag{20}$$

The second term is the MPC in one time period and the first term is the depreciation replacement incurred on the cumulative change in consumption up to period $\tau - 1$.

We can rearrange and rewrite this expression for a recursive relationship:

$$\frac{\partial}{\partial b}\mathbb{E}[s \times c(x_{\tau-1})|x_0 = x] + \delta\left(\frac{\partial}{\partial b}\mathbb{E}\left[\sum_{t=0}^{\tau-2} s \times c(x_t)dt \mid x_0 = x\right]\right) = (r+\delta) MPX_\tau^D(x) \tag{21}$$

$$s \times \left(\frac{\partial}{\partial b}\mathbb{E}\left[c(x_{\tau-1}) + \delta\left(\sum_{t=0}^{\tau-2} c(x_t)dt\right) \mid x_0 = x\right]\right) = (r+\delta) MPX_\tau^D(x) \tag{22}$$

$$s \times \left(\frac{\partial}{\partial b}\mathbb{E}\left[\left(\sum_{t=0}^{\tau-1} c(x_t)dt\right) - (1-\delta)\left(\sum_{t=0}^{\tau-2} c(x_t)dt\right) \mid x_0 = x\right]\right) = (r+\delta) MPX_\tau^D(x) \tag{23}$$

Plugging in the discrete-time definition for the MPC gives:

$$s \times (MPC_\tau(x) - (1-\delta) MPC_{\tau-1}(x)) = (r+\delta) MPX_\tau^D(x). \tag{24}$$

Starting with the one-period MPC, which simplifies to $s \times MPC_1(x) = (r+\delta) MPX_1^D(x)$, we iterate forward for the expression in the text (t is measured in months):[39]

$$\text{MPC}_t = \left(\frac{r+\delta}{s}\right)\sum_{i=1}^{t}(1-\delta)^{t-i}\text{MPX}_i^D. \tag{25}$$

A.3 Auto Consumption Share Calculations

We use the Consumer Expenditure Survey (CEX) to compute calendar-year, weight-adjusted mean expenditures by category. Building on Aguiar and Bils (2015), total household expenditures are defined as TOTEXP net of nondiscretionary items (cash contributions to persons outside the CU, life-insurance premiums, and retirement/pension contributions).

We report two measures of vehicle outlays: a cash-flow measure based on quarterly payment flows and a net-outlay measure that books the purchase amount in the period of purchase. Both our measures and the CEX vehicle expenditure convention use net payments for purchases and thus implicitly deduct the value of trade-ins.

Relative to Aguiar and Bils (2015), we do not restrict the sample to four-interview households, and we adopt a narrower purchase concept focused on new and used automobiles/light trucks, excluding finance charges, leases, sales offsets, motorcycles, and other vehicles.

Appendix Table A.1 reports annual vehicle purchase expenditure shares under both definitions (Cash-Flow and Net Outlay) for 2016–2023, along with pooled means for 2016–2019 and 2020–2022. Appendix Table A.2 presents the same shares by number of children (0–4+), with period pools shown as Panel A (2016–2019) and Panel B (2020–2022). Estimates are reported for the average consumer unit and by number of children; pooled rows are simple averages across years. Our aggregates are in line with those in the CEX Annual Report.

[39]For brevity, in the main text, we denote by MPX the marginal propensity to spend on autos, which corresponds to MPX^D in their notation.

Table A.1: Vehicle Purchase Expenditure Shares by Year

	Cash-Flow			Net Outlay		
Year	Total (%)	Used (%)	New (%)	Total (%)	Used (%)	New (%)
2016	6.35	3.27	3.08	7.87	4.11	3.76
2017	6.50	3.42	3.08	8.34	4.26	4.08
2018	6.43	3.42	3.01	8.02	4.20	3.82
2019	6.58	3.54	3.04	8.28	4.59	3.69
2020	6.89	3.79	3.10	8.85	4.73	4.12
2021	6.58	3.50	3.08	8.71	4.59	4.12
2022	6.31	3.19	3.12	7.72	3.82	3.90
2023	6.67	3.28	3.39	8.50	3.99	4.51
2016–2019	6.46	3.41	3.05	8.13	4.29	3.84
2020–2022	6.59	3.49	3.10	8.43	4.38	4.05
All Years	6.54	3.43	3.11	8.29	4.29	4.00

Notes: Shares are expressed as percentages of net total expenditures at the calendar-year level. Calendar-year means follow the CEX Interview methodology with survey weights (`FINLWT21`); pooled rows are simple averages over the listed years. *Cash-Flow* uses the CEX flow variables for new and used vehicles (`ECARTKN`, `ECARTKU`). *Net Outlay* uses the corresponding quarterly purchase-amount variables for new and used vehicles (e.g., `CARTKN`, `CARTKU`). Net total expenditures equal `TOTEXP` net of `CASHCO`, `LIFINS`, and `RETPEN`. Vehicle scope is restricted to new/used automobiles and light trucks; finance charges, leases, sales offsets, motorcycles, and other vehicles are excluded.

Table A.2: Vehicle Purchase Expenditure Shares by Number of Children

Panel A: 2016–2019

Number of Children	Cash-Flow Total (%)	Cash-Flow Used (%)	Cash-Flow New (%)	Net Outlay Total (%)	Net Outlay Used (%)	Net Outlay New (%)
0	6.23	3.08	3.16	7.80	3.82	3.98
1	6.98	3.84	3.14	8.80	4.84	3.96
2	7.00	3.89	3.11	8.86	5.20	3.66
3	6.22	4.23	1.99	7.97	5.22	2.74
4+	6.79	5.07	1.73	8.79	5.99	2.80
Average	6.46	3.41	3.05	8.13	4.29	3.84

Panel B: 2020–2022

Number of Children	Cash-Flow Total (%)	Cash-Flow Used (%)	Cash-Flow New (%)	Net Outlay Total (%)	Net Outlay Used (%)	Net Outlay New (%)
0	6.36	3.16	3.20	7.89	3.92	3.98
1	7.02	3.98	3.05	8.67	4.65	4.02
2	7.16	4.07	3.09	10.18	5.39	4.79
3	6.56	4.24	2.32	8.44	5.46	2.98
4+	6.91	4.28	2.63	10.53	6.53	4.00
Average	6.59	3.49	3.10	8.43	4.38	4.05

Notes: Panel A pools 2016–2019; Panel B pools 2020–2022. Shares are percentages of net total expenditures. Weighting follows the CEX Interview methodology with `FINLWT21`; the "Average" row is the household-pooled mean within each period. Children are counted from MEMI as CU members with `AGE` ≤ 17 and `CU_CODE` $\in \{3,4\}$ (child or grandchild), top-coded at 4+. *Cash-Flow* uses the CEX flow variables for new and used vehicles (`ECARTKN`, `ECARTKU`). *Net Outlay* uses the corresponding quarterly purchase-amount variables for new and used vehicles (e.g., `CARTKN`, `CARTKU`). Net total expenditures equal `TOTEXP` net of `CASHCO`, `LIFINS`, and `RETPEN`. Vehicle scope is restricted to new/used automobiles and light trucks; finance charges, leases, sales offsets, motorcycles, and other vehicles are excluded.

A.4 Microfoundation and Potential Bias from Household Structure

Demand curve for autos. We derive the demand function for autos Q and non-durable consumption C for a consumer with income Y. The price of non-durables is normalized to one and the relative price of autos is P. Consumers maximize utility

$$U(Q,C) = (\alpha Q^\rho + \beta C^\rho)^{1/\rho}, \tag{26}$$

subject to the budget constraint $PQ + C \leq Y$.

Optimization implies relative demand given by

$$\frac{\alpha}{\beta}\left(\frac{Q}{C}\right)^{\rho-1} = P. \tag{27}$$

Substitute for C using the budget constraint, which binds under non-satiation and rearrange:

$$\frac{Q}{Y-PQ} = \left(\frac{\beta}{\alpha}P\right)^{1/(\rho-1)} \tag{28}$$

$$Q = \frac{\left(\frac{\beta}{\alpha}\right)^{1/(\rho-1)} Y}{P\left(P^{\rho/(1-\rho)} + \left(\frac{\beta}{\alpha}\right)^{1/(\rho-1)}\right)} \tag{29}$$

With Cobb-Douglas preferences ($\rho \to 0$), we have

$$Q = \frac{\alpha Y}{(\alpha+\beta)P} \tag{30}$$

If $\beta = 1-\alpha$, then $Q = \alpha Y/P$.

Returning to the CES case, take logs, then linearize around a reference price P_0, and denote $A = (\beta/\alpha)^{1/(\rho-1)}$ (lower case letters x denote $\ln(X)$):

$$q = \ln A + y - p + \ln\left(A + P^{\rho/(1-\rho)}\right) \tag{31}$$

$$q \approx \ln A + y - p + \ln\left(A + P_0^{\rho/(1-\rho)}\right) + \frac{\rho P_0^{(2\rho-1)/(1-\rho)}}{(1-\rho)\left(A + P_0^{\rho/(1-\rho)}\right)}(p - p_0) \tag{32}$$

$$q \approx y - bp + e \tag{33}$$

where b and e are functions of parameters.[40] This equation corresponds to our estimating equation in the DD analysis, under the assumption that car price fluctuations are absorbed in the time and region fixed effects. It also corresponds to the demand curve in the partial equilibrium model.

Potential bias from number of kids. Index (33) by group g, allow group specific coefficients b on a common national price, and let the constant e include idiosyncratic group and national shocks:

$$q_{gt} = y_{gt} - b_g p_t + e_{gt}. \tag{34}$$

The first difference for each group is given by:

$$\Delta q_g = \Delta y_g - b_g \Delta p + \Delta e_g. \tag{35}$$

Taking the difference between two groups, we have:

$$\Delta q_1 - \Delta q_2 = (\Delta y_1 - \Delta y_2) - (b_1 - b_2)\Delta p + (\Delta e_1 - \Delta e_2). \tag{36}$$

The coefficient on income does not depend on the share of consumption going to autos (i.e.,

[40] Specifically, b is given by $b = 1 - \frac{\rho P_0^{(2\rho-1)/(1-\rho)}}{(1-\rho)\left(A+P_0^{\rho/(1-\rho)}\right)}$, and e is given by $e = \ln A + \ln\left(A + P_0^{\rho/(1-\rho)}\right) - \frac{\rho P_0^{(2\rho-1)/(1-\rho)}}{(1-\rho)\left(A+P_0^{\rho/(1-\rho)}\right)} p_0$.

α). In other words, under these preferences we should not expect the coefficient on income shocks to vary with expenditure share differences, for instance, due to household structure.

Note in the Cobb-Douglas case, both the price and income elasticities are independent of the expenditure share. However, in the CES case, the coefficient on price does depend on α (through the A term). This could introduce bias if the expenditure shares differ across groups, for example, if larger families spend relatively more on autos than smaller families. The sign of the bias depends on the elasticity of substitution between autos and other goods.

While we show in Appendix A.3 that expenditure shares on autos in the CEX do not vary systematically with the number of kids, we quantify this potential bias here to emphasize that it does not have a material effect on our DD estimates. Specifically, we simulate a two-period data set of 1,000,000 households, half of whom receive transfers equal to 3.3% of income (i.e., families with relatively fewer children) and half of whom receive 5.3% (i.e., families with relatively more children). We calibrate the elasticity of substitution ($1/(1-\rho)$) of 1.2 in one set of simulations and 0.8 in another, indicating substitutes and complements, respectively. We set the α for the high transfer group equal to 0.07 and the low transfer group to 0.06. We calibrate a common price increase of 25%. True demand is given by (29).

To quantify the bias, we estimate a short regression that compares the high- (treatment) and low-stimulus (control) groups before and after the transfer. We then augment the regression by interacting the price increase with a treatment indicator. Appendix Table A.3 shows a very small bias for the DD coefficient in the short regression, but the estimates are essentially unchanged (columns 1-2 and 3-4 show the substitute and complement cases, respectively). Only in the case of an unrealistic difference in expenditures shares (0.6 vs. 0.06) do we see a substantial bias in the DD estimate (columns 5 and 6).

Table A.3: Simulation of Bias from Household Structure

	(1)	(2)	(3)	(4)	(5)	(6)
Post period=1 × Treatment group=1	0.0194	0.0197	0.0185	0.0195	-0.0006	0.0193
	(0.0006)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Log price of cars		-1.1985		-0.8948		-0.8949
		(0.0014)		(0.0014)		(0.0014)
Treatment group=1 × Log price of cars		-0.0021		-0.0073		-0.0891
		(0.0020)		(0.0020)		(0.0020)

Notes: $N = 2,000,000$. The table presents simulated difference-in-differences estimates of the effect of a stimulus transfer on the log quantity of cars purchased. Columns 1-2 and 3-4 show the results for a CES demand curve with an elasticity of substitution of 1.2 and 0.8, respectively. The expenditure share parameters are 0.07 and 0.06 for the high- and low-transfer groups, respectively. Columns 5-6 show the results for a CES demand curve with an elasticity of substitution of 1.2, but with a difference in expenditure share parameters of 0.6 versus 0.06. All households face a common price shock of 25%.

A.5 Partial Equilibrium Model Derivations

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

$$\Delta q_d = \mathbb{E}[\Delta q_{di}] = -\varepsilon_d \Delta p + \gamma \mathbb{E}[\Delta y_i] + \Delta \nu_d, \tag{37}$$

where the expectations are size-weighted to achieve aggregation. 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 the equation in the text. Plugging in for the quantity response using the definition of Δq_s gives the equation for Δq in the text.

DD Estimator versus Aggregates. The difference-in-differences (DD) estimator between any two locations $i = 1$ and $i = 2$ is given by:[41]

$$\mathbb{E}[\Delta q_{d1} - \Delta q_{d2}] = \hat{\gamma}(\Delta y_1 - \Delta y_2). \tag{38}$$

The aggregate DD estimator across all locations is:

$$\hat{q} = \hat{\gamma}\mathbb{E}[\Delta y_i], \tag{39}$$

where $\hat{\gamma}$ is the coefficient on the change in income estimated in our research design. Rewriting the expressions for Δq and Δp in terms of $\hat{q}$ gives the expressions in the text.

Relative Importance of Supply and Demand Shocks. The system of equations governing equilibrium ((8) and (9)) provides a decomposition for the total price change into relative contributions from stimulus, supply, and demand shocks. Specifically, the aggregate demand shock equals:

$$\Delta \nu_d = \Delta q - \hat{q} + \varepsilon_d \Delta p, \tag{40}$$

and the aggregate supply shock equals:

$$-\Delta \nu_s = -\Delta q + \varepsilon_s \Delta p. \tag{41}$$

Combining these two equations with the expression for Δp gives the final expressions for ζ_d and ζ_s in the text.

A.6 Other Fiscal Transfers

We augment our baseline prediction, which focuses on stimulus payments and the advanced child tax credit, by calculating the expected boost to demand coming other fiscal transfers that occurred during the COVID pandemic, including UI expansions, student loan forbearance, and PPP loans.

To estimate the net financial benefit from UI transfers in excess of lost wages, we use the total UI transfers from the daily Treasury Statement during the pandemic that total just over \$860 billion and the median comprehensive replacement rate of 134% from Ganong, Noel

[41] 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, $q_{dit} - \bar{q}_{di} - \bar{q}_{dt}$ identifies γ with the right hand side being $\gamma(y_{it} - \bar{y}_i - \bar{y}_t)$.

and Vavra (2020). Together, these imply that the net benefit to households from UI was $220 billion during the sample period.

For student loan forbearance, we estimate an additional $157 billion in savings during the sample period, which we treat as an income shock.[42] To estimate this value of this forbearance, we use results from Cooper and Haddix (2025) that reports there were 17 million borrowers with average payments of $280/month that benefited from the payment pause.

For the PPP, we assume that 90% of the benefits ultimately accrue to business owners. The rest of the benefits accrue to other stakeholders (mainly workers, but also landlords and suppliers) but do not constitute a positive income shock for them. Thus, we assume that $720 billion of the $800 billion was an income boost to owners.

We assume the same MPX for autos of 0.17, estimated in our research design, for the UI and student loan program recipients. Business owners are disproportionately wealthy, likely with much lower MPXs than the general population. So we assume an MPX of 0.01 for them.

These assumptions imply an additional $37 billion, $27 billion, and $7 billion in auto purchases from the UI, student loan, and PPP programs, respectively. The total additional auto purchases from all fiscal programs is approximately 2.7 million vehicles, which we add to the baseline of 5.6 million in the *All Fiscal Transfers* scenario in Table 5.

In addition, we also estimate the demand impact for households from mortgage refinances, which increased substantially during the sample period. We estimate that excess mortgage refinances from both cash-out and rate refinances provided an additional $275 billion to households. Using data from ICE McDash from 2017-2022 we estimate the share of refinances that are excess during 2020-2021 (roughly 80%) compared to the prepandemic level. We then use the average cash out amounts ($82,000) and payment changes ($150/month increase for cash out, $220/month decrease for rate refinances) reported in Haughwout, Lee, Mangrum, Scally and van der Klaauw (2023). Applying our baseline MPX for autos, this translates into $47 billion in auto spending (1.7 million sales).

We think of this demand boost as reflecting the influence of expansionary monetary policy, akin to the monetary shock in our model. For this reason, we exclude the demand boost from the *All Fiscal Transfers* scenario in Table 5.

A.7 Liquid Wealth Shock

Abdelrahman, Oliveira and Shapiro (2024) show that household liquid wealth increased from $12 trillion to $16 trillion during the first year of the pandemic, which subsequently stabilizes and then falls. The sources of this increase include reduced consumption, monetary policy through the refinancing channel ($275 billion), and fiscal policy through stimulus checks and childcare tax credits ($900 billion), PPP loans ($800 billion), UI payments ($900 billion), SNAP extensions ($150 billion), and student loan forbearance ($160 billion). They forecast growth in liquid wealth in the absence of the pandemic and fiscal response to $14 trillion. Therefore, an upper bound on the share of excess wealth attributable to transfers excluding stimulus checks and the advanced CTC is approximately: $\frac{1850}{2750} \times \frac{16-14}{14} \approx 10\%$./footnoteAlternatively, Greig, Deadman and Noel (2021) use JPMorgan Chase data and find a mean increase in checking

[42]Note that we omit the wealth effects from the suspension of interest accrual during the forbearance period as the consumption from wealth is likely to be smaller than from income.

accounts of around $2 thousand, with most of the dollar growth coming at the high end of the distribution. Assuming their sample is representative of all 128 million households in the US, this implies an increase of only $250 billion. Because that data does not include accounts at other financial institutions, we prefer the broader measure from the Federal Reserve data.

To shed light on the distribution of the liquid wealth shock, we follow the approach of Abdelrahman, Oliveira and Shapiro (2024) and use the Federal Reserve's Distributional Financial Accounts (DFA) to decompose the rise by wealth and income group.[43] Appendix Figure B.9 and Appendix Tables C.14 and C.15 show that a substantial share of the aggregate increase comes from households in the top decile and top percentile of the wealth distributions. Thus, the data reveal that the groups expected to have higher MPCs show smaller increases in liquid wealth in aggregate terms.

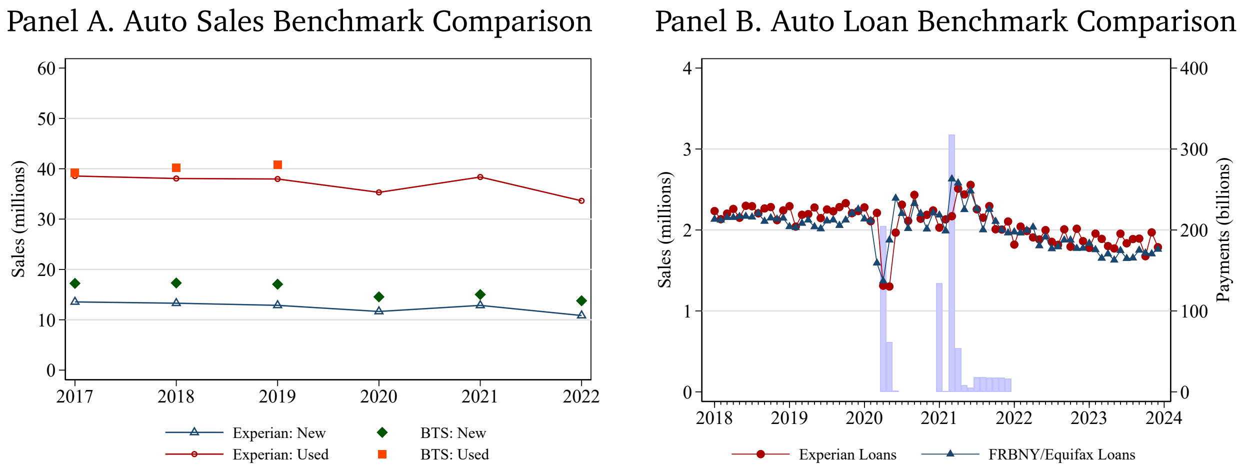
Accounting for each group's relative share of total liquid wealth, we find the bottom 50% in net worth account for just 7% of the total increase, the 50-90% account for 40%, the next 9.9% account for 33%, and the top 0.1% account for 20%. Relative to their 2019 level, the bottom 50% show an increase of 37% through the end of 2021, the 50-90% show an increase of 28%, the next 9.9% show an increase of 15%, and the top 0.1% show an increase of nearly 50%. Thus the increases are largest in proportional terms for the bottom and very top groups, who entered the pandemic with relatively low levels of liquid wealth. However, the aggregate increase is skewed toward those at the top of the distribution.

Overall, the distribution of excess wealth is broad-based, but skews toward the middle and top of the wealth distribution. Similar patterns hold for the distribution of wealth and income. This fact supports using a calibrated uniform wealth shock as a simple approximation, given the limited heterogeneity in our model.

[43] Our aggregate estimates do not exactly match Abdelrahman, Oliveira and Shapiro (2024) because we use a simple linear trend to fit a counterfactual, whereas they use an unreported local projection model.

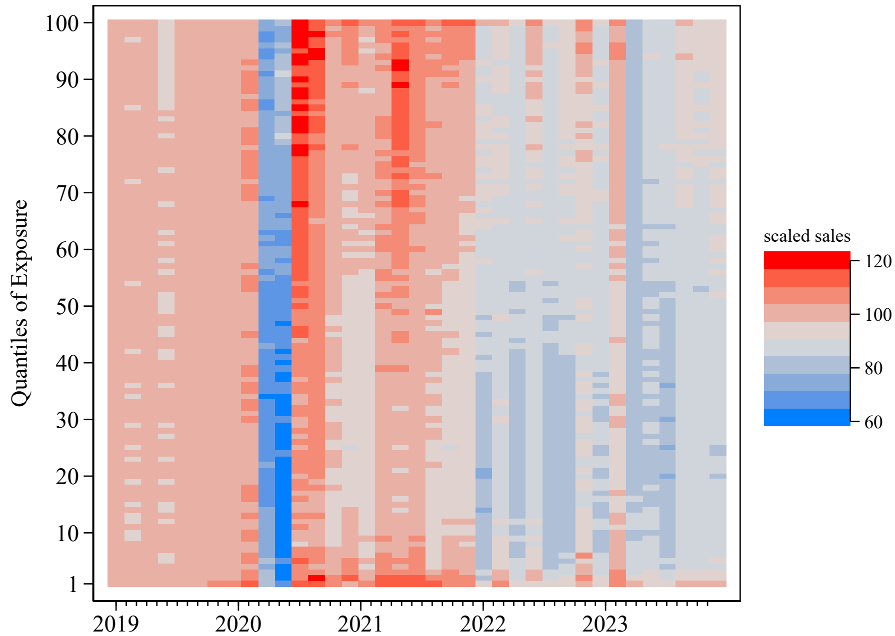
B Appendix Figures

Figure B.1: Benchmarking Experian Data



Notes: Panel A compares auto sales from Experian with those reported by the Bureau of Transportation Statistics (BTS). Data include new auto sales/leases through 2022 and used sales through 2019. Data for Experian are shown for the main analysis sample, adjusted to be nationally representative (the analysis sample represents roughly 90% of households). Panel B compares auto loans from Experian with those reported by the Federal Reserve Board of New York Consumer Credit Panel (CCP)/Equifax, a 5% anonymized random sample of all individuals with credit reports. To facilitate comparisons between the data in Panel B, we scale the CCP data to be nationally representative.

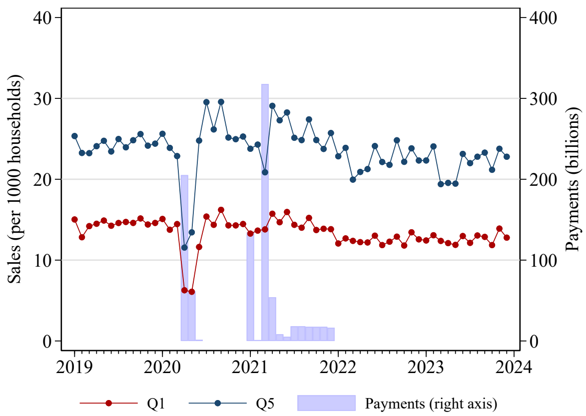
Figure B.2: Used Sales by Quantiles of Exposure



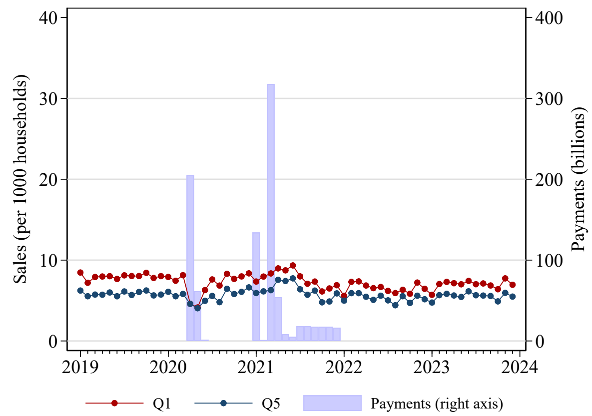
Notes: The figure shows a difference-in-differences calendar-time heatmap of used monthly sales for ZIPs divided into 100 quantiles and sorted based on program exposure. Columns correspond to months and rows correspond to quantiles formed by grouping of ZIPs based on ex ante (2018) exposure. Shading corresponds to a level of used monthly auto sales scaled by average monthly used sales in 2018 (100=2018).

Figure B.3: The Effect of Transfers on Aggregate Auto Sales per 1,000 Households

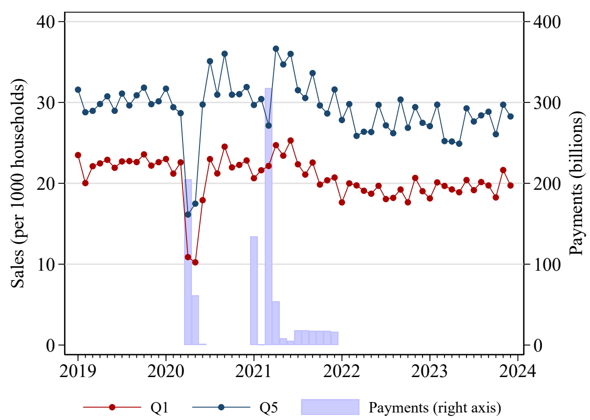
Panel A. Used Autos



Panel B. New Autos

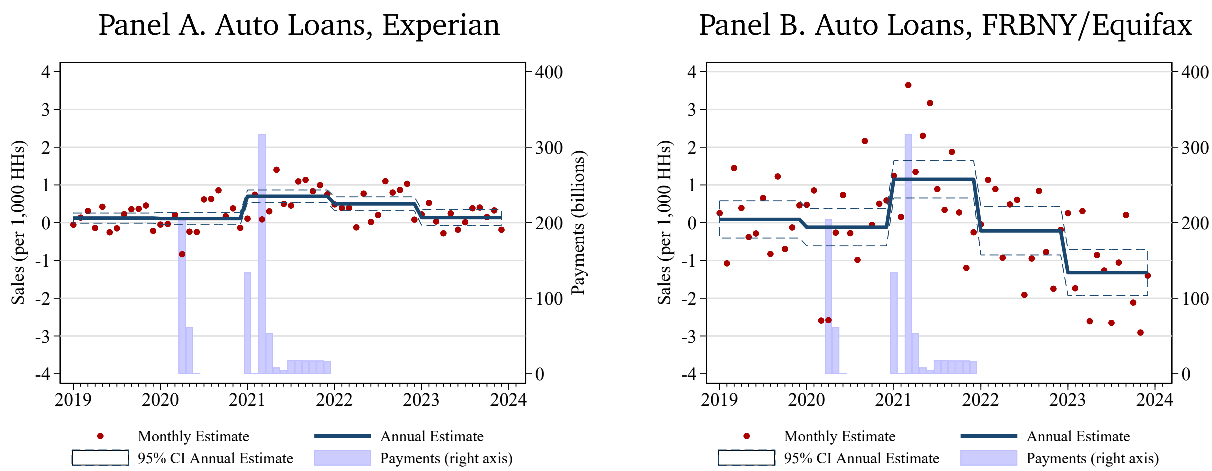


Panel C. Total Autos



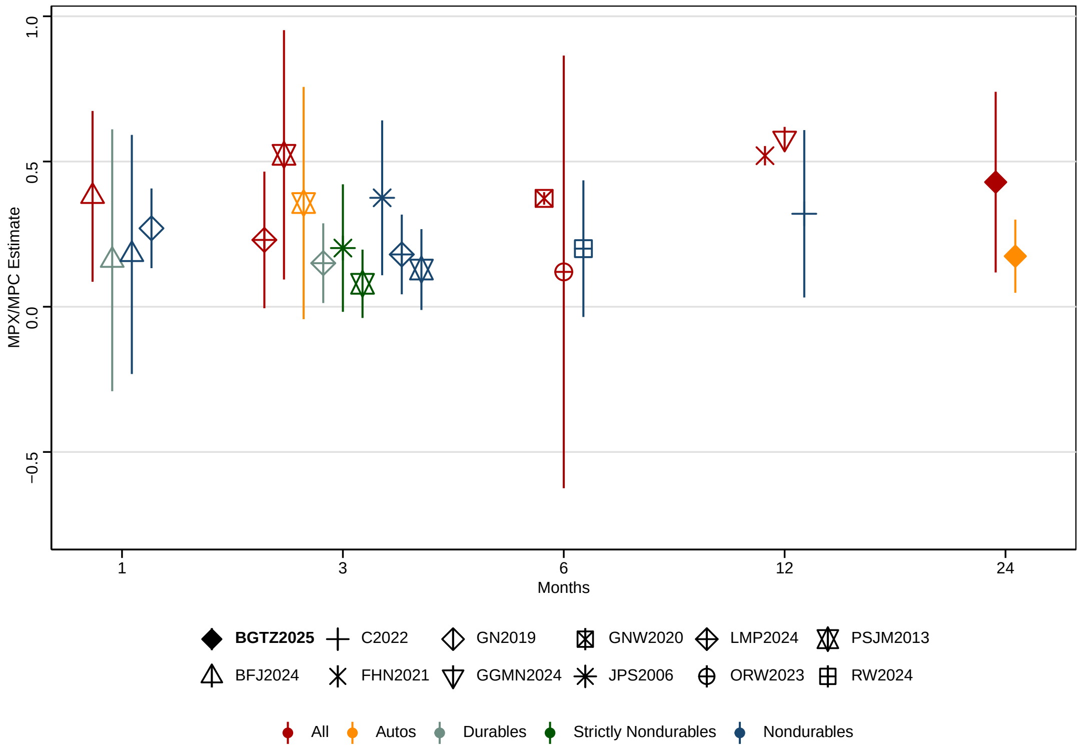
Notes: This figure plots monthly auto sales per 1,000 households at the ZIP level for the top and bottom quintiles of pandemic transfer exposure. Panels A, B, and C plot series for used autos, new autos, and total auto sales, respectively.

Figure B.4: Auto Loan Robustness



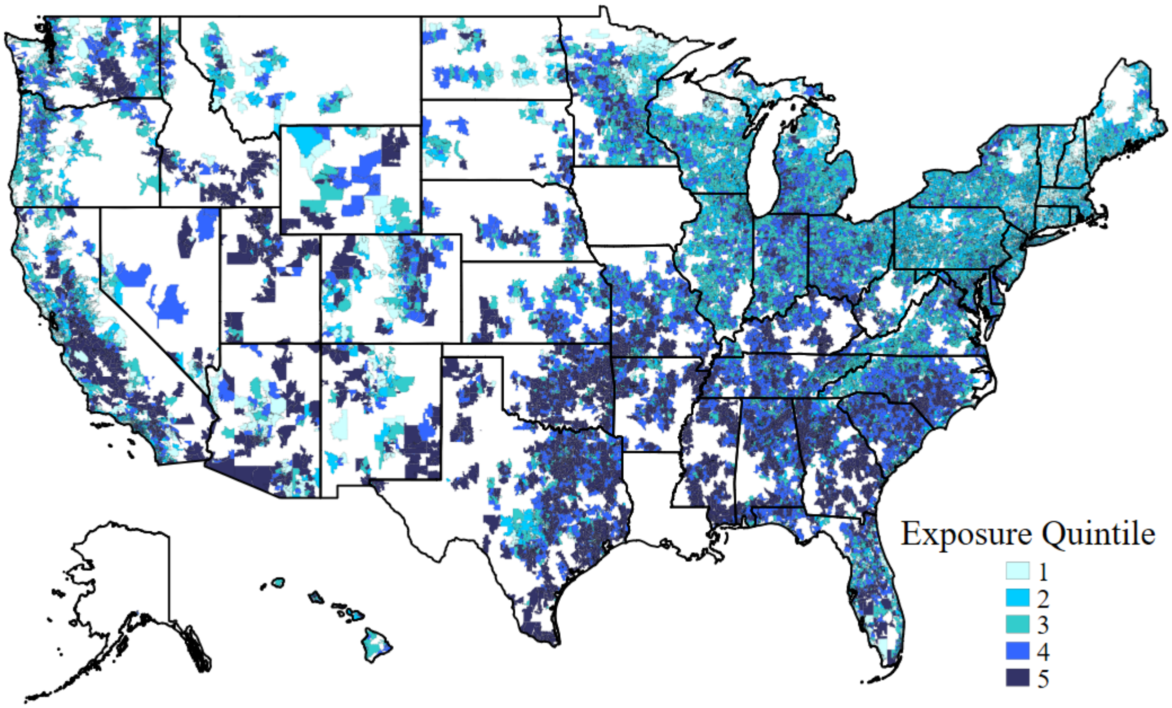
Notes: This figure shows that results for loan financed sales are similar in our analysis sample based on auto registration data and to new auto loan data from individual credit reports. The figure replicates our baseline regression results (Figure 7) for loan financed auto transactions. Panel A uses our baseline sample. Panel B uses data from the FRBNY CCP/Equifax, a 5% anonymized random sample of credit reports. To facilitate comparisons between panels we scale the data in Panel B to be nationally representative.

Figure B.5: MPC Estimates



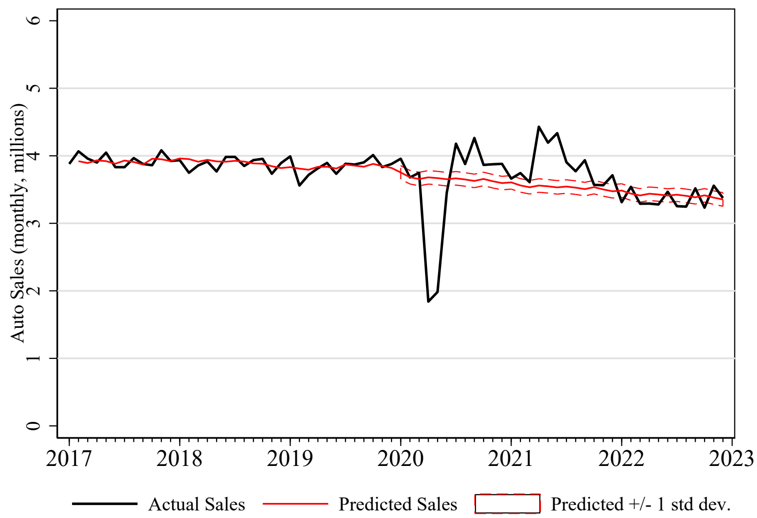
Notes: This figure shows MPC estimates with 95% CIs from literature by spending category at horizons of 1, 3, 6, and 12 months. Legend tags use the first letters of author surnames followed by the year, with multi-word surnames abbreviated by their first letter. BGTZ2025 denotes estimates from this paper at a horizon of about 24 months. Acronym–paper mapping: JPS2006—Johnson, Parker and Souleles (2006); PSJM2013—Parker, Souleles, Johnson and McClelland (2013); GN2019—Ganong and Noel (2019); C2022—Commault (2022); RW2024—Orchard, Ramey and Wieland (2023); BFJ2024—Boehm, Fize and Jaravel (2025); LMP2024—Lewis, Melcangi and Pilossoph (2024); MS2013—Mian, Rao and Sufi (n.d.); TV2021—Tauber and Van Zandweghe (2021); JP2014—Jappelli and Pistaferri (2014); GNW2020—Gross, Notowidigdo and Wang (2020); FHN2021—Fagereng, Holm and Natvik (2021); ORW2023—Orchard, Ramey and Wieland (2025); GGMN2024—Golosov, Graber, Mogstad and Novgorodsky (2023).

Figure B.6: National Variation in Stimulus Exposure



Notes: The figure shows regional variation in exposure to pandemic stimulus 5-digit zip codes in the analysis sample, using the 2018 county-level average CTC value per tax return as the exposure measure.

Figure B.7: Aggregate Car Sales and No-Pandemic Counterfactual

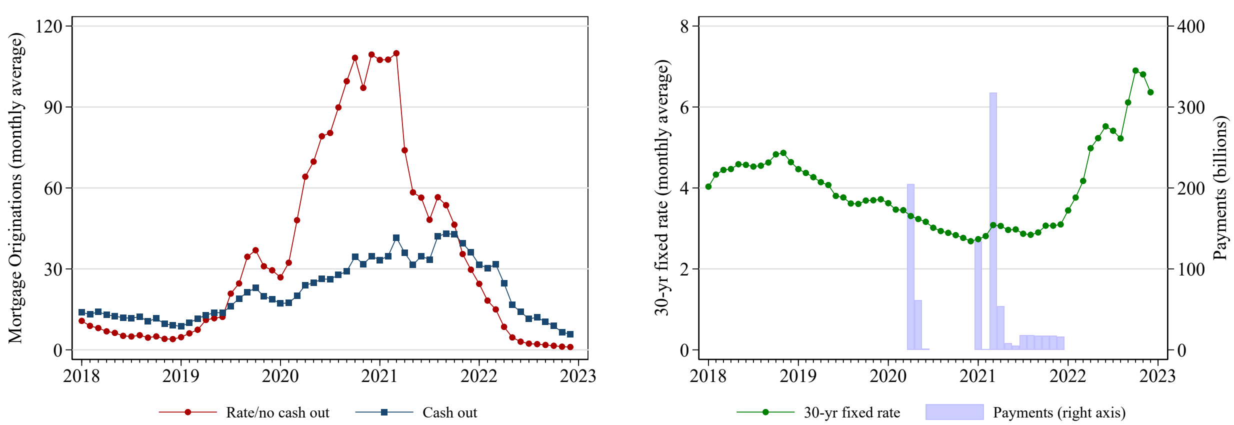


Notes: This figure shows seasonally adjusted total auto sales in black and predicted sales in red. Predicted sales estimate the counterfactual level of auto sales absent the changes in economic conditions during the COVID pandemic, including the additional fiscal stimulus measures and non-fiscal shocks. Predicted sales are estimated from a regression of auto sales (monthly) on real GDP, lagged real GDP levels (quarterly) and lagged unemployment rate (monthly) for 2017-19. Out of sample predicted auto sales in 2020 and beyond use these estimated parameters and predicted values of GDP and the unemployment rate that are estimated using a time series model that includes a linear trend and month/quarterly time indicators estimated for 2017-2019 and projected for 2020 and later years.

Figure B.8: Mortgage Market Activity during the Pandemic

Panel A.

Panel B.

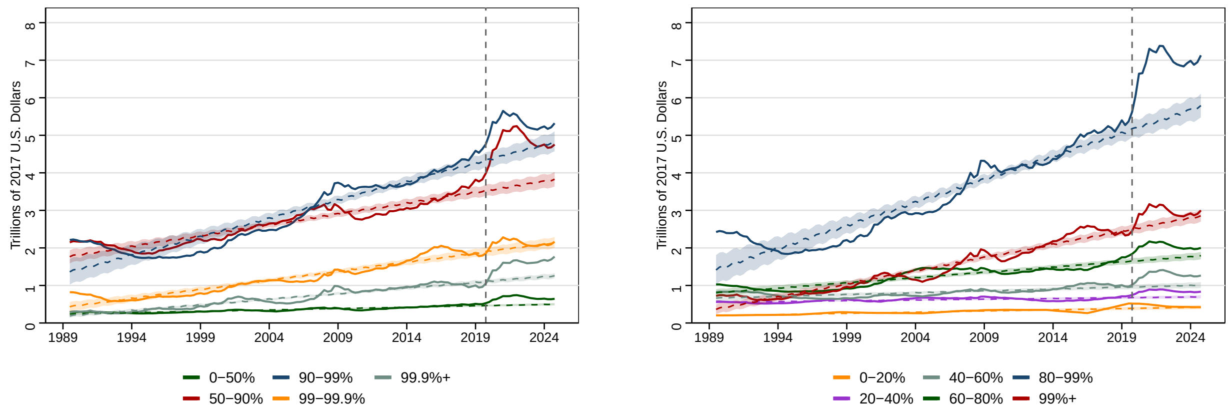


Notes: This figure shows mortgage refinance activity in the left panel, including both cash-out refinances (blue) and rate/no cash out refinances (red) using data from ICE McDash. The right hand side shows the average 30-year mortgage rate (data from FRED) and aggregate monthly payments.

Figure B.9: The Distribution of the Pandemic Increase in Liquid Assets

Panel A. By Net Worth Group

Panel B. By Income Group



Notes: This figure plots quarterly liquid assets in trillions of 2017 U.S. dollars. Panel A decomposes the series by net-worth groups, and Panel B by income groups. Data are from the Distributional Financial Accounts (DFAs). Liquid assets are defined as the sum of “Deposits” and “Money market fund shares” from DFA datasets. Quarterly data are adjusted for inflation using the overall personal consumption expenditures (PCE) price index. Solid lines show actual values; dashed lines show a counterfactual path projected from a linear time trend with quarter fixed effects estimated on data through 2019Q4; shaded bands are 95% confidence intervals (Newey–West with 4 lags). The vertical dashed line marks 2019Q4, the end of the estimation sample.

C Appendix Tables

Table C.1: Family Vehicle Regressions

	(1)	(2)	(3)	(4)	(5)	(6)
	Total		New		Used	
	Family	NonFamily	Family	NonFamily	Family	NonFamily
2019	0.147	0.074	0.086	0.017	0.055	0.053
	[0.078]	[0.055]	[0.033]	[0.018]	[0.071]	[0.050]
2020	0.465	0.354	0.092	0.029	0.357	0.319
	[0.115]	[0.071]	[0.033]	[0.021]	[0.104]	[0.060]
2021	0.977	0.425	0.280	0.049	0.682	0.376
	[0.120]	[0.071]	[0.036]	[0.024]	[0.107]	[0.061]
2022	0.466	0.327	0.329	0.114	0.122	0.202
	[0.136]	[0.085]	[0.039]	[0.027]	[0.117]	[0.072]
2023	0.037	0.174	0.198	0.064	-0.165	0.108
	[0.148]	[0.094]	[0.045]	[0.027]	[0.124]	[0.082]
2018 Mean	20.77	11.64	4.82	2.24	15.95	9.40
R2	0.781	0.593	0.516	0.461	0.786	0.609
N						1,587,888

Notes: All outcomes are monthly sales per 1,000 households. Family cars include all non-sport and non-luxury models. Sample size, number of zipcodes 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.2: Used Vehicle Quality Results

	(1)	(2)
	Age	Mileage
2019	-0.013	68.92
	[0.021]	[373.7]
2020	0.028	-470.7
	[0.029]	[532.5]
2021	-0.068	-1781
	[0.028]	[546.3]
2022	-0.120	-2450
	[0.030]	[633.2]
2023	-0.051	-1424
	[0.032]	[632.8]
2018 Mean	9.89	147,691
N		1,587,888

Notes: Outcome in column (1) is age of car in years and in column (2) is the odometer readming in miles for used car sales. Sample size, number of zipcodes 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.3: Used Sales Robustness

	(1)	(2)	(3)	(4)
	Pre-period (2019)	Policy-period (2020-2022)	Post-period (2023)	Sample Size
Baseline	0.066	0.660	-0.053	1,587,888
	[0.088]	[0.131]	[0.183]	
Population weighted	-0.005	0.800	0.079	1,587,888
	[0.064]	[0.112]	[0.205]	
Sales weighted	0.123	0.645	-0.049	1,587,888
	[0.108]	[0.150]	[0.204]	
No state travel restrictions	0.161	1.035	0.290	829,944
	[0.122]	[0.212]	[0.283]	
State travel restrictions	-0.053	0.249	-0.412	757,944
	[0.124]	[0.149]	[0.239]	
2017 exposure and controls	-0.024	1.034	-0.163	1,587,888
	[0.148]	[0.212]	[0.319]	
Fixed effects only	0.014	0.362	-0.642	1,587,888
	[0.075]	[0.113]	[0.188]	
Number of children interactions	0.150	1.252	-0.157	1,587,888
	[0.135]	[0.227]	[0.323]	
Additional controls	0.020	0.399	-0.171	1,587,888
	[0.094]	[0.136]	[0.182]	
Gas/diesel	0.053	0.627	-0.085	1,587,888
	[0.086]	[0.125]	[0.177]	
Hybrid/electric	0.009	0.021	0.026	1,587,888
	[0.005]	[0.005]	[0.008]	
Ln sales	0.004	0.033	0.022	1,587,888
	[0.003]	[0.006]	[0.008]	

Notes: All outcomes are monthly sales per 1,000 households. Standard errors clustered at the CBSA level are in brackets. Population and sales weights are based on 2018 number of tax returns and 2017 sales respectively. Travel restrictions are based on data from Balletopedia. The 2017 exposure and controls replace all 2018 SOI data with data from 2017 SOI files. Number of children interactions replace the exposure measure with the average number of dependents per tax return in 2018. Additional controls include controls for student loans, and the full set of GPS, PPP and mortgage variables shown in Table 1.

Table C.4: New Sales Robustness

	(1)	(2)	(3)	(4)
	Pre-period (2019)	Policy-period (2020-2022)	Post-period (2023)	Sample Size
Baseline	0.151	0.397	0.397	1,587,888
	[0.049]	[0.057]	[0.075]	
Population weighted	0.168	0.566	0.700	1,587,888
	[0.040]	[0.081]	[0.089]	
Sales weighted	0.149	0.393	0.362	1,587,888
	[0.055]	[0.058]	[0.081]	
No state travel restrictions	0.132	0.470	0.510	829,944
	[0.070]	[0.098]	[0.125]	
State travel restrictions	0.182	0.321	0.267	757,944
	[0.067]	[0.066]	[0.089]	
2017 exposure and controls	0.242	0.714	0.637	1,587,888
	[0.084]	[0.105]	[0.134]	
Fixed effects only	0.141	0.460	0.309	1,587,888
	[0.038]	[0.058]	[0.071]	
Number of children interactions	0.243	0.559	0.826	1,587,888
	[0.081]	[0.114]	[0.138]	
Additional controls	0.142	0.427	0.330	1,587,888
	[0.049]	[0.054]	[0.070]	
Gas/diesel	0.093	0.278	0.262	1,587,888
	[0.039]	[0.041]	[0.058]	
Hybrid/electric	-0.008	0.013	0.003	1,587,888
	[0.006]	[0.010]	[0.020]	
Ln sales	0.016	0.042	0.031	1,587,888
	[0.005]	[0.006]	[0.008]	

Notes: All outcomes are monthly sales per 1,000 households. Standard errors clustered at the CBSA level are in brackets. Population and sales weights are based on 2018 number of tax returns and 2017 sales respectively. Travel restrictions are based on data from Balletopedia. The 2017 exposure and controls replace all 2018 SOI data with data from 2017 SOI files. Number of children interactions replace the exposure measure with the average number of dependents per tax return in 2018. Additional controls include controls for student loans, and the full set of GPS, PPP and mortgage variables shown in Table 1.

Table C.5: Total Sales Robustness

	(1)	(2)	(3)	(4)
	Pre-period (2019)	Policy-period (2020-2022)	Post-period (2023)	Sample Size
Baseline	0.216	1.035	0.308	1,587,888
	[0.113]	[0.154]	[0.216]	
Population weighted	0.162	1.299	0.648	1,587,888
	[0.087]	[0.134]	[0.238]	
Sales weighted	0.256	1.012	0.284	1,587,888
	[0.118]	[0.171]	[0.240]	
No state travel restrictions	0.266	1.465	0.752	829,944
	[0.141]	[0.246]	[0.337]	
State travel restrictions	0.135	0.547	-0.174	757,944
	[0.144]	[0.172]	[0.274]	
2017 exposure and controls	0.257	1.737	0.427	1,587,888
	[0.174]	[0.240]	[0.357]	
Fixed effects only	0.155	0.842	-0.318	1,587,888
	[0.079]	[0.138]	[0.202]	
Number of children interactions	0.415	1.757	0.588	1,587,888
	[0.168]	[0.283]	[0.390]	
Additional controls	0.157	0.803	0.126	1,587,888
	[0.110]	[0.157]	[0.215]	
Gas/diesel	0.141	0.918	0.186	1,587,888
	[0.094]	[0.149]	[0.210]	
Hybrid/electric	0.002	0.029	0.021	1,587,888
	[0.009]	[0.011]	[0.023]	
Ln sales	0.007	0.036	0.022	1,587,888
	[0.003]	[0.005]	[0.006]	

Notes: All outcomes are monthly sales per 1,000 households. Standard errors clustered at the CBSA level are in brackets. Population and sales weights are based on 2018 number of tax returns and 2017 sales respectively. Travel restrictions are based on data from Balletopedia. The 2017 exposure and controls replace all 2018 SOI data with data from 2017 SOI files. Number of children interactions replace the exposure measure with the average number of dependents per tax return in 2018. Additional controls include controls for student loans, and the full set of GPS, PPP and mortgage variables shown in Table 1.

Table C.6: Variable Details and Sources

(1)	(2)	(3)	(4)
Description	Baseline Control Set	Form	Source
Exposure	-	Child tax credit ($1000s) per tax return	Statistics of Income, Internal Revenue Service, 2018
Auto Sales			
Auto Sales	-	Monthly sales per 1,000 tax returns 2018	Experian/Velocity, 2018-2023
Zip & County Cross Sectional Characteristics			
		Tax Return Data	
Adjusted gross income (AGI)	Y	LN(AGI per tax return)	Statistics of Income, Internal Revenue Service, 2018
Number of tax returns	Y	LN(# of returns)	
Married tax returns	Y	Share of married tax returns	
Student loans	N	Share of tax returns with student loan interest deduction	
Household growth rate	Y	Percent increase in number of tax returns (2015-2018)	
		Political Alignment Data	
Political alignment	Y	Republican Party Vote Share (county level)	MIT Election Lab, 2016
		Ex ante Unemployment Data	
Unemployment	Y	Unemployment rate, ages 18 to 64	American Community Survey, 2016-19
Vehicle miles traveled	N	Miles driven for weekday travel	Bureau of Transportation Statistics estimates derived from American Community Survey Data, 2012–16
White population	Y	White population	American Community Survey, 2014-18
Elderly population	Y	Age 65+ population	
Zip & County Time Varying Characteristics			
		COVID-19 Data	
Cases	Y	Cumulative amounts (county level)	U.S. COVID Tracker, COVID ActNow, 2020-2023
Deaths	Y		
Vaccinations	Y		
		GPS Tracking Data	
Work	Y	GPS tracking, indexed relative to Jan 3–Feb 5 2020 (county level)	Opportunity Insights, Economic Tracker
Home	N		
Retail	N		
		Unemployment Data	
Unemployment	Y	Local area unemployment rate estimates (county level)	Bureau of Labor Statistics, 2018-23
		Paycheck Protection Program (PPP) data	
Loans	Y	Number per 2018 tax returns	Small Business Administration, 2020-23
Amounts	Y	Amount ($1000s) per 2018 tax returns	
Mortgage Market Data			
Originations	N	Number per 2018 tax returns	Intercontinental Exchange (ICE) McDash, 2018-2023
Refinances, cash out	Y		
Refinances, no cash out	N		

Notes: Data are zip code level unless otherwise noted. All time constant zip-level varaibles enter the control set as interactions with time dummies for each month beginning in January 2019.

Table C.7: Auto Demand Elasticity Estimates

Paper	Parameter	Value
Bordley (1993)	Market elasticity for new cars (p.460)	-1.0
Leard (2022)	Market elasticity for new cars (p.42)	-0.3
Grieco, Murry and Yurukoglu (2024)	Market elasticity for new cars (p.1224)	-1.3
Ankney and Leard (2025)	Elasticity of used car demand (p.566)	-1.5
Bento, Roth and Zuo (2018)	Scrappage elasticity (p.170)	-0.4
Jacobsen and van Benthem (2015)	Scrappage elasticity (p.1325)	-0.7
	Mean	-0.9

Notes: The table lists papers that estimate price elasticities of demand for new cars or scrappage elasticities, which we use as a proxy for used cars.

Table C.8: Elastic Supply Counterfactuals for All Cars: Peak & Cumulative (0–3 periods)

Scenario	Pr. Peak	Pr. Cum.	Qty. Peak	Qty. Cum.
Income only	-0.041	-0.224	3.256	4.066
Wealth shock only (10%)	-0.215	-0.763	5.746	4.621
Wealth shock only (20%)	-0.205	-1.011	18.646	21.838
Preference shift only (+1 pp)	-0.130	-4.259	13.725	8.067
Borrowing relaxation only	0.625	0.479	27.773	14.098
Income + wealth (10%)	-0.118	-0.613	9.812	17.923
Income + wealth (20%)	-0.280	-1.240	21.277	26.199
Income + borrowing relaxation	0.604	0.323	32.983	20.660
Income + preference shift	-0.211	-4.140	16.581	21.106
Income + wealth (10%) + borrowing	0.456	-0.310	39.892	28.581
Income + wealth (10%) + preference	-0.477	-4.750	27.532	30.871
Full combo (wealth (10%) + others)	-0.572	-4.477	51.333	40.605
Income + wealth (20%) + borrowing	0.295	-1.021	45.501	34.587
Income + wealth (20%) + preference	-0.709	-5.472	36.710	37.478
Full combo (wealth (20%) + others)	-0.796	-5.285	54.548	44.844

Notes: This table presents peak and cumulative impulse responses for prices and quantities under a variety of shock counterfactuals described in the text. The responses are reported in percentage points. This table focuses on outcomes for both used and new cars in the case of elastic supply of new cars.

Table C.9: Elastic Supply Counterfactuals for New Cars: Peak & Cumulative (0–3 periods)

Scenario	Pr. Peak	Pr. Cum.	Qty. Peak	Qty. Cum.
Income only	0.000	0.000	2.837	3.488
Wealth shock only (10%)	0.000	0.000	4.710	3.080
Wealth shock only (20%)	0.000	0.000	15.772	16.875
Preference shift only (+1 pp)	0.000	0.000	9.714	2.445
Borrowing relaxation only	0.000	0.000	24.437	10.611
Income + wealth (10%)	0.000	0.000	8.163	14.557
Income + wealth (20%)	0.000	0.000	18.062	20.521
Income + borrowing relaxation	0.000	0.000	28.959	16.254
Income + preference shift	0.000	0.000	12.414	13.671
Income + wealth (10%) + borrowing	0.000	0.000	34.563	22.179
Income + wealth (10%) + preference	0.000	0.000	21.727	21.135
Full combo (wealth (10%) + others)	0.000	0.000	42.396	28.458
Income + wealth (20%) + borrowing	0.000	0.000	39.157	26.439
Income + wealth (20%) + preference	0.000	0.000	29.495	26.014
Full combo (wealth (20%) + others)	0.000	0.000	44.926	31.380

Notes: This table focuses on outcomes for new cars in the case of elastic supply of new cars. See the note to Appendix Table C.8 for more details.

Table C.10: Elastic Supply Counterfactuals for Used Cars: Peak & Cumulative (0–3 periods)

Scenario	Pr. Peak	Pr. Cum.	Qty. Peak	Qty. Cum.
Income only	-0.167	-0.924	3.902	4.956
Wealth shock only (10%)	-0.886	-3.141	7.343	6.998
Wealth shock only (20%)	-0.846	-4.163	23.080	29.495
Preference shift only (+1 pp)	-0.537	-17.544	19.913	16.738
Borrowing relaxation only	2.574	1.972	32.918	19.477
Income + wealth (10%)	-0.486	-2.525	12.356	23.116
Income + wealth (20%)	-1.154	-5.109	26.235	34.958
Income + borrowing relaxation	2.489	1.329	39.189	27.456
Income + preference shift	-0.869	-17.051	23.009	32.576
Income + wealth (10%) + borrowing	1.878	-1.279	48.112	38.456
Income + wealth (10%) + preference	-1.966	-19.565	36.488	45.890
Full combo (wealth (10%) + others)	-2.356	-18.441	65.120	59.345
Income + wealth (20%) + borrowing	1.216	-4.207	55.287	47.157
Income + wealth (20%) + preference	-2.922	-22.537	47.840	55.163
Full combo (wealth (20%) + others)	-3.280	-21.770	69.393	65.615

Notes: This table focuses on outcomes for used cars in the case of elastic supply of new cars. See the note to Appendix Table C.8 for more details.

Table C.11: Inelastic Supply Counterfactuals for All Cars: Peak & Cumulative (0–3 periods)

	Supply inelastic for 2 periods				Supply inelastic for 4 periods				Supply inelastic permanently			
Scenario	Pr. Peak	Pr. Cum.	Qty. Peak	Qty. Cum.	Pr. Peak	Pr. Cum.	Qty. Peak	Qty. Cum.	Pr. Peak	Pr. Cum.	Qty. Peak	Qty. Cum.
Income only	0.118	0.004	2.614	3.868	0.668	0.955	1.548	3.287	0.120	-0.023	2.560	3.847
Wealth shock only (10%)	0.653	0.312	2.051	3.425	0.652	0.202	2.015	3.678	0.652	0.202	2.015	3.680
Wealth shock only (20%)	3.239	4.487	5.227	10.036	3.456	5.789	3.009	7.278	3.477	5.767	2.862	6.986
Preference shift only (+1 pp)	1.394	2.646	4.805	4.609	0.856	-1.106	4.663	7.671	0.873	-1.082	4.641	7.551
Borrowing relaxation only	8.178	11.949	2.971	-2.169	6.956	6.556	2.340	5.260	6.908	6.375	2.430	5.368
Income + wealth (10%)	2.538	4.601	5.698	9.407	2.662	5.939	2.414	6.141	2.634	5.838	2.407	6.023
Income + wealth (20%)	5.450	9.032	13.827	18.776	6.776	15.209	2.683	6.227	6.703	14.844	2.665	6.233
Income + borrowing relaxation	9.555	14.142	3.262	5.840	9.219	12.684	2.571	6.543	9.093	12.194	2.593	6.422
Income + preference shift	3.468	6.691	5.137	7.292	2.950	3.781	5.066	9.633	2.944	3.699	5.032	9.477
Income + wealth (10%) + borrowing	10.754	16.078	12.804	18.433	11.839	20.006	4.043	8.907	11.619	19.133	4.032	8.646
Income + wealth (10%) + preference	7.192	11.942	6.378	12.650	6.992	11.218	6.465	12.630	6.919	10.842	6.426	12.399
Full combo (wealth (10%) + others)	18.194	27.511	8.153	17.965	17.919	26.554	7.887	16.057	17.687	25.568	7.651	15.270
Income + wealth (20%) + borrowing	11.180	16.432	16.458	23.002	13.828	25.614	4.723	11.112	13.651	24.889	4.573	10.891
Income + wealth (20%) + preference	11.060	17.781	8.640	20.499	11.671	20.874	7.126	15.142	11.484	20.076	7.138	15.056
Full combo (wealth (20%) + others)	19.071	28.684	11.942	25.352	20.853	35.009	8.736	18.070	20.233	32.739	8.815	18.161

Notes: This table focuses on outcomes for new and used cars in the case of inelastic supply of new cars. We consider temporary supply shocks for 2 periods, 4 periods, and permanent. See the note to Appendix Table C.8 for more details.

Table C.12: Inelastic Supply Counterfactuals for New Cars: Peak & Cumulative (0–3 periods)

	Supply inelastic for 2 periods				Supply inelastic for 4 periods				Supply inelastic permanently			
Scenario	Pr. Peak	Pr. Cum.	Qty. Peak	Qty. Cum.	Pr. Peak	Pr. Cum.	Qty. Peak	Qty. Cum.	Pr. Peak	Pr. Cum.	Qty. Peak	Qty. Cum.
Income only	0.143	0.216	2.293	3.326	0.570	0.960	1.433	2.952	0.143	0.188	2.251	3.316
Wealth shock only (10%)	0.711	0.836	1.638	2.352	0.707	0.731	1.609	2.579	0.707	0.730	1.608	2.581
Wealth shock only (20%)	2.955	4.578	4.081	8.002	3.152	5.682	2.506	5.651	3.162	5.625	2.390	5.419
Preference shift only (+1 pp)	3.176	5.431	2.414	0.746	2.682	2.311	2.228	3.105	2.691	2.310	2.212	3.006
Borrowing relaxation only	6.228	9.177	3.696	-0.699	5.180	4.638	3.040	5.389	5.133	4.464	3.118	5.462
Income + wealth (10%)	2.291	4.283	4.688	8.220	2.425	5.436	2.227	5.412	2.396	5.325	2.222	5.309
Income + wealth (20%)	4.759	8.127	11.526	16.446	5.942	13.419	2.675	5.895	5.867	13.065	2.676	5.892
Income + borrowing relaxation	7.387	10.971	4.144	6.651	7.098	9.717	3.501	7.207	6.977	9.238	3.492	7.044
Income + preference shift	4.917	8.800	2.996	3.599	4.456	6.367	2.906	5.434	4.441	6.250	2.880	5.307
Income + wealth (10%) + borrowing	8.524	12.816	10.628	17.549	9.442	16.115	4.875	9.480	9.235	15.288	4.838	9.175
Income + wealth (10%) + preference	8.050	13.411	4.203	8.290	7.875	12.791	4.256	8.206	7.794	12.394	4.212	7.971
Full combo (wealth (10%) + others)	16.428	25.145	6.974	14.437	16.189	24.355	6.735	12.745	15.964	23.384	6.508	11.999
Income + wealth (20%) + borrowing	9.097	13.659	13.718	20.938	11.301	21.231	5.342	11.275	11.133	20.531	5.191	11.032
Income + wealth (20%) + preference	11.280	18.402	6.929	15.318	11.817	21.005	5.134	10.837	11.638	20.245	5.131	10.707
Full combo (wealth (20%) + others)	17.364	26.593	9.705	20.713	18.835	31.744	7.513	14.705	18.286	29.726	7.552	14.618

Notes: This table focuses on outcomes for new cars in the case of inelastic supply of new cars. We consider temporary supply shocks for 2 periods, 4 periods, and permanent. See the note to Appendix Table C.8 for more details.

Table C.13: Inelastic Supply Counterfactuals for Used Cars: Peak & Cumulative (0–3 periods)

	Supply inelastic for 2 periods				Supply inelastic for 4 periods				Supply inelastic permanently			
Scenario	Pr. Peak	Pr. Cum.	Qty. Peak	Qty. Cum.	Pr. Peak	Pr. Cum.	Qty. Peak	Qty. Cum.	Pr. Peak	Pr. Cum.	Qty. Peak	Qty. Cum.
Income only	0.039	-0.656	3.109	4.704	0.974	0.942	1.726	3.805	0.048	-0.683	3.038	4.667
Wealth shock only (10%)	0.470	-1.323	2.689	5.081	0.480	-1.450	2.643	5.372	0.482	-1.444	2.642	5.376
Wealth shock only (20%)	4.125	4.203	6.994	13.173	4.402	6.123	3.786	9.787	4.461	6.212	3.589	9.403
Preference shift only (+1 pp)	-0.093	-6.039	8.494	10.569	-3.376	-11.765	8.419	14.716	-3.334	-11.665	8.390	14.563
Borrowing relaxation only	14.260	20.595	2.327	-4.436	12.495	12.537	2.138	5.061	12.442	12.338	2.102	5.224
Income + wealth (10%)	3.313	5.595	7.256	11.237	3.758	7.507	2.703	7.265	3.747	7.437	2.692	7.124
Income + wealth (20%)	7.604	11.857	17.376	22.371	9.379	20.792	2.742	6.738	9.309	20.393	2.795	6.759
Income + borrowing relaxation	16.317	24.030	1.903	4.588	15.833	21.937	2.697	5.517	15.691	21.414	2.571	5.462
Income + preference shift	1.563	0.110	8.440	12.990	0.324	-4.285	8.398	16.112	0.339	-4.257	8.352	15.911
Income + wealth (10%) + borrowing	17.710	26.251	16.161	19.795	19.315	32.141	2.950	8.024	19.053	31.124	2.798	7.830
Income + wealth (10%) + preference	4.516	7.359	9.733	19.374	4.239	6.312	9.871	19.456	4.189	6.002	9.841	19.230
Full combo (wealth (10%) + others)	23.701	34.893	9.973	23.406	23.316	33.414	9.665	21.166	23.061	32.381	9.415	20.317
Income + wealth (20%) + borrowing	17.677	25.081	20.687	26.186	21.709	39.285	3.774	10.859	21.504	38.484	3.867	10.674
Income + wealth (20%) + preference	10.373	15.843	11.279	28.490	11.215	20.466	10.197	21.782	11.004	19.550	10.235	21.766
Full combo (wealth (20%) + others)	24.396	35.205	15.393	32.508	27.147	45.193	10.625	23.260	26.307	42.135	10.764	23.626

Notes: This table focuses on outcomes for used cars in the case of inelastic supply of new cars. We consider temporary supply shocks for 2 periods, 4 periods, and permanent. See the note to Appendix Table C.8 for more details.

Table C.14: Cumulative Increase in Excess Savings Relative to the Counterfactual, by Net Worth Group (2019Q4 Baseline)

	0-50%			50-90%			90-99%			99-99.9%			99.9%+			Aggregate		
Quarter	Δ ($T)	Δ (%)	Fitted ($T)	Δ ($T)	Δ (%)	Fitted ($T)	Δ ($T)	Δ (%)	Fitted ($T)	Δ ($T)	Δ (%)	Fitted ($T)	Δ ($T)	Δ (%)	Fitted ($T)	Δ ($T)	Δ (%)	Fitted ($T)
2020 Q1	0.03	5.49	0.47	0.21	5.35	3.55	0.24	4.94	4.37	0.12	6.26	1.92	0.15	14.37	1.12	0.74	6.11	11.43
2020 Q2	0.09	17.44	0.46	0.61	15.37	3.54	0.59	12.45	4.35	0.30	15.95	1.91	0.35	33.59	1.11	1.95	15.97	11.36
2020 Q3	0.10	19.30	0.47	0.65	16.23	3.56	0.52	10.86	4.40	0.25	13.40	1.93	0.35	33.07	1.12	1.86	15.28	11.46
2020 Q4	0.14	25.67	0.47	0.83	20.75	3.60	0.59	12.46	4.45	0.28	15.08	1.95	0.40	38.21	1.14	2.24	18.37	11.61
2021 Q1	0.18	33.75	0.47	1.08	27.08	3.61	0.77	16.18	4.47	0.36	19.63	1.97	0.51	48.90	1.15	2.91	23.85	11.67
2021 Q2	0.18	33.85	0.47	1.07	26.86	3.59	0.71	14.97	4.45	0.34	18.13	1.95	0.52	49.92	1.14	2.82	23.17	11.61
2021 Q3	0.18	33.84	0.47	1.05	26.31	3.61	0.61	12.75	4.49	0.28	15.25	1.97	0.51	48.83	1.15	2.63	21.59	11.70
2021 Q4	0.19	36.67	0.48	1.13	28.24	3.66	0.62	13.09	4.55	0.30	15.95	2.00	0.55	52.84	1.17	2.79	22.93	11.85
2022 Q1	0.20	37.22	0.48	1.13	28.24	3.67	0.56	11.78	4.56	0.26	14.00	2.02	0.55	52.30	1.18	2.69	22.10	11.91
2022 Q2	0.18	34.60	0.48	1.04	26.02	3.65	0.45	9.51	4.55	0.21	11.29	2.00	0.54	50.97	1.17	2.42	19.85	11.85
2022 Q3	0.16	31.10	0.48	0.92	23.17	3.67	0.31	6.44	4.59	0.14	7.31	2.02	0.50	47.90	1.18	2.03	16.69	11.94
2022 Q4	0.14	25.61	0.49	0.75	18.87	3.71	0.17	3.49	4.64	0.07	3.71	2.05	0.45	42.95	1.20	1.57	12.92	12.09
2023 Q1	0.11	21.05	0.49	0.63	15.90	3.73	0.12	2.42	4.66	0.04	2.25	2.06	0.45	42.43	1.21	1.35	11.06	12.15
2023 Q2	0.10	19.38	0.49	0.59	14.87	3.71	0.11	2.35	4.64	0.05	2.66	2.05	0.47	44.57	1.20	1.32	10.87	12.09
2023 Q3	0.09	16.82	0.49	0.52	12.97	3.73	0.05	1.03	4.69	0.03	1.47	2.07	0.47	44.77	1.21	1.15	9.46	12.18
2023 Q4	0.09	16.18	0.49	0.50	12.56	3.77	0.05	1.00	4.74	0.03	1.81	2.10	0.49	46.24	1.23	1.15	9.46	12.33
2019 Q4 Level		0.53			3.99			4.76			1.85			1.05			12.18	

Notes: The table reports the cumulative increase in liquid assets relative to a counterfactual, by net worth group, using 2019Q4 as the baseline. All monetary values are expressed in trillions of 2017 U.S. dollars. “Fitted” columns show predicted liquid asset levels projected from a linear time trend with quarter fixed effects estimated on data through 2019Q4. The first two columns for each group report the estimated deviation from the counterfactual in a given quarter, net of the deviation in 2019Q4, both in absolute terms ($T) and as a percentage of the group’s 2019Q4 level. Data are from the Distributional Financial Accounts (DFAs). Liquid assets are defined as the sum of “Deposits” and “Money market fund shares” in the DFA. All series are deflated using the overall personal consumption expenditures (PCE) price index.

Table C.15: Cumulative Increase in Excess Savings Relative to the Counterfactual, by Income Group (2019Q4 Baseline)

	0–20%			20–40%			40–60%			60–80%			80–99%			99%+			Aggregate		
Quarter	Δ ($T)	Δ (%)	Fitted ($T)	Δ ($T)	Δ (%)	Fitted ($T)	Δ ($T)	Δ (%)	Fitted ($T)	Δ ($T)	Δ (%)	Fitted ($T)	Δ ($T)	Δ (%)	Fitted ($T)	Δ ($T)	Δ (%)	Fitted ($T)	Δ ($T)	Δ (%)	Fitted ($T)
2020 Q1	0.00	0.06	0.39	0.03	4.41	0.68	0.06	6.10	0.97	0.07	3.93	1.66	0.41	7.25	5.21	0.17	6.94	2.53	0.74	6.11	11.43
2020 Q2	-0.00	-0.27	0.39	0.09	11.89	0.68	0.18	18.03	0.96	0.20	10.80	1.65	1.03	18.27	5.17	0.45	18.40	2.51	1.95	15.97	11.36
2020 Q3	-0.01	-2.32	0.39	0.09	12.26	0.68	0.20	19.52	0.96	0.19	10.40	1.67	0.99	17.55	5.22	0.41	16.61	2.54	1.86	15.28	11.46
2020 Q4	-0.02	-4.06	0.40	0.11	14.98	0.68	0.26	25.41	0.97	0.23	12.72	1.68	1.18	20.97	5.30	0.48	19.46	2.58	2.24	18.37	11.61
2021 Q1	-0.03	-5.41	0.39	0.14	19.09	0.68	0.34	33.12	0.98	0.30	16.43	1.69	1.53	27.26	5.33	0.62	25.37	2.60	2.91	23.85	11.67
2021 Q2	-0.04	-7.85	0.40	0.14	18.77	0.68	0.34	33.46	0.97	0.29	15.58	1.68	1.51	26.81	5.29	0.59	24.12	2.59	2.82	23.17	11.61
2021 Q3	-0.05	-10.26	0.40	0.14	18.20	0.68	0.34	33.27	0.97	0.26	14.22	1.69	1.42	25.21	5.35	0.53	21.72	2.61	2.63	21.59	11.70
2021 Q4	-0.07	-12.81	0.40	0.14	19.08	0.69	0.37	36.13	0.98	0.27	14.73	1.71	1.52	26.97	5.42	0.56	23.03	2.65	2.79	22.93	11.85
2022 Q1	-0.08	-14.74	0.40	0.14	18.64	0.69	0.37	36.52	0.99	0.26	13.93	1.71	1.48	26.33	5.45	0.52	21.36	2.67	2.69	22.10	11.91
2022 Q2	-0.09	-17.10	0.40	0.13	17.04	0.68	0.35	34.43	0.98	0.22	11.85	1.71	1.36	24.22	5.42	0.45	18.39	2.66	2.42	19.85	11.85
2022 Q3	-0.10	-18.90	0.41	0.11	15.26	0.68	0.32	31.33	0.98	0.17	9.31	1.72	1.18	21.03	5.47	0.35	14.14	2.68	2.03	16.69	11.94
2022 Q4	-0.10	-19.85	0.41	0.09	12.32	0.69	0.27	26.58	0.99	0.12	6.29	1.74	0.96	17.12	5.55	0.24	9.67	2.72	1.57	12.92	12.09
2023 Q1	-0.10	-19.71	0.41	0.08	10.69	0.69	0.23	22.98	1.00	0.08	4.56	1.74	0.87	15.53	5.57	0.18	7.34	2.74	1.35	11.06	12.15
2023 Q2	-0.11	-20.48	0.41	0.08	10.31	0.69	0.22	22.01	0.99	0.07	3.91	1.74	0.88	15.58	5.54	0.18	7.45	2.73	1.32	10.87	12.09
2023 Q3	-0.11	-21.44	0.41	0.07	9.31	0.69	0.20	19.90	0.99	0.04	2.37	1.75	0.80	14.21	5.59	0.15	6.10	2.75	1.15	9.46	12.18
2023 Q4	-0.11	-22.20	0.41	0.07	9.10	0.69	0.20	19.67	1.00	0.04	2.01	1.77	0.81	14.38	5.67	0.15	6.32	2.79	1.15	9.46	12.33
2019 Q4 Level		0.52			0.74			1.02			1.84			5.62			2.45			12.18	

Notes: The table reports the cumulative increase in liquid assets relative to a counterfactual, by income group, using 2019Q4 as the baseline. All monetary values are expressed in trillions of 2017 U.S. dollars. “Fitted” columns show predicted liquid asset levels projected from a linear time trend with quarter fixed effects estimated on data through 2019Q4. The first two columns for each group report the estimated deviation from the counterfactual in a given quarter, net of the deviation in 2019Q4, both in absolute terms ($T) and as a percentage of the group’s 2019Q4 level. Data are from the Distributional Financial Accounts (DFAs). Liquid assets are defined as the sum of “Deposits” and “Money market fund shares” in the DFA. All series are deflated using the overall personal consumption expenditures (PCE) price index.