How Did COVID-19 and Stabilization Policies Affect Spending and Employment? A New Real-Time Economic Tracker Based on Private Sector Data*

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Abstract

We build a publicly available platform that tracks economic activity at a granular level in real time using anonymized data from private companies. We report daily statistics on consumer spending, business revenues, employment rates, and other key indicators disaggregated by county, industry, and income group. Using these data, we study the mechanisms through which COVID-19 affected the economy by analyzing heterogeneity in its impacts across geographic areas and income groups. We first show that high-income individuals reduced spending sharply in mid-March 2020, particularly in areas with high rates of COVID-19 infection and in sectors that require physical interaction. This reduction in spending greatly reduced the revenues of businesses that cater to high-income households in person, notably small businesses in affluent ZIP codes. These businesses laid off most of their low-income employees, leading to a surge in unemployment claims in affluent areas. Building on this diagnostic analysis, we use event study designs to estimate the causal effects of policies aimed at mitigating the adverse impacts of COVID. State-ordered reopenings of economies have little impact on local employment. Stimulus payments to low-income households increased consumer spending sharply, but had modest impacts on employment in the short run, perhaps because very little of the increased spending flowed to businesses most affected by the COVID-19 shock. Paycheck Protection Program loans have also had little impact on employment at small businesses. These results suggest that traditional macroeconomic tools – stimulating aggregate demand or providing liquidity to businesses – may have diminished capacity to restore employment when consumer spending is constrained by health concerns. During a pandemic, it may be more fruitful to mitigate economic hardship through social insurance. More broadly, this analysis illustrates how real-time economic tracking using private sector data can help rapidly identify the origins of economic crises and facilitate ongoing evaluation of policy impacts.

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I Introduction

Since the pioneering work of Kuznets (1941), macroeconomic policy decisions have been made on the basis of data collected from recurring surveys of households and businesses conducted by the federal government. Although such statistics have great value for understanding the economy, they have two limitations that have become apparent during the COVID-19 pandemic. First, such data are typically available only at a low frequency with a significant time lag. For example, disaggregated quarterly data on consumer expenditures are typically available with a one year lag in the Consumer Expenditure Survey (CEX). Second, such statistics typically cannot be used to assess granular variation across geographies or subgroups; due to limitations in sample sizes, most statistics are typically reported only at the national or state level and breakdowns by subgroups or sectors are often unavailable.

In this paper, we address these challenges by building a new, freely accessible platform that tracks economic activity at a high-frequency, granular level using anonymized and aggregated data from private companies. Combining data from credit card processors, payroll firms, and financial services firms, we construct statistics on consumer spending, employment rates, business revenues, job postings, and other key indicators described in detail in Section II below. We report these statistics in real time using an automated pipeline that ingests data from businesses and reports statistics publicly on the data visualization platform, typically less than seven days after the relevant transactions occur. We present fine disaggregations of the data, reporting each statistic by county and by industry and, where feasible, by initial (pre-crisis) income level and business size.

Many firms already analyze their own data internally to inform business decisions and some firms have begun sharing aggregated data with policymakers and researchers during the current crisis. Our contribution is to (1) combine these disparate data sources into a single, publicly accessible platform that eliminates the need to write contracts with specific companies to access relevant data; (2) systematize these data sources by documenting the samples they cover and benchmarking them to existing public series; and (3) provide the combined series in an interactive data visualization tool that facilitates comparisons across outcomes, areas, and subgroups.

Unlike official government statistics, which are based on sampling frames designed to provide representative information, our statistics reflect the behavior of the clients of the firms from which we obtain data. To mitigate selection biases that can arise from this approach, we use data from companies that have large samples (e.g., at least one million individuals), span well-defined sectors or subgroups (e.g., small businesses, bottom-income-quintile workers), and track publicly available benchmarks in historical data. Although there is no guarantee that the statistics from such data sources capture total economic activity accurately, we believe they contain useful information because the shocks induced by major crises such as COVID-19 are large relative to plausible biases due to non-representative sampling, as shown e.g., by Aladangady et al. (2019) and Dunn, Hood, and Driessen (2020).

We use these new data to analyze the economic impacts of the coronavirus pandemic (COVID-19). Government statistics show that COVID led to a very sharp reduction in GDP and an unprecedented surge in unemployment. Our goal is to demonstrate how the publicly available data we have constructed can shed light on the sources of these macroeconomic changes in near-real-time, in particular by disaggregating these changes across subgroups and areas. We therefore base all of our analysis purely on the statistics that we release publicly rather than the underlying (confidential) microdata that we obtain from data partners.

National accounts data reveal that most of the reduction in GDP came from a reduction in consumer spending (rather than business investment, government purchases, or exports). We therefore begin our analysis by examining the drivers of changes in consumer spending, focusing in particular on credit and debit card spending. We first establish that card spending closely tracks historical benchmarks on retail spending and services, which together constitute a large fraction of the reduction in total spending in the national accounts. We then show that the vast majority of the reduction in consumer spending in the U.S. came from reduced spending by *high-income* households. As of June 10, more than half of the total reduction in card spending since January had come from households in the top quartile of the income distribution; only 5% had come from households in the bottom income quartile.[1] This is both because the rich account for a larger share of total spending to begin with and because high-income households reduced their spending by 17%, whereas low-income households reduced their spending by only 4% as of June 10.

Most of the reduction in spending is accounted for by reduced spending on goods or services that require in-person physical interaction and thereby carry a risk of COVID infection, such as hotels, transportation, and food services, consistent with the findings of Alexander and Karger

1. We impute income as the median household income (based on Census data) in the cardholder's ZIP code. We verify the quality of this imputation procedure by showing that our estimates of the gap in spending reductions by income group are aligned with those of Cox et al. (2020), who observe income directly for JPMorgan Chase clients, as of mid-April 2020, the last date available in their series. We find that spending levels of low-income households increased much more sharply than those of high-income households since mid-April largely as a result of stimulus payments.

(2020). The composition of spending cuts – with a large reduction in services – differs sharply from that in prior recessions, where service spending was essentially unchanged and durable goods spending fell sharply. Zooming into specific subcategories, we find that spending on luxury goods that do not require physical contact – such as landscaping services or home swimming pools – did not fall, while spending at salons and restaurants plummeted. Businesses that offer fewer in person services, such as financial and professional services firms, also experienced much smaller losses. The fact that spending fell in proportion to the degree of physical exposure required across sectors suggests that the reduction in spending by the rich was driven primarily by health concerns rather than a reduction in income or wealth. Indeed, the incomes of the rich have fallen relatively little in this recession (Cajner et al. 2020). Consistent with the centrality of health concerns, we find that the reductions in spending and time spent outside home were larger in high-income, high-density areas with higher rates of COVID infection, perhaps because high-income individuals can self-isolate more easily (e.g., by substituting to remote work). Together, these results suggest that consumer spending in the pandemic fell because of changes in firms' ability to supply certain goods (e.g., restaurant meals that carry no health risk) rather than because of a reduction in purchasing power.[2]

Next, we turn to the impacts of the consumer spending shock on businesses. To do so, we exploit the fact that many of the sectors in which spending fell most are non-tradable goods produced by small local businesses (e.g., restaurants) who serve customers in their local area. Building on the results on the heterogeneity of the spending shock, we use differences in average incomes and rents across ZIP codes as a source of variation in the spending shock that businesses face. This geographic analysis is useful both from the perspective of understanding mechanisms and because prior work shows that geography plays a central role in the impacts of economic shocks due to low rates of migration that can lead to hysteresis in local labor markets (Austin, Glaeser, and Summers 2018, Yagan 2019).

Small business revenues in the most affluent ZIP codes in large cities fell by more than 70% between March and late April, as compared with 30% in the least affluent ZIP codes. These reductions in revenue resulted in a much higher rate of small business closure in high-rent, high-income areas within a given county than in less affluent areas. This is particularly the case for

2. This explanation may appear to be inconsistent with the fact that the Consumer Price Index (CPI) shows little increase in inflation, given that one would expect a supply shock to increase prices. However, the CPI likely understates inflation in the current crisis because it does not capture the extreme shifts in the consumption bundle that have occurred as a result of the COVID crisis (Cavallo 2020).

non-tradable goods that require physical interaction – e.g., restaurants and accommodation services – where revenues fell by more than 80% in the most affluent neighborhoods in the country, such as the Upper East Side of Manhattan or Palo Alto, California. Small businesses that provide fewer in-person services – such as financial or professional services firms – experience much smaller losses in revenue even in affluent areas.

As businesses lost revenue, they passed the incidence of the shock on to their employees. Low-wage hourly workers in small businesses in affluent areas are especially likely to have lost their jobs. In the highest-rent ZIP codes, more than 65% of workers at small businesses were laid off within two weeks after the COVID crisis began; by contrast, in the lowest-rent ZIP codes, fewer than 30% lost their jobs. Workers at larger firms and in tradable sectors (e.g., manufacturing) were much less likely to lose their jobs than those working in small businesses producing non-tradable goods, irrespective of their geographic location. Job postings also fell much more sharply in more affluent areas, particularly for lower-skilled positions. As a result of these changes in the labor market, unemployment claims surged even in affluent counties, which have generally had relatively low unemployment rates in prior recessions. For example, more than 15% of residents of Santa Clara county – the richest county in the United States, located in Silicon Valley – filed for unemployment benefits by May 2. Perhaps because they face higher rates of job loss and worse future employment prospects, low-income individuals working in more affluent areas cut their *own* spending much more than low-income individuals working in less affluent areas.

In summary, the initial impacts of COVID-19 on economic activity appear to be largely driven by a reduction in spending by higher-income individuals due to health concerns, which in turn affected businesses that cater to the rich – e.g., small businesses in affluent areas – and ultimately reduced the incomes and expenditure levels of low-wage employees of those businesses. In the final part of the paper, we analyze the impacts of three major policy efforts that were enacted in an effort to break this chain of events and mitigate the economic impacts of the crisis: state-ordered reopenings, stimulus payments to households, and loans to small businesses.[3]

Reopenings of economies had modest impacts on economic activity. Spending and employment remained well below baseline levels even after reopenings, and in particular did not rise more rapidly in states that reopened earlier relative to comparable states that reopened later. Spending

3. Of course, this set of policies is by no means exhaustive: a vast set of other policy efforts ranging from changes in monetary policy to various state-level programs were also undertaken in response to the crisis. We focus on these three policies because they illustrate the ways in which the new high-frequency data we have assembled can be used for real-time policy analysis, and we hope that future work will use these data to analyze other policies.

and employment also fell well *before* state-level shutdowns were implemented, consistent with other recent work examining data on hours of work and movement patterns (Bartik et al. 2020, Villas-Boas et al. 2020).

Stimulus payments provided through the CARES Act increased spending among low-income households sharply, nearly restoring their spending to pre-COVID levels as of May 10, consistent with evidence from Baker et al. (2020). Most of this increase in spending was in sectors that require limited physical interaction. Purchases of durable goods surged, while consumption of in-person services (e.g., restaurants) increased very little. As a result, very little of the increased spending flowed to businesses most affected by the COVID-19 shock, such as small businesses in affluent areas – potentially limiting the capacity of the stimulus to increase economic activity and employment in the communities where job losses were largest.

Loans to small businesses as part of the Paycheck Protection Program (PPP) also have had little impact on employment rates at small businesses to date. Employment rates at small firms in the hardest-hit sectors trended similarly to those at larger firms that were likely to be ineligible for PPP loans, and remained far below baseline levels as of May 30. These results suggest that providing liquidity itself may be inadequate to restore employment at small businesses, at least in the short run.[4]

In sum, our analysis suggests that the primary barrier to economic activity is depressed consumer spending due to the threat of COVID-19 itself as opposed to government restrictions on economic activity, inadequate income among consumers, or a lack of liquidity for firms. Hence, the only path to full economic recovery in the long run may be to restore consumer confidence by addressing the virus itself (e.g., Allen et al. 2020, Romer 2020). Traditional macroeconomic tools – stimulating aggregate demand or providing liquidity to businesses – may have diminished short-run impacts in an environment where consumer spending is fundamentally constrained by health concerns.

In the meantime, it may be more fruitful to approach this economic crisis from the lens of providing social insurance to reduce hardship rather than stimulus to increase economic activity. Rather than attempt to put workers back to work in sectors where spending is temporarily depressed because of health concerns, it may be best to focus on mitigating income losses for those who have

4. The PPP also includes price incentives to rehire workers in the form of loan forgiveness for firms that employ the same number of workers as of June 30 as they did in February. Firms may rehire workers in light of this incentive in the coming month, a possibility that can be evaluated in real time using the data in the tracker. What is clear at this stage is that liquidity itself – absent this price incentive or fundamental changes in the public health situation – appears to be insufficient to restore employment to pre-recession levels.

lost their jobs, consistent with the normative predictions of the theoretical framework developed by Guerrieri et al. (2020). For instance, providing support to workers who have lost their jobs (e.g., via the unemployment benefit system) may be preferable to stimulus payments to all households, irrespective of their employment situation. Our findings also suggest that may be useful to consider additional place-based assistance targeted at low-income individuals in areas that have suffered the largest losses – such as affluent, urban areas – since historical experience suggests that relatively few people move to other labor markets to find new jobs after recessions (Yagan 2019).

Of course, all of these results could change over time: the recession may turn into a more traditional economic shock with Keynesian spillovers across a wider set of sectors and areas as time passes, in which case tools such as stimulus and liquidity could become much more impactful (Guerrieri et al. 2020). The tracker constructed here can be used to monitor the changing dynamics of the crisis and evaluate policy impacts on an ongoing basis.

Our work builds on and contributes to a rapidly evolving literature on the economic impacts of COVID-19 as well as a long literature in macroeconomics on the measurement of economic activity at business cycle frequencies. Several recent papers have used private sector data analogous to what we assemble here to analyze consumer spending (e.g., Baker et al. 2020, Chen, Qian, and Wen 2020, Cox et al. 2020), business revenues (e.g., Alexander and Karger 2020), labor market trends (e.g., Bartik et al. 2020, Cajner et al. 2020, Kurmann, Lalé, and Ta 2020, Kahn, Lange, and Wiczer 2020), and social distancing (e.g., Allcott et al. 2020, Chiou and Tucker 2020, Goldfarb and Tucker 2020, Mongey, Pilossoph, and Weinberg 2020). These papers have identified a number of important results consistent with our findings, such as concentrated impacts on spending in certain industries such as food and accommodation; social distancing that is a result of voluntary choices rather than legislation; and large employment losses for low-income workers. Each of these papers analyzes a subset of data sources, obtained through a data use agreement with the relevant firm. By combining these and other datasets and benchmarking them to national aggregates, we are able to trace the macroeconomic impacts of the COVID shock from consumer spending to businesses to labor markets. More generally, by integrating these datasets into a unified, freely accessible platform, we eliminate the need for researchers or local policymakers to obtain specific permissions to use confidential data from companies. We demonstrate that it is feasible to construct aggregates from these data that protect privacy while providing sufficient granularity for economic analysis in real time, thereby providing a new tool for economic policy analysis in this crisis and beyond.

The paper is organized as follows. The next section describes the data we use to construct the economic tracker. In Section 3, we analyze the effects of COVID-19 on spending, revenue, and employment. Section 4 analyzes the impacts of policies enacted to mitigate COVID's impacts. Section 5 concludes. Technical details on data, methods, and supplementary analyses are available in an online appendix.

II Data and Methods

We use anonymized data from several private companies to construct indices of spending, employment, and other metrics. In this section, we describe how we construct each series. To facilitate comparisons between series, we adopt the following set of principles when constructing each series (wherever feasible given data availability constraints).

First, the central challenge in using private sector data to measure economic activity is that they capture information exclusively about the customers each company serves, and thus are not necessarily representative of the full population. Instead of attempting to adjust for this non-representative sampling, we characterize the portion of the economy that each series captures by comparing the characteristics of each sample we use to national benchmarks.[5]

Second, we clean each series to remove artifacts that arise from changes in the data providers' coverage or systems. For instance, firms' clients often change discretely, sometimes leading to discontinuous jumps in series, particularly in small cells. We systematically search for large jumps in series (e.g., >80%), seek to understand their root causes, and address such discontinuities by imposing continuity as described below.

Third, many series exhibit substantial periodic fluctuations across days. We address such fluctuations through aggregation, e.g. reporting 7-day moving averages to smooth daily fluctuations. Certain series – most notably consumer spending and business revenue – exhibit strong weekly fluctuations that are autocorrelated across years (e.g., a surge in spending around the holiday season). We de-seasonalize such series by normalizing each week's value in 2020 relative to corresponding values for the same week in 2019 in our baseline analysis, but also report raw values for 2020 for researchers who prefer to make alternative seasonal adjustments.

Fourth, to protect confidentiality of business market shares, we do not report levels of the series.

5. An alternative approach is to reweight samples based on observable characteristics – e.g., industry – to match national benchmarks. We do not pursue such an approach here because the samples we work with track relevant national benchmarks – at least for the scale of shocks induced by the COVID crisis – without such reweighting. However, the disaggregated data we report by industry and county can be easily reweighted as desired in future applications.

Instead, we report indexed values that show percentage changes relative to mean values in January 2020.[6] We also suppress small cells and exclude outliers to protect the privacy of individuals and businesses, with thresholds that vary across datasets as described below.

Finally, we seek to release data series at the highest possible frequency. To limit revisions, we permit a sufficient lag to adjust for reporting delays (typically one week). We disaggregate each series by two-digit NAICS industry code; by county, metro area, and state; and by income quartile where feasible.[7]

We now describe each of the series in turn, discussing the raw data sources, construction of key variables, and cross-sectional comparisons to publicly available benchmarks.[8] All of the data series described below can be freely downloaded from the Economic Tracker website: www.tracktherecovery.org.

II.A Consumer Spending: Affinity Solutions

We measure consumer spending using aggregated and anonymized consumer purchase data collected by Affinity Solutions Inc, a company that aggregates consumer credit and debit card spending information to support a variety of financial service products.

We obtain raw data from Affinity Solutions at the county-by-ZIP code income quartile-by-industry-by-day level starting from January 1, 2019. Industries are defined by grouping together similar merchant category codes. ZIP code income quartiles are constructed at the national level using Census data on population and median household income by ZIP. Cells with fewer than five unique card transactions are masked.

The raw data include several discontinuous breaks caused by entry or exit of credit card providers from the sample. We identify these breaks using data on the total number of active cards in the cell. We then estimate the discontinuous level shift in spending resulting from the break (using a standard regression discontinuity estimator). At the state level (including Washington, DC), we adjust the series within each cell by adding the RD estimate back to the raw data to obtain a smooth series. At the county-level, there is too much noise to implement a reliable correction, so we exclude counties that exhibit such breaks from the sample. After cleaning the raw data in

6. We always norm after summing to a given cell (e.g. geographic unit, industry, etc.) rather than at the firm or individual level. This dollar-weighted approach overweights bigger firms and higher-income individuals, but leads to smoother series and is arguably more relevant for certain macroeconomic policy questions (e.g., changes in aggregate spending).

7. We construct metro area values for large metro areas using a county to metro area crosswalk described in the Appendix.

8. We benchmark trends in each series over time to publicly-available data in the context of our analysis in the next section.

this manner, we construct daily values of the consumer spending series using a seven-day moving average of the current day and previous six days of spending. We then seasonally adjust the series by dividing each calendar date's 2020 value by its corresponding value from 2019.[9] Finally, we index the seasonally-adjusted series relative to pre-COVID-19 spending by dividing each day's value by the mean of the seasonally-adjusted seven-day moving average from January 8-28.

Comparison to QSS and MRTS. Total debit and credit card spending in the U.S. was $7.08 trillion in 2018 (Board of Governors of the Federal Reserve System 2019), approximately 50% of total personal consumption expenditures recorded in national accounts. Affinity Solutions captures nearly 10% of debit and credit card spending in the U.S. To assess which categories of spending are covered by the Affinity data, Appendix Figure 1 compares the spending distributions across sectors to spending captured in the nationally representative Quarterly Services Survey (QSS) and Monthly Retail Trade Survey (MRTS). Affinity has broad coverage across industries. However, as expected, it over-represents categories where credit and debit cards are used for purchases. In particular, accommodation and food services and clothing are a greater share of the Affinity data than financial services and motor vehicles. We therefore view Affinity as providing statistics that are representative of total card spending (but not total consumer spending). We assess whether Affinity captures changes in card spending around the crisis in Section 3.1 below.

II.B Small Business Revenue: Womply

We obtain data on small business transactions and revenues from Womply, a company that aggregates data from several credit card processors to provide analytical insights to small businesses and other clients. In contrast to the Affinity series on consumer spending, which is a cardholder-based panel covering total spending, Womply is a firm-based panel covering total revenues of small businesses. The key distinction is that location in Womply refers to the location where the business transaction occurred as opposed to the location where the cardholder lives.

We obtain raw data on small business transactions and revenues from Womply at the ZIP-industry-day level starting from January 1, 2019.[10] Small businesses are defined as businesses with annual revenue below Small Business Administration thresholds. To reduce the influence of outliers, firms outside twice the interquartile range of firm annual revenue within this sample are excluded and the sample is further limited to firms with 30 or more transactions in a quarter and

9. We divide the daily value for February 29, 2020 by the average value between the February 28, 2019 and March 1, 2019.

10. We crosswalk Womply's transaction categories to two-digit NAICS codes using an internally generated Womply category-NAICS crosswalk, and then aggregate to NAICS supersectors.

more than one transaction in 2 out of the 3 months.

We aggregate these raw data to form two publicly available series at the county by industry level: one measuring total small business revenue and another measuring the number of small businesses open. We measure small business revenue as the sum of all credits (generally purchases) minus debits (generally returns). We define small businesses as being open if they have a transaction in the last three days. We exclude counties with a total average revenue of less than $250,000 during the pre-COVID-19 period (January 4-31).

For each series, we construct daily values in exactly the same way that we constructed the consumer spending series. We first take a seven-day moving average, then seasonally adjust by dividing each calendar date's 2020 value by its corresponding value from 2019. Finally, we index relative to pre-COVID-19 by dividing the series by its average value over January 4-31.

Comparison to QSS and MRTS. Appendix Figure 1 shows the distribution of revenues observed in Womply across industries in comparison to national benchmarks. Womply revenues are again broadly distributed across sectors, particularly those where card use is common. A larger share of the Womply revenue data come from industries that have a larger share of small businesses, such as food services, professional services, and other services, as one would expect given that the Womply data only cover small businesses.

II.C Employment and Earnings: Earnin and Homebase

We use two data sources to obtain information on employment and earnings for low-income workers: Earnin and Homebase.

Earnin is a financial management application that provides its members with access to their income as they earn it. Workers sign up for Earnin individually using a cell phone app, which records payroll information from bank accounts. Many lower-income workers across a wide spectrum of firms – ranging from the largest firms and government employers in the U.S. to small businesses – use Earnin; we discuss the characteristics of these workers further below. We obtain raw data from Earnin at the paycheck level with information on home ZIP, workplace ZIP, industry and firm size decile from January 2020 to present.[11] We restrict this sample to workers who are paid on a weekly or bi-weekly paycycle. We then use these data to measure employment and earnings for low-income employees. We assign workers to locations using their workplace ZIP codes. We suppress estimates for ZIP codes with fewer than 50 worker-days observed in Earnin over the period January 4-31.

11. We map each firm to a NAICS code using firm names and a custom-built crosswalk constructed by Digital Divide Data. We obtain data on firm sizes from Reference USA.

Homebase provides scheduling tools for small businesses (on average, 8.4 employees) such as restaurants (64% of employees for whom sectoral data are available) and retail stores (15% of employees for whom sectoral data are available). Unlike Earnin, Homebase provides a complete roster of workers at a given firm, but only covers workers at small businesses. We obtain de-identified individual-level data on hours and total pay for employees at firms that contract with Homebase at the establishment-worker-day level, starting on January 1, 2018. We restrict this sample to non-salaried employees. We then form each aggregate series at the county and industry level, assigning location based on the ZIP code of establishment. To protect confidentiality, we suppress estimates for cells with fewer than 10 Homebase clients in January 2020.

In both datasets, we measure total employment as a seven-day moving average of total number of active employees, expressed as a percentage change relative to January 4-31, and total earnings using a seven-day moving average of earnings divided by the average daily total earnings between January 4-31. In the Homebase data, employment and earnings are observed on a daily basis. In the Earnin data, where we observe paychecks, we distribute each worker's earnings at the end of their pay period over each day in their pay period, and assume that workers are employed over their full pay period.

We also observe wages in both datasets. In the Homebase data, we measure hourly wage rates using the change in the first reported hourly wage rate in the current week and the average reported wage between January 4-31, 2020, divided by that average. In the Earnin data, where we do not observe individual identifiers, we measure wages as the seven-day moving average of daily mean wages, expressed as a percentage change from daily mean wages between January 4-31.

Comparisons to OES and QCEW. Appendix Figure 2 compares the industry composition of the Earnin and Homebase samples to nationally representative statistics from the Quarterly Census of Employment and Wages (QCEW). The Earnin sample is fairly representative of the broader industry mix in the U.S., although high-skilled sectors (such as professional services) are under-represented. Homebase has a much larger share of workers in food services, even relative to small establishments (those with fewer than 50 employees) in the QCEW, as expected given its client base.

Overall, annualizing January earnings would imply median earnings of roughly $23K per year ($11-12 per hour). In Appendix Table 1, we compare the median wage rates of workers in Earnin and Homebase to nationally representative statistics from the BLS's Occupational Employment Statistics. Workers enrolled in Earnin have median wages that are at roughly the 10th percentile of the wage distribution within each NAICS code. The one exception is the food and drink industry, where the median wages are close to the population median wages in that industry (reflecting that most workers in food services earn relatively low wages). Homebase exhibits a similar pattern, with lower wage rates compared to industry averages, except in sectors that have low wages, such as food services and retail.

We conclude based on these comparisons that Earnin and Homebase provide statistics that may be representative of low-wage (bottom-quintile) workers. Earnin provides data covering such workers in all industries, whereas Homebase is best interpreted as a series that reflects workers in the restaurant and retail sector.

II.D Job Postings: Burning Glass

We obtain data on job postings from 2007 to present from Burning Glass Technologies. Burning Glass aggregates nearly all jobs posted online from approximately 40,000 online job boards in the United States. Burning Glass then removes duplicate postings across sites and assigns attributes including geographic locations, required job qualifications, and industry.

We obtain raw data on job postings at the industry-week-job qualification-county level from Burning Glass. Industry is defined using select NAICS supersectors, aggregated from 2-digit NAICS classification codes assigned by a Burning Glass algorithm. Job qualifications are defined using ONET Job Zones. These job zones are mutually exclusive categories that classify jobs into five groups: needing little or no preparation, some preparation, medium preparation, considerable preparation, or extensive preparation. We also obtain analogous data broken by educational requirements (e.g., high school degree, college, etc.).

Comparison to JOLTS. Burning Glass data have been used extensively in prior research in economics; for instance, see Hershbein and Kahn (2018) and Deming and Kahn (2018). Carnevale, Jayasundera, and Repnikov (2014) compare the Burning Glass data to government statistics on job openings and characterize the sample in detail. In Appendix Figure 3, we compare the distribution of industries in the Burning Glass data to nationally representative statistics from the Bureau of Labor Statistics' Job Openings and Labor Market Turnover Survey (JOLTS) in January 2020. In general, Burning Glass is well aligned across industries with JOLTS, with the one exception that it under-covers government jobs. We therefore view Burning Glass as a sample representative of private sector jobs in the U.S.

II.E Education: Zearn

Zearn is an education nonprofit that partners with schools to provide a math program, typically used in classrooms, that combines in-person instruction with digital lessons. Many schools continued to use Zearn as part of their math curriculum after COVID-19 induced schools to shift to remote learning.

We obtain data on the number of students using Zearn Math and the number of lessons they completed at the school-grade-week level. The data we obtain are masked such that any county with fewer than two districts, fewer than three schools, or fewer than 50 students on average using Zearn Math during the pre-period is excluded. We fill in these masked county statistics with the commuting zone mean whenever possible. We winsorize values reflecting an increase of greater than 300% at the school level. We exclude schools who did not use Zearn Math for at least one week from January 6 to February 7 and schools that never have more than five students using Zearn Math during our analysis period. To reduce the effects of school breaks, we replace the value of any week for a given school that reflects a 50% decrease (increase) greater than the week before or after it with the mean value for the three relevant weeks.

We measure online math participation as the number of students using Zearn Math in a given week. We measure student progress in math using the number of lessons completed by students each week. We aggregate to the county, state, and national level, in each case weighting by the average number of students using the platform at each school during the base period of January 6-February 7, and we normalize relative to this base period to construct the indices we report.

Comparison to American Community Survey. In Appendix Table 2, we assess the representativeness of the Zearn data by comparing the demographic characteristics of the schools for which we Zearn data (based on the ZIP codes in which they are located) to the demographic characteristics of K-12 students in the U.S. as a whole. In general, the distribution of income, education, and race and ethnicity of the schools in the Zearn sample is similar to that in the U.S. as a whole suggesting that Zearn likely provides a fairly representative picture of online learning for public school students in the U.S.

II.F Public Data Sources: UI Records, COVID-19 Incidence, and Google Mobility Reports

Unemployment Benefit Claims. We collect county-level data by week on unemployment insurance claims starting in January 2020 from state government agencies since no weekly, county-level national data exist. Location is defined as the county where the filer resides. We use the initial claims reported by states, which sometimes vary in their exact definitions (e.g., including or excluding certain federal programs). In some cases, states only publish monthly data. For these cases, we impute the weekly values from the monthly values using the distribution of the weekly state claims data from the Department of Labor (described below). We construct an unemployment claims rate by dividing the total number of claims filed by the 2019 Bureau of Labor Statistics labor force estimates. Note that county-level data are available for 22 states, including the District of Columbia.

We also report weekly unemployment insurance claims at the state level from the Office of Unemployment Insurance at the Department of Labor. Here, location is defined as the state liable for the benefits payment, regardless of the filer's residence. We report both new unemployment claims and total employment claims. Total claims are the count of new claims plus the count of people receiving unemployment insurance benefits in the same period of eligibility as when they last received the benefits.

COVID-19 Data. We report the number of new COVID-19 cases and deaths each day using publicly available data from the New York Times available at the county, state and national level.[12] We also report daily state-level data on the number of tests performed per day per 100,000 people from the COVID Tracking Project.[13] For each measure - cases, deaths, and tests – we report two daily series per 100,000 people: a seven-day moving average of new daily totals and a cumulative total through the given date.

Google Mobility Reports. We use data from Google's COVID-19 Community Mobility Reports to construct measures of daily time spent at parks, retail and recreation, grocery, transit locations, and workplaces.[14] We report these values as changes relative to the median value for the corresponding day of the week during the five-week period from January 3rd - February 6, 2020. Details on place types and additional information about data collection is available from Google. We use these raw series to form a measure of time spent outside home as follows. We first use the American Time Use survey to measure the mean time spent inside home (excluding time asleep) and outside home in January 2018 for each day of the week. We then multiply time spent inside home in January

12. See the New York Times data description for a complete discussion of methodology and definitions. Because the New York Times groups all New York City counties as one entity, we instead use case and death data from New York City Department of Health data for counties in New York City.

13. We use the Census Bureau's 2019 population estimates to define population when normalizing by 100,000 people. We suppress data where new counts are negative due to adjustments in official statistics.

14. Google Mobility trends may not precisely reflect time spent at locations, but rather "show how visits and length of stay at different places change compared to a baseline." We call this "time spent at a location" for brevity.

with Google's percent change in time spent at residential locations to get an estimate of time spent inside the home for each date. The remainder of waking hours in the day provides an estimate for time spent outside the home, which we report as changes relative to the mean values for the corresponding day of the week in January 2018.

III Economic Impacts of COVID-19

In this section, we analyze the economic impacts of COVID-19, both to shed light on the COVID crisis itself and to demonstrate the utility of private sector data sources assembled above as a complement to national accounts data in tracking economic activity.

To structure our analysis, we begin from national accounts data released by the Bureau of Economic Analysis (2020). GDP fell by \$247 billion (an annualized rate of 5%) from the fourth quarter of 2019 to the first quarter of 2020, shown by the first bar in Figure 1a. GDP fell primarily because of a reduction in personal consumption expenditures (consumer spending), which fell by \$230 billion.[15] Government purchases did not change significantly, while net exports increased by \$65 billion and private investment fell by \$90 billion.[16] We therefore begin our analysis by studying the determinants of this sharp reduction in consumer spending. We then turn to examine downstream impacts of the reduction in consumer spending on business activity and the labor market.

III.A Consumer Spending

We analyze consumer spending using data on aggregate credit and debit card spending. National accounts data show that spending that is well captured on credit and debit cards – essentially all spending excluding housing, healthcare, and motor vehicles – fell by approximately \$138 billion, comprising roughly 60% of the total reduction in personal consumption expenditures.[17]

15. GDP is released at a quarterly level in the U.S. The reduction in consumer spending occurred in the last two weeks of March (Figure 2 below); hence the first quarter GDP estimates capture about one-sixth of the reduction in spending due to the COVID shock.

16. Most of the reduction in private investment was driven by a reduction in inventories and equipment investment in the transportation sector, both of which are plausibly a response to reductions in current and anticipated consumer spending. The increase in net exports was driven primarily by a reduction in imports, with a large reduction in imports of travel and transporation services in particular, again reflecting a change in domestic consumer spending behavior.

17. The rest of the reduction is largely accounted for by healthcare and motor vehicle expenditures; housing expenditures did not change significantly. We view the incorporation of data sources to study these other major components of spending as an important direction for future work; however, we believe that the mechanisms discussed below may apply at least qualitatively to those sectors as well.

Benchmarking. We begin by assessing whether the credit card data track patterns in corresponding spending categories in the national accounts. Figure 1b plots spending on retail services (excluding auto-related expenses) in the Affinity Solutions credit card data alongside the Monthly Retail Trade Survey (MRTS), one of the main inputs used to construct the national accounts. Both series are indexed to have a value of 1 in January 2020; each point shows the level of spending in a given month divided by spending in January 2020. Figure 1c replicates Figure 1b for spending on food services. In both cases, the credit/debit card spending series closely tracks the inputs that make up the national accounts. In particular, both series show a rapid drop in food services spending in March and April 2020 and a smaller drop in retail spending, along with an increase in May. Given that credit card spending data closely tracks the MRTS at the national level, we proceed to use it to disaggregate the national series in several ways to understand why consumer spending fell so sharply.

Heterogeneity by Income. We begin by examining spending changes by household income. We do not directly observe cardholders' incomes in our data; instead, we proxy for cardholders' incomes using the median household income in the ZIP code in which they live (based on data from the 2014-18 American Community Survey). ZIP-codes are strong predictors of income because of the degree of segregation in most American cities; however, they are not a perfect proxy for income and can be prone to bias in certain applications, particularly when studying tail outcomes (Chetty et al. 2020). To evaluate the accuracy of our ZIP code imputation procedure, we compare our estimates to those of Cox et al. (2020), who observe cardholder income directly based on checking account data for clients of JPMorgan Chase. Our estimates are closely aligned with those estimates, suggesting that the ZIP code proxy is reasonably accurate in this application.[18]

Figure 2a plots a seven-day moving average of total daily card spending for households in the bottom vs. top quartile of ZIP codes based on median household income.[19] The solid line shows data from January to May 2020, while the dashed line shows data for the same days in 2019 as a reference. Spending fell sharply on March 15, when the National Emergency was declared and the threat of COVID became widely discussed in the United States. Spending fell from $7.9 billion

18. Cox et al. (2020) report an eight percentage point (pp) larger decline in spending for the highest income quartile relative to the lowest income quartile in the second week of April. Our estimate of the gap is also eight pp at that point, although the levels of the declines in our data are slightly smaller in magnitude for both groups. The JPMorgan Chase data cannot themselves be used for the analysis that follows because there are no publicly available aggregated series based on those data at present.

19. We estimate total card spending by multiplying the raw totals in the Affinity Solutions data by the ratio of total spending on the categories shown in the last bar of Figure 1a in PCE to total spending in the Affinity data in January 2020.

per day in February to \$5.4 billion per day by the end of March (a 31% reduction) for high-income households; the corresponding change for low-income households was \$3.5 billion to \$2.7 billion (a 23% reduction). Because high-income households both cut spending more in percentage terms and accounted for a larger share of aggregate spending to begin with, they account for a much larger share of the decline in total spending in the U.S. than low-income households. We estimate that as of mid-April, top-quartile households accounted for 39% of the aggregate spending decline after the COVID shock, while bottom-quartile households accounted for only 13% of the decline. This gap grew even larger after stimulus payments began in mid-April. By mid June, top-quartile households accounted for over half of the total spending decline in the U.S. and were still spending 15% less than their January levels, whereas bottom-quartile households were spending almost the same amount they were in 2019. This heterogeneity in spending changes by income is much larger than that observed in previous recessions (Petev, Pistaferri, and Eksten 2011, Figure 6) and plays a central role in understanding the downstream impacts of COVID on businesses and the labor market, as we show below.

Heterogeneity Across Sectors. Next, we disaggregate the change in total spending across categories to understand why households cut spending so rapidly. In particular, we seek to distinguish two channels: reductions in spending due to loss of income vs. fears of contracting COVID.

The left bar in Figure 2b plots the share of the total decline in spending from the pre-COVID period to mid-April accounted for by various categories. Nearly three-fourths of the reduction in spending comes from reduced spending on goods or services that require in-person contact (and thereby carry a risk of COVID infection), such as hotels, transportation, and food services.[20] This is particularly striking given that these goods accounted for only one-third of total spending in January, as shown by the right bar in Figure 2b.

Next, we zoom in to specific subcategories of spending that differ sharply in the degree to which they require physical interaction in Figure 2c. Spending on luxury goods such as installation of home pools and landscaping services – which do not require in-person contact – *increased* slightly after the COVID shock; by contrast, spending on restaurants, beauty shops, and airlines all plummeted sharply. Consistent with these substitution patterns, spending at online retailers increase sharply: online purchases comprised 11% of retail sales in 2019 vs. 22% in April and May of 2020 (Mastercard 2020).[21] A conventional reduction in income or wealth would typically reduce spending on all goods

20. The relative shares of spending reductions across categories are similar for low- and high-income households (Appendix Figure 4); what differs is the level of spending reduction, as discussed above.

21. We are unable to distinguish online and in-store transactions in the Affinity Solutions data.

as predicted by their Engel curves (income elasticities); the fact that the spending reductions vary so sharply across goods that differ in terms of their health risks lends further support to the hypothesis that it is health concerns rather than a lack of purchasing power that drove spending reductions.

These patterns of spending reductions are particularly remarkable when contrasted with those observed in prior recessions. Figure 2d compares the change in spending across categories in national accounts data in the COVID recession and the Great Recession in 2009-10. In the Great Recession, nearly all of the reduction in consumer spending came from a reduction in spending on goods; spending on services was almost unchanged. In the COVID recession, 67% of the reduction in total spending came from a reduction in spending on services, as anticipated by Mathy (2020).

Heterogeneity by COVID Incidence. To further evaluate the role of health concerns, we next turn to directly examine the association between incidence of COVID across areas and changes in spending. Figure 3a presents a binned scatterplot of changes in spending from January to April vs. the rate of detected COVID cases by county. To construct this figure, we divide the x variable (COVID cases) into 20 bins, each of which contain 5% of the population, and plot the mean value of the x and y variables within each bin. Areas with higher rates of COVID infection experience significantly larger declines in spending, a relationship that holds conditional on controls for median household income and state fixed effects (Appendix Figure 5).[22]

To examine the mechanism driving these spending reductions more directly, in Figure 3b, we present a binned scatterplot of the amount of time spent outside home (using anonymized cell phone data from Google) vs. COVID case rates, separately for low- and high-income counties (median household income in the bottom vs. top income quartile). In both sets of areas, there is a strong negative relationship: people spend considerably less time outside home in areas with higher rates of COVID infection. The reduction in spending on services that require physical, in-person interaction (e.g., restaurants) is mechanically related to this simple but important change in behavior.

At all levels of COVID infection, higher-income households spend less time outside. Figure 3c establishes this point more directly by showing that time spent outside home falls monotonically with household income across the distribution. These results help explain why the rich reduce spending more, especially on goods that require in-person interaction: high-income people apparently self-isolate more, perhaps by working remotely or because they have larger living spaces.

22. Note that there is a substantial reduction in spending even in areas without high rates of realized COVID infection, which is consistent with widespread concern about the disease even in areas where outbreaks did not actually occur at high rates.

In sum, disaggregated data on consumer spending reveals that spending in the initial stages of the pandemic fell primarily because of health concerns rather than a loss of current or expected income. Indeed, income losses were relatively modest because relatively few high-income individuals lost their jobs (Cajner et al. 2020) and lower-income households who experienced job loss had their incomes more than replaced by unemployment benefits (Ganong, Noel, and Vavra 2020). As a result, national accounts data actually show an *increase* in total income of 13% from March to April 2020. This result implies that the central channel emphasized in Keynesian models that have guided policy responses to prior recessions – a fall in aggregate demand due to a lack of purchasing power – has been less important in the early stages of the pandemic, partly as a result of policies such as increases in unemployment benefits that offset lost earnings. Rather, the key driver of residual changes in aggregate spending is a contraction in firms' ability to supply certain goods, namely services that carry no health risks. We now show that this novel source of spending reductions leads to a distinct pattern of downstream impacts on businesses and the labor market, potentially calling for different policy responses than in prior recessions.

III.B Business Revenues

We now turn to examine how reductions in consumer spending affect business activity. Conceptually, we seek to understand how a change in revenue for a given firm affects its decisions: whether to remain open, how many employees to retain, what wage rates to pay them, how many new people to hire. Ideally, one would analyze these impacts at the firm level, examining how the customer base of a given firm affected its revenues and employment decisions. Lacking firm-level data, we use geographic variation as an instrument for the spending shocks that firms face. The motivation for this geographical approach is that spending fell primarily among high-income households in sectors that require in-person interaction, such as restaurants. Most of these goods are non-tradable products produced by small local businesses who serve customers in their local area.[23] We therefore use differences in average incomes and rents across ZIP codes as a source of variation in the magnitude of the spending shock that small businesses face.[24]

23. 56% of workers in food and accommodation services and retail (two major non-tradeable sectors) work in establishments with fewer than 50 employees.

24. We focus on small businesses because their customers are typically located near the business itself; larger businesses' customers (e.g., large retail chains) are more dispersed, making the geographic location of the business less relevant. One could also in principle use other groups (e.g., sectors) instead of geography as instruments. We focus primarily on geographic variation because the granularity of the data by ZIP code yields much sharper variation than what is available across sectors and arguably yields comparisons across more similar firms (e.g., restaurants in different neighborhoods rather than airlines vs. manufacturing).

Benchmarking. We measure small business revenues using data from Womply, which records revenues from credit card transactions for small businesses (as defined by the Small Business Administration). Business revenues in Womply closely track patterns in the Affinity total spending data, especially in sectors with a large share of small businesses, such as food and accommodation services (Appendix Figure 6).[25]

Heterogeneity Across Areas. We begin our analysis of the Womply data by examining how small business revenues changed in low- vs. high-income ZIP codes from a baseline period prior the COVID shock (January 5 to March 7, 2020) to the weeks immediately after the COVID shock before the stimulus program began (March 22 to April 20, 2020). Figure 4 maps the change in small business revenue by ZIP code in three large metro areas: New York City, San Francisco, and Chicago (analogous ZIP-level maps for other cities are available here). There is substantial heterogeneity in revenue declines across areas. For example, average revenue declines range from -87% (or below) in the lowest-income-decile of ZIP codes to -12% (or above) in the top-income-decile in New York City.[26]

In all three cities, revenue losses are largest in the most affluent parts of the city. For example, small business lost 73% of their revenue in the Upper East Side in New York, compared with 14% in the East Bronx; 67% in Lincoln Park vs. 38% in Bronzeville on the South Side of Chicago; and 88% in Nob Hill vs. 37% in Bayview in San Francisco. Revenue losses are also large in the central business districts in each city (lower Manhattan, the Loop in Chicago, the Financial District in San Francisco), likely a direct consequence of the fact that many workers who used to work in these areas are now working remotely. But even within predominantly residential areas, businesses located in more affluent neighborhoods suffered much larger revenue losses, consistent with the heterogeneity in spending reductions observed in the Affinity data.[27] More broadly, cities that have experienced the largest declines in small business revenue on average tend to be affluent cities – such as New York, San Francisco, and Boston (Table 1, Appendix Figure 8).

Figure 5a generalizes these examples by presenting a binned scatter plot of percent changes in small business revenue vs. median household incomes, by ZIP code across the entire country. We observe much larger reductions in revenue at local small businesses in affluent ZIP codes. In the

25. In sectors that have a bigger share of large businesses – such as retail – the Womply small business series exhibits a larger decline during the COVID crisis than Affinity (or MRTS). This pattern is precisely as expected given other evidence that consumers shifted spending toward large online retailers such as Amazon (Alexander and Karger 2020).

26. Very little of this variation is due to sampling error: the reliability of these estimates across ZIP codes within counties exceeds 0.8, i.e., more than 80% of the variance within each of these maps is due to signal rather than noise.

27. We find a similar pattern when controlling for differences in industry mix across areas; for instance, the maps look very similar when we focus solely on small businesses in food and accommodation services (Appendix Figure 7).

richest 5% of ZIP codes, small business revenues fell by 60%, as compared with 40% in the poorest 5% of ZIP codes.[28]

As discussed above, spending fell most sharply not just in high-income areas, but particularly in high-income areas with a high rate of COVID infection. Data on COVID case rates are not available at the ZIP code level; however, one well established predictor of the rate of spread of COVID is population density: the infection spreads more rapidly in dense areas. Figure 5b shows that small business revenues fell more heavily in more densely populated ZIP codes.[29]

Figure 5c combines the income and population density mechanisms by plotting revenue changes vs. median rents (for a two bedroom apartment) by ZIP code. Rents are a simple measure of the affluence of an area that combine income and population density: the highest rent ZIP codes tend to be high-income, dense areas such as Manhattan. Figure 5c shows a particularly steep gradient of revenue changes with respect to rents: revenues fell by less than 30% in the lowest-rent ZIP codes, compared with more than 60% in the highest-rent ZIP codes. This relationship is essentially unchanged when controlling for worker density in the ZIP code and county fixed effects (Appendix Table 3).

In Figure 5d, we examine heterogeneity in this relationship across sectors that require different levels of physical interaction: food and accommodation services and retail trade (which largely require in-person interaction) vs. finance and professional services (which largely can be conducted remotely). Revenues fall much more sharply for food and retail in higher-rent areas; in contrast, there is essentially no relationship between rents and revenue changes for finance and professional services. These findings show that businesses that cater *in person* to the rich are those that lost the most businesses. Naturally, many of those businesses are located in high-income areas given people's preference for geographic proximity in consuming services.

As a result of this sharp loss in revenues, small businesses in high-rent areas are much more likely to close entirely. We measure closure in the Womply data as reporting zero credit card revenue for three days in a row. Appendix Figure 10 shows that 55% of small businesses in the highest-rent ZIP codes closed, compared with 40% in the lowest rent ZIP codes. The extensive

28. Of course, households do not restrict their spending solely to businesses in their own ZIP code. An alternative way to establish this result at a broader geography is to relate small business revenue changes to the degree of income inequality across counties. Counties with higher Gini coefficients experienced large losses of small business revenue (Appendix Figure 9a). This is particularly the case among counties with a large top 1% income share (Appendix Figure 9b). Poverty rates are not strongly associated with revenue losses at the county level (Appendix Figure 9c), showing that it is the presence of the rich in particular (as opposed to the middle class) that is most predictive of economic impacts on local businesses.

29. Consistent with this pattern, total spending levels and time spent outside also fell much more in high population density areas.

margin of business closure accounts for most of the decline in total revenues.

Because businesses located in high-rent areas lose more revenue in percentage terms and tend to account for a greater share of total revenue to begin with, they account for a very large share of the total loss in small business revenue. More than half of the total loss in small business revenues comes from business located in the top-quartile of ZIP codes by rent; only 8% of the revenue loss comes from businesses located in the bottom quartile. We now examine how the incidence of this shock is passed on to their employees.

III.C Impacts on Employment Rates and Low-Income Workers

We analyze the impacts of the COVID shock on employment using data from two sources: Earnin, which provides data on hours, wages, and employment rates for low-wage (bottom quintile) workers across a broad range of industries and Homebase, which provides analogous data for hourly workers in small businesses, especially restaurants and retail shops.

Benchmarking. As with the other series analyzed above, we begin by benchmarking changes in these series to nationally representative benchmarks. Figure 6a plots employment rates from the nationally representative Current Employment Statistics for all workers alongside the overall Earnin series and Homebase series. We also include three series constructed using data from ADP, a large payroll processor that covers nearly 20% of employment in the U.S. The ADP data are reweighted to provide estimates that are intended to represent all workers in the U.S. The first series is the monthly National Employment Report. The second and third series come from Cajner et al. (2020), who report estimates for all workers and also use ADP data to report estimates of the decline in employment by worker wage quintile, showing that employment rates fell much more sharply for lower-wage workers. We plot the estimate they report for workers in the bottom quintile in Figure 6a. Consistent with the findings of Cajner et al. (2020), the CES and ADP series for all workers exhibit smaller declines in employment rates than the series that focuses on low-wage (bottom quintile) workers. The ADP estimate for low-wage workers is roughly aligned with decline observed in Earnin. However, Homebase exhibits a much larger decline than Earnin.

The differences between trends in the Homebase data and other series is largely explained by differences in industry and size composition. Figure 6b establishes this result by replicating Figure 6a for workers in Accommodation and Food Services.[30] The Earnin series and overall ADP series are very closely aligned here, consistent with the fact that workers in the food services sector tend

30. Since estimates for Accommodation and Food Services are unavailable in ADP's National Employment Report, we use their Leisure and Hospitality Series.

to have low wage rates (Appendix Table 1). When we further restrict Earnin to small firms – with less than 50 employees, comparable to the typical sizes of firms in the Homebase data – we find closer alignment between the Earnin and Homebase data in terms of the magnitude of decline in employment.[31]

Based on this benchmarking exercise, we conclude that Earnin provides a good representation of employment rates for low-wage workers across sectors, while Homebase provides estimates that are representative of workers at small businesses, particularly in restaurants (who comprise 64% of workers in the Homebase data for whom sectoral data are available). We therefore use Earnin as our primary dataset for analyzing labor market outcomes for low-income workers, and supplement it with Homebase to look more closely at workers in restaurants.

Consistent with the results of Bartik et al. (2020), we find that wage rates have remained unchanged through the COVID shock for workers who retained their jobs. Additionally, changes in employment rates are virtually identical to changes in hours because the extensive margin accounts for the vast majority of hours reductions. As a result, the employment changes in Figure 6 are almost identical to observed changes in workers' hours and earnings (Appendix Figure 11).

Heterogeneity Across Areas. We now use the Earnin and Homebase data to examine the drivers of employment losses for low-wage workers. Building on the approach developed above, we focus on geographic heterogeneity in spending reductions and the resulting revenue losses faced by business. Figure 7 presents maps of changes in hours of work for small- and mid-size businesses (fewer than 500 employees) in the Earnin data by ZIP code in New York, San Francisco, and Chicago (analogous ZIP-level maps for other cities are available here).[32] The patterns closely mirror those observed for business revenues above, with a wide range of variation across ZIP codes. Hours of work fell by more than 80% in the most affluent areas of these cities, as compared with 30% in the least affluent areas. We observe very similar spatial patterns when we focus solely on workers in food and accommodation services in the Earnin and Homebase data (Appendix Figure 14) and when examining variation across counties at the national level (Appendix Figure 12).

Figure 8a presents a binned scatter plot of changes in hours of work vs. median rents by employer ZIP code in the Homebase data. Consistent with the results for revenues, we see much

31. One area of discrepancy between the datasets is that Homebase data exhibits a larger increase in employment starting in mid-April than any of the other series. This may be because employment in small restaurants recovered particularly quickly or because of specific trends in Homebase's clients.

32. We focus on small and mid-size businesses here because larger firms exhibit significantly smaller declines in employment (Appendix Figure 13) and because, as noted above, their markets are likely to extend well beyond the ZIP code in which they are located.

larger reductions in hours of work for workers who work in high-rent areas than low-rent areas. Figure 8b replicates this result in the Earnin data, separating workers who work in firms with fewer than 60,000 vs. more than 60,000 employees (which include large multi-establishment firms such as McDonalds, Starbucks, Home Depot, etc.). Hours fell by more than 55% for workers in the smaller group of firms located in high-rent ZIP codes, as compared with 25% for workers in low-rent ZIP codes.

Interestingly, we observe a similar gradient with respect to local rents for workers at very large firms: from near zero in the lowest-rent ZIPs to 25% in the highest-rent ZIPs. This presumably reflects the fact that multi-establishment firms such as Starbucks face larger revenue losses at stores located in more affluent neighborhoods for the reasons documented above, which in turns induces them to reduce employment in those areas more heavily.[33] While there is a similar gradient with respect to rent levels, the overall level of employment losses for workers at large firms is lower than at smaller firms. This may be because large firms lost less revenue as a result of the COVID shock given their line of business (e.g., fast food vs. sit-down restaurants), have a greater ability to substitute to other modes of business (delivery, online retail), or have more liquidity.

Because businesses located in high-rent areas lay off more workers and account for a greater share of employment to begin with, they account for a large share of the total loss in employment among low-income workers. 36% of the total loss in employment observed in the Earnin data comes from business located in the top-quartile of ZIP codes by rent; 11% comes from businesses located in the bottom quartile.

Job Postings. Prior work suggests that the labor market impacts of the recession may depend as much upon job postings as they do on the rate of initial layoffs (e.g., Diamond and Blanchard 1989, Elsby, Michaels, and Ratner 2015). We therefore now turn to examine how the spending shocks and revenue losses have affected job postings. We measure job postings at the county level using data from Burning Glass, which prior work has shown is fairly well aligned with government statistics based on the Job Openings and Labor Turnover Survey (Carnevale, Jayasundera, and Repnikov 2014, Kahn, Lange, and Wiczer 2020).[34] We conduct this analysis at the county level, pooling firms of all sizes and sectors because workers can substitute across firms and areas when searching for a new job, making it less relevant which exact firm or ZIP code they work in.

33. We cannot measure changes in revenue by establishment for large firms because the Womply data on revenues only cover small businesses. Moreover, one would need data on revenues by *establishment* within large companies to conduct such an analysis.

34. Burning Glass measures the sum of job postings, whereas JOLTS measures job openings at a given point in time. Hence, jobs that are posted and quickly filled will be included in Burning Glass but not in JOLTS.

Figure 8c presents a binned scatter plot of the change in job postings pre- vs. post-COVID vs. median rents by county for jobs that require minimal education. We find a pattern similar to what we find with current employment: job postings for lower-skilled workers in high-rent areas have fallen much more sharply (by approximately 30%) than for workers in lower-rent areas. Hence, low-wage workers in such areas are not only more likely to have lost their jobs to begin with, they also have poorer prospects of finding a new job. Figure 8d replicates Figure 8c for job postings that require higher levels of education. For this group, which is much more likely to be employed in tradable sectors that are less influenced by local conditions (e.g., finance or professional services), there is no relationship between local rents and the change in job postings, consistent with our findings above in Figure 5d.[35]

Unemployment Rates. The low rates of job postings combined with high rates of job loss in affluent areas combined to create very tight labor markets that produce unemployment in such areas that are unprecedented in recent history. To illustrate this, we contrast rates of employment losses by county in the COVID recession (from Feb-April 2020) with the Great Recession (from 2007-2010) using statistics on employment from the Bureau of Labor Statistics.[36]

Figure 9 shows that in the Great Recession, counties with lower median incomes tended to account for a greater share of job losses. In particular, the first set of bars in Figure 9 show that counties in the bottom quartile (25%) of household median income distribution comprised a disproportionate (30%) share of job losses. In contrast, in the recent recession they account for actually less than 25% of the job losses, consistent with the evidence above that employment losses from the COVID shock have been concentrated among low-income employees in affluent areas. In the final set of bars, we show that in the recent recession this has led to the surprising pattern that UI claims are almost equally likely to come from high versus low-income counties.[37]

Santa Clara CA is the highest income county on the West Coast, yet 16% of its labor force

35. The magnitude of the reduction in job postings for highly educated workers is substantial, at approximately 27%. This contrasts with evidence that higher-skilled workers have experienced much lower rates of job loss to date, and suggests that unemployment rates could begin to rise even for higher-skilled workers going forward.

36. One notable feature of the current COVID-induced recession is that the increase in unemployment rates between February and April 2020 (11%) is only two-thirds as large as the decrease in employment (16%). The difference is due to a 5% decline in the labor force: many people have lost their jobs but are not actively searching for a new job in the midst of the pandemic. In the three prior recessionary periods, the labor force continued to grow by 0.3% to 0.8% annually.

We therefore focus on the decline in employment rates.

37. Unlike our analyses of private data, the publicly released unemployment claims data do not allow us disaggregagate changes in employment by individuals' income or ZIP code. Given the evidence above that job losses are concentrated among low-wage workers in high-income areas, there is strong reason to believe that the unemployment claims in high-income counties are coming from lower-income individuals living in those counties.

claimed UI between March 15th to May 2nd. This claim rate is identical to the share of the labor force that claimed UI in Fresno CA, a low-income county in California's Central Valley. Unemployment rates above 10% have happened regularly in Fresno during prior recessions, but are unprecedented in Santa Clara. In Montgomery County, MD, long one of the richest counties in the U.S., workers have historically been quite insulated from prior recessions. During the 1991 and 2001 recessions the unemployment rate in Montgomery remained 3%. In 2010 it only hit 6%, one of the lowest in the country. In May 2020 employment losses and unemployment claims in Montgomery exceeded 12% of the labor force, resembling many counties with much lower average incomes.

In the Great Recession, the areas of the country that experienced the largest increases in unemployment took many years to recover because workers did not move to find new jobs and job vacancies remained depressed in hard-hit areas well after the national recession ended (Yagan 2019). Appendix Figure 15 shows early signs of a similar pattern in this recession: job postings went up significantly in late May in the U.S., but remained significantly lower in high-rent counties than in low-rent counties (where postings recovered nearly to pre-COVID levels by the end of May). If this pattern persists going forward, the recovery for low-income workers may take the longest in the richest parts of the U.S.

III.D Spending by Low-Income Workers

We close our analysis by showing job loss induced by working for firms in affluent areas affected the consumption of low-income workers themselves. To do so, we return to the credit card spending data from Affinity Solutions and ask whether low-income individuals working in high-rent ZIP codes reduce spending more than those working in low-rent ZIP codes.

Because we cannot measure workplace location in the credit card data itself, we use data from the Census LEHD Origin-Destination Employment Statistics (LODES) database, which provides information on the matrix of residential ZIP by work ZIP for all workers in the U.S. in 2017. Using this matrix, we compute the average workplace median rent level for each residential ZIP. Figure 10a presents a binned scatter plot of changes in hours of work by *home* (residential) ZIP code and average workplace rent, restricting the sample to low-income (bottom income quartile) ZIP codes. This figure confirms that low-income individuals who work in high-rent areas are more likely to lose their jobs, verifying that the LODES data linked to residential ZIPs produce the same result as directly using workplace ZIP codes in the Earnin data.

Figure 10b replicates Figure 10a using spending changes on the y axis. Low-income individuals who work in high-rent ZIP codes cut spending by 35% on average from the baseline period to mid-April 2020, compared with 15% for those working in low-rent ZIPs. In Appendix Table 4, we present a set of regression specifications showing that the relationship remains similar when we compare ZIP codes within the same county by including county fixed effects, control for rents in the home (residential) ZIP code, and include other controls. Intuitively, these results show that among two equally low-income ZIP codes in Queens, those who live in a ZIP code where many work in an affluent area (perhaps because of a proximate subway line into Manhattan) are more likely to lose their jobs and, as a result, cut their own spending more following the COVID shock.

IV Evaluation of Policy Responses to COVID-19

We have seen that a chain of events led to substantial employment losses following the COVID-19 shock: (1) reductions in spending by high-income individuals due to health concerns, (2) revenue losses for businesses catering to those customers, and (3) job losses for low-income workers working at those businesses. We now turn to study what type of policies can mitigate the economic impacts of the pandemic, focusing in particular on increasing employment among low-income workers. We study three sets of policies that target different points of the economic chain: (1) state-ordered business reopenings that remove barriers to economic activity; (2) stimulus payments to households, which aim to spur consumer spending and thereby increase employment; and (3) loans to small businesses, which provide liquidity to keep workers on payroll.

IV.A State-Ordered Reopenings

One direct approach to changing consumer spending and employment is via executive orders. Many states enacted stay-at-home orders and shutdowns of businesses in an effort to limit the spread of COVID infection and later reopened their economies by removing these restrictions. We begin by examining how such executive orders affect economic activity, exploiting variation across states in the timing of shutdowns and reopenings. Throughout this section, we define the reopening date to be the day that a state *began* the reopening process. In most states, reopening was a gradual process in which certain industries and types of businesses opened before others, but there was a lot of heterogeneity across states in the precise form that the reopening took. Our estimates should therefore be viewed as an assessment of the average impact of typical re-opening efforts on aggregate economic activity; we defer a more detailed analysis of how different types of re-openings affect different sectors (which can be undertaken with the data we have made publicly available) to future work.

We begin with a case study comparing Colorado and New Mexico that is representative of our broader findings. These two states both issued stay-at-home orders during the final week of March (New Mexico on March 24, Colorado on March 26). Colorado then partially reopened its economy, permitting a larger group of businesses to operate, on May 1, while New Mexico did not re-open until two weeks later, on May 16.[38]

Figure 11a plots consumer spending (using the Affinity Solutions data) in Colorado and New Mexico. Spending evolved on a nearly identical path in these two states: in particular, there is no evidence that the earlier reopening in Colorado did anything to boost spending during the two intervening weeks before New Mexico reopened.

Figure 11b generalizes the case study in Figure 11a by studying partial reopenings in the 20 states that issued such orders on or before May 4. For each reopening date (of which there are five: April 20, 24th, and 27, as well as May 1 and 4), we compare the trajectory of spending in treated states to a group of control states selected from the group of 13 states that did not issue reopening orders until after May 18. We select the control states for each of the five reopening dates by choosing nearest-neighbor matches on pre-period levels of spending (relative to January) during the weeks ending March 31, April 7, and April 19. Appendix Table 5 lists the control states we use for each date. We then calculate unweighted means of the outcome variables in the control and treatment states to construct the two series for each reopening date. Finally, we pool these five event studies together (redefining calendar time as time relative to the reopening date) to create Figures 11b.

Just as in the case study of Colorado vs. New Mexico, the trajectories of spending in the treated states almost exactly mirror that in the control states. Figure 11c shows that the same is true for low-wage workers' employment rates (using Earnin data). Given that earlier reopenings had no impact on consumer behavior, it is not surprising that it also had little or no downstream impact on employment.[39] These results are consistent with the findings of Lin and Meissner (2020), who use a state-border discontinuity design and find no impact of stay-at-home orders on job losses.

38. Specifically, on 1 May Colorado allowed retail businesses to open to the public beyond curbside pick-up and delivery, and permitted personal services businesses to re-open.

39. We emphasize that these results apply to *average* employment rates for *low-income* workers and are thus not inconsistent with evidence of modest impacts in specific subsectors, particularly at higher wage levels, as identified e.g., by Cajner et al. (2020).

Why did these reopenings have so little immediate impact on economic activity?[40] The evidence in Section 3 suggests that health concerns among consumers were the primary driver of the sharp decline in economic activity in March and April. Consistent with that evidence, spending fell sharply in most states *before* formal state closures (Appendix Figure 16). If health concerns are the core driver of reductions in spending rather than government-imposed restrictions, governments may have limited capacity to restore economic activity through reopenings, especially if those reopenings are not interpreted by consumers as a clear signal of reduced health risks.

IV.B Stimulus Payments to Households

The Coronavirus Aid, Relief, and Economic Security (CARES) Act made direct payments to nearly 160 million people, totaling $267 billion as of May 31, 2020. Individuals earning less than $75,000 received a stimulus payment of $1,200; married couples earning less than $150,000 received a payment of $2,400; and households received an additional $500 for each dependent they claimed. These payments were reduced at higher levels of income and phased out entirely for households with incomes above $99,000 (for single filers without children) or $198,000 (for married couples without children). The vast majority of these stimulus payments were deposited on exactly April 15, 2020, while some households received payments on April 14 (Appendix Figure 17).

The goal of these stimulus payments was to increase consumer spending and restore employment.[41] Was the stimulus effective in achieving these goals? In this section, we analyze this question using high-frequency event studies examining spending and employment changes in the days surrounding April 15, comparing outcomes for lower-income and higher-income households.

Impacts on Consumer Spending. We begin in Figure 12a by plotting a weekly moving average of spending changes relative to mean levels in January for low-income (bottom income quartile) vs. high-income (top income quartile ZIP codes) households. As noted above, high-income households decreased spending by more than low-income households in the immediate aftermath of the COVID shock; in the week ending April 13th, spending in top-income-quartile households was down by 36% relative to pre-COVID levels, as compared with 28% for bottom-income-quartile households. Starting on April 15, spending rose very sharply for those in the bottom income quartile, increasing by nearly 20 percentage points within a week. Spending among top-income-quartile households increased as well, but by only about 9 percentage points. This simple analysis suggests that the

40. Reopenings could have a lagged effect on spending, particularly if they serve as a signal of changes in health risks; going forward, the real-time data in the tracker can be used to assess such lagged impacts.

41. The Congressional Budget Office (2020) estimates that these payments will cost $293 billion, a considerably larger sum than similar direct stimulus in 2001 and 2008.

stimulus payments had a large positive effect on spending, especially for low-income families.[42]

To estimate the causal effect of the stimulus payments more precisely, we use a regression discontinuity estimator with the daily spending data.[43] Figures 12b and 12c plot daily spending levels relative to baseline for low- and high-income households, respectively, for the month of April. Spending levels jumped sharply from April 13th to 15th. Fitting a linear approximation to the points on either side of the stimulus, we estimate that spending levels rose discontinuously on April 15 by 26pp in low-income households and 9pp in high-income households.[44] Both effects are statistically significantly different from 0, as well as from each other. These findings are consistent with Baker et al. (2020) and Karger and Rajan (2020), who use individual transaction data on incomes and spending patterns of approximately 15,000 primarily low-income individuals to estimate a large and immediate effect of receiving the stimulus check on spending, especially among the very poorest households.

In Figures 12d and 12e, we investigate the composition of goods on which households spent their stimulus checks. We pool all households in these figures to maximize precision. Figure 12d shows that spending on durable goods rose by 21 pp following the arrival of the stimulus payments and further increased thereafter, rising well above pre-crisis levels. But Figure 12e shows that spending on in-person services rose by only 7 pp, remaining more than 50% below pre-crisis levels. Durable goods accounted for 44% of the recovery in spending levels from the beginning to the end of April, despite accounting for just 23% of pre-crisis spending. In-person services accounted for just 18% of the recovery, despite making up 32% of pre-crisis spending (Appendix Figure 18).[45] These results show that the stimulus increased the overall level of spending, but did not increase spending in the sectors where spending fell most following the COVID shock (Figure 2b). As a result, the stimulus did not channel money back to the businesses that lost the most revenue as a result of the COVID shock.

Impacts on Business Revenue Across Areas. Next, we investigate how the stimulus program affected business revenues across areas. In particular, did the businesses that lost the most revenue –

42. We expect the stimulus program to have a smaller impact on high-income households for three reasons. First, lower-income households simply received more money than high-income households. Second, low-income households spend half as much as high-income households prior to the COVID shock (Figure 2a), and hence one would expect a larger impact on their spending levels as a percentage of baseline spending. Finally, many studies have found higher marginal propensities to consume (MPCs) among lower-income households, who are often more liquidity constrained.

43. We use the raw daily data, not the 7-day moving average.

44. We omit the partially treated date of April 14 (denoted by a hollow dot) since a small fraction of stimulus payments arrived on that day when estimating this RD specification.

45. The other major spending categories (non-durable goods and remote services) each accounted for 19% of the recovery and 23% and 21% of pre-crisis spending, respectively.

those in high-rent areas – gain business as as result of the stimulus? Figures 13a and 13b replicate the analysis above using Womply data on small business revenues as the outcome, separately for lowest-rent-quartile and highest-rent-quartile ZIP codes. We see a sharp increase of 21 pp in revenues in small businesses in low-rent neighborhoods exactly at the time when households received stimulus payments. In contrast, Panel B shows a small, statistically insignificant increase in revenues of 4 pp for small businesses in high-rent areas.

This geographic heterogeneity illustrates another important dimension in which the stimulus did not channel money back to the business that lost the most revenue from the COVID shock. In fact, the stimulus actually *amplified* the difference in small business revenue losses rather than narrowing it across areas. Those in low-rent areas have nearly returned to pre-crisis levels following the stimulus payments, while those in high-rent areas remained nearly 40% down relative to January levels in the second half of April (Figure 13c, solid lines).

Impacts on Low-Income Employment. Finally, we investigate whether the increase in spending induced by the stimulus increased employment rates, as one would expect in a traditional Keynesian stimulus. Here, we do not use the RD design as we do not expect employment to respond immediately to increased spending. Instead, we analyze the evolution of employment of low-income workers in the Earnin data in low vs. high-rent ZIP codes over time in Figure 13c (dashed lines). In high-rent areas, low-wage employment remains 45% below pre-COVID levels – perhaps not surprisingly, since revenues have not recovered significantly there. But even in low rent areas, payroll has recovered only slightly, which is a surprising contrast with the sharp recovery of small business revenues. It is unclear why revenues and employment both *fell* in tandem at very similar rates when the COVID shock hit, but revenues recovered much more quickly than employment in low-rent areas. One possibility is that businesses have reopened temporarily with a minimal staff (Lazear, Shaw, and Stanton 2016) and are planning to recall or hire new workers going forward. A more worrisome possibility is a "jobless" recovery, in which economic activity shifts away from in-person labor intensive production, reducing employment opportunities in the longer term (Berger 2012).

In summary, our analysis suggests that stimulus substantially increased total consumer spending but did not directly undo the initial spending reductions by returning money back to the businesses that lost the most revenue. In a frictionless model where businesses and workers could costlessly reallocate their capital and labor to other sectors, this reallocation of spending might have no consequence for employment levels. But if workers' ability to switch jobs is constrained – e.g., because of job-specific skills that limit switching across industries or costs that limit moving across geographic areas, as suggested by Yagan (2019) – the ability of the stimulus to foster a uniform recovery in employment to pre-COVID levels is likely to be hampered.

IV.C Loans to Small Businesses

We now turn to evaluate the Paycheck Protection Program (PPP), a policy that sought to reduce employment losses by providing direct support to small businesses. Congress appropriated nearly $350 billion for loans to small businesses in an initial tranche that was paid beginning on April 3, followed by another $175 billion in a second round beginning on April 27. The program offered loan forgiveness for businesses that maintained sufficiently high employment levels through June 30 (relative to pre-crisis levels), providing an incentive for small businesses to keep employees on payroll.

How effective was the PPP program in increasing employment, particularly among low-income workers? We study this question by exploiting the fact that eligibility for the PPP depended on business size. Firms with fewer than 500 employees before the COVID crisis qualified for PPP loans, while those with more than 500 employees generally did not.[46] One important exception to this rule is the food service industry, which was treated differently because of the prevalence of franchises. We therefore omit the food services sector from the analysis that follows.[47]

We estimate the causal effects of the PPP using a difference-in-differences research design, comparing trends in employment for firms below the 500 employee cutoff (the treated group) vs. those above the 500 employee cutoff (the control group) before vs. after April 3, when the PPP program began. Figure 14a plots the average change in employment rates (inferred from payroll deposits) relative to January by decile of business size in the Earnin data. To adjust for the fact that industry composition varies across firms of different sizes, we reweight firms within each decile to match the average (2 digit NAICS) industry composition in the sample as a whole when computing mean employment rates by decile. Recognizing that our size measures in the Earnin data do not correspond exactly to those used to determine PPP eligibility by the Small Business

46. The eligibility rules vary across industries, with some exceptions that allow larger firms to obtain loans. Appendix Figure 19 plots a histogram of the exact size cutoffs weighting by employees in the national sample in Reference USA data (Panel A) and employees in Earnin data (Panel B), in both cases restricting to workers in companies with 300-700 employees. More than 90% of employees work at firms that face the 500 employee threshold. In addition to employment thresholds, firms may also qualify based on revenue thresholds set by the Small Business Administration; however, using the distribution of firm size and revenue from Reference USA, we estimate that in practice the size threshold is the binding constraint for the vast majority of firms. Given these results, we use a pre-COVID employee size cutoff of 500 to define treatment and control groups.

47. We find no differences in employment trends below vs. above the 500 employee threshold in the food services sector as well (Appendix Figure 20), consistent with our results below.

Administration, we plot trends in employment for firms of various sizes, not just those just above vs. below the 500 employee cutoff. We focus in particular on firms in the 3rd-6th deciles of firm size in Figure 14a. The 3rd and 4th deciles have an average of about 45 and 130 employees, respectively, and therefore consist of firms that would almost certainly be eligible for the PPP. Most businesses in the 5th size decile (with an average of 413 employees) were also likely to be eligible. Firms in the 6th decile are largely above the 500 employee threshold (with an average of roughly 1,500 employees).

Before April 3, trends in employment are extremely similar across the four groups, showing that larger businesses (in the 6th decile) are likely to provide a good counterfactual for employment trends one would have observed in smaller firms absent the PPP program. After April 3, the trends remain extremely similar across firms of all sizes: in particular, there is no evidence that employment went up in firms in the smaller deciles relative to larger firms after April 3, as one would expect if PPP had a substantial treatment effect on employment rates.

Figure 14b plots the change in employment vs. average firm size, by decile. Again, we see that the decline in payroll is stable across firm size, varying between -36% and -39% between firms with an average size ranging from 5 to 30,000 employees.

Figure 14c replicates Figure 14a, splitting businesses into those located in the highest-rent (top quartile) ZIP codes and lowest-rent (bottom quartile) ZIP codes. To simplify the plot, we combine the 3rd and 4th deciles into a single "PPP eligible" group and omit the partially treated 5th size decile. As noted in Section 3, the decline in hours worked is about 35% larger in high-rent areas than in low-rent areas. But there is no evidence that the PPP had any significant impact on employment rates in either of these groups. As in Panels A and B, there is little or no difference in hours worked across businesses by size. In particular, employment fell about as much as business revenue did in these areas (Figure 5c). We therefore conclude that the PPP had little material impact on employment at small businesses: we cannot rule out a small positive employment effect of the program (of e.g., 3-4 pp on employment rates), but it is clear that the program did not restore the vast majority of jobs that were lost following the COVID shock.[48]

Why did the PPP have small effects on employment rates? One potential explanation is that the loans were taken by firms that intended not to layoff many employees to begin with, i.e. firms

48. We do not directly observe the loans provided to each firm, and as a result we cannot estimate the "first stage" of the program on receipt of loans. From a reduced form perspective, we can conclude that the effect of the policy on aggregate employment was not large, but we cannot directly estimate the causal effect of receiving a PPP loan on employment rates.

that were inframarginal recipients of loans. Consistent with this, Granja et al. (2020) show that states and congressional districts that experienced more job losses prior to April 3 actually received *fewer* PPP loans. Moreover, PPP loans also were not distributed to the industries most likely to experience job losses from the COVID crisis. For example, firms in the professional, scientific, and technical services industry received a greater share of the PPP loans than accommodation and food services (SBA 2020). Yet accommodation and food services accounted for half of the total decline in employment between February and March (prior to PPP enactment) in BLS statistics, while employment in professional, scientific and technical services accounted for less than 5% of the decline.

V Conclusion

Data held by private companies provide an unprecedented capacity to measure economic activity at a granular level very rapidly. These data have become integral to corporations in business decisions. In this paper, we have constructed a freely available platform that harnesses the same data with the aim of supporting public policy.

We use these new data to analyze the initial impacts of COVID-19 on people, businesses, and communities. We find that COVID-19 induced high-income households to self-isolate and sharply reduce spending in sectors that require physical interaction. This spending shock in turn led to losses in business revenue and layoffs of low-income workers at firms that cater to high-income consumers, ultimately reducing their own consumption levels. Because the root cause of the shock appears to be self-isolation driven by health concerns, there is limited capacity to restore economic activity without addressing the virus itself. In particular, we find that state-ordered reopenings of economies have only modest impacts on economic activity; stimulus checks increase spending particularly among low-income households, but very little of the additional spending flows to the businesses most affected by the COVID shock; and loans to small businesses have little impact on employment rates. Our analysis therefore suggests that the only effective approach to mitigating economic hardship in the short run may be to provide benefits to those who have lost their incomes to mitigate consumption losses while public health measures restore consumer confidence and ultimately increase spending.

We focused here on the short-run economic consequences of the COVID-19 crisis. However, this economic shock could also have long-lasting scarring effects that warrant attention. As an illustration of how private sector data can be useful in tracking these impacts as well, Figure 15 plots weekly student progress (lessons completed) on Zearn, an online math platform used by many elementary school students as part of their regular school curriculum. Children in high-income areas experience a temporary reduction in learning on this platform when the COVID crisis hit, but soon recover to baseline levels; by contrast, children in lower-income areas remain 50% below baseline levels persistently. Although this platform captures only one aspect of education, these findings raise the concern that COVID-19 may reduce social mobility and ultimately further amplify inequality by having particularly negative effects on human capital development for lower-income children.

Going forward, our analysis illustrates two roles for real-time tracking using private sector data to support economic policy in this crisis and beyond. First, the data can be used to learn rapidly from heterogeneity across areas, as different places are often hit by differential shocks and pursue different local policy responses. This approach can permit rapid diagnosis of the root factors underlying an economic crisis. Second, the data can permit rapid evaluation of ongoing policies, potentially helping to fine-tune policy responses.

More broadly, the platform built here can be viewed as a preliminary prototype for a system of "real time national accounts" using administrative data from the private sector, much as the Bureau of Economic Analysis, building on a prototype developed by Kuznets (1941), instituted a set of systematic, recurring surveys of businesses and households that are the basis for the National Income accounts of the United States. The analysis in this paper demonstrates that even this prototype can yield timely insights that are not apparent in existing data, suggesting that a more systematic platform that aggregates data from several private companies has great potential for improving our understanding of economic activity and policymaking going forward.

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Supplementary Appendix

In this appendix, we describe additional details about the key dates in the COVID-19 crisis as well as geographic definitions used in our analysis.

Key Dates for COVID-19 Crisis. The Economic Tracker includes information about key dates relevant for understanding the impacts of the COVID-19 crisis. At the national level, we focus on three key dates:

- First U.S. COVID-19 Case: 1/20/2020
- National Emergency Declared: 3/13/2020
- CARES Act Signed in to Law: 3/27/2020

At the state level we collect information on the following events:

- Schools closed statewide: Sourced from COVID-19 Impact: School Status Updates by MCH Strategic Data, available here. Compiled from public federal, state and local school information and media updates.
- Nonessential businesses closed: Sourced from the Institute for Health Metrics and Evaluation state-level data (available here), who define a non-essential business closure order as: "Only locally defined 'essential services' are in operation. Typically, this results in closure of public spaces such as stadiums, cinemas, shopping malls, museums, and playgrounds. It also includes restrictions on bars and restaurants (they may provide take-away and delivery services only), closure of general retail stores, and services (like nail salons, hair salons, and barber shops) where appropriate social distancing measures are not practical. There is an enforceable consequence for non-compliance such as fines or prosecution."
- Stay-at-home order goes into effect: Sourced from the New York Times stay at home order data, available here.
- Stay-at-home order ends: Sourced from the New York Times reopening data, available here. Defined as the date at which the state government lifted or eased the executive action telling residents to stay home.
- Partial business reopening: Sourced from the New York Times reopening data, available here. Defined as the date at which the state government allowed the first set of businesses to reopen.

Geographic Definitions. For many of the series we convert from counties to metros and ZIP codes to counties. We use the HUD-USPS ZIP Code Crosswalk Files to convert from ZIP code to county. When a ZIP code corresponds to multiple counties, we assign the entity to the county with the highest business ratio, as defined by HUD-USPS ZIP Crosswalk. We generate metro values for a selection of large cities using a custom metro-county crosswalk, available in Appendix Table 6. We assigned metros to counties and ensured that a significant portion of the county population was in the metro of interest. Some large metros share a county, in this case the smaller metro was subsumed into the larger metro.

Table 1
Cities with Largest Small Business Revenue Losses Following COVID Shock

City	State	% Change in Small Bus. Revenue (Womply)	% Change in Low-Wage Worker Hours, Small Restaurants/Retail (HomeBase)	% Change in Low-Wage Worker Hours (Earnin)
(1)	(2)	(3)	(4)	(5)
New Orleans	Louisiana	-80.8%	-76.6%	-60.9%
Washington	District of Columbia	-72.9%	-73.2%	-60.2%
Honolulu	Hawaii	-62.7%	-75.8%	-25.3%
Miami	Florida	-62.2%	-68.7%	-51.1%
Boston	Massachusetts	-60.6%	-79.5%	-60.9%
Philadelphia	Pennsylvania	-58.7%	-66.6%	-51.8%
Fresno	California	-58.7%	-60.7%	-36.6%
San Jose	California	-58.6%	-61.5%	-51.9%
New York City	New York	-57.0%	-78.7%	-63.4%
Las Vegas	Nevada	-56.1%	-66.4%	-53.0%

Notes: This table shows the ten cities with the largest small business revenue declines as measured in the Womply data (among the fifty largest cities in the U.S.). The decline is defined as net revenue normalized by revenue in 2019 from March 25th 2020 to April 14th 2020 over the normalized net revenue from Jan 8th to March 10th 2020. The changes in low-wage worker hours (both for small restaurants/retail - HomeBase and in general - Earnin) are defined as the change in hours from March 25th 2020 to April 14th 2020 relative to total hours from Jan 8th to March 10th 2020.

Appendix Table 1
Hourly Wage Rates By Industry

		2019 BLS Wages			Median in Private Datasets	
		10th Percentile (Pre Tax)	25th Percentile (Pre Tax)	Median (Pre Tax)	Earnin (Post Tax)	Homebase (Pre Tax)
NAICS Code	NAICS Description	(1)	(2)	(3)	(4)	(5)
22	Utilities	18.56	26.82	38.06	15.00	
55	Management of Companies and Enterprises	16.09	22.42	34.74	12.34	
54	Professional, Scientific, and Technical Services	14.85	21.62	34.00	12.63	13.00
51	Information	12.90	19.56	32.13	12.49	
52	Finance and Insurance	14.25	18.40	27.42	12.77	
21	Mining, Quarrying, and Oil and Gas Extraction	15.36	19.11	25.82	15.69	
61	Educational Services	11.54	16.18	24.47	13.25	11.50
23	Construction	13.78	17.51	23.92	13.94	
42	Wholesale Trade	12.30	15.73	22.05	11.79	
48-49	Transportation and Warehousing	12.07	15.49	20.89	13.20	15.00
31-33	Manufacturing	12.36	15.35	20.77	12.66	
53	Real Estate and Rental and Leasing	11.31	14.14	19.31	12.64	
62	Health Care and Social Assistance	11.18	13.59	19.27	11.68	14.00
81	Other Services (except Public Administration)	9.73	12.02	16.57	10.97	14.00
56	Administrative Support	10.33	12.26	15.71	11.82	
71	Arts, Entertainment, and Recreation	9.21	11.17	14.09	10.38	12.00
11	Agriculture, Forestry, Fishing and Hunting	11.28	11.89	13.38	11.56	
44-45	Retail Trade	9.49	11.18	13.36	9.76	12.00
72	Accommodation and Food Services	8.68	9.61	11.81	9.26	11.00

Notes: This table reports wages at various percentiles for two-digit NAICS sectors. 2019 BLS Wages (1-3) come from the May 2019 Occupational Employment Statistics and are inflated to 2020 dollars using the Consumer Price Index. Columns (4) and (5) report median wages in two private employment datasets, Earnin and Homebase. In Earnin and Homebase, the median wage is the 50th percentile of hourly wages for workers of the given industry during the pre-COVID period (January 8th - March 10th). In Earnin (4), wages are calculated by dividing the payment deposited in the individual's bank account by hours worked and are thus post-tax. Homebase wages are pre-tax. Industries missing from the Homebase data are left blank.

Appendix Table 2
Demographic Characteristics of Zearn Users

	Zearn Users (1)	US Population (2)
Panel A: Income		
ZIP Median Household Income		
25th Percentile	43,766	45,655
Median	54,516	57,869
75th Percentile	70,198	77,014
Number of ZIP codes	5,148	33,253
Number of People	803,794	322,586,624

	Zearn Users	US K-12 Students
Panel B: School Demographics		
Share of Black Students		
25th Percentile	1.4%	1.5%
Median	5.6%	5.8%
75th Percentile	21.3%	19.1%
Share of Hispanic Students		
25th Percentile	4.3%	5.6%
Median	10.9%	15.0%
75th Percentile	35.7%	40.6%
Share of Students Receiving FRPL		
25th Percentile	33.8%	28.2%
Median	55.5%	50.1%
75th Percentile	78.5%	74.8%
Number of Schools	8,801	88,459
Number of Students	767,310	49,038,524

Notes: This table reports demographic characteristics for US schools. Household income percentiles are calculated using the 2017 median household income in each school's ZIP code. The share of students who are Black, Hispanic, or receive Free or Reduced Price Lunch (FRPL) in a given school are calculated using school demographic data from the Common Core data set from MDR Education, a private education data firm. Percentile distributions for each demographic variable are calculated separately and weighted by the number of students in each school. Column (1) reports school characteristics for students using Zearn, while Column (2) reports income data for the entire US population and shares of students who are Black, Hispanic, or receive FRPL for all US elementary school students.

Appendix Table 3
Association Between Changes in Business Revenue and Area Rents

Outcome:	% Change in Small Business Revenues				% Change in Small Business Revenue in Food Services and Accommodation	
	(1)	(2)	(3)	(4)	(5)	(6)
Median 2BR Rent	-0.0110 (0.0006)	-0.0199 (0.0011)	-0.0110 (0.0007)	-0.0173 (0.0011)	-0.0244 (0.0025)	-0.0212 (0.0025)
Controls:						
County Fixed Effects		X		X	X	X
Worker Density (Log)			X	X		X
Observations	16,477	16,475	16,469	16,467	9,913	9,910

Notes: This table shows OLS regressions of average percentage changes in business revenue by ZCTA code (using Womply data) on average ZCTA code median two-bedroom rent and median household income. Standard errors are reported in parentheses. The dependent variable is scaled from 0 to 100, such that, for example, the coefficient of -0.011 in Column (1) implies that a $100 increase in monthly workplace rent is associated with a 1.1% larger drop in total revenue. Columns (1)-(4) use the percent change in all small business revenue while Columns (5) and (6) use the percent change in food services and accommodation small business revenue as the outcome. Column (1) shows the baseline regression without any controls while the rest of the columns add county fixed effects and the log of worker density.

Appendix Table 4

Association Between Changes in Consumer Spending Home Area and Workplace Area Rents

Dep. Var.: % Change in Total Credit Card Spending			
	(1)	(2)	(3)
Median Workplace 2BR Rent	-0.0129	-0.0089	-0.0121
	(0.0006)	(0.0012)	(0.0039)
Median Home 2BR Rent		-0.0065	
		(0.0017)	
Controls:			
County Fixed Effects			X
Observations	8,934	6,682	8,934

Notes: This table shows OLS regressions of average percentage changes in consumer spending by ZCTA code (using data from Affinity Solutions) on average workplace ZCTA code median two-bedroom rent. Standard errors are reported in parentheses. Workplace ZCTA code rent is computed by using data from the Census LEHD Origin-Destination Employment Statistics (LODES) database as described in the text. The dependent variable is scaled from 0 to 100 such that, for example, the coefficient of -0.0129 in Column (1) implies that a $100 increase in monthly workplace rent is associated with a 1.2% larger drop in total spending. Column (1) shows the baseline regression without any controls, Column (2) adds median home two bedroom rent and Column (3) adds county level fixed effects.

Appendix Table 5
List of Partial Re-Openings and Control States for Event Study

Date	States that Re-Opened	Affinity Controls	Earnin Controls
April 20th, 2020	South Carolina	Kentucky, New Hampshire	Illinois, Kentucky, Maryland, New Hampshire, New Mexico, Oregon, South Dakota, Virginia, Washington, Wisconsin
April 24th, 2020	Alaska, Georgia, Oklahoma	Delaware, Illinois, Louisiana, Maryland, New Jersey, New York, Oregon, South Dakota, Virginia	New Mexico, Oregon, South Dakota, Virginia
April 24th, 2021	Minnesota, Mississippi, Montana, Tennessee	Illinois, New Jersey	Illinois, South Dakota

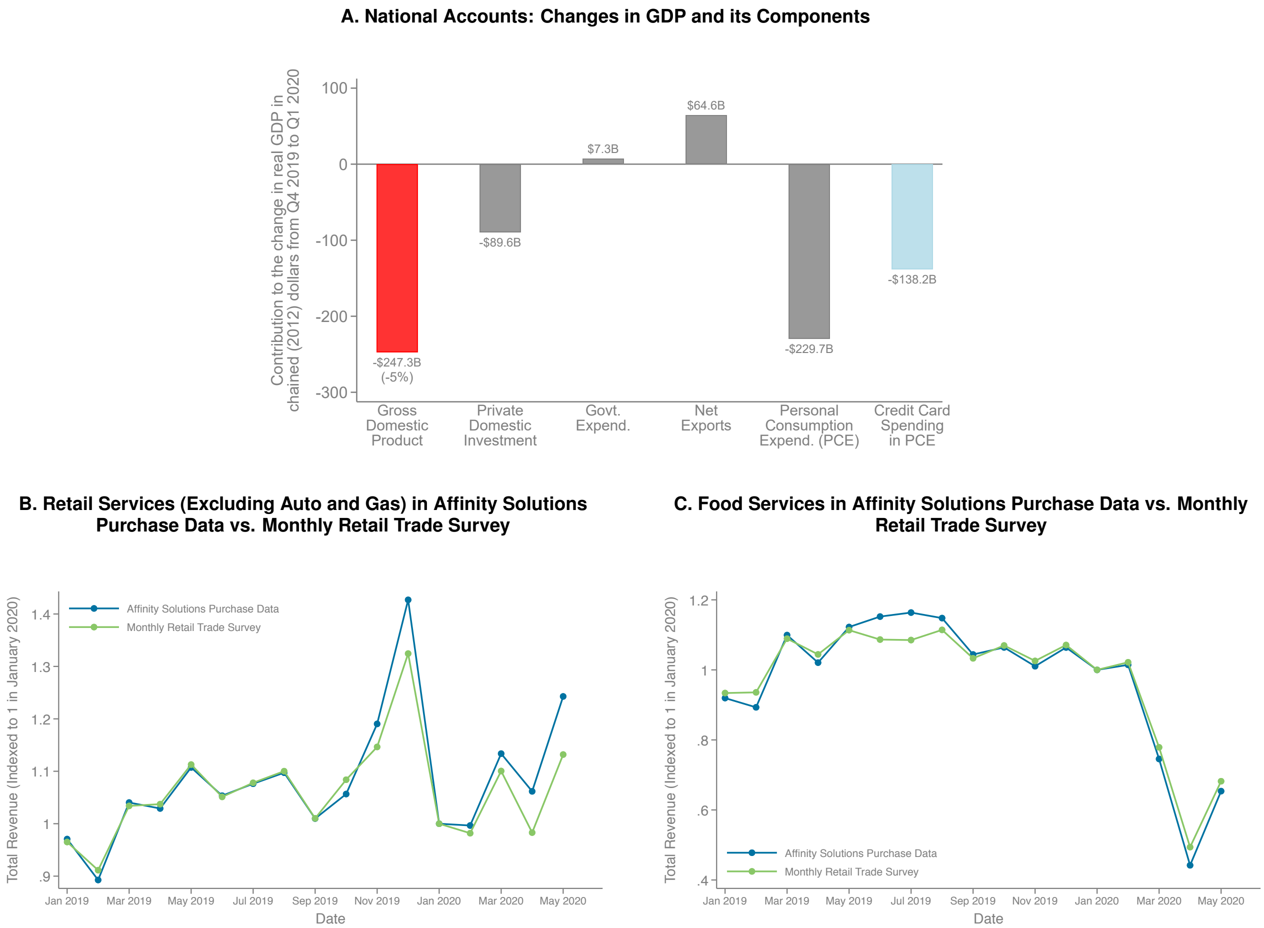
Notes : This table lists the treatment and control states for each opening date in Figures 11b-11c.

Appendix Table 6
City to County Crosswalk

City Name	State Name	County	County Fips Code
Los Angeles	California	Los Angeles	6037
New York City	New York	Richmond	36085
New York City	New York	Kings	36047
New York City	New York	Queens	36081
New York City	New York	New York	36061
New York City	New York	Bronx	36005
Chicago	Illinois	Cook	17031
Houston	Texas	Harris	48201
Phoenix	Arizona	Maricopa	4013
San Diego	California	San Diego	6073
Dallas	Texas	Dallas	48113
Las Vegas	Nevada	Clark	32003
Seattle	Washington	King	53033
Fort Worth	Texas	Tarrant	48439
San Antonio	Texas	Bexar	48029
San Jose	California	Santa Clara	6085
Detroit	Michigan	Wayne	26163
Philadelphia	Pennsylvania	Philadelphia	42101
Columbus	Ohio	Franklin	39049
Austin	Texas	Travis	48453
Charlotte	North Carolina	Mecklenburg	37119
Indianapolis	Indiana	Marion	18097
Jacksonville	Florida	Duval	12031
Memphis	Tennessee	Shelby	47157
San Francisco	California	San Francisco	6075
El Paso	Texas	El Paso	48141
Baltimore	Maryland	Baltimore	24005
Portland	Oregon	Multnomah	41051
Boston	Massachusetts	Suffolk	25025
Oklahoma City	Oklahoma	Oklahoma	40109
Louisville	Kentucky	Jefferson	21111
Denver	Colorado	Denver	8031
Washington	District of Columbia	District Of Columbia	11001
Nashville	Tennessee	Davidson	47037
Milwaukee	Wisconsin	Milwaukee	55079
Albuquerque	New Mexico	Bernalillo	35001
Tucson	Arizona	Pima	4019
Fresno	California	Fresno	6019
Sacramento	California	Sacramento	6067
Atlanta	Georgia	Fulton	13121
Kansas City	Missouri	Jackson	29095
Miami	Florida	Dade	12086
Raleigh	North Carolina	Wake	37183
Omaha	Nebraska	Douglas	31055
Oakland	California	Alameda	6001
Minneapolis	Minnesota	Hennepin	27053
Tampa	Florida	Hillsborough	12057
New Orleans	Louisiana	Orleans	22071
Wichita	Kansas	Sedgwick	20173
Cleveland	Ohio	Cuyahoga	39035
Bakersfield	California	Kern	6029
Honolulu	Hawaii	Honolulu	15003
Boise	Idaho	Ada	16001
Salt Lake City	Utah	Salt Lake	49035
Virginia Beach	Virginia	Virginia Beach City	51810
Colorado Springs	Colorado	El Paso	8041
Tulsa	Oklahoma	Tulsa	40143

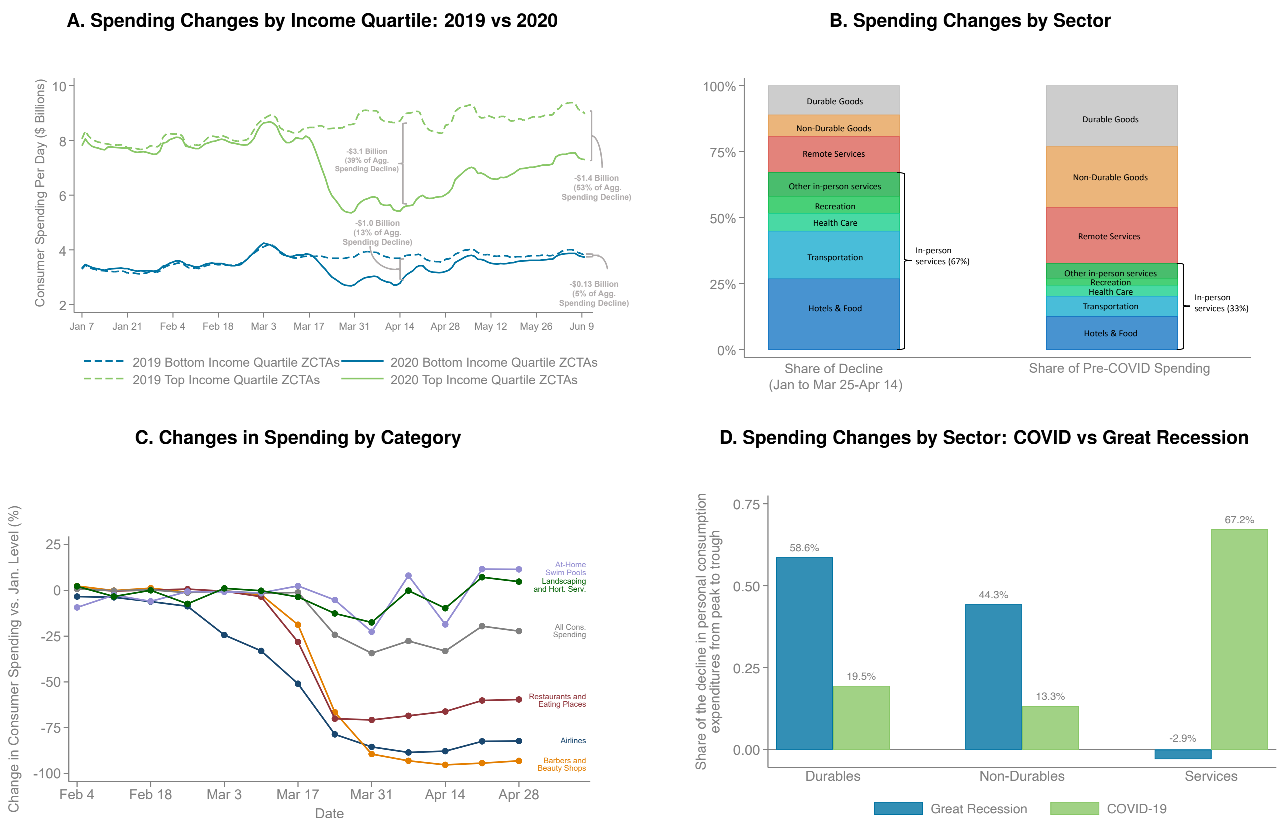
Notes: This table shows our metro area (city) to county crosswalk. We assigned metros to counties and ensured that a significant portion of the county population was in the metro of interest. Some large metros share a county, in this case the smaller metro was subsumed into the larger metro.

FIGURE 1: Changes in Consumer Spending: National Accounts vs. Credit Card Data



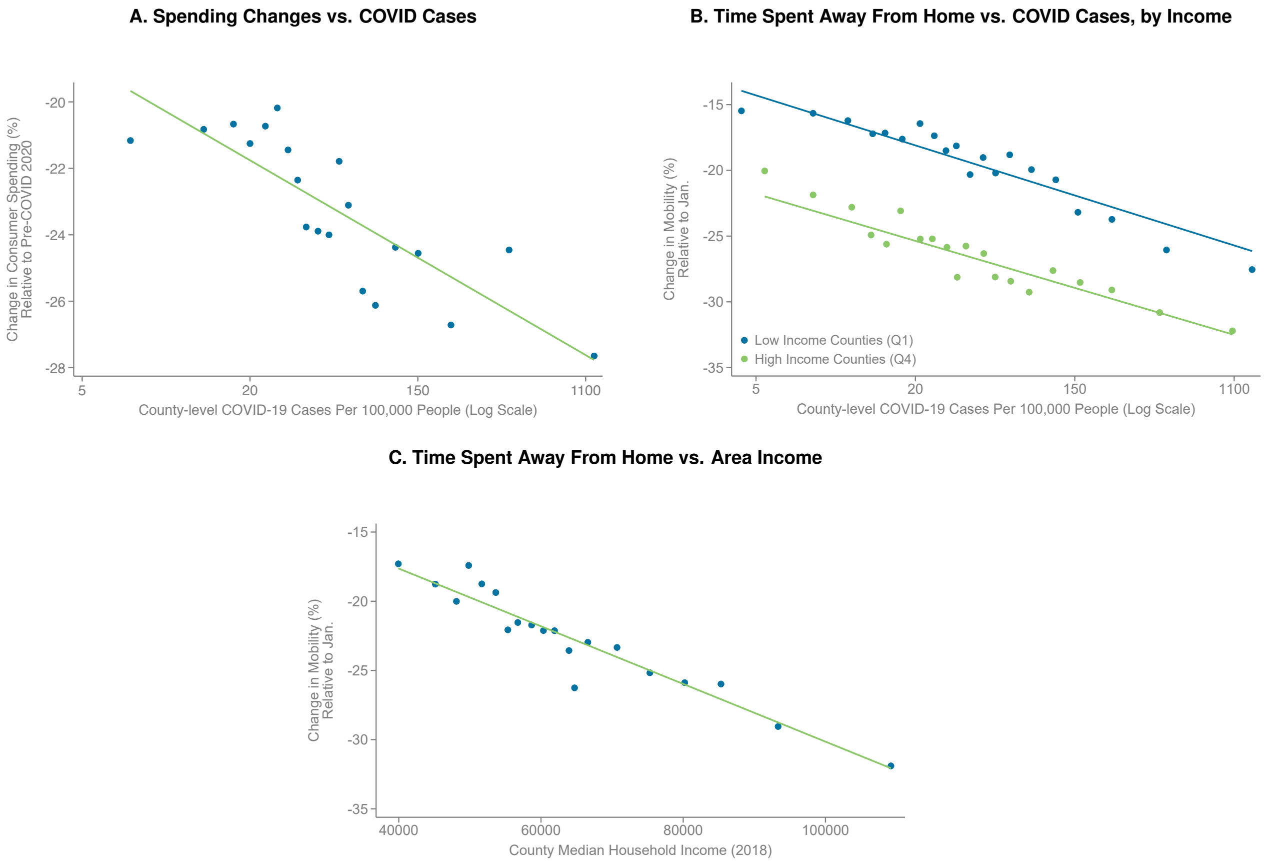
Notes: This figure relates official measurement sources of spending changes to measures of consumer spending from Affinity Solutions. Panel A summarizes NIPA data (Tables 1.1.2, 1.1.6 and 2.3.2) comparing Q4 2019 and Q1 2020. The first bar shows the seasonally adjusted change in real GDP in chained (2012) dollars (-\$247.3B). In parentheses under the first bar is the compound annual growth rate corresponding to this change in real GDP (-5.0%). Bars two through five show the contribution to the change in real GDP of its components. These contributions are estimated by multiplying the change in real GDP (-\$247.3B) by the contributions to the percent change in real GDP given in Table NIPA 1.1.2. The final bar shows the contribution of components of Personal Consumption Expenditures (PCE) that are likely to be captured in credit card spending (-\$138.2B). This includes all components of PCE except for motor vehicles and parts, housing and utilities, health care and the final consumption expenditures of nonprofit institutions serving households. This bar is computed by multiplying the change in PCE (-\$229.7B) by the contributions to the percent change in PCE given in NIPA Table 2.3.2 (excluding the aforementioned subcategories). Panels B and C report monthly spending from Affinity Solutions compared with that of the Monthly Retail Trade Survey (MRTS), a Census survey providing current estimates of sales at retail and food services stores across the United States. Panel B restricts to specifically retail trade sectors (NAICS code 44-45) excluding motor vehicles (NAICS code 441) and gas (NAICS code 447). Panel C restricts to food services (NAICS code 722) in the MRTS and food services (NAICS code 722) as well as accommodations (NAICS code 721) in Affinity Solutions. Both series are normalized relative to January 2020 spending (Jan 1 - Jan 31). Data source: Affinity Solutions

FIGURE 2: Changes in Consumer Spending by Sector



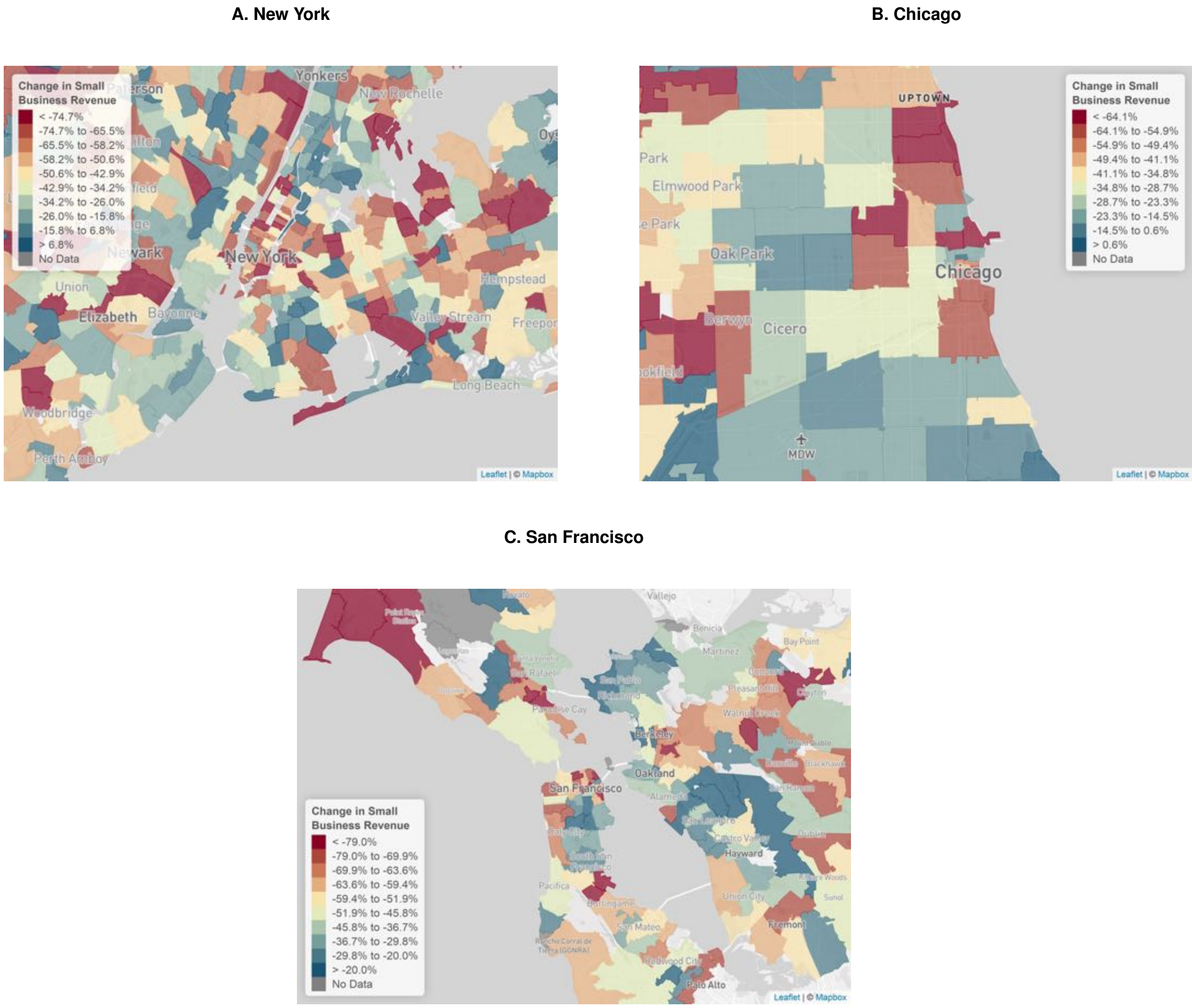
Notes: This figure disaggregates spending changes by income and sector. Panel A plots the 7-day moving average of consumer spending for the lowest and highest ZCTA median household income quartiles in 2020 and 2019. We scale the 2020 (2019) series by multiplying by the ratio of January 2020 total spending for components of PCE that are likely captured in credit card spending to the January 2020 (2019) total spending in the Affinity data. The ZCTA median household income quartiles are constructed using population-weighted 2014-2018 ACS median household income. We impute February 29, 2019 with the average of February 22, 2019 and March 7, 2019. Panel B disaggregates spending changes into Merchant Category Codes (MCCs). The first bar for Panel B shows the share of the decline in spending which can be attributed to the different sectors. The total decline is defined as ((Spending in March 25 through April 14 2020) - (Spending in March 26 through April 15 2019)) - ((Spending in January 8 through January 28 2020) - (Spending in January 8 - January 28 2019)). The second bar shows the share of spending in January 8-28 of 2020 for each sector. Merchant category codes (MCCs) which we were unable to identify are excluded from this figure. We define durable goods as the following MCC groups: motor vehicles, sporting goods and hobby, home improvement centers, consumer electronics, and telecommunications equipment. Non-durable goods include wholesale trade, agriculture, forestry and hunting, general merchandise, apparel and accessories, health and personal care stores, and grocery stores. Remote services include utilities, professional/scientific services, public administration, administration and waste services, information, construction, education, and finance and insurance. In-person services include real estate and leasing, recreation, health care services, transportation and warehousing services, and accommodation and food, as well as barber shops, spas, and assorted other services. Non-durables consist of 5.2% of the decline as show in the left-hand side bar and 23.0% of January spending. Excluding grocery stores from non-durable spending, non-durables constitute 11.6% of the decline and 10.5% of January spending. Panel C compares trends in consumer spending in the Affinity data for six categories of goods and services: at-home swimming pools; landscaping and horticultural services; restaurants and eating places; airlines; barbers and beauty shops; and pooled consumer spending across all categories. Panel D decomposes the change in personal consumption expenditures (PCE) for the COVID-19 shock and the Great Recession using NIPA data (Table 2.3.6U). PCE is defined here as the sum of services, durables and non-durables in seasonally adjusted, chained (2012) dollars. For COVID-19 (Great Recession) the peak is defined as January 2020 (December 2007) and the trough is April 2020 (June 2009). Data source: Affinity Solutions

FIGURE 3: Association Between COVID-19 Incidence, Spending, and Time Outside Home



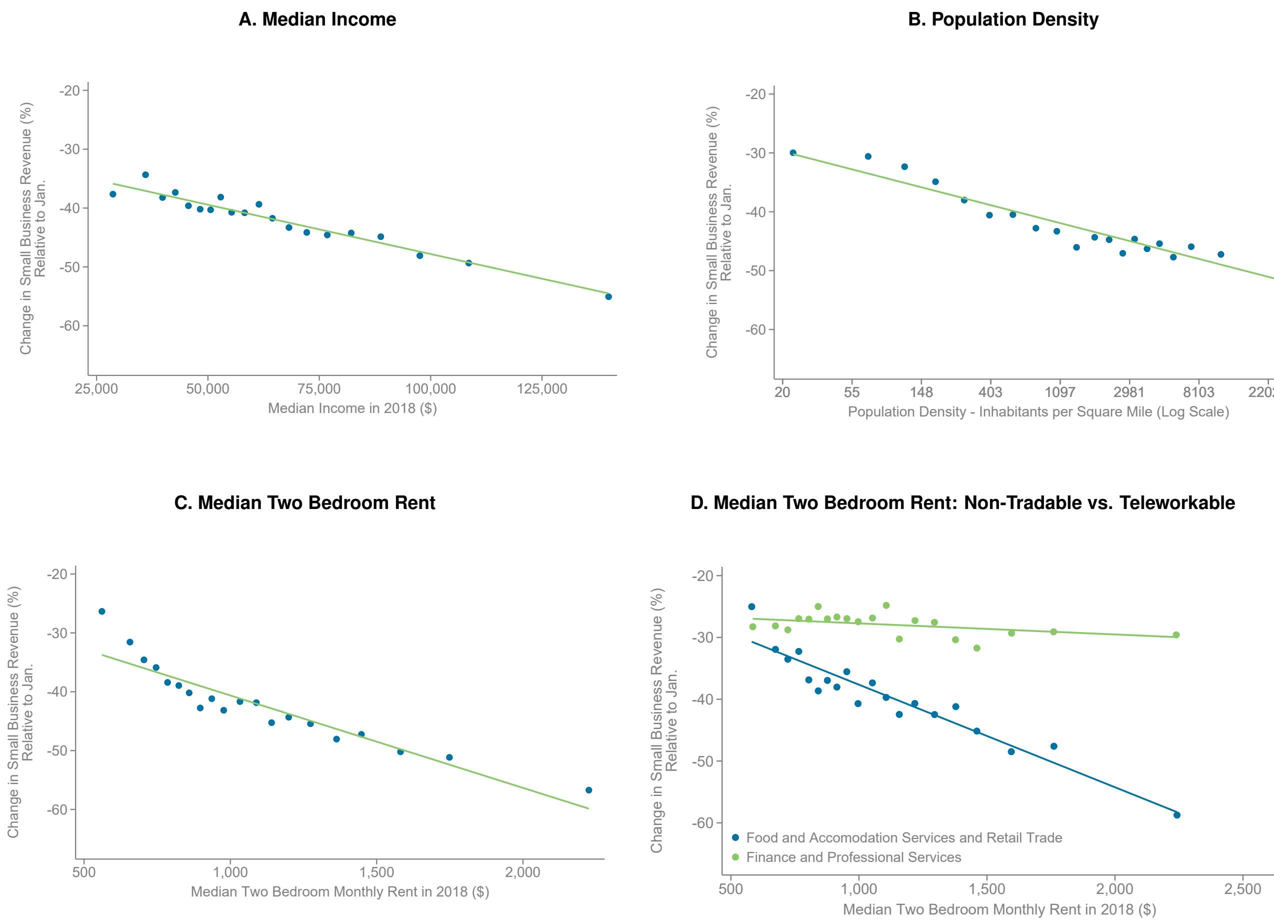
Notes: This figure plots three binned scatter plots showing the relationship between changes in spending or time spent away from home with median income and COVID case rates at the county level. To construct each binned scatter plot, we divide the x-axis variable into twenty equal-sized bins weighting by the county's population, and plot the (population-weighted) means of the y-axis and x-axis variables within each bin. Panel A presents a binned scatter plot of the change in average weekly consumer spending (using data from Affinity Solutions) in a county from the base period (January 8 - January 28) to the two-week period from April 1 - April 14 vs. the county's COVID case rate over the two week period from April 1 - April 14. Panel B presents a second binned scatter plot of the change in time spent outside the home in a county between January and the three-week period from March 25 - April 14 vs. the county's COVID case rate separately for low and high-income counties over the three week period from March 25 - April 14. Low-income and high-income counties have median household income in the bottom 25% and top 25% of all counties respectively, weighted by county population. Panel C presents a binned scatter plot of the change in time spent outside home in each county between January and the three-week period from March 25 - April 14 vs. the county's median household income as measured in the 2012-2016 ACS. Data sources: Affinity Solutions, Google Mobility

FIGURE 4: Changes in Small Business Revenues by ZIP Code



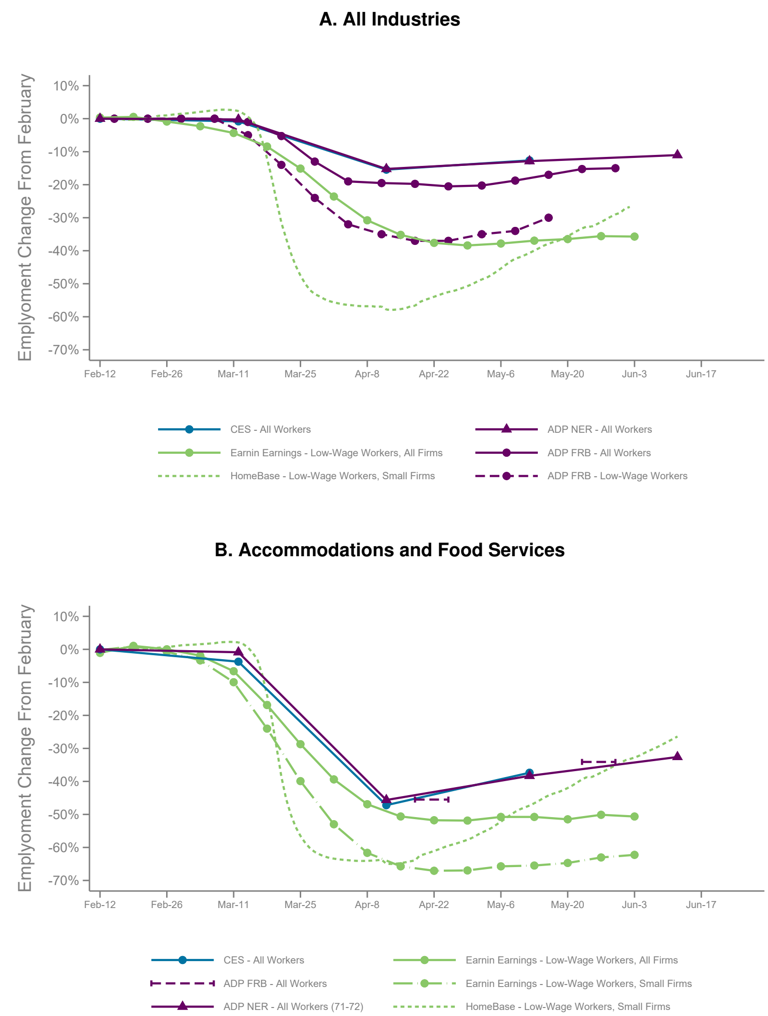
Notes: This figure shows ZCTA-level maps of the MSAs corresponding to New York City, San Francisco, and Chicago, colored by their respective deciles of normalized changes in small businesses revenue within each MSA using data from Womply. The change in revenue is defined as net revenue normalized by revenue in 2019 from March 22th 2020 to May 4th 2020 over the normalized net revenue from Jan 5th to March 7th 2020. Panel A is of the New York-Newark-Jersey City, NY-NJ-PA MSA. Panel B is of the San Francisco-Oakland-Hayward, CA MSA. Panel C is of the Chicago-Naperville-Elgin, IL-IN-WI MSA. For all panels, please note that although the entire MSA may not be shown in the view of the map, all of the ZCTA-level data within the MSA is being used to calculate the deciles in the legend. Additionally, each ZCTA can represent a different number of people, as ZCTAs are drawn according to ZIP codes, thus perceptions of smaller, denser ZCTAs do not necessarily indicate denser populations. Dark gray areas represent missing data, while lighter gray areas that are not covered by a ZCTA (as ZCTAs are based on ZIP codes and do not cover all of the nation's land area). Data source: Womply

FIGURE 5: Changes in Small Business Revenues vs. ZIP Code Characteristics



Notes: This figure plots three binned scatter plots showing the relationship between changes in small business revenue using data from Womply and different measures of economic activity at the ZCTA level. Binned scatter plots are constructed as indicated in Figure 3. The changes in business revenue are estimated by comparing the post-COVID period (March 22th 2020 to April 20nd 2020) against the base period (Jan 5th to March 7th 2020). We exclude from the sample ZCTA where the average total revenue in the base period was less than 1.000 USD and where the changes where larger than 200%. This does not affect results in any significant way. Panel A plots the declines in revenue against median household income at the ZCTA level taken from the 2014-2018 ACS. Panel B plots the declines in revenue against to the log number of inhabitants per square mile. Panel C plots the declines in revenue against median 2BR rent from the 2014-2018 ACS. Finally, Panel D replicates Panel C for two sectors of the economy: non-tradable business sectors, defined as Food and Accommodation (NAICS 72) and Retail Trade (NAICS 44 and 45), vs. sectors in which workers are more likely to be able to telework, defined as Finance and Professional Services (NAICS 52 and NAICS 54). Data source: Womply

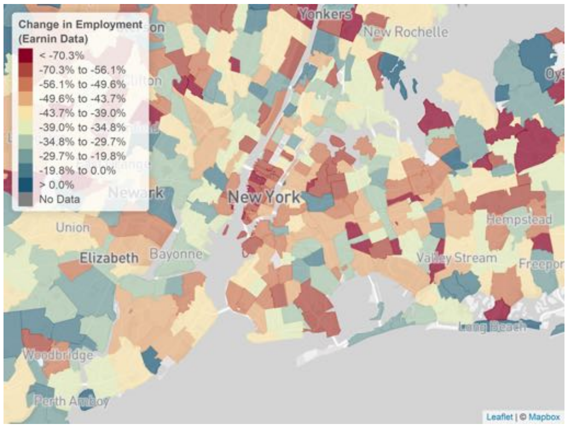
FIGURE 6: Changes in Employment Rates Over Time



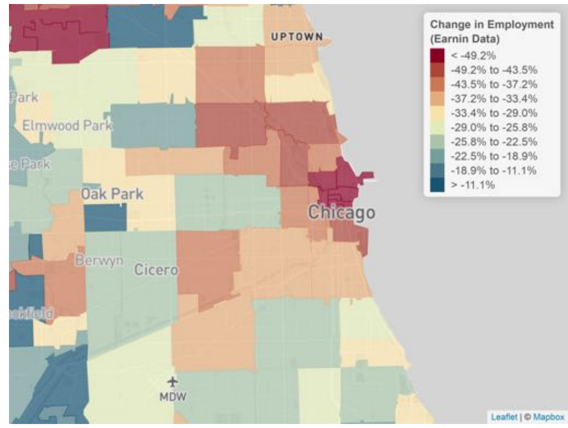
Notes: This figure compares employment changes relative to February 2020 within various datasets. In Panel A, we construct a daily employment series for Homebase for all industries by first summing the total number of employees in each day. We then construct an employment index by averaging employment over the prior seven days and then norming to the average value of the seven day moving average over the period, February 8 - February 29, 2020. In Earnin, we plot a weekly series of earnings by summing total earnings over each week and dividing by the average value for the three week period starting on February 13th. The Current Employment Statistics (CES) data are available monthly, so we plot changes in each month relative to February 2020 using the establishment-level data. The CES reports employment for the pay period including the 12th of each month, so we plot the monthly series on the 12th of the month. The ADP NER series is the ADP National Employment Report, put out from the ADP Research Institute and the solid ADP FRB series is the paid employment series from figure 2 of Cajner et al. 2020 (dated June 21, 2020). The dashed ADP FRB series is the decline in employment in ADP for the bottom quintile of workers taken from figure 5 of Cajner et al. 2020. Panel B replicates the Earnin, Homebase, and CES series from figure A but instead restricts to employment in the two-digit NAICS sector 72, Accommodations and Food Services. In addition, we plot a series for small NAICS 72 firms in the Earnin data, defining small as the third decile of Earnin employees, which corresponds to employers of mean size around 45 employees. The ADP NER series restricts to firms in NAICS 71 and 72. The ADP FRB series is the Accomodations and Food Services Series taken from table 1 of Cajner et al. 2020. Data sources: Earnin, HomeBase

FIGURE 7: Changes in Employment Rates by ZIP Code

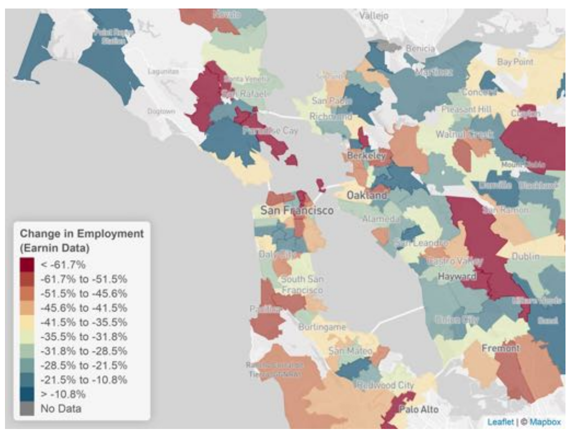
A. New York



B. Chicago

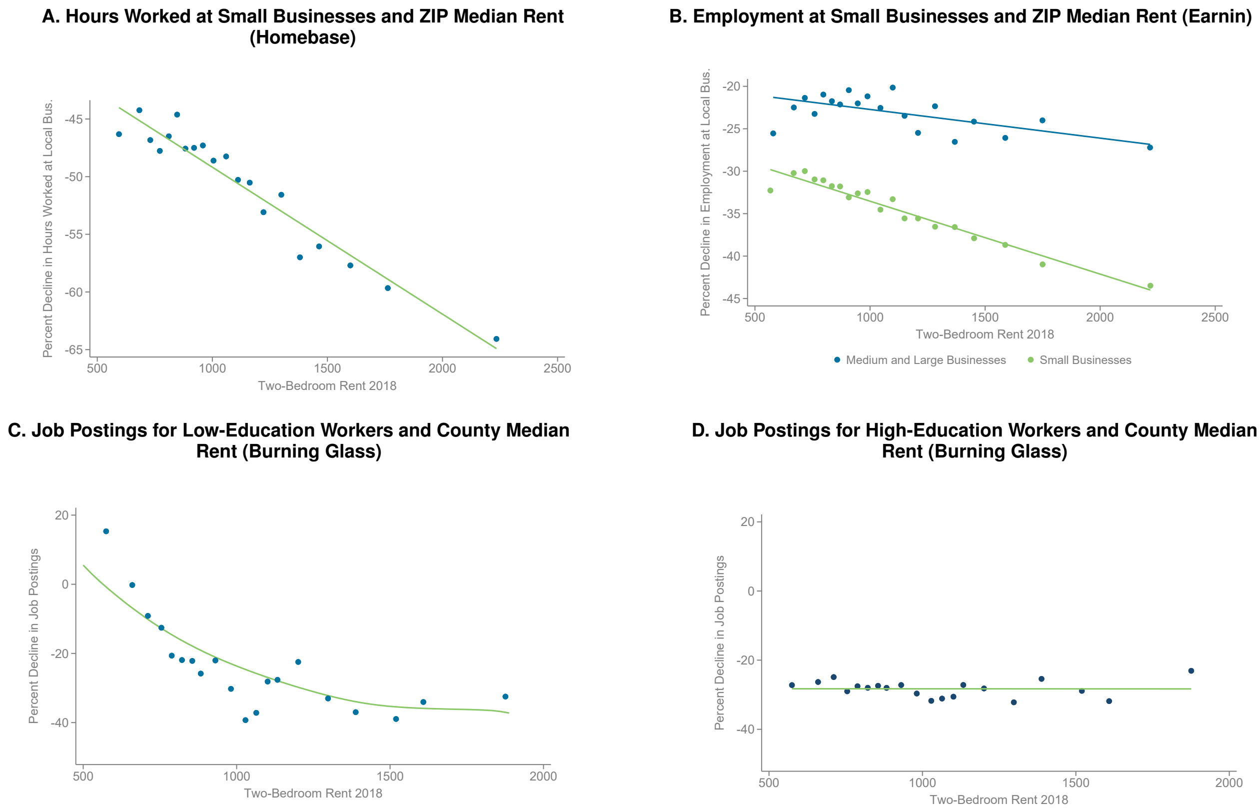


C. San Francisco



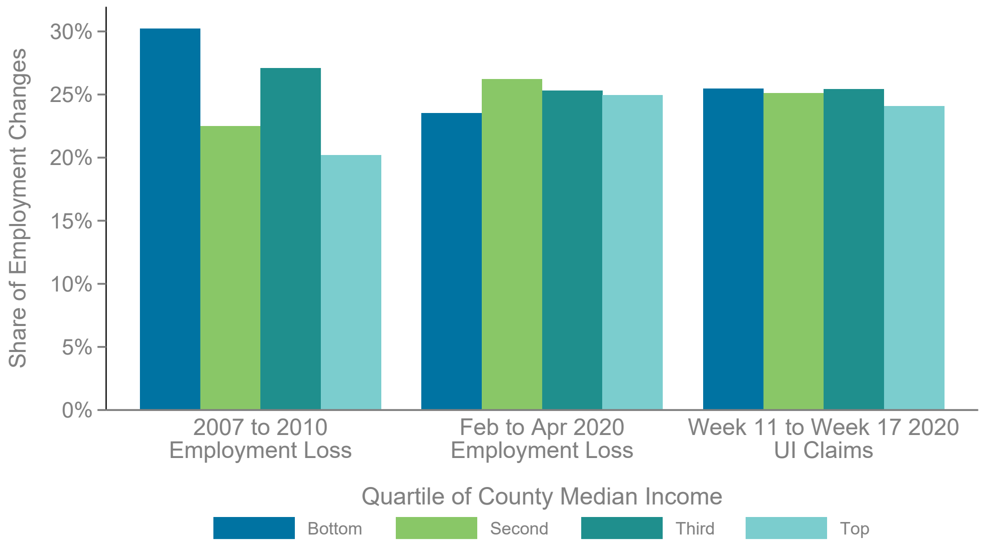
Notes: This figure replicates Figure 4 using changes in employment at small businesses based on data from Earnin. The change in employment is defined as the average decrease inemployment at the ZCTA level from the period of January 8th to March 10th, 2020 to the period of April 8th to April 28th, 2020. Data sources: Earnin, HomeBase

FIGURE 8: Changes in Employment and Job Postings vs. Rent



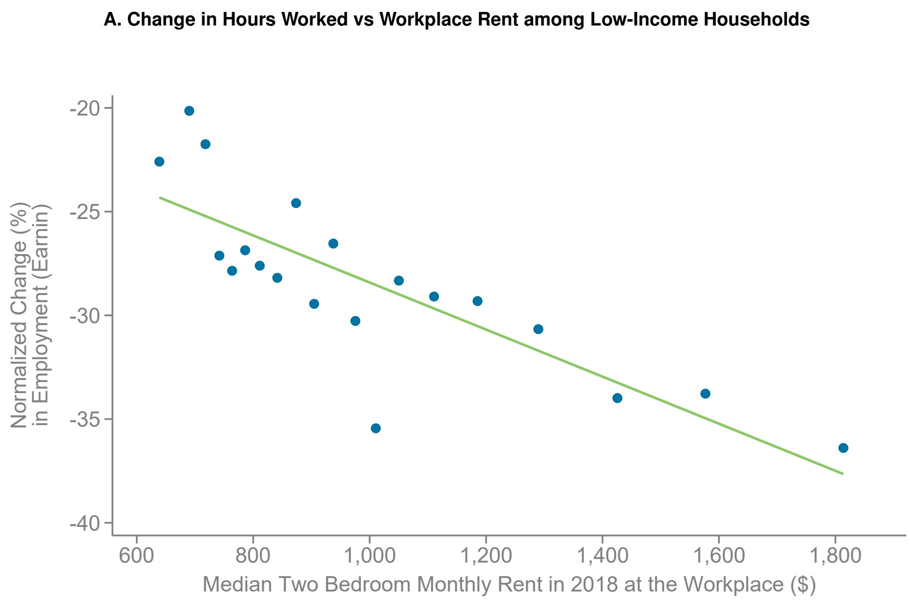
Notes: This figure shows binned scatterplots of the relationship between median rent and both employment and job postings. Binned scatter plots are constructed as indicated in Figure 3 by binning areas based on their median rent into 20 equally sized bins and computing the mean change in the outcome variable within each bin. Panels A presents the binned scatter plots of the relationship between the average change in hours worked at businesses in the Homebase data between January and April and median 2 bedroom rent at the ZCTA level using data from the. Panel B presents a similar binned scatter plot showing the relationship between employment changes in the Earnin data and median 2 bedroom rent at the ZCTA level. Both panels measure the percentage change from January 8-28th, 2020 to April 8-28th, 2020. The change in hours worked in Panel A is constructed using Data from Homebase, which is comprised of small businesses. The change in hours worked in Panel B is constructed using data from Earnin, and is shown separately for businesses above vs. below the 8th decile of firm size in the Earnin data. Panel C presents a binned scatterplot of the relationship between the percentage change in job postings for workers with minimal or some education and median 2 bedroom rent (from the 2014-2018 ACS) at the county level. Panel D presents a binned scatterplot of the relationship between percentage change in job postings for workers with moderate, considerable or extensive education and median 2 bedroom rent, with a lowess fit. Data sources: Burning Glass, Earnin, Homebase

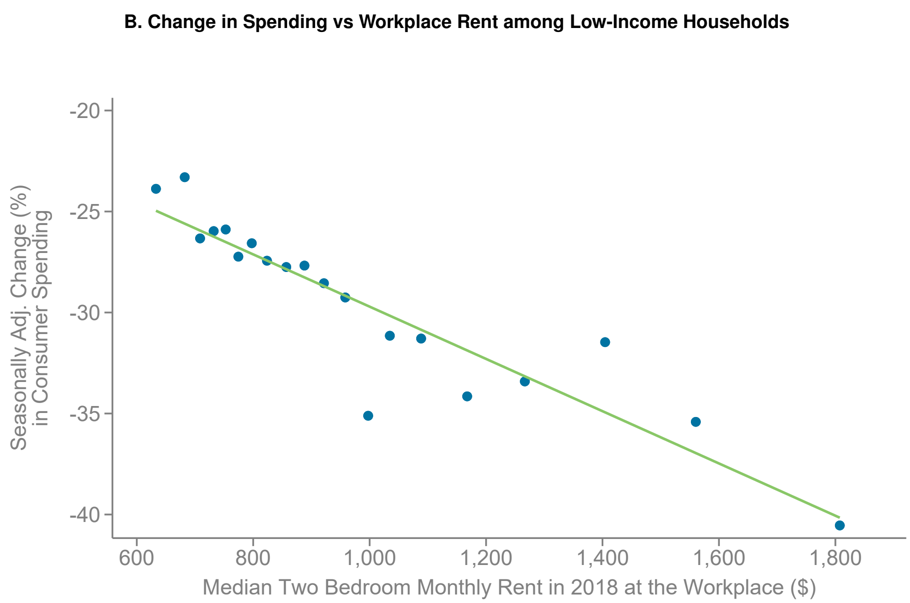
FIGURE 9: Geography of Unemployment in the Great Recession vs. COVID Recession



Notes: This figure displays the share of job losses occurring in counties with differing median incomes, for both the Great Recession and the COVID recession. To construct the first set of four bars, we first calculate national employment loss between 2007 and 2010 using data from the BLS. We then group counties by median income, and compute the share of employment loss that occurred in counties in each quartile of the distribution of county median income. The second set of bars replicates the first set of bars using total job losses that occurred between February 2020 and April 2020. The third set of bars reports the allocation of county-level UI claims summed between March 15 and May 2 across counties in different income quartiles. In the first set of bars, county median income is calculated using the 2006 ACS; in the second and third sets of bars, county median income is calculated using the 2014-2018 ACS.

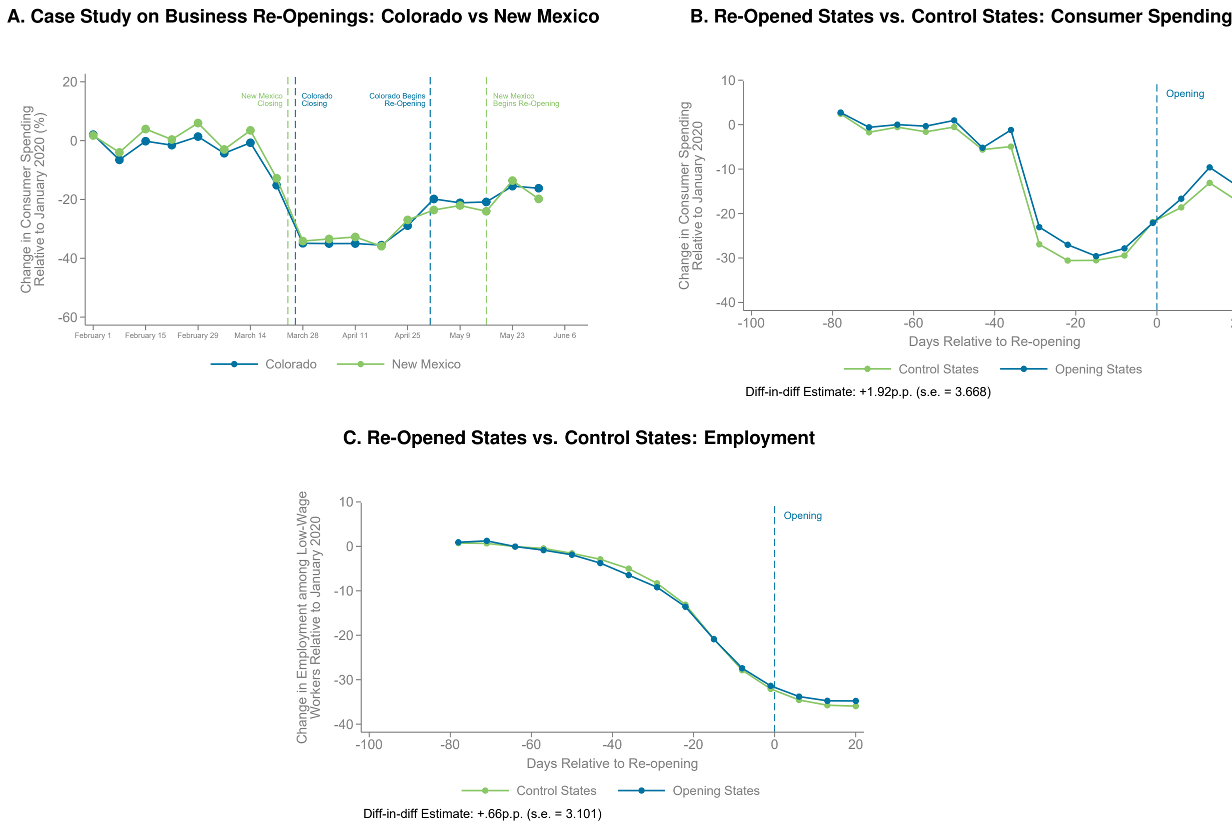
FIGURE 10: Changes in Consumer Spending vs. Workplace Rent for Low-Income Households





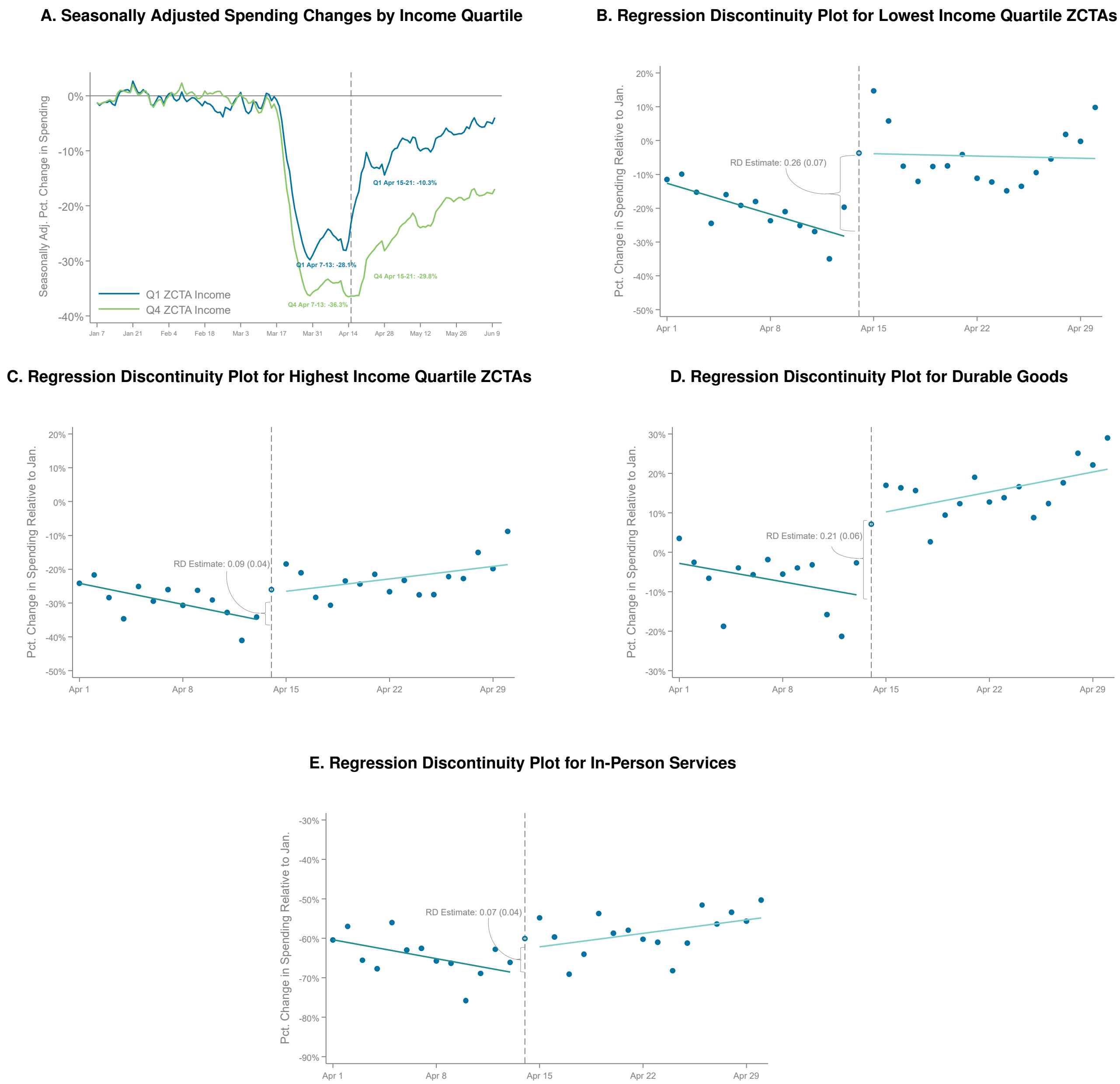
Notes: This figure plots changes in hours worked (Panel A) or in consumer spending (Panel B) by ZCTA vs. the average median 2 bedroom rent in the workplace ZIPs of individuals who live in a given ZCTA, restricting to ZCTAs in the bottom quartile of the household income distribution. We construct the average median 2 bedroom rent variable by combining data on the matrix of home residence by workplace ZCTAs taken from Census' LEHD Origin-Destination Employment Statistics (LODES) with data on median rents from the 2014-2018 ACS. In particular, we assign median rents from the ACS to each ZCTA of workplace in the LODES data and then collapse workplace rents to each home ZCTA, weighting by the number of jobs in each workplace ZCTA. In Panel A, the change in employment variable is based on data from Earnin. The change is computed from Jan 5th to March 7th 2020 to the period of April 8th 2020 - April 28th 2020. In Panel B, the spending change variable is based on data from Affinity Solutions on total card spending, and the change is computed from the period of Jan 5th to March 7th 2020 to the period of March 22th 2020 - April 20nd 2020. Data sources: Affinity Solutions, Earnin.

FIGURE 11: Causal Effects of Re-Openings on Economic Activity: Event Studies



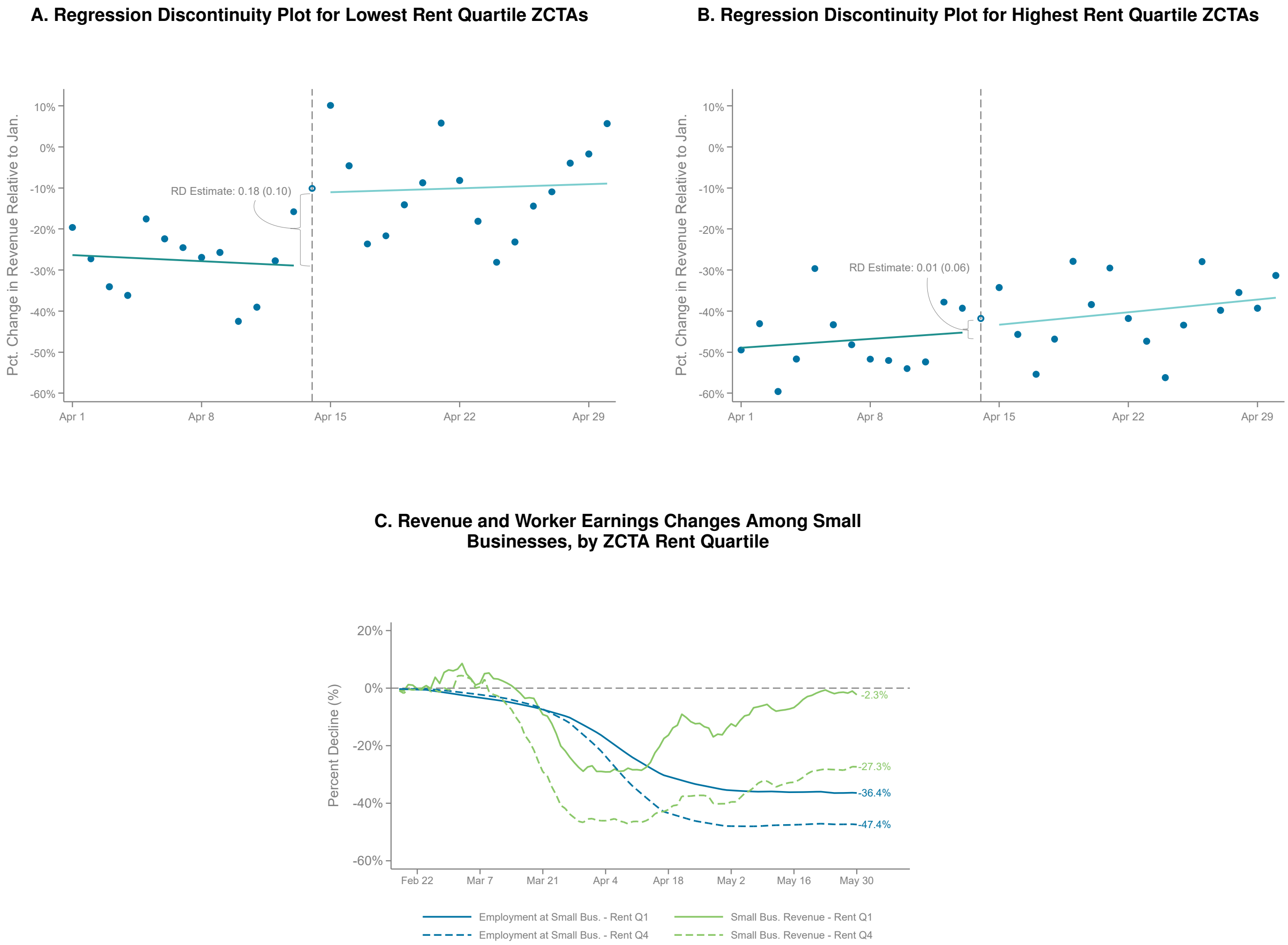
Notes: Panels A and B show seasonally-adjusted percent change in consumer spending in the Affinity Solutions data (see Section 2.1 for more details about the seasonal adjustment). Panel A shows the series for both New Mexico and Colorado; Colorado partially reopened non-essential businesses on May 1, while New Mexico did not do so until May 16. Panel B presents an event study of states that partially reopened non-essential businesses between April 20th and May 4th, compared to a matched control group. We construct the control group separately for states on each opening day and then stack the resulting event studies to align the events. Panel C replicates Panel B but instead plotting the percent change in employment of workers using Earnin data. In Panels B-C, we provide the coefficient from a difference-in-difference comparing treated vs. untreated states in the two weeks following and the two weeks prior to the partial re-opening. Data sources: Affinity Solutions, Earnin

FIGURE 12: Impact of Stimulus Payments on Consumer Spending



Notes: This figure studies the effect of the stimulus payments on spending in the Affinity Solutions data. Panel A plots the percent change in seasonally-adjusted consumer spending for both the lowest and highest population-weighted ZCTA median household income quartiles. We use the ZCTA population and median household income estimates in the 2014-2018 ACS. For panels B-D, each point is the national level of spending on that day divided by the average level of spending in January. The points are residualised by day of week and first of the month fixed effects. We estimate the fixed effects using data from January 1, 2019, to May 10, 2019. The hollow-point and dashed line correspond to April 14th, which is excluded from the regression. Panel B restricts to ZCTAs in the lowest income quartile. Panel C restricts to ZCTAs in the highest income quartile. Panel D restricts to spending on durable goods as defined in the notes for Figure 2. Panel E restricts to spending on in-person services as defined in the notes for Figure 2. Data source: Affinity Solutions

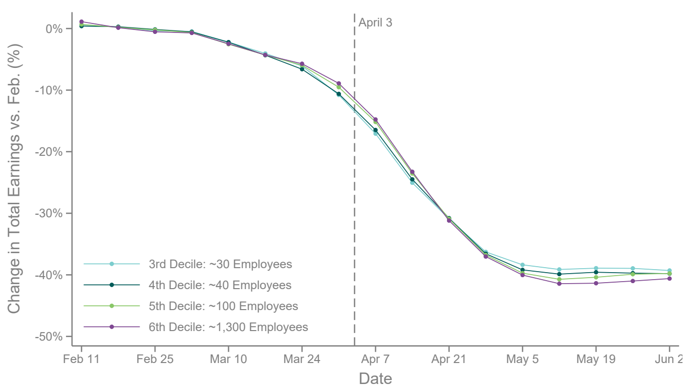
FIGURE 13: Impact of Stimulus Payments on Business Revenue and Employment



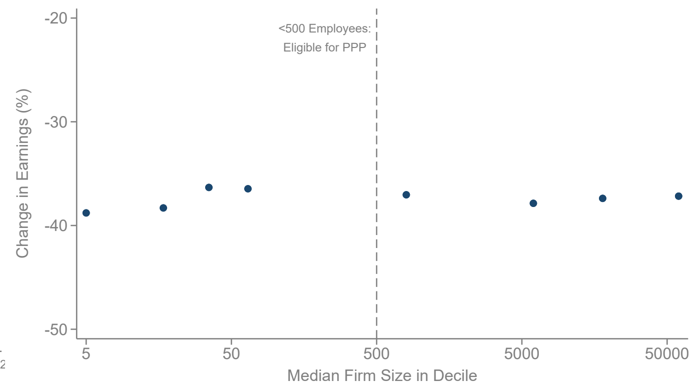
Notes: Panels A and B of this figure study the effect of the stimulus payments on small business revenue in the Womply data. In these panels, each point is the level of spending (in that ZCTA median 2-bedroom rent quartile) on that day divided by the average level of spending in January. The points are residualised by day of week and first of the month fixed effects. We estimate the fixed effects using data from January 1, 2019, to May 10, 2019. The hollow-point and dashed line correspond to April 14th, which is excluded from the regression. Panel C plots the percent change in the seven-day moving average of small-business revenue using the Womply data and change in employment among Earnin users by ZCTA rent-quartile and restricts to small businesses in the Earnin sample, as defined by being in the bottom seven deciles of employer size. The revenue series is seasonally-adjusted and the employment change series is relative to January 2020. Data sources: Earnin, Womply

FIGURE 14: Impact of Paycheck Protection Program on Worker Earnings

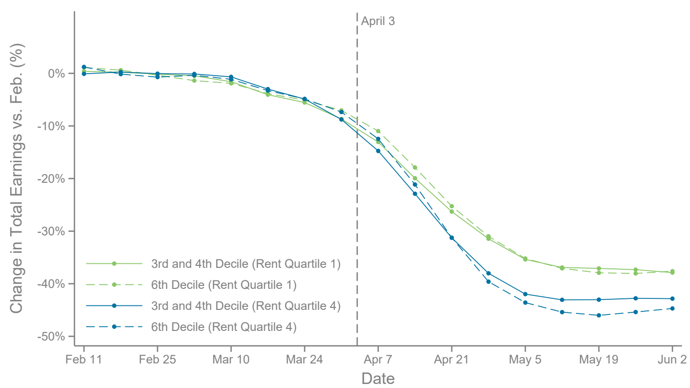
A. Change in Total Earnings by Decile of Firm Size, All Industries Excl. NAICS 72



B. Change in Total Earnings vs Decile of Firm Size, All Industries Excl. NAICS 72

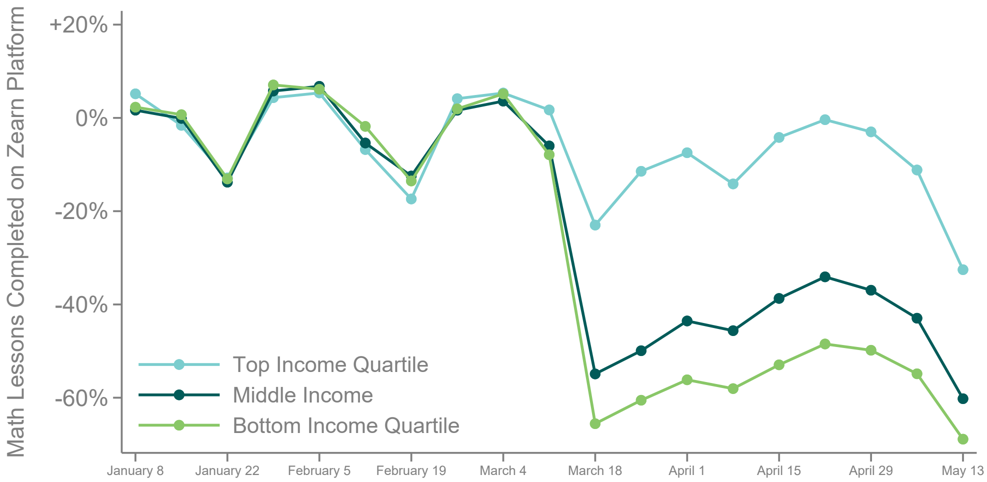


C. Change in Total Earnings by Firm Size and Employer ZCTA Rent Quartile



Notes: Panels A-C show the change in total earnings in a repeated cross-section of Earnin users, by decile of employer size. Each panel excludes workers in the Accommodation and Food Services sector (NAICS 72). The percent change for each week is computed with respect to the average earnings between January 29th and February 25th. We estimate the size of firm deciles 3-8 by matching Earnin employer names and locations to employer names and locations in ReferenceUSA data. We estimate the size of firm deciles 1-2 by rescaling the number of Earnin users to total number of employees to match the national distribution of firm sizes using data from the Statistics of U.S. Business (SUSB). The grey dashed line corresponds to April 3, 2020, the first day for enrollment in the Paycheck Protection Program (PPP). Panels A and BC are both reweighted so that industry composition is constant across firm size deciles. The change in earnings is first calculated within each two-digit NAICS code, and then reweighted so that the composition of industries within each decile of firm size matches the composition of industries within all deciles plotted. Panel B plots the average percent change in earnings between April 8th and May 5th against the median firm size in each decile. As NAICS code is not observed for firms in deciles 1-2 of Earnin data, the change in earnings for deciles 1-2 reflects the change in earnings in all industries pooled, whereas the change in earnings for deciles 3-8 reflects the change in earnings in all industries other than Accommodation and Food Services. Panel C restricts to firms that are eligible for the PPP (the 3rd and 4th deciles of employer size) and those that are ineligible (the 6th decile of employer size) for the PPP, separately by rent quartile of work ZCTA. The population-weighted ZCTA rent income quartiles were constructed using 2014-2018 ACS estimates of population and median-household income. Data source: Earnin

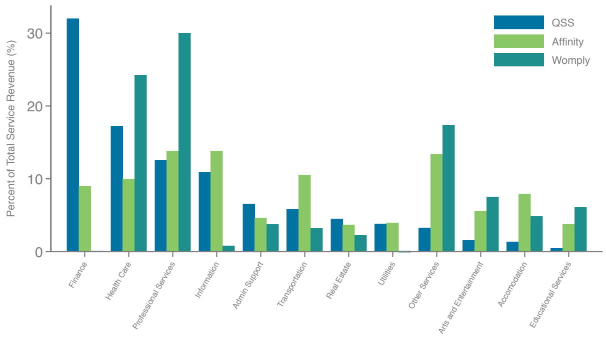
FIGURE 15: Effects of COVID on Educational Progress by Income Group



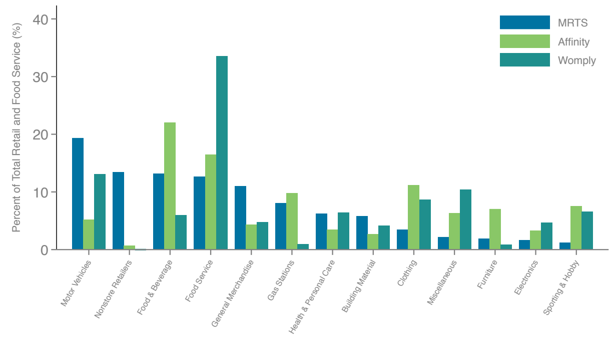
Notes: We construct this series using data from Zearn Inc. at the class-week level, which we aggregate to the national-week-income level according to the median household income of the Zip codes of Zearn schools (weighting by the average number of students using the platform at each school during the base period). The key outcome is student progress, defined as the number of accomplishment badges earned in Zearn in each week, relative to the base period of January 6th-February 7th. Our sample includes all classes with more than 10 students using Zearn during the base period, excluding those with fewer than five users in all weeks. We index student progress to pre-COVID student progress by dividing weekly progress at the school level by average weekly progress during the base period and then subtracting 1 to center the data around 0% change. Data source: Zearn Inc.

APPENDIX FIGURE 1: Industry Shares of Consumer Spending and Business Revenues Across Datasets

A. Compared to QSS

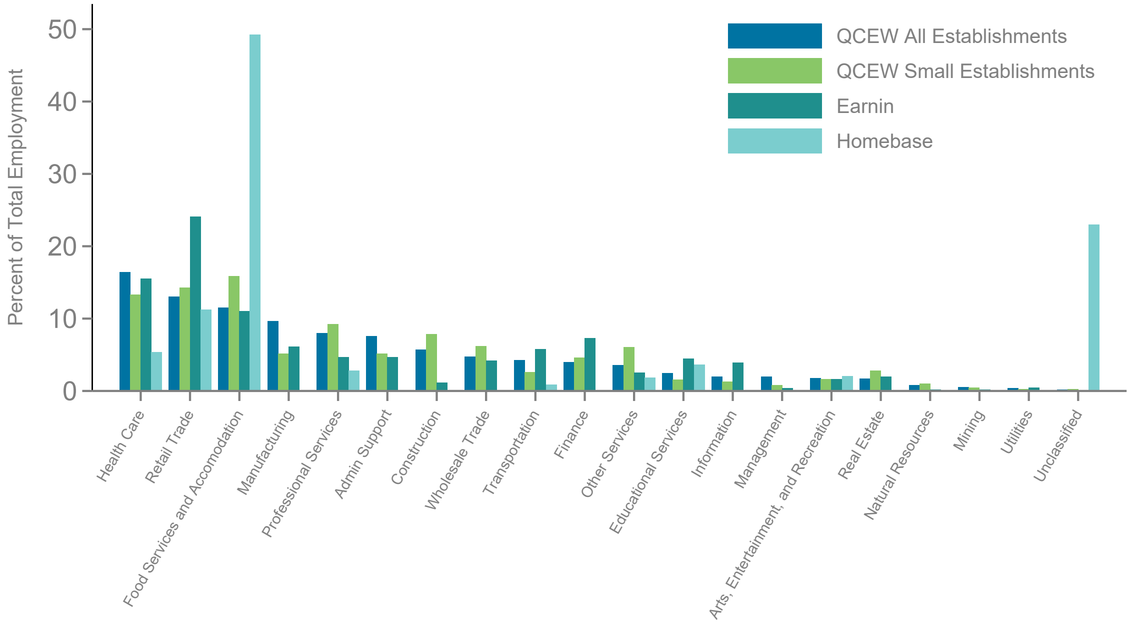


B. Compared to MRTS



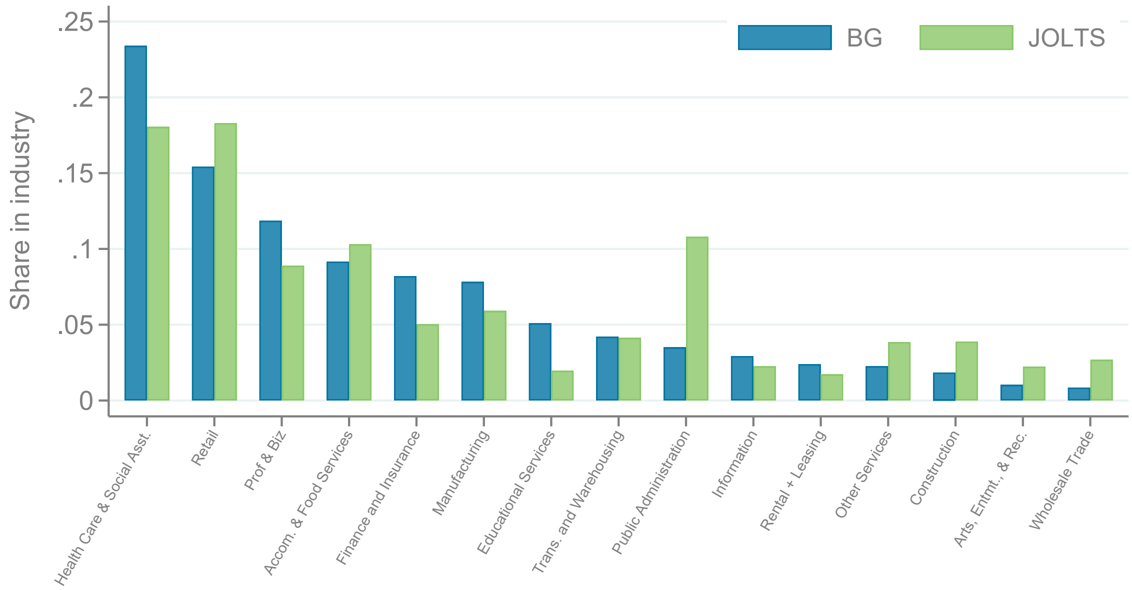
Notes: Panel A shows the NAICS two-digit industry mix for two private business credit card transaction datasets compared with the Quarterly Services Survey (QSS), a survey dataset providing timely estimates of revenue and expenses for selected service industries. Subsetting to the industries in the QSS, each bar represents the share of revenue in the specified sector during Q1 2020. We construct spending and revenue shares for the private datasets, Affinity and Womply, by aggregating firm revenue (from card transactions) in January through March of 2020. Panel B shows the NAICS three-digit industry mix for the same two private datasets compared with the Monthly Retail Trade Survey (MRTS), another survey dataset which provides current estimates of sales at retail and food services stores across the United States. Subsetting to the industries in the MRTS, each bar represents the share of revenue in the specified sector during January 2020. We construct revenue shares for the private datasets, Affinity and Womply, by aggregating firm revenue (from card transactions) in January 2020. Data sources: Affinity Solutions, Womply

APPENDIX FIGURE 2: Industry Shares of Employment Across Datasets



Notes: This figure shows the NAICS two-digit industry mix for two private employment-based datasets compared with the Quarterly Census of Employment and Wages (QCEW), an administrative dataset covering the near-universe of firms in the United States. Each bar represents the share of employees in the given dataset who work in the specified sector. We construct data for all establishments and small establishments using employment data from the Q1 2019 QCEW. Small establishments are defined as having fewer than 50 employees. We construct employment shares for the private datasets, Earnin and Homebase, using January 2020 employment. We define employment in Earnin as the total number of worker-days in the month. We define employment in Homebase as the number of unique individuals working a positive number of hours in the month. Data sources: Earnin, HomeBase

APPENDIX FIGURE 3: Industry Shares of Job Postings in Burning Glass and Job Openings in JOLTS

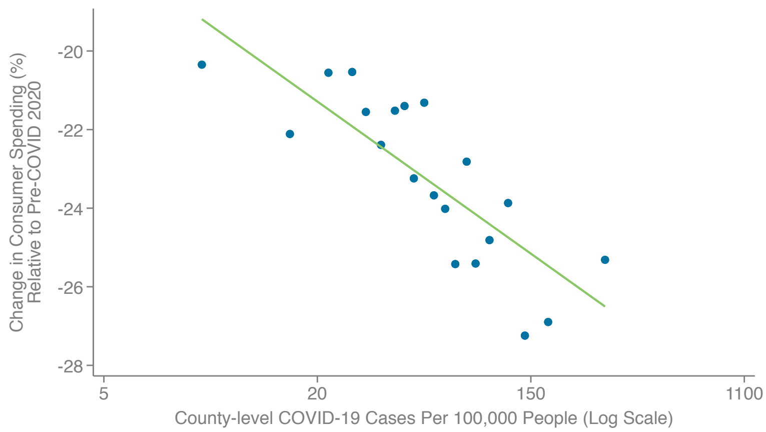


Notes: This Figure displays the NAICS two-digit industry mix of job postings in Burning Glass and job openings in JOLTS, the Job Openings and Labor Turnover Survey data provided by the U.S. Bureau of Labor Statistics, in January 2020. Data source: Burning Glass

APPENDIX FIGURE 4: Spending Changes by Sector and Income Quartile

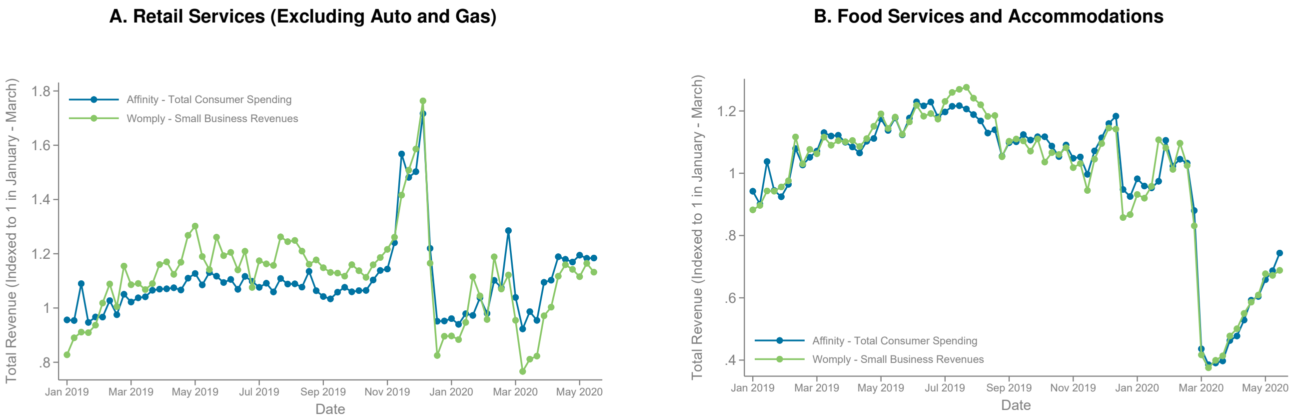
Notes: This figure displays the change in spending by sector for the four quartiles of ZCTA median household income (constructed using 2014-2018 ACS population and income estimates). These sectors were constructed by grouping together similar merchant category codes, not all merchant category codes were used in this plot. The change in spending displayed is (the log difference-in-difference of spending -1)*100, where the pre-period used is January 8th-28th and the post-period is March 25th-April 14th. Data source: Affinity Solutions

APPENDIX FIGURE 5: Spending Changes vs COVID Cases, by County



Notes: To construct this figure, we divide the log COVID cases into 20 bins, each of which contain 5% of the population, and plot the mean value of the log of COVID cases and change of spending variables within each bin, controlling for state fixed effects and median-household income. COVID cases and decline in spending are both measured during the two week period of April 1st to April14th, and is benchmarked to the pre-period of January 8th to January 28th. Data source: Affinity Solutions

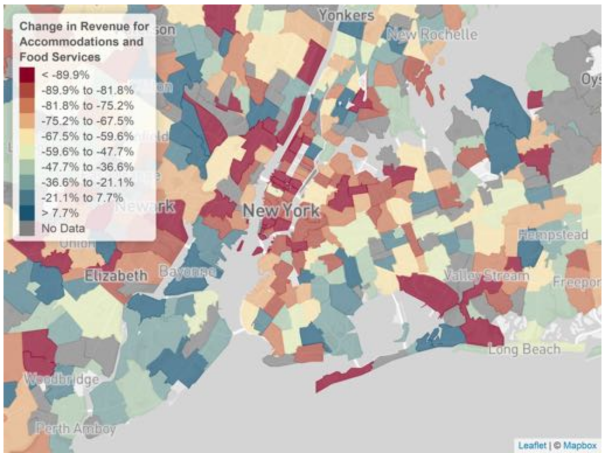
APPENDIX FIGURE 6: Small Business Revenue Changes vs. Local Income Distribution



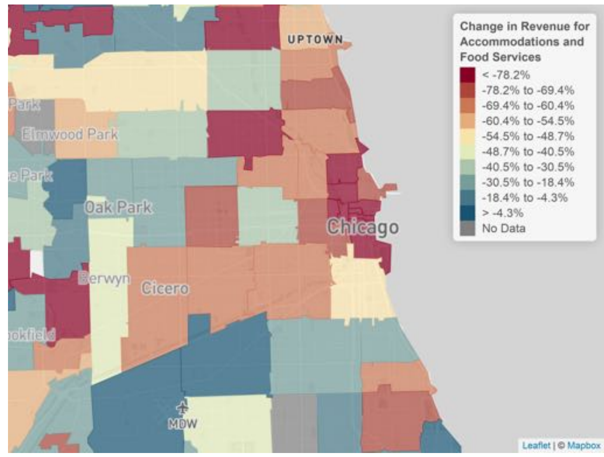
Notes: This figure compares weekly total consumer spending (from Affinity Solutions purchase data) and small business revenue (from Womply) normalized to the average pre-COVID levels of each year. The pre-COVID period is defined as January 8 - March 10 and we normalize within each calendar year to account for year fixed effects. Following the sectors defined in the Monthly Retail Trade Survey (MRTS), Panel A restricts to specifically retail trade sectors (NAICS code 44-45) excluding motor vehicles (NAICS code 441) and gas (NAICS code 447), and Panel B restricts specifically to food services and accommodations (NAICS code 72). Data sources: Affinity Solutions, Womply

APPENDIX FIGURE 7: Changes in Small Business Revenues by ZIP Code for Food and Accommodation Service Businesses

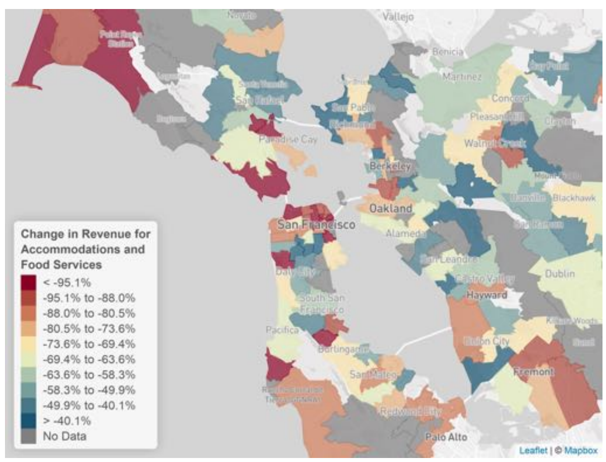
A. New York City



B. Chicago

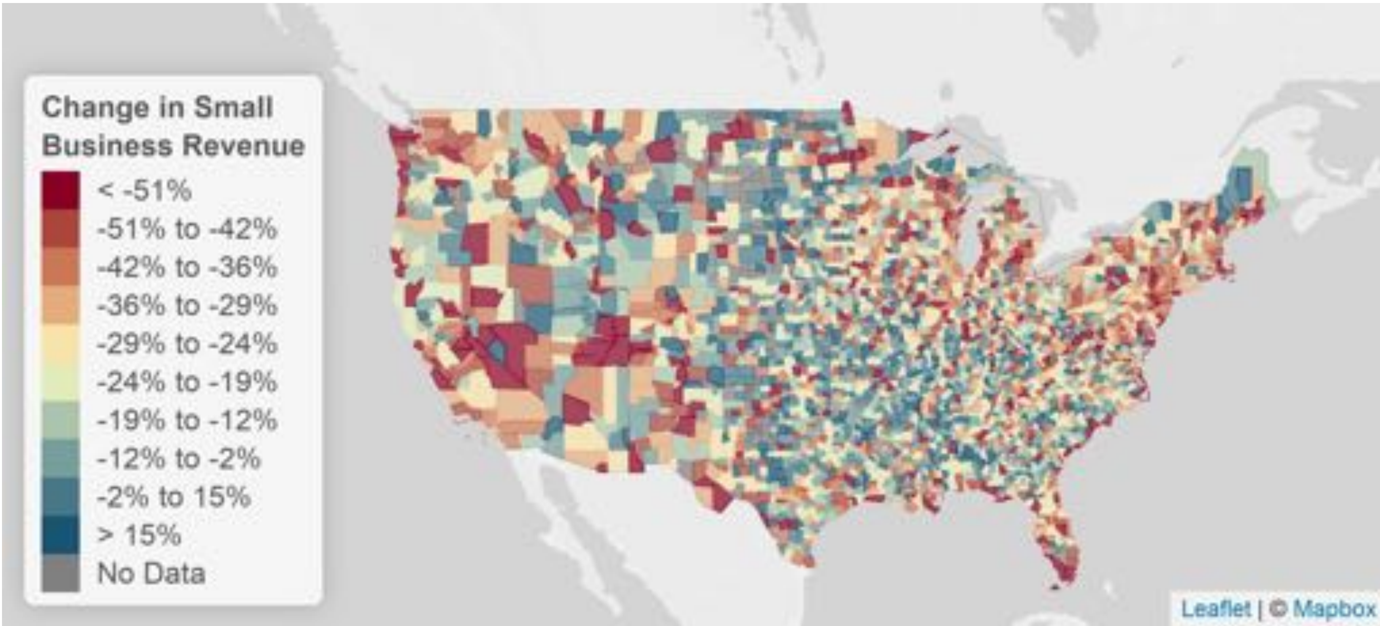


C. San Francisco



Notes: This Figure displays ZCTA-level maps of the MSAs corresponding to New York City, San Francisco, and Chicago, colored by their respective deciles of Womply change in revenue for small businesses classified as NAICS 72 within each MSA. This figure corresponds to the process described in the notes for Figure 4. Data source: Womply

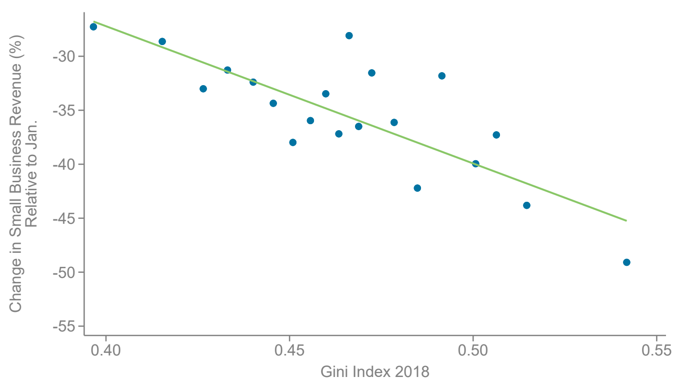
APPENDIX FIGURE 8: Changes in Small Business Revenues by County



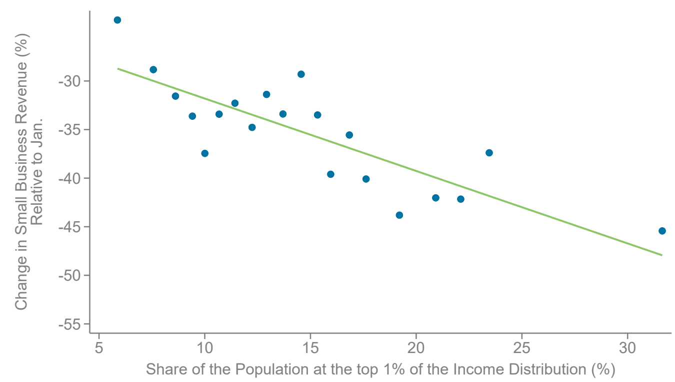
Notes: This figure replicates Figure 4 but for the entire United States instead of a single city and its surrounding area and graphing counties instead of ZCTAs. See notes to Figure 4 for details. Data source: Womply

APPENDIX FIGURE 9: Womply Business Revenue vs. Poverty Share, Top 1% Share, and Gini by County

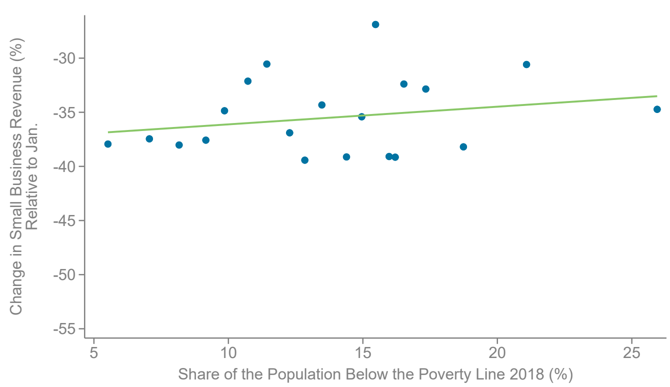
A. Gini Index



B. Share of Population in Top 1% of Income Distribution



C. Share of Population below Poverty Line

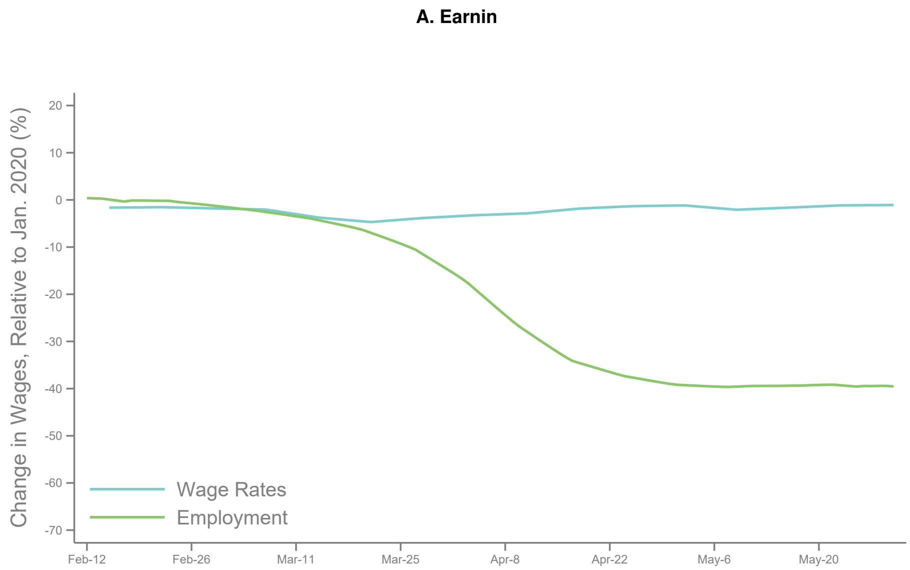


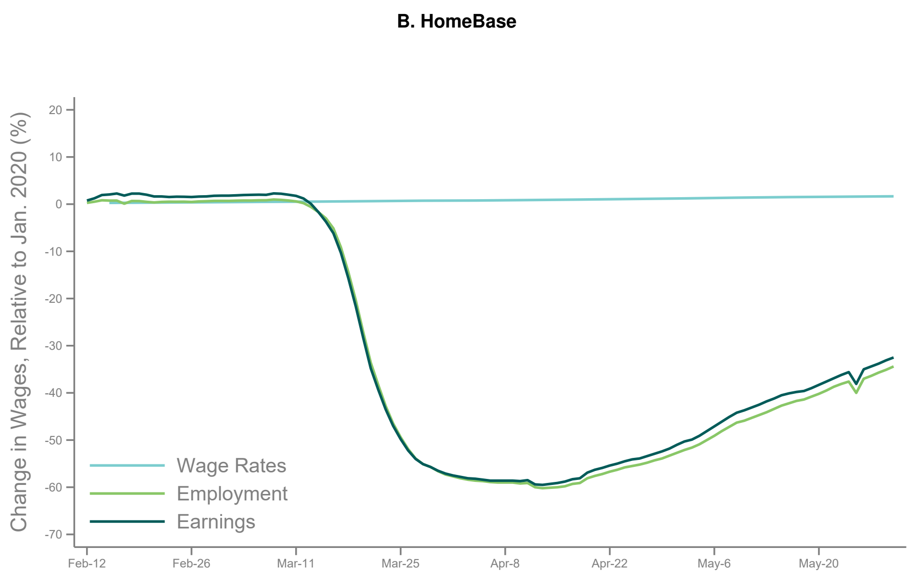
Notes: This Figure replicates Figure 5 but compares the declines with different measures of inequality. Panel A compares the within county Gini index against the declines. Panel B uses the share of the county with incomes at the top 1% of the income distribution. Panel C compares the declines with the share of the county population with incomes below the poverty line in the 2010 decennial census. See notes to Figure 5 for details. Data source: Womply

APPENDIX FIGURE 10: Womply Business Closures vs. Rent by ZIP

Notes: This figure replicates Panel C of Figure 5 but shows average changes in small businesses that remain open instead of changes in revenue. See notes to Figure 5 for details. Data source: Womply

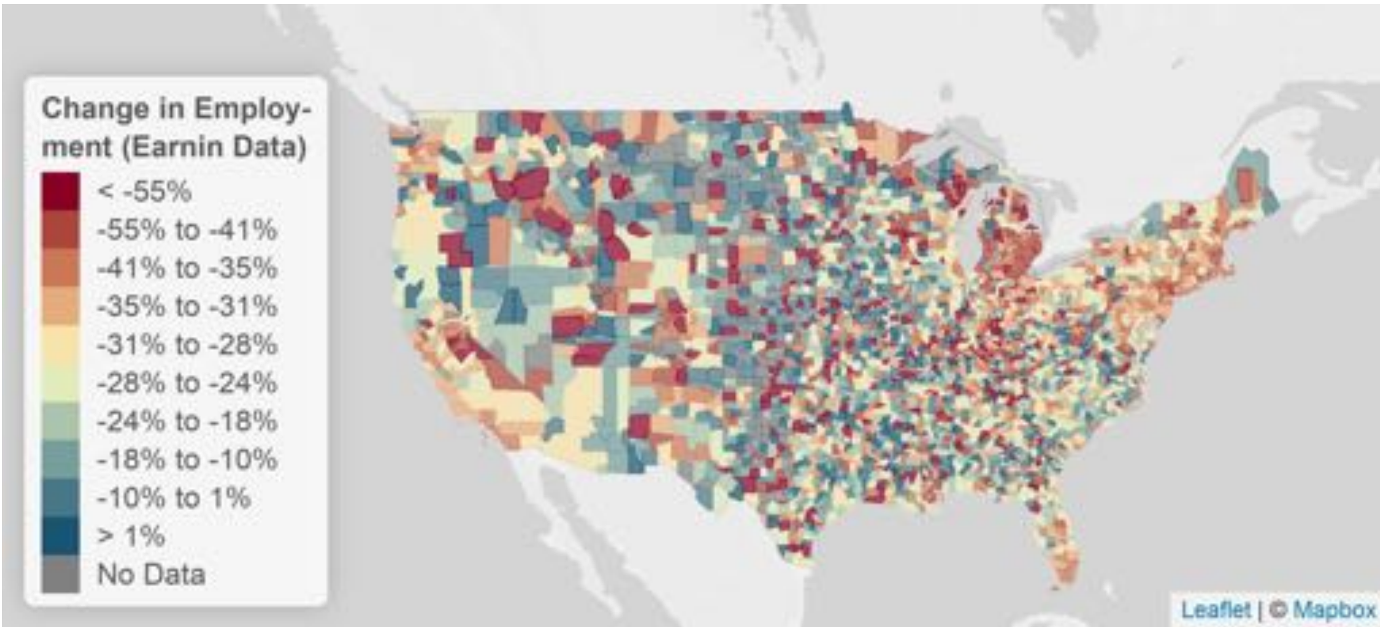
APPENDIX FIGURE 11: Changes in Wages, Hours Worked and Earnings Over Time





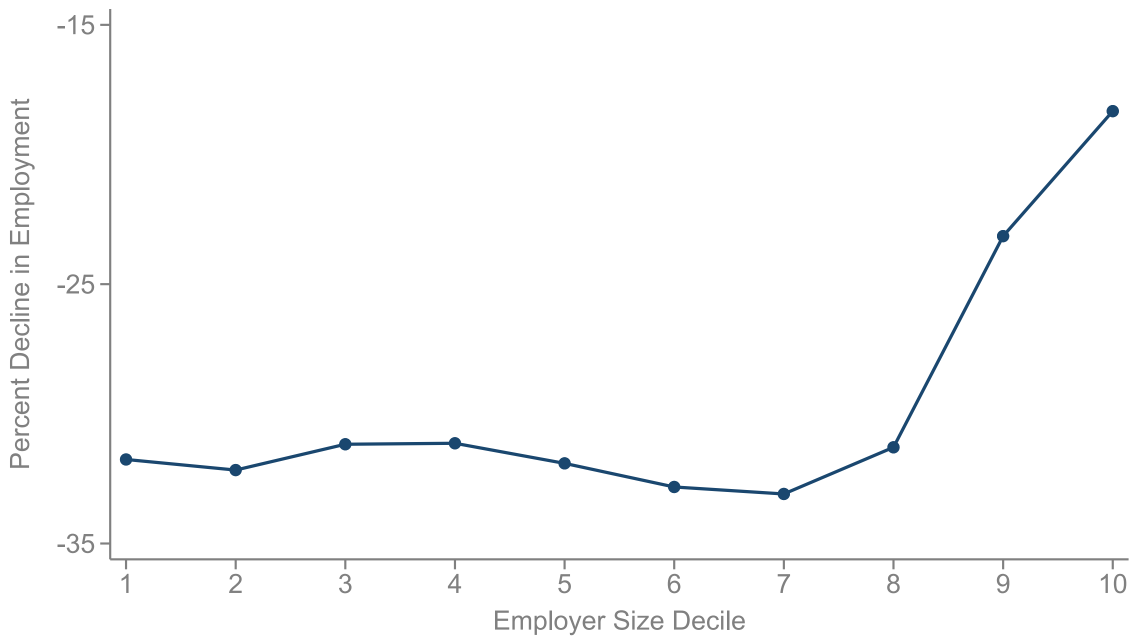
Notes: This figure compares changes in mean wages and employment relative to January 2020 within the Earnin (Panel A) and HomeBase (Panel B) datasets.We construct daily wages for both Earnin and HomeBase by calculating the mean wage on each day. In the HomeBase dataset, we condition on workers being employed by restricting the sample to workers who are observed working in every week of the series. We construct employment in the Earnin and Homebase data and earnings in the Homebase data by summing the total number of hours worked in each day and the total wages earned in each day, respectively. We then take the mean value of each series over the prior seven days and norm to the average value of the seven-day moving average over the period January 4 - January 31, 2020. Data sources: Earnin, HomeBase

APPENDIX FIGURE 12: Changes in Employment Rates by County



Notes: This figure replicates Figure 7 but for the entire United States instead of a single city and its surrounding area. See notes to Figure 7 for details. Data sources: Earnin

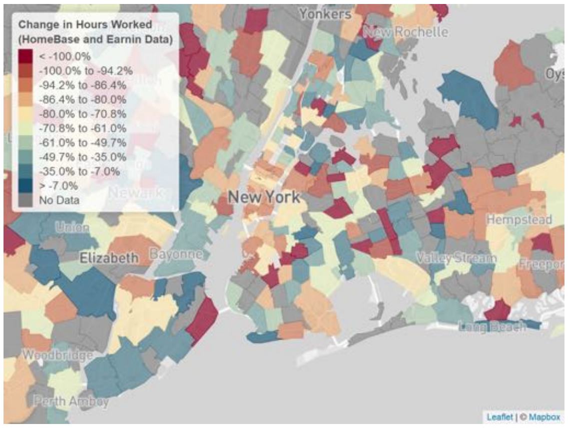
APPENDIX FIGURE 13: Changes in Total Employment by Firm Size



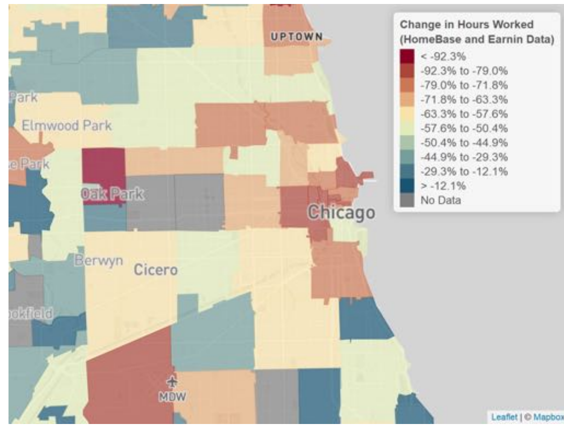
Notes: This figure displays the average declines in employment among workers in the Earnin data, separately for each firm size decile. The decline is calculated by taking total employment at the firm decile level in a pre-period that spans from January 8th, 2020 to January 28th, 2020, and comparing to employment in a post-period that spans from April 1, 2020 to April 21, 2020. Firms are classified into firm size deciles based on total number of Earnin users at the firm. Data source: Earnin

APPENDIX FIGURE 14: Changes in Employment Rates by ZIP Code for Food and Accommodation Service Businesses

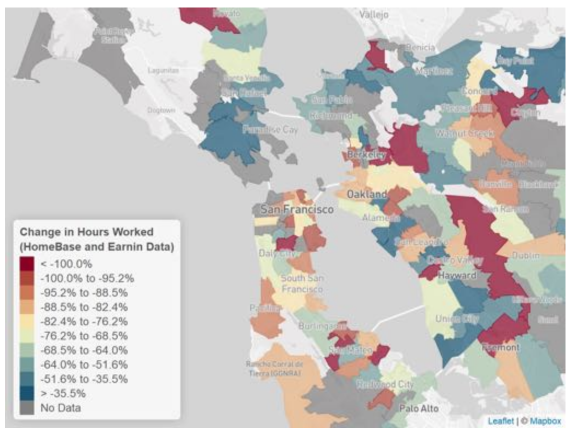
A. New York City



B. Chicago



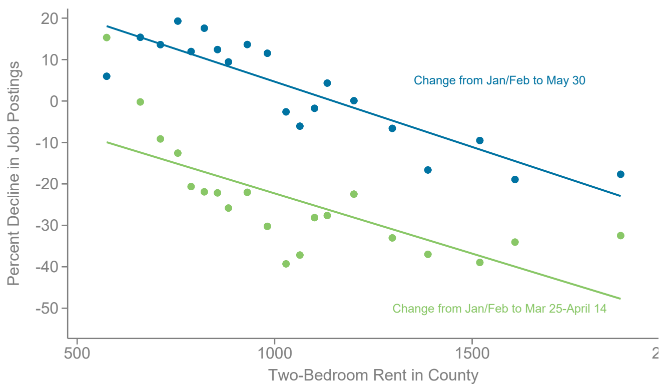
C. San Francisco



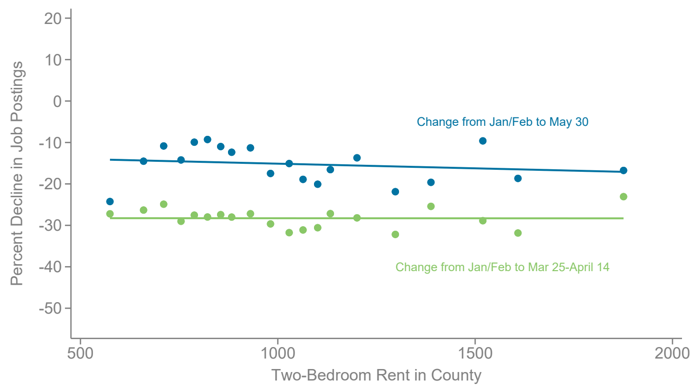
Notes: This Figure displays ZCTA-level maps of the MSAs corresponding to New York City, San Francisco, and Chicago, coloured by their respective deciles of change in hours worked in businesses classified as NAICS 72 within each MSA. We calculate total hours worked in each ZCTA by summing total hours worked in Earnin data with total hours worked in Homebase data, restricting to NAICS 72 employers in both datasets. We then calculate changes in hours worked in each ZCTA as described in the notes to Figure 7. Data sources: Earnin, HomeBase

APPENDIX FIGURE 15: Changes in Job Postings vs. Rent Over Time

A. Job Postings for Low-Education Workers and County Median Rent, Over Time



B. Job Postings for High-Education Workers and County Median Rent, Over Time

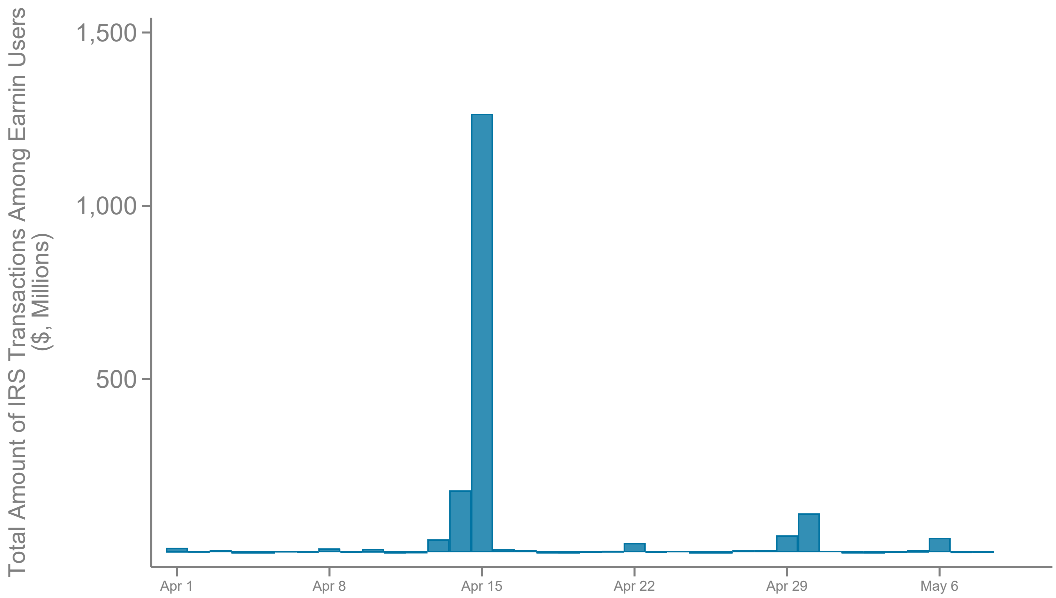


Notes: This figure shows binned scatterplots of the relationship between median rent and changes in job postings between a pre-period of January 8 - March 10 and the periods March 25 - April 14 or the period May 30-June 5. The change in job postings is computed using Burning Glass data. Median two-bedroom rent is computed using the 2014-2018 ACS at the county level. Panel C presents a binned scatterplot of the relationship between the percentage change in job postings for workers with minimal or some education and median 2 bedroom rent. Panel D presents a binned scatterplot of the relationship between the percentage change in job postings for workers with moderate, considerable or extensive education and median 2 bedroom rent. Data source: Burning Glass

APPENDIX FIGURE 16: Legislated Stay-at-Home Orders and Non-Essential Business Closures

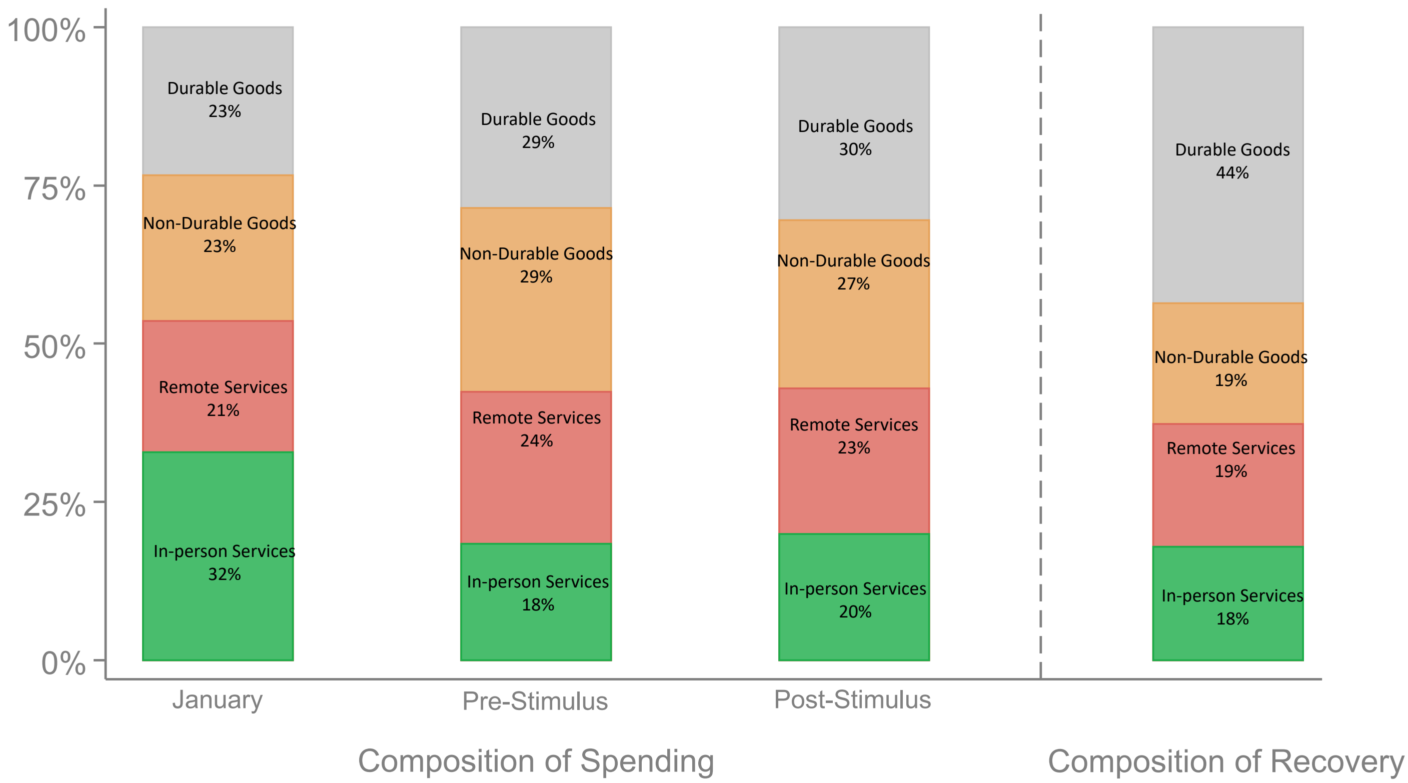
Notes: This figure shows percent change in seasonally-adjusted consumer spending in the Affinity Solutions data, pooling together states that closed non-essential business early (between March 19th and March 24th), states that closed non-essential businesses late (between March 30th and April 6th), and those that never closed. Data source: Affinity Solutions

APPENDIX FIGURE 17: IRS Transactions Among Earnin Users



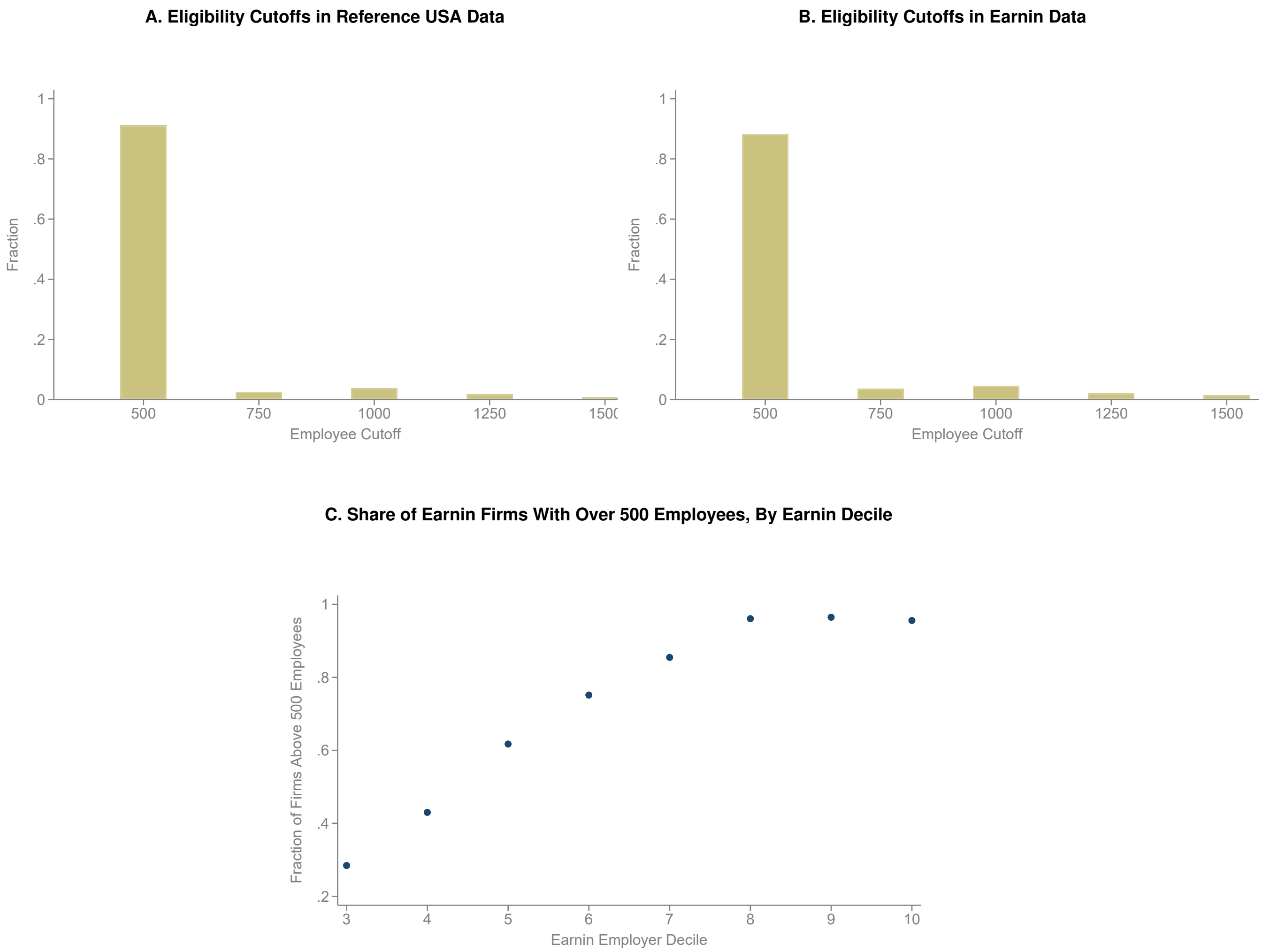
Notes: This figure displays the total dollar amount of IRS transactions for Earnin users. Data source: Earnin

APPENDIX FIGURE 18: Impact of Stimulus on the Composition of Consumer Spending



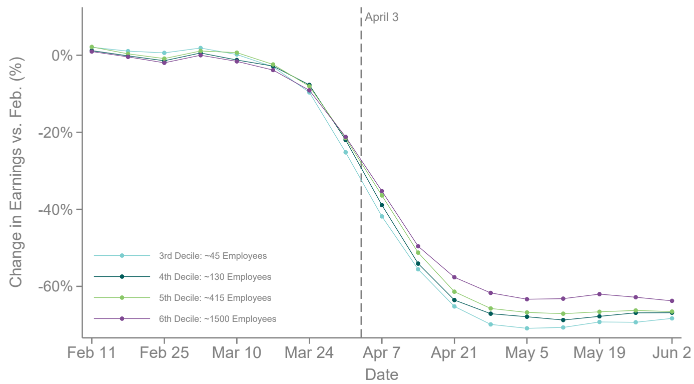
Notes: See notes of Figure 2 Panel B. The pre-stimulus, post-COVID period is defined as March 25th-April 14th. The post-stimulus period is defined as April 29th to May 5th. The total recovery is computed use the post-stimulus period and the average weekly spending in the pre-stimulus period. This figure disaggregates spending by Merchant Category Codes (MCCs), grouping together similar MCCs.We define durable goods as the following MCC groups: motor vehicles, sporting goods and hobby, home improvement centers, consumer electronics, and telecommunications equipment. Non-durable goods include wholesale trade, agriculture, forestry and hunting, general merchandise, apparel and accessories, health and personal care stores, and grocery stores. Remote services include utilities, professional/scientific services, public administration, administration and waste services, information, construction, education, and finance and insurance. In-person services include real estate and leasing, recreation, health care services, transportation and warehousing services, and accommodation and food, as well as barber shops, spas, and assorted other services. Data source: Affinity Solutions

APPENDIX FIGURE 19: Histograms of PPP Eligibility Firm Size Cutoffs for Firms with 300 to 700 Employees



Notes: This figure plots a histogram of the firm size cutoffs for PPP eligibility in the set of firms in Reference USA and the set of firms in the Earnin sample. In the reference USA data, we take the establishment-size-weighted distribution of PPP employee-based eligibility thresholds, which are based on parent company size (except in the case of NAICS 72, which is not included here). In the Earnin sample, we assign a firm size threshold for which the individual's firm would be eligible for PPP loans. Panel C shows the proportion of firms in the Earnin data whose parent company has more than 500 employees, split by firm size deciles based on number of Earnin users.

APPENDIX FIGURE 20: Impact of Paycheck Protection Program on NAICS 72



Notes: This figure replicates Figure 14a for NAICS 72. See notes for Figure 14. Data source: Earnin