The Economic Impacts of COVID-19: Evidence from a New Public Database Built Using Private Sector Data

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Abstract

We build a publicly available database 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 ZIP code, industry, income group, and business size. Using these data, we study the mechanisms through which COVID-19 affected the economy by analyzing heterogeneity in its impacts. 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 in-person 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 many of their employees, leading to widespread job losses especially among low-wage workers in affluent areas. High-wage workers experienced a “V-shaped” recession that lasted a few weeks in terms of employment loss, whereas low-wage workers experienced much larger job losses that persisted for several months. 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-19. State-ordered reopenings of economies had small impacts on spending and employment. Stimulus payments to low-income households increased consumer spending sharply, but little of this increased spending flowed to businesses most affected by the COVID-19 shock, dampening its impacts on employment. Paycheck Protection Program loans increased employment at small businesses by only 2%, implying a cost of $377,000 per job saved. These results suggest that traditional macroeconomic tools – stimulating aggregate demand or providing liquidity to businesses – 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 shows how public statistics constructed from private sector data can support many research and policy analyses without compromising privacy, providing a new tool for real-time empirical macroeconomics.

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Introduction

Since Kuznets (1941), macroeconomic policy decisions have been made on the basis of publicly available statistics constructed from recurring surveys of households and businesses conducted by the federal government. Although such statistics have great value for understanding total economic activity, they have two limitations. First, survey-based data are typically available only at low frequencies, often with a significant time lag. For example, disaggregated data on consumer spending are only available at a quarterly frequency 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 relatively small sample sizes, most statistics are typically reported only at the national or state level and breakdowns for demographic subgroups or sectors are generally unavailable. Because of these limitations, existing publicly available macroeconomic statistics typically cannot be used to uncover the sources of economic fluctuations or the causal impacts of macroeconomic policies in a timely manner.

In this paper, we address these challenges by (1) building a publicly accessible database that measures spending, employment, and other outcomes at a high-frequency, granular level using anonymized data from private companies and (2) demonstrating how this new database can be used to obtain insights into the effects of the coronavirus pandemic (COVID-19) and subsequent stabilization policies in near real-time – within three weeks of the shock or policy change of interest.

We organize the paper in three parts. First, we describe how we construct statistics on consumer spending, business revenues, employment rates, job postings, and other key indicators – disaggregated by area (ZIP code or county), industry, income level, and business size – by combining data from credit card processors, payroll firms, and financial services firms. The main challenge in using private sector data sources to measure economic activity is a tension between research value and privacy protection. For research, it is beneficial to use raw, disaggregated data – ideally down to the individual consumer or business level – to maximize precision and flexibility of research designs. But from a privacy perspective, it is preferable to aggregate and mask data to reduce the risk of disclosure of information about businesses or their clients. To balance these conflicting interests, one must construct statistics that are sufficiently aggregated and masked to mitigate privacy concerns yet sufficiently granular to support research and policy analysis.1

1. An alternative approach – pursued by several recent studies summarized at the end of this section – is to use confidential private data sources for research analyses under non-disclosure agreements with companies. Although a valuable complement to the public data approach we pursue here, that approach has limits in terms of scale and timeliness. The need to write contracts and acquire data from each company separately typically leads most studies...
We navigate this tradeoff by combining several statistical methods: reporting only changes since January 2020 (rather than raw levels), masking certain cells, and pooling data from multiple companies to comply with regulations governing the disclosure of material non-public information. We then clean the raw transaction data by removing data artifacts (e.g., breaks that arise from changes in platforms) and smoothing seasonal fluctuations. Finally, we address the challenge that these statistics reflect the behavior of the clients of the firms from which we obtain data and hence may not be representative of the broader population. To minimize potential selection biases, we start by obtaining data from companies that have large samples (e.g., at least one million individuals) and span well-defined sectors or subgroups (e.g., small businesses, bottom-wage-quartile workers). We then compare each time series to publicly available benchmarks based on representative surveys and proceed to use only the series that track publicly available data closely. After establishing these protocols, we report the final statistics using an automated pipeline that ingests data from businesses and reports statistics shortly after the relevant transactions occur (typically within one week).

In the second part of the paper, we use these new public statistics to analyze the economic impacts of COVID-19. National accounts data reveal that most of the initial reduction in GDP following the COVID-19 shock 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 primarily 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. High-income households cut spending more heavily when the COVID shock first hit in mid-March and increased spending more gradually since that point. As of July 31st, 44% of the reduction in spending since January came from households in the top quartile of the income distribution; only 9% had come from households in the to use one or two datasets and limits the number of researchers and policymakers who can analyze data most relevant to their region or sector of interest in a timely manner. Our goal is to assess whether one can eliminate the need to write such contracts by producing aggregated public statistics that can deliver analogous insights.

2. This benchmarking work proves to be quite important in constructing representative series. For example, many studies have used data from Homebase, a company that helps small businesses track their employees’ hours (e.g., Bartik et al. 2020, Bartlett and Morse 2020, Granja et al. 2020, Altonji et al. 2020), to study employment in the COVID pandemic. As noted by Bartik et al. (2020), the time series patterns in the Homebase data differ significantly from representative statistics on small business employment (although the patterns are generally similar for the sectors it covers). We therefore turn to other sources of employment data to produce publicly available series that track representative benchmarks more closely, which are now available for future research.
bottom income quartile.\textsuperscript{3} This is both because the rich account for a larger share of total spending to begin with, and because high-income households were spending 13\% less by the end of July than they were in January, whereas low-income households were spending only 4\% less.

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 findings from contemporaneous work by Alexander and Karger (2020) and Cox et al. (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.

The fact that spending fell in proportion to the degree of in-person exposure required across sectors suggests that the reduction in spending by the rich was driven primarily by health concerns (either their own or altruistic concerns about others’ health) rather than a reduction in income or wealth. Indeed, we find that the incomes of the rich fell relatively little in the COVID recession. 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.\textsuperscript{4}

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) which 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

\textsuperscript{3} 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.

\textsuperscript{4} 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) or changes in product variety and promotions offered to consumers (Jaravel and O’Connell 2020).
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 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, particularly low-wage workers. Building on results first established by Cajner et al. (2020), we find that employment rates fell by 36% around the trough of the COVID recession (April 15, 2020) for workers with wages rates in the bottom quartile of the pre-COVID wage distribution. By contrast, employment rates fell by 14% for those in the top wage quartile. Employment for high-wage workers also rebounded much more quickly: employment levels for workers in the top wage quartile were back to pre-COVID levels by the end of May, but remained 19% below baseline for low-wage workers even as of September 2020. The greater persistence of job losses for low-wage workers is not explained purely by sectoral differences; even in sectors where spending rebounded to baseline levels, such as retail trade, employment of low-wage workers remained far below baseline levels, suggesting that firms may have shifted their modes of production to use less low-wage labor (Jaimovich and Siu 2020).

Low-wage individuals working at small businesses in affluent areas were especially likely to lose their jobs. In the highest-rent ZIP codes, more than 45% 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 25% 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 positions requiring less education. 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 – showing that some workers were not fully insured against job loss despite the substantial expansion of safety net programs such as unemployment insurance.

In summary, the impacts of COVID-19 on economic activity in the first three months after the shock 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 third and final part of the paper, we analyze the impacts of three sets of policies that were enacted shortly after the crisis began in an effort to break this chain of events and mitigate economic losses: state-ordered shutdowns and reopenings, stimulus payments to households, and loans to small businesses.5

State-ordered shutdowns and reopenings of economies had modest impacts on economic activity. Spending and employment remained well below baseline levels even after reopenings, and trended similarly in states that reopened earlier relative to comparable states that reopened later. Spending 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). As a result, relatively little of the cross-state variation in spending and employment patterns is explained by the timing of shutdown orders or re-openings, consistent with the findings of Goolsbee and Syverson (2020) from cell phone location data.

Stimulus payments made to households in mid-April 2020 increased spending among low-income households sharply, nearly restoring their spending to pre-COVID levels by late April, consistent with evidence from Baker et al. (2020) and recent models of consumption that generate excess sensitivity via frictions (e.g., Kaplan and Violante 2014). However, 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 by much less. As a result, very little

5. This set of policies is not 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 data we have assembled here can be used for real-time policy analysis, and we hope that future work will use these data to analyze other policies.
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 and sectors where job losses were largest because of diminished secondary multiplier effects (a broken Keynesian cross), as discussed in Guerrieri et al. (2020).

Loans to small businesses as part of the Paycheck Protection Program (PPP) also had small impacts on employment rates at small businesses. Employment rates at firms with fewer than 500 employees (which were eligible for PPP assistance) increased only slightly – by about 2 percentage points – relative to larger firms that were ineligible for PPP when the PPP program began. Our point estimates imply that the cost per job saved by the PPP was $377,000 ($119,000 at the lower bound of the 95% confidence interval). The PPP had modest marginal impacts on employment likely because the vast majority of PPP loans went to inframarginal firms that were not planning to lay off many workers. These results are consistent with those of more recent studies by Granja et al. (2020), Autor et al. (2020), and Hubbard and Strain (2020) using alternative data sources and research designs. Together, these findings suggest that providing liquidity to firms is an expensive way to maintain employment rates in the short run, although it remains possible that the PPP may have long-term benefits by reducing business closures.

Our findings suggest that economic recovery from a pandemic ultimately requires restoring consumer confidence by addressing the root health concerns themselves (e.g., Allen et al. 2020, Romer 2020). Traditional macroeconomic tools – stimulating aggregate demand or providing liquidity to businesses – have diminished short-run impacts when consumer spending is fundamentally constrained by health concerns. In such a setting, it may be more fruitful to provide social insurance to reduce hardship for those who have lost their jobs (e.g., via unemployment benefit extensions), consistent with the normative predictions of Guerrieri et al. (2020). In addition, the disparate impacts of the shock across areas suggest it may be useful to target employment assistance to places that have suffered the largest job losses (such as affluent, urban areas), since geographic disparities in unemployment persist for many years (Blanchard and Katz 1992, Yagan 2019).

Our work builds on two literatures: a longstanding literature on the measurement of economic activity and a nascent literature on the economics of pandemics. In the macroeconomic measurement literature, our work is most closely related to recent studies showing that private sector data sources can be used to forecast government statistics (e.g., Abraham et al. 2019, Aladangady et al. 2019, Ehrlich et al. 2019, Cajner et al. 2019, Gindelsky, Moulton, and Wentland 2019, Dunn,
Hood, and Driessen 2020). In the COVID-19 pandemic literature, several recent papers have used confidential private sector data similar to those we use to construct our platform to document related results on 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, Forsythe et al. 2020), and social distancing (e.g., Allcott et al. 2020, Chiou and Tucker 2020, Goldfarb and Tucker 2020, Mongey, Pilossoph, and Weinberg 2020).

Our paper makes two main contributions to these literatures. First, our analysis sheds light on the mechanisms through which pandemics affect economic activity. Other contemporaneous studies of the COVID-19 pandemic have focused on a specific subset of outcomes (e.g., spending or employment or job postings) at broad geographies. By combining data sources on multiple outcomes at the ZIP code level, we provide an integrated picture of how COVID-19 affected the macroeconomy – from changes in consumer spending to in-person business revenue losses to employment changes and impacts on displaced workers. In addition, analyzing a suite of outcomes allows us to characterize the impacts of major stabilization policies more fully, from changes in consumer behavior to impacts on businesses’ employment and hiring decisions. These findings also suggest new directions for future research. The sharp heterogeneity in impacts we document across ZIP codes provides a novel source of local variation to understand macroeconomic dynamics, similar to the geographic variation widely exploited to understand the Great Recession (Mian and Sufi 2009).

Second, and more broadly, this study opens new approaches to empirical macroeconomics by demonstrating that it is feasible to construct public statistics that are sufficiently disaggregated to answer many research and policy questions yet are sufficiently aggregated to meet privacy and data protection requirements. Unlike the aforementioned studies of COVID-19 – each of which draws upon confidential data sources – all the results reported here are produced from what are now publicly available data. This ability to analyze impacts in a timely, publicly verifiable manner creates new paths for evidence-based macroeconomic policy and research. For instance, one could potentially modify the parameters of policies as one observes their impacts on the economy (as the Paycheck Protection Program was repeatedly modified, but without the benefit of evidence on its ongoing impacts). Moreover, these impacts can be analyzed heterogeneously across areas, permitting tailored responses by local governments and analyses of disaggregated data by a much larger set of researchers. In this sense, the data assembled here provide a prototype for a system of real-time, granular national accounts that we hope will be refined in future work, much as Kuznets
(1941) and Summers and Heston (1991) developed prototypes for national accounts within and across countries that were refined in subsequent work (e.g., Feenstra, Inklaar, and Timmer 2015).

The paper is organized as follows. The next section describes how we construct the public data series. In Section III, we analyze the effects of COVID-19 on spending, revenue, and employment. Section IV analyzes the impacts of policies enacted to mitigate COVID’s impacts. Section V concludes. Technical details are available in an online appendix, and the data used to produce the results can be downloaded from this Github repository.

II Data and Methods

We use anonymized data from several private companies to construct indices of spending, employment, and other outcomes. To systematize our approach and facilitate comparisons between series, we adopt the following set of principles when constructing each series (wherever feasible given data availability constraints).

First, we remove artifacts in transaction data that arise from changes in 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%), study their root causes by consulting with the data provider, and address such discontinuities by imposing continuity using series-specific methods described below.

Second, we smooth low- and high-frequency fluctuations in the data. We address high-frequency fluctuations through aggregation, e.g. by reporting 7-day moving averages to smooth fluctuations across days of the week. Certain series – most notably consumer spending and business revenue – exhibit strong lower-frequency seasonal fluctuations that are autocorrelated across years (e.g., a surge in spending around the holiday season). Where feasible, we de-seasonalize such series by normalizing each week’s value in 2020 relative to corresponding values for the same week in 2019, but we also report raw values for 2020 for researchers who prefer to make alternative seasonal adjustments.

Third, we take a series of steps to protect the confidentiality of businesses and their clients. Instead of reporting levels of each series, we report indexed values that show percentage changes relative to mean values in January 2020. We suppress small cells and exclude outliers to meet

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
privacy and data protection requirements, with thresholds that vary across datasets as described below. For data obtained from publicly traded firms – whose ability to disclose data is restricted by Securities and Exchange Commission regulations governing the disclosure of material non-public information – we combine data from multiple firms so that the statistics we report do not reveal information about any single company’s activities.\footnote{For publicly traded firms, a key contribution of our platform is that it serves as a coordination device that allows multiple firms to pool and release their data in an environment where each firm faces restrictions that limit its capacity to share its own data publicly.}

Finally, we address the challenge that our data sources capture information about the customers each company serves rather than the general population. Instead of attempting to adjust for this non-representative sampling, we characterize the portion of the economy that each series represents by comparing each sample we use to national benchmarks. We explicitly label the sector and population subgroup that each series represents and exclude data sources that do not track established benchmarks for that sector/subgroup closely. We examined several sources of spending, revenue, and employment data in addition to those used in the final analysis below and excluded them because they failed benchmarking tests.

We release each data series at the highest available frequency using an automated pipeline that ingests data from data providers, constructs the relevant statistics and conducts quality control tests, and outputs the series publicly (see Appendix A for details on the engineering of this pipeline). To limit revisions, we allow for a lag to account 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.

In the rest of this section, we describe each of the series in turn, discussing the raw data sources, construction of key variables, and cross-sectional comparisons to publicly available benchmarks. We benchmark trends in each series over time to publicly available data in the context of our analysis in Section III. All of the data series described below can be freely downloaded from the Economic Tracker website.

II.A Consumer Spending: Affinity Solutions and CoinOut

\textit{Affinity Solutions.} We measure consumer spending primarily using aggregated and anonymized data on credit and debit card spending collected by \textit{Affinity Solutions Inc}, a company that aggregates consumer credit and debit card spending information to support a variety of financial service
products, such as loyalty programs for banks. Affinity Solutions captures nearly 10% of debit and credit card spending in the U.S. 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. We first remove discontinuous breaks caused by entry or exit of card providers from the sample (see Appendix B for details). We then construct daily values of the consumer spending series using a seven-day moving average of the current day and the 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. Finally, we index the seasonally-adjusted series relative to pre-COVID-19 spending by dividing each value by the mean of the seasonally-adjusted average spending level in the first four complete weeks of 2020.

**CoinOut Cash Spending Series.** A concern with card-based spending measures is that they omit cash transactions, which account for 6.3% of consumer spending in the United States (Greene and Stavins 2020) and could potentially respond differently to the COVID shock and subsequent policies. To address this concern, we measure cash spending using transaction data from CoinOut, a company that allows individuals to receive rewards by uploading photos of their receipts to a mobile app. We obtain anonymized data from CoinOut starting from January 1, 2018 on the date and amount of each transaction; the user’s ZIP code; and the date and time the receipt was uploaded. We identify cash transactions by searching for the string “cash” in the text of each receipt and construct series on the total number and amount of cash transactions by day. The CoinOut data are not disaggregated by industry; however, since cash is almost always used in person, we view this series as representing spending on in-person goods (e.g., at grocery stores).

**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. Appendix Figure 1 compares the spending distributions across sectors in the Affinity data to spending captured in the nationally representative Quarterly Services Survey (QSS) and Monthly Retail Trade Survey (MRTS). The Affinity series has broad coverage across industries. However, as expected, it over-represents categories in which credit and debit cards are used for purchases. In particular, accommodation and food services and clothing are a greater share of the card spending data than financial services and motor vehicles. We therefore view the Affinity series as providing statistics that are representative of total card spending (but not total consumer spending). We assess whether the Affinity series accurately captures changes in total card spending around the crisis in Section III.A 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. After excluding outliers and large firms, 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 (see Appendix C for details). For each series, we construct daily values in exactly the same way that we constructed the consumer spending series.

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: Paychex, Intuit, Earnin, and Kronos

We combine several data sources to obtain information on employment and earnings: payroll data from Paychex and Intuit, worker-level data from Earnin, and time sheet data from Kronos. We describe each of these data sources in turn and then discuss how we combine them to construct a weekly series that measures private non-farm employment rates in the U.S. Further details are provided in Appendix D.

Paychex. Paychex provides payroll services to approximately 670,000 small- and medium-sized businesses across the United States and pays nearly 10% of U.S. private-sector workers (Paychex 2020). We obtain aggregated weekly data on total employment, hours worked, and payroll earnings for each county by industry (two-digit NAICS) by 2019 hourly wage quartile by 2019 firm size.

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8. The private payroll providers from whom we obtain data have limited coverage of government agencies; we therefore do not attempt to measure government employment here.
bin by pay frequency. Hourly wage quartiles are based on fixed thresholds of the hourly wage distribution in 2019 (≤$13.00, $13.00-$18.18, $18.18-29.17, >$29.17). Firm size is measured in Dun & Bradstreet data on employment, broken into a set of broad groups (e.g., 1-49 employees, 50-99 employees, 100-199 employees, ..., 900-999 employees, >999 employees). We obtain data from Paychex on checks processed by week in each of these groups. We convert these data into an employment series using methods analogous to those developed by Cajner et al. (2019); see Appendix D for details.

**Intuit.** Intuit offers payroll services to businesses as part of its Quickbooks program, covering approximately one million businesses as of January 2020. Businesses that use Quickbooks tend to be very small (fewer than 20 employees). Employment is defined in the Intuit data as the total number of workers who were paid a non-zero amount in the prior month. We obtain anonymized, aggregated data on month-on-month and year-on-year changes in total employment and average earnings at the state and county level by month, based on repeated cross-sections. To develop a national series, we take population-weighted averages of state changes in each month.

**Earnin.** Earnin is a financial management application that provides its members with access to their income as they earn it, in advance of their paychecks. Workers sign up for Earnin individually using a cell phone app, which connects to the bank account in which paychecks are deposited. Earnin complements the firm-based payroll datasets discussed above by providing a worker-level sample. This yields insight into employment rates at a much wider spectrum of firms – ranging from the largest firms in the U.S. to small businesses – at the expense of having fewer workers per firm. Since employment and hours decisions are highly correlated across workers within firms at business cycle frequencies, Earnin’s coverage of a large set of firms proves to be a valuable complement to the firm-based payroll datasets for our analysis. Because its users tend to have lower income levels, Earnin primarily provides information on employment for the bottom quartile of the wage distribution; we discuss the characteristics of the workers who use Earnin further below.

We obtain anonymized data from Earnin from January 2020 to present at the paycheck level with information on home ZIP code, workplace ZIP code, unemployment status, earnings, and hours worked. We assign firm sizes and NAICS codes to Earnin employers by matching to firm size data provided by ReferenceUSA and matching to NAICS codes using a custom-built crosswalk constructed by Digital Divide Data (see Appendix D for details).

**Kronos.** Kronos is a workforce management service used by many firms across the U.S. The data we obtain from Kronos cover approximately 30,000 mid-sized firms which together employed...
about 3.2 million workers pre-COVID. We obtain anonymized and aggregated weekly data on the total number of “punches,” representing an employee clocking into work on an electronic time sheet. We obtain these data by county, industry, and firm size at the point that the firm entered the Kronos database. The employees in the database are primarily hourly workers who must record their time, and are concentrated in the bottom quartile of the wage distribution: assuming full-time employment, their wage rates translate to average earnings of $24,000 per year (with a 10th-90th percentile range of $7,200 to $45,600).

The Kronos data differ from the other data sources above in that they measure data from time sheets rather than paychecks. The advantage of time sheets is that they provide more timely information on employment, with a lag of just 2-3 days. The disadvantage of time sheets is that they do not capture total wage employment (e.g., workers may remain on payroll despite clocking fewer hours) and, naturally, only provide information for the subset of workers who are required to record their time.

Homebase. Homebase provides scheduling tools for small businesses (on average, 8.4 employees) such as restaurants (which comprise 64% of employees for whom sectoral data are available). We obtain de-identified individual-level data on hours and total pay for employees and and construct employment series at the county and industry level, assigning location based on the ZIP code of establishment. We include Homebase as a point of comparison because it has been widely used in other studies of small business employment in the COVID pandemic, but we do not include it in our primary employment indices because it does not track benchmarks of overall employment at small businesses as closely as our other data sources (see Section III.C below).

Combined Employment Series. To protect business privacy and maximize precision, we combine the data sources above to construct our primary employment series (see Appendix D for details). Because Paychex covers all sectors and wage levels, we use Paychex data at the industry x wage quartile level for each geography (county, state and national) as the base for the combined employment series. We then use Earnin and Intuit to refine the series in cells represented by those datasets. Earnin best represents workers in the bottom wage quartile (see Appendix Table 2 below). We therefore combine Earnin data with Paychex data to construct employment estimates for the bottom wage quartile. Next, we combine Intuit with the Paychex+Earnin data, accounting for the fact that Intuit data are available at a lower frequency and are not broken down by wage level or industry. We report seven-day moving averages of these series, expressed as a percentage change relative to January 4-31.
The employment series constructed based on payroll data is generally available only with a one month lag because people are paid after completing work over multiple prior weeks. To obtain more current estimates, we use Kronos time sheet data along with Paychex data from firms with weekly paycycles to forecast employment rates (see Appendix D for the forecasting model).

Comparisons to QCEW and OES. Appendix Table 1 compares industry shares in each of the data sources above to nationally representative statistics from the Quarterly Census of Employment and Wages (QCEW). The Earnin-Paychex combined sample is broadly representative of the industry mix in the U.S., although high-wage sectors (such as professional services) are slightly under-represented in Earnin as expected given that it consists primarily of lower-income workers. Intuit is concentrated primarily in professional services, construction, other services, and healthcare and social assistance. Kronos has fairly broad coverage, but over-represents the manufacturing and transportation and warehousing sectors. Homebase covers primarily food services.

Next, we assess how these datasets compare to national benchmarks in terms of wage rates by comparing the median wage rates of workers in Paychex, Intuit, and Earnin to nationally representative statistics from the BLS’s Occupational Employment Statistics (Appendix Table 2). Median wage rates in Paychex closely match the OES estimates. Average wages in Intuit closely mirror OES estimates in the industries that Intuit covers. Workers who use the Earnin app 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 our combined datasets provide a representative picture of private non-farm employment in the United States, and that Earnin provides good coverage of workers at the bottom of the wage distribution, who are a group of particular interest given their volatile employment rates over the business cycle.

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. Job qualifications are defined using ONET job zones, which classify jobs into five groups based on the amount of preparation they require. We also obtain analogous data broken down by educational requirements.

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) show that the Burning Glass data are reasonably well aligned with government survey-based statistics on job openings and characterize the sample in detail. In Appendix Figure 2, 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 a non-profit math curriculum publisher that combines in-person instruction with digital lessons. Zearn was used by approximately 925,000 students in the U.S. in Spring 2020. 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 (see Appendix E for details).

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. After excluding outliers, 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 3, we assess the representativeness of the Zearn data by comparing the demographic characteristics of the schools for which we obtain 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. 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 provides a 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

In addition to the new private sector data sources described above, we also collect and use three sets of data from public sources to supplement our analysis: data on unemployment benefit claims obtained from state government agencies; data on COVID-19 cases and deaths obtained from the New York Times; and data on the amount of time people spend at home vs. other locations obtained from Google’s COVID-19 Community Mobility Reports. Further details on these data sources are provided in Appendix F.

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 $1.73 trillion (an annualized rate of 31.7%) from the first quarter of 2020 to the second 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 $1.35 trillion. Government purchases and net exports did not change significantly, while private investment fell by $0.47 trillion. 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

9. Most of the reduction in private investment was driven by a reduction in inventories and equipment investment in the transportation and retail sectors, both of which are plausibly a response to reductions in current and anticipated consumer spending. In the first quarter of 2020, consumer spending accounted for an even larger share of the reduction in GDP, further supporting the view that the initial shock to the economy came from a reduction in consumer spending.
spending excluding housing, healthcare, and motor vehicles – fell by approximately $1.03 trillion, comprising roughly 75% of the total reduction in personal consumption expenditures.\textsuperscript{10}

\textit{Benchmarking}. We begin by assessing whether our Affinity data track patterns in corresponding spending categories in the national accounts. Figure 1b plots spending on retail services (excluding auto-related expenses) and food services in the Affinity data vs. corresponding series from the Monthly Retail Trade Survey (MRTS), one of the main inputs used to construct the national accounts.\textsuperscript{11} All series are indexed to have a value of 1 in January of each calendar year; each point shows the average level of daily spending in a given month divided by spending in January of that year. The Affinity spending series tracks the MRTS closely both before and after the COVID crisis. In particular, both series show a rapid drop in food services spending in March and April 2020, while total retail spending fluctuates much less.

Figure 1c plots the change in spending from January to April 2020 in the Affinity spending series against the decline in consumer spending as measured in the MRTS. Despite the fact that the MRTS category definitions are not perfectly aligned with those in the card spending data, the relative declines are generally well aligned across sectors, with a correlation of 0.91. Given that Affinity data tracks the MRTS at the national level quite well, we proceed to use it to disaggregate the national series in several ways to understand why consumer spending fell so sharply.\textsuperscript{12}

\textit{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

\textsuperscript{10} The rest of the reduction is largely accounted for by healthcare expenditures; housing and motor vehicle 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.

\textsuperscript{11} The series are not perfectly comparable because the category definitions differ slightly across the datasets. For example, we observe food and accommodation services combined together in the card data but only food services in the MRTS. In addition, the MRTS includes corporate card transactions, whereas we exclude them in order to isolate consumer spending. Hence, we would not expect the series to track each other perfectly even if the card spending data provided a perfect representation of national spending patterns.

\textsuperscript{12} Of course, our national benchmarking exercise does not guarantee that our statistics capture economic activity in every subgroup accurately. We cannot benchmark most datasets at the local level: this is precisely the value of the private sector data that we introduce here. To assuage concerns about differential selection bias across regions, we show that each of main results is obtained in multiple different data sources, likely because any biases due to non-representative sampling are small relative to the scale of changes induced by COVID-19.
estimates to those in contemporaneous work by 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.\(^{13}\)

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.\(^{14}\) The solid line shows
data from January to August 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 per day in February to $5.5 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 22% 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. In Column 1 of Table 1a, we estimate that as of mid-April, top-quartile households accounted for roughly 40% 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-July, top-quartile households accounted for roughly 45% of the total spending decline in the U.S. (Table 1, Column 3) and were still spending 18% less than their January levels, whereas bottom-quartile households were spending around 17% less than they were in 2019. This heterogeneity in spending changes by income is 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.

A potential concern with our card-based estimates of spending changes is bias from substitution out of cash purchases; for instance, if individuals sought to use more contactless methods to pay or began placing more orders online, trends in card spending might exhibit excess volatility relative to

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\(^{13}\) 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 percentage points 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.

\(^{14}\) 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.
overall spending. To assess the importance of such substitution, we examine cash purchases using receipts data from CoinOut. Appendix Figure 3a plots aggregate cash purchases in the CoinOut data vs. aggregate card spending at grocery stores over time.\textsuperscript{15} The time trends are very similar between the two series (with a signal correlation of 0.9 at the weekly level), showing a sharp spike in spending in late March (as households stocked up on groceries), followed by a more sustained increase in spending from the latter half of April. These results – together with the fact that our card spending series closely track estimates from the MRTS (Figures 1b and 1c) – indicate that aggregate fluctuations in card spending do not appear to have been offset by opposite-signed changes in cash spending. Rather, households shifted spending similarly across both modes of payment.\textsuperscript{16} We therefore proceed to focus on card spending in the rest of our analysis given the larger sample sizes and greater granularity of the card spending data.

\textit{Heterogeneity Across Sectors.} Next, we disaggregate the change in total card 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 or spreading 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. Consistent with the findings of Cox et al. (2020), roughly two-thirds 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. 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. Panel B of Table 1 shows that these gaps have only grown larger as the pandemic has progressed, as consumer spending increased above pre-pandemic levels for durable and non-durable goods by mid-July, but remained sharply depressed for in-person services.

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, online spending increased sharply: online

\textsuperscript{15} We focus on grocery spending in the card data because cash spending in CoinOut is concentrated in certain sectors such as groceries; unfortunately, we are unable to disaggregate the CoinOut data by sector or align sectoral definitions more precisely across the datasets.

\textsuperscript{16} Appendix Figures 3b and 3c compare the patterns for spending in high- vs. low-rent areas; the patterns also appear quite similar between the CoinOut spending and grocery card spending across areas as well.
purchases increased by 37% from the first to the second quarter of 2020 (U.S. Department of Commerce 2020). A conventional reduction in income or wealth would typically reduce spending on all goods 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, separately for low- and high-income counties (median household income in the bottom vs. top income quartile). Spending fell more in counties with higher rates of COVID infection in both low- and high-income areas.\textsuperscript{17}

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, again separately for low- and high-income counties. 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.

Figures 3a-b show that at all levels of COVID infection, higher-income households reduced spending and time spent outside more than lower-income households. Figure 3c establishes this point more directly by showing that change in time spent outside home falls monotonically with household income across the distribution. These results help explain why the rich reduce spending

\textsuperscript{17} The relationship shown in Figure 3a also holds in a county-level regression of changes in consumer spending on cases per 100,000 people with controls for household income and state fixed effects (coefficient = -2.25, S.E. = 0.28). 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.
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.

In sum, disaggregated data on consumer spending reveal that spending in the initial stages of the pandemic fell primarily because of health concerns rather than a loss of current or expected income. Income losses were relatively modest because few high-income individuals lost their jobs—as we show in Section III.C below—and lower-income households who experienced job loss had their incomes more than replaced by supplemental unemployment benefits (Ganong, Noel, and Vavra 2020), which led unemployed households to increase their spending relative to pre-COVID levels (Farrell, Ganong, Greig, Liebeskind, Noel, and Vavra 2020). Indeed, national accounts data actually show an increase in total income of 13% from March to April 2020. This finding 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—was 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.

18. It may be surprising that we do not see a decline in aggregate spending in Figure 2a when supplemental unemployment benefits expired at the end of July 2020. Farrell, Ganong, Greig, Liebeskind, Noel, Sullivan, et al. (2020) estimate that the expiration of supplemental unemployment benefits led to a $96 reduction in weekly spending, around 16% of the reduction in unemployment benefits. As of the last week of July, federal spending on supplemental unemployment benefits was $15.59 billion (Morath 2020), implying that aggregate spending would decline by roughly $2.49 billion in the week following the expiration of supplemental benefits, about 1.6% of mean weekly card spending in January 2020. From July to September 2020, the root-mean-squared-error from a regression of total weekly consumer spending on a linear trend is 1.6%. Hence, the aggregate effect of the expiry of supplemental unemployment benefits is small relative to typical fluctuations around trend, explaining why the impacts of the expiration of UI benefits are not visible in the aggregate data shortly after benefits expire. However, as emphasized by Farrell, Ganong, Greig, Liebeskind, Noel, Sullivan, et al. (2020), households maintained consumption by depleting savings they had built up when receiving supplemental benefits; as they exhaust their assets, expenditure could fall more sharply and have a larger impact on aggregate spending.

19. Of course, these results only apply to the period we study (the initial months after the COVID shock hit the U.S.), when the federal government was injecting substantial income into the economy via increased UI benefits and stimulus checks to households. Recessions induced by pandemics could produce more traditional economic shocks with Keynesian spillovers across a wider set of sectors and areas as time passes or in the absence of income support for the unemployed (Farrell, Ganong, Greig, Liebeskind, Noel, Sullivan, et al. 2020), in which case tools such as stimulus and liquidity could become much more impactful (Guerrieri et al. 2020).
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 shocks to firms’ revenues. 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. 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.

**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 4).

**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 (formally, ZIP code Tabulation Areas - ZCTAs) from a baseline period prior to the COVID shock (January 8 to March 8, 2020) to the weeks immediately after the COVID shock before the stimulus program began (March 25 to April 14, 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 -73% (or below) in the hardest-hit (lowest decile) of ZIP codes.

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20. 56% of workers in food and accommodation services and retail (two major non-tradeable sectors) work in establishments with fewer than 50 employees.

21. 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).

22. 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).
to -13% (or above) in the top decile within New York City.\textsuperscript{23}

In all three cities, revenue losses are largest in the most affluent parts of the city. For example, small business lost 67% of their revenue in the Upper East Side in New York, compared with 45% in the East Bronx; 66% in Lincoln Park vs. 41% in Bronzeville on the South Side of Chicago; and 84% in Nob Hill vs. 27% 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.\textsuperscript{24} 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 (Appendix Table 4, Appendix Figure 6a).

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 richest 5% of ZIP codes, small business revenues fell by 50%, as compared with 37% in the poorest 5% of ZIP codes.\textsuperscript{25}

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.\textsuperscript{26}

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

\textsuperscript{23} Very little of this variation is due to sampling error: the reliability of these estimates across ZIP codes within counties is around 0.8, i.e., 80% of the variance within each of these maps is due to signal rather than noise.

\textsuperscript{24} 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 5).

\textsuperscript{25} 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 7a). This is particularly the case among counties with a large top 1% income share (Appendix Figure 7b). Poverty rates are not strongly associated with revenue losses at the county level (Appendix Figure 7c), 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.

\textsuperscript{26} Consistent with this pattern, total spending levels and time spent outside also fell much more in high population density areas.
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 50% in the highest-rent ZIP codes. This relationship is essentially unchanged when we compare ZIP codes within the same county by regressing revenue changes on rent with county fixed effects (Table 2, Column 2). It is also unaffected by controlling for the (pre-COVID) density of high-wage workers in a ZIP code to account for differences that may arise from shifts to remote work in business districts (Table 2, Column 3). Furthermore, these spatial differences between high-rent and low-rent ZIP codes persisted even as the economy began to recover. In July, small business revenue was still around 25% below its January level in the highest-rent ZIP codes, but only around 5% below the January level in the lowest-rent ZIP codes (Appendix Figure 8a).

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 7d shows that 42% of small businesses in the highest-rent ZIP codes closed, compared with 27% in the lowest-rent ZIP codes. The extensive 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. Almost half of the total loss in small business revenues comes from business located in the top-quartile of ZIP codes by rent; less than 15% 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 Employment Rates

We study the impacts of COVID on employment rates using data from payroll companies. We begin by benchmarking these data sources to employment statistics from nationally representative surveys conducted by the Bureau of Labor Statistics and then disaggregate the data by wage level and geography to analyze how the shock in consumer spending and business revenue affected employment rates.

**Benchmarking.** Figure 6a plots employment rates from the nationally representative Current Employment Statistics (a survey of businesses) and Current Population Survey (a survey of households) for all workers alongside our combined Paychex-Intuit-Earnin employment series, constructed as described in Section II.C. Our payroll-based series is broadly aligned with the survey-based measures, generally falling between estimates obtained from the two surveys.

Figure 6b examines how our series performs in matching national statistics on trends across sectors. For illustration, we focus on two sectors that experienced very different trajectories: food services, where employment fell heavily, and professional services, where it did not. In both cases, our Paychex-Intuit-Earnin series closely tracks data from the CES. Appendix Figure 9d shows more generally that changes in employment rates across sectors (two-digit NAICS) are very closely aligned in our series and the CES, with a correlation of 0.94.

For comparison, we also examine trends in employment based on data from Homebase, a dataset that has been used to examine employment trends in the COVID recession in many studies. Homebase exhibits a much larger decline in employment than the other series (56.1% at the trough vs. 15.2% in the CES). This is primarily because 64% of individuals in the Homebase data work in the food services sector, which suffered particularly large employment losses as noted above; however, even within food services, Homebase exhibits a larger decline in employment at the trough (63.8%) relative to the CES (46.8%), as shown in Figure 6b. Because Homebase does not track overall national benchmarks on employment very closely, we do not use it for the analysis that follows, although we note that it exhibits qualitative patterns similar to the other series within food services.

In Appendix Figures 9a-b, we compare trends by wage quartile in our data with estimates based on the Current Population Survey and estimates reported in Cajner et al. (2020), who report employment changes by wage quintile using data from ADP in the initial weeks after the COVID shock. We find broadly similar trends in all three datasets. We also examine employment changes by state and find that in almost all states (excluding North Dakota and Hawaii), employment
changes from January-April in our combined series align very closely with changes in the CES, with an overall correlation of 0.99 (Appendix Figure 9c).

Based on these benchmarking exercises, we conclude that our combined employment series provides a good representation of employment rates across sectors, wage groups, and geographic areas. Consistent with the results of Cajner et al. (2020), we find that wage rates remained relatively constant when restricting to workers who are observed in consecutive periods to avoid selection effects (Appendix Figure 10). 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. We therefore focuses solely on employment changes in what follows to characterize the incidence of the COVID shock on workers.

**Heterogeneity by Wage Rates.** Figure 7a plots the combined employment series by wage quartile. To construct this figure, we first construct hourly wage quartiles based on fixed thresholds of the hourly wage distribution in 2019 ($<13.00, $13.00-$18.18, $18.18-29.17, >$29.17). The solid lines plot total employment (based on repeated daily cross-sections) in each of these bins relative to the January baseline, based on the combined Paychex-Intuit-Earnin data. Consistent with the findings of Cajner et al. (2020) in prior work using ADP data, we find sharp heterogeneity in job losses by wage rate. Employment rates fell by 36% around the trough of the recession (April 15) for workers in the bottom wage quartile (i.e., the total number of jobs paying $<13/hour was 36% lower as of April 15 than in January). By contrast, employment rates fell by only 14% for those in the top wage quartile as of April 15.

High-wage workers not only were less likely to lose their jobs to begin with, but also experienced a much more rapid recovery. By late May, employment for high-wage workers had returned nearly to the pre-COVID baseline. But employment rates for low-wage workers remained 17% below baseline levels even as of late August. Using time sheet data from Kronos, and payroll data from firms with weekly paycycles in Paychex – both of which are available with a shorter lag than payroll-based employment data containing all paycycles – we construct a prediction of employment rates up to September 25 as described in Section II.C (shown by the dashed lines in Figure 7a). These predictions suggest that the rate of recovery remained slow for low-wage workers in September.

In sum, COVID induced a short-term “V-shaped” recession for high-wage workers in terms of employment opportunities, but led to a much deeper and more prolonged recession for lower-wage workers. Why did employment trajectories for low-wage workers differ so sharply from those for
high-wage workers? One potential explanation is that low-wage workers work in different sectors or areas that may have experienced larger reductions in consumer demand. We evaluate this hypothesis in Figure 7b by plotting employment for workers in the bottom wage quartile, reweighting the series to match baseline employment shares by county and industry (2 digit NAICS) in the top wage quartile. This reweighting closes very little of the gap between the two series, showing that differences in industry and location do not explain the differences in employment trajectories.

Figure 7c provides a specific illustration of this result by showing trends in employment and spending in the retail trade sector. Total retail spending is nearly 10% higher as of August 15 relative to the pre-COVID baseline. Employment of high-wage workers is comparable to baseline levels, yet employment of low-wage workers is still down by over 15%. More broadly, even though consumer spending was only 5.9% below baseline levels as of August 15, low-wage employment levels remained 20.2% lower.

One explanation for these patterns is that firms shifted their production processes to use more technology or find other efficiencies (Lazear, Shaw, and Stanton 2016), potentially reducing demand for routine occupations more permanently – a phenomenon documented in previous recessions by Jaimovich and Siu (2020). For example, retail spending may have shifted toward online retailers and larger firms that may use more capital (or imports) than low-wage labor in the United States to produce goods. These results raise the possibility of a “jobless recovery” absent efforts to help workers who have been displaced from their prior jobs (Berger 2012).

**Heterogeneity Across Areas.** To shed further light on why employment rates for low-wage workers fell so much, we next turn to examine geographic heterogeneity in employment losses, in connection to the heterogeneity in spending changes and business revenue losses examined above. We begin by using the Earnin data – which is publicly available at the ZIP code level – to analyze heterogeneity across the ZIP codes where people *work* (not necessarily where they live). Figure 8 maps changes in employment rates from January to the trough in mid-April for low-wage workers at small- and mid-size businesses (fewer than 500 employees) by ZIP code in New York, San Francisco, and Chicago (analogous ZIP-level maps for other cities are available here). The patterns closely mirror those observed for business revenues above. Employment rates for low-wage workers 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 examining variation across commuting

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27. We focus on small and mid-size businesses here because larger firms exhibit significantly smaller declines in employment (Appendix Figure 11) and because, as noted above, their markets are likely to extend well beyond the ZIP code in which they are located.
zones (aggregates of counties) at the national level using the combined Paychex-Intuit-Earnin data (Appendix Figure 6b).

Figure 9a presents a binned scatter plot of changes in employment rates (from January to the mid-April trough) vs. median rents in the employer’s ZIP code, by firm size. Employment rates fell much more at businesses located in high-rent areas than low-rent areas in all groups, supporting the view that the sharp reductions in business revenue in affluent areas induced firms to lay off low-wage workers. As employment recovered in the months after April 2020, the spatial differences observed in Figure 9 persisted. In July, low-wage employment was approximately 15% below baseline levels in low-rent ZIP codes, but remained 30% below baseline in the highest-rent ZIP codes (Appendix Figure 8b).

We observe a similar gradient with respect to local rents for workers at very large firms: from 25% in the lowest-rent ZIPs to over 35% in the highest-rent ZIPs. This presumably reflects that 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. 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) or have a greater ability to substitute to other modes of business (delivery, online retail).

Table 2b presents a set of regression estimates quantifying these impacts. Low-wage workers consistently face larger employment losses in higher-rent ZIP codes, even when comparing ZIP codes within the same county (Column 2) and controlling for the density of high-wage workers in the ZIP code (Column 3).

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 using data from Burning Glass, as in prior work by Forsythe et al. (2020). We conduct this analysis at the county level, pooling firms of all

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28. 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.

29. Employment in the combined Paychex-Intuit-Earnin data is only available at the county level. We see a similar pattern when regressing Paychex-Intuit-Earnin employment changes for low-wage workers on rent at the county level (coefficient = -8.69% per thousand dollars, S.E. = 1.09% per thousand dollars), although the magnitude of the gradient is attenuated as expected given the coarser geographic measures.
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.

Figure 9b 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-wage 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 9c replicates Figure 9b 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.\footnote{30}

\textit{Unemployment Rates.} The low rates of job postings combined with high rates of job loss in affluent areas combined to create very high rates of unemployment in such areas. 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.\footnote{31}

Figure 10 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 10 shows 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.\footnote{32}

\footnote{30. The magnitude of the reduction in job postings for highly educated workers is substantial, at approximately 27%. This contrasts with evidence that higher-wage workers have experienced much lower rates of job loss to date, and suggests that unemployment rates could begin to rise even for higher-wage workers going forward.}

\footnote{31. One notable feature of the COVID 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 lost their jobs but were not actively searching for a new job in the midst of the pandemic. In the three prior recessions, the labor force continued to grow by 0.3% to 0.8% annually. We therefore focus on the decline in employment rates to obtain comparable statistics on job loss across recessions.}

\footnote{32. Unlike our analyses of private data, the publicly released unemployment claims data do not allow us disaggregate 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.}
Santa Clara county in California is the highest income county on the West Coast, yet 16% of its labor force 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 8c shows early signs of a similar pattern in this recession: job postings began to increase starting in late May in the U.S., but remained significantly lower in high-rent counties than in low-rent counties even at the end of July. If this pattern persists going forward, the recovery for low-income workers may take the longest in the richest parts of the country.

III.D Impacts on Displaced Workers

The analysis in the last section focused on changes in employment rates for workers by location in repeated cross-sections, showing for example that low-wage employment rates fell sharply in Manhattan relative to the Bronx. We close our analysis by examining how the COVID shock affected workers’ employment and consumption trajectories longitudinally, tracking workers who lost their jobs and examining whether they were able to find employment elsewhere as the economy recovered. Did people who worked in Manhattan pre-COVID remain out of work and have lower spending levels than comparable workers in the Bronx (who were less likely to lose their jobs)? Or do they find jobs elsewhere, so that the initial incidence of the shock is effectively shared across workers over time, as one would expect in a frictionless labor market?

The traditional approach to studying displacement effects is to compare the employment trajectories of displaced vs. comparable non-displaced workers in individual-level panel data (e.g., Jacobson, LaLonde, and Sullivan 1993, Sullivan and Wachter 2009). Here, we implement such an analysis using our publicly available aggregate data. To do so, we track employment rates
by workers’ ZIP code of residence; since relatively few people moved during the pandemic (Pew Research Center 2020), this effectively provides a panel of employment rates for a given set of workers. We then 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 low-income workers in the U.S. in 2017, to compute the average workplace median rent level for each residential ZIP.

Figure 11a presents a binned scatter plot of changes in low-income employment by home (residential) ZIP code vs. average workplace rent. This figure shows that low-income individuals who were working in high-rent areas pre-COVID are much less likely to be employed after the shock hits in April – consistent with our findings above. Strikingly, these employment losses then persist over time with no convergence in employment rates for workers who live in different areas over time. Figure 11b plots employment trends for low-wage workers living in low-income ZIP codes, by the average workplace rent. As of April 15, employment fell for workers in high-workplace-rent (top quartile) ZIP codes by 45%, whereas employment fell by 32% among workers in low workplace rent (bottom quartile) ZIP codes. This gap in employment rates persisted for several months; in mid-July, employment in high workplace rent ZIP codes was around 33% below baseline levels, whereas employment in low workplace rent ZIP codes was 17% below baseline levels. More broadly, across the entire distribution of workplace rents, the relationship between employment rates and workplace rents is almost the same in July as it was in April, as shown in Appendix Figure 8d. These gaps in employment persist even when comparing workers who live in the same county: adding county fixed effects to a regression of changes in employment (as of July) on ZIP workplace changes the slope coefficient from -22%/\$1000 (s.e. = 2.5%) to -37%/\$1000 (s.e. = 12.9%).

These results show that the spatial patterns in the maps in Figure 8 are driven not by people switching from working in high-rent areas to low-rent areas, but rather by persistently lower employment rates for those who happened to be working in high-rent areas pre-COVID. A worker working at a restaurant in Manhattan remains less likely to be employed months later than a worker in the Bronx. One explanation for this lack of re-equilibration even within labor markets is that, unlike in prior recoveries, many workers may simply returned to their previous jobs as the economy recovered rather than shifting to new jobs. Given the high levels of churn typically observed in low-wage labor markets, one would expect greater convergence in employment rates

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33 In the Earnin microdata, we find similar results even when comparing workers employed at the same firm (e.g., a chain restaurant): people working in high-rent ZIP codes in January remain less likely to have a job (anywhere) in July than their co-workers working in a different establishment of the same firm in lower-rent ZIP codes.
across workers with different initial conditions over time. Still, this evidence suggests that there are significant frictions within labor markets that contributed to persistent impacts of COVID on low-wage workers who happened to work in areas and sectors where demand fell sharply.

Finally, we analyze how these differential shocks to employment affected the consumption of displaced low-wage workers. Using the card spending data from Affinity Solutions, Figure 11c shows that low-income individuals working in high-rent ZIP codes reduced spending more when COVID hit than those working in low-rent ZIP codes. Low-income individuals who work in high-rent ZIP codes cut spending by 40% on average from the baseline period to mid-April 2020, compared with 25% for those working in low-rent ZIPs. The relationship remains similar when we compare ZIP codes within the same county by including county fixed effects and control for rents in the home (residential) ZIP code (Appendix Table 5).

In sum, reductions in spending by high-income households due to health concerns led to persistent negative impacts on employment and spending for low-income workers who happened to work in affluent areas and lose their jobs.

**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) persistent employment 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.

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34. Since we cannot disaggregate the Affinity data by individual-level income, we restrict this figure to households living in low-income ZIPS. As the Earnin data already represent low-income workers, we do not restrict to low-income ZIPs in the analysis above; however, the patterns are very similar when restricting to low-income ZIPs in the Earnin data.

35. In future work, this source of geographic variation in displacement rates could potentially be used to study further downstream impacts on a range of other financial and economic outcomes, as in Mian and Sufi (2009).
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 (see Appendix G for details). 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 retail and personal service businesses to open to the public, on May 1, while New Mexico did not re-open until two weeks later, on May 16. Figure 12a plots consumer spending (using the Affinity Solutions data) in Colorado and New Mexico. Spending evolved nearly identically in these two states: in particular, there is no evidence that the earlier reopening in Colorado boosted spending during the two intervening weeks before New Mexico reopened.

Figure 12b generalizes the case study in Figure 12a by studying partial reopenings in the five states that issued such orders on or before April 24. For each reopening date (of which there are three: April 20, 24, and 27), we compare the trajectory of spending in treated states to a group of control states that had not reopened at the time that the treated state reopened. We select at least three control states (listed in Appendix Table 6) for each of the reopening dates by matching on pre-period levels of spending (relative to January) during the three weeks prior to reopening. 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 three event studies together (redefining calendar time as time relative to the reopening date) to create Figure 12b.

As in the case study of Colorado vs. New Mexico, the trajectories of spending in the treated
states almost exactly mirror those in the control states. We formalize the estimate from this design using a difference-in-differences (DD) design that compares the two weeks before the reopening in the treated states and two weeks after. We estimate that reopenings led to a 1.45% increase in spending. This DD estimate also appears in Table 3, Column 1. Column 2 replicates that specification, but focuses on reopenings where we can go out three weeks after the event before control states begin to re-open; the DD estimate is unchanged with this wider window, at 1.47 percentage points. Figure 12c shows that we also find little impact of reopenings on employment (using the Paychex-Intuit-Earnin data). Finally, Figure 12d shows that there was a 3.52 percentage point increase in the fraction of small businesses open after states allowed businesses to reopen (using data from Womply) – confirming that state orders do have some mechanical impact on the fraction of businesses that are open. However, this mechanical effect does not appear to translate to noticeable impacts on total employment or spending.

In line with these small treatment effect estimates, reopenings account for a relatively small share of the overall variation in economic conditions across states. To demonstrate this, we first calculate the actual variance in spending levels and other outcomes across states. We then counterfactually add our estimate effect of reopening to all states that were not yet open as of May 18, and recalculate the variance. Figure 12e then plots the 1 minus the ratio of the counterfactual variance to the actual variance, which is a measure of the importance of early reopenings in explaining the variation in economic activity observed on May 18. These ratios are very low, showing that early reopenings did not play an important role in explaining why some states had stronger employment trajectories than others. These results are consistent with the findings of other contemporaneous studies showing that little of the state-level variation in employment, job vacancies, or time spent outside home is related to state-level stay-at-home orders or business closures (Bartik et al. 2020, Forsythe et al. 2020, Goolsbee and Syverson 2020, Lin and Meissner 2020, Villas-Boas et al. 2020).

Why did these reopenings have so little immediate impact on economic activity? The evidence in Section III 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 12). If individuals’ own health concerns are the core driver of reductions in spending during pandemics, governments may have limited capacity to restore economic activity through reopenings, especially if those reopenings are

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36. We emphasize that these results apply to average employment rates 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).
not interpreted by consumers as a clear signal of reduced health risks.\textsuperscript{37}

\textbf{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). IRS statistics show that 72\% of stimulus payments made in April were direct-deposited on exactly April 15, 2020, while some households received payments on April 14 (Bureau of the Fiscal Service 2020).

The goal of these stimulus payments was to increase consumer spending and restore employment.\textsuperscript{38} Was the stimulus effective in achieving these goals? In this section, we analyze this question using high-frequency event studies examining spending, business revenue and employment changes in the days surrounding April 15, comparing outcomes for lower-income and higher-income households.

\textit{Impacts on Consumer Spending.} We begin in Figure 13a by plotting a weekly moving average of spending changes relative to mean levels in January for low-income (bottom income quartile ZIP codes) 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 37\% 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 more than 15 percentage points within a week. Spending among top-income-quartile households increased as well, but by only about 7 percentage points. This simple analysis suggests that the stimulus payments had a large positive effect on spending, especially for low-income families.\textsuperscript{39}

\textsuperscript{37} Of course, these conclusions only apply to the initial stages of the pandemic that we study here. If health concerns diminish over time (e.g., due to quarantine fatigue), government restrictions could have larger effects on economic activity.

\textsuperscript{38} 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.

\textsuperscript{39} 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
To estimate the causal effect of the stimulus payments more precisely, we use a regression discontinuity estimator with the daily spending data.\textsuperscript{40} Figures 13b and 13c 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.\textsuperscript{41} Both effects are statistically significantly different from 0, as well as from each other. Panel A of Table 4 shows these regression discontinuity estimates under a variety of bandwidths. These findings are consistent with contemporaneous work in 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 13d and 13e, we investigate the composition of goods on which households spent their stimulus checks. We pool all households in these figures to maximize precision. Figure 13d shows that spending on durable goods rose by 21 percentage points following the arrival of the stimulus payments and further increased thereafter, rising well above pre-crisis levels. But Figure 13e shows that spending on in-person services rose by only 7 percentage points, 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 33% of pre-crisis spending (Appendix Figure 13).\textsuperscript{42} 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 – those in high-rent areas – gain business as as result of the stimulus? Figures 14a and 14b replicate

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\textsuperscript{40} We use the raw daily data, not the 7-day moving average.

\textsuperscript{41} We omit the partially treated date of April 14 (denoted by a hollow dot) when estimating this RD specification since a small fraction of stimulus payments arrived on that day.

\textsuperscript{42} 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.
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 18 percentage points 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 1 percentage point 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 14c, blue lines). Panel B of Table 4 shows these regression discontinuity estimates under a variety of bandwidths.

**Impacts on 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-wage workers in the Earnin data in low- vs. high-rent ZIP codes over time in Figure 14c (orange 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, employment has recovered only partially, despite the fact that small business revenues have reverted to pre-COVID baseline levels. This result echoes the divergence between employment and revenue at the sectoral level documented above in Figure 7c and again raises the specter of a jobless recovery for low-wage workers.

In summary, the 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. This empirical impact contrasts with theoretical motivations for stimulus in response to shocks. In particular, Farhi and Werning (2016) show that optimal macroprudential policy involves a stimulus that increases spending in sectors and areas whose demand is depressed. In a frictionless model where businesses and workers could costlessly reallocate their capital and labor to other sectors, the reallocation of spending across sectors and areas 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, perhaps explaining why employment levels remained well below baseline even as total spending recovered after April 15.

IV.C Loans to Small Businesses

We now turn to evaluating the Paycheck Protection Program (PPP), a policy that sought to reduce employment losses by providing financial 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 (relative to pre-crisis levels).

According to the House Committee on Small Business (2020), the stated primary purpose of the PPP was to encourage businesses to maintain employment even as they lost revenue. The Small Business Administration (2020a) emphasized the employment impacts of the PPP as a key measure of the program’s success, noting that the PPP “ensure[d] that over approximately 50 million hardworking Americans stay[ed] connected to their jobs” based on self-reports of the number of jobs retained by firms that received PPP assistance.

Here, we study the marginal impacts of the PPP on employment directly using payroll data, 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. One important exception to this rule is the food services industry, which was treated differently because of the prevalence of franchises. We therefore omit the food services sector from the analysis that follows.

We estimate the causal effect of the PPP on employment rates at small businesses 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 15a plots the average change in employment rates (inferred from payroll deposits) relative to January for firms in the Paychex-

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43. The eligibility rules vary across industries, with some exceptions that allow larger firms to obtain loans. Using the national sample in ReferenceUSA data, restricted to workers in firms with 100-800 employees, we estimate that 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.
Earnin data employing 100-500 employees, which were eligible for PPP loans, vs. firms employing 501-800 employees, which were generally ineligible for PPP loans. To adjust for the fact that industry composition varies across firms of different sizes, we reweight by two-digit NAICS code so that the distribution of industries in the below-500 and above-500 employee groups match the overall distribution of industries in January 2020 when computing mean employment rates by firm size. We also residualize employment rates by county x wage quartile x week fixed effects, to account for the differential time patterns of employment rates by county and wage quartile shown in Section III.C.

Before April 3, trends in employment are similar among eligible vs. ineligible firms, showing that larger businesses provide a good counterfactual for employment trends one would have observed in smaller firms absent the PPP program. After April 3, employment continues to follow almost an identical trajectory in the treated group (<500 employees) and the control group, implying that the PPP program had little impact on employment at small businesses under the identification assumption that employment trends in the two groups would have remained similar absent the PPP. Notably, even after PPP implementation, employment levels remain well below baseline – showing that the program did not close a significant share of the loss in jobs resulting from the COVID shock.

Figure 15b plots the change in employment from January 8-January 31 to June 1-June 23 by firm size bin. The decline in employment is quite similar across firm sizes, and in particular is not markedly smaller for firms below the 500 employee eligibility threshold. Appendix Figure 14 shows that we obtain very similar results in the Earnin and Kronos data: the employment trajectory of ineligible firms closely matches the employment trajectory of eligible firms, both prior to and after the beginning of the PPP.

In Table 5, we quantify the impacts of the PPP using a set of regression models of the form:

$$\text{Emp}_{scqit} = \alpha_{cqt} + \delta \text{Eligible}_s + \gamma \text{Post-PPP}_t + \beta_{DD} \text{Eligible}_s \cdot \text{Post-PPP}_t + \varepsilon_{scqit},$$

where \(\text{Emp}_{scqit}\) is the change in employment within each firm size category \(s \times \text{county } c \times \text{wage quartile } q \times 2\)-digit NAICS industry \(i\) on week \(t\) cell, relative to January 4-January 31, \(\text{Eligible}_s\) is an indicator variable for whether firm size is 500 or fewer employees in the pre-COVID period.

44. Since Intuit consists primarily of firms with fewer than 20 employees, we omit it from this analysis.
45. Because of differences in the measurement of firm sizes in our data and the SBA data used to determine PPP eligibility (see below), there is no sharp discontinuity in eligibility at the 500 cutoff. Hence, we do not interpret this plot using an RD design, but rather view it as showing that our estimates are insensitive to the bandwidth used to define the treatment and control groups in the DD analysis.
Post-PPP is an indicator variable for the date being on or after 3 April 2020, and $\alpha_{cqt}$ represents a county-week-wage quartile fixed effect. We estimate this regression on the sample of firms with 100-800 employees using data from March 11 to August 15. We reweight by two-digit NAICS code so that the distribution of industries in the below-500 and above-500 employee groups match the overall distribution of industries in January 2020. We cluster standard errors at the county-by-industry-by-firm-size level to permit correlation in errors across firms and over time within counties and estimate the regression using OLS, weighting by the total number of employees in the cell from January 4-31, 2020.

Column 1 presents the baseline estimate obtained by estimating (1) of $\beta_{DD} = 1.71$ (s.e. = 1.98), an estimate that matches the figure plotted in Figure 15a and is similar to that obtained in confidential ADP data by Autor et al. (2020). The mean decline in employment among firms in the control group to August 15 was 18.7%, implying that the PPP saved 5.6% of the jobs that would otherwise have been lost. In Column 2, we reduce the bandwidth to focus more narrowly around the 500-employee size threshold; the estimates remains statistically indistinguishable from that in Column 1. Columns 3 and 4 replicate the specification in Column 1, using data only from Earnin and Kronos, respectively. Since these data sources only cover workers in the lowest wage quartile, we include fixed effects only at the county-by-week level. The estimates from these data sources are also similar to our baseline estimate.

Note that our difference-in-differences research design identifies the causal effect of the PPP on eligible firms under the assumption that the PPP did not have a causal effect on employment at PPP-ineligible firms. However, it is possible that the PPP reduced employment at ineligible firms (relative to the no-PPP counterfactual) through an employment substitution channel: ineligible firms might have hired workers laid off from eligible firms in the absence of the PPP. In the presence of such substitution, our DD estimate would overstate the causal effect of the PPP on employment at small businesses, providing an upper bound for the partial equilibrium impact of the PPP (ignoring general equilibrium effects that may have influenced consumer demand and employment at all firms).

Measurement Error in Firm Sizes. Our measures of firm size – which are based on employment levels in 2018 from the ReferenceUSA database for the Earnin sample, employment levels in 2019 from Dun & Bradstreet data for the Paychex sample, and 2019 employment for the Kronos data – do not correspond precisely to the measures used by the Small Business Administration to determine

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46. Our data use agreements do not permit us to report results based solely on Paychex data.
PPP eligibility. Such measurement error in firm size attenuates the estimates of $\beta_{DD}$ obtained from (1) relative to the true causal effect of PPP eligibility because some of the firms classified as having more than 500 employees may have actually received PPP (and vice versa). We estimate the degree of this attenuation bias by matching our data on firm sizes to data publicly released by the Small Business Administration (SBA) on a selected set of PPP recipients and assessing the extent to which firms are misclassified around the threshold. We estimate that our reduced-form estimates are attenuated by 35% based on this matched data (see Appendix D for details). Under standard assumptions required to obtain a local average treatment effect in the presence of non-compliance – no direct effect of being classified as having more than 500 workers independent of the PPP and a monotonic treatment effect – we can estimate the LATE of the PPP on employment rates by multiplying the raw estimates reported in Table 1 by 1.35 (Angrist, Imbens, and Rubin 1996). This gives us a final preferred point estimate for the effect of PPP eligibility on employment of 2.41 percentage points.

Costs Per Job Saved. Using SUSB data, we calculate that approximately 53.6 million workers work at firms eligible for PPP assistance, excluding firms in NAICS 72 (for details, see Appendix D). Under the assumption that the PPP’s effects on firms with between 100 and 500 employees are the same in percentage terms as the PPP’s effects on all eligible firms, our baseline estimates in the combined Paychex-Earnin data (Column 1 of Table 5), adjusted for attenuation bias, imply that the PPP saved 0.02 × 53.6M = 1.29 million jobs from April through August 15.47 Given a total expenditure on the PPP program of $486 billion through August 8 (excluding firms in food services), this translates to an average cost per job saved by the PPP of $377,000. Even at the upper bound of the 95% confidence interval for employment impact, we estimate a cost per job saved of $119,000. For comparison, mean annual earnings for workers at PPP-eligible firms are only $45,000.

Why did the PPP have relatively small effects on employment rates despite having a very high takeup rate among small businesses? One potential explanation is that the loans were taken by firms that did not intend to layoff many employees to begin with, i.e. firms that were inframarginal recipients of loans. Consistent with this hypothesis, 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

47. If the treatment effect of the PPP program on food services was the same in percentage terms as in other sectors, we estimate a total of 1.51 million jobs saved by the PPP.
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.

Although the PPP had modest impacts on employment – consistent with the estimates from studies using other data sources – the program may have had other important benefits, such as reducing the rate of small business closures. As emphasized by Hubbard and Strain (2020), if the disruption costs of closing and restarting businesses are sufficiently large, the PPP may have still have significant benefits over time – an important question for future research.

V Conclusion

Transactional data held by private companies have great potential for measuring economic activity, but to date have been accessible only internally within companies or through contracts to work with confidential microdata. In this paper, we have constructed a public platform to measure economic activity at a high-frequency, granular level using data from private companies. By carefully aggregating and masking the underlying micro data, we construct series that can be released publicly without disclosing sensitive information, yet are well suited to answer a variety of research questions.

We apply these new data to analyze the economic impacts of COVID-19. 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 is self-isolation driven by health concerns there was limited capacity to restore economic activity without addressing the virus itself, at least in the initial months after the pandemic began in mid-March. 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 most effective approach to mitigating economic hardship in the midst of a pandemic may be to provide benefits to those who have lost their incomes to mitigate consumption losses while investing in public health measures to
restore consumer confidence and ultimately increase spending.

We focused in this paper on the short-run economic consequences of the COVID-19 pandemic. However, such shocks can also have long-lasting scarring effects that warrant attention. Private sector data can be useful in measuring these impacts in real time as well. As an illustration, Figure 16 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 experienced a temporary reduction in learning on this platform when the COVID crisis hit and schools shifted to remote instruction, but soon recovered to baseline levels. By contrast, children in lower-income areas remained 50% below baseline levels persistently. Although this platform captures only one aspect of education, these findings raise the concern that pandemics may reduce social mobility and ultimately further amplify inequality by having particularly negative effects on human capital development for lower-income children.

More broadly, beyond its implications for the economics of pandemics, our analysis demonstrates two broad ways in which the public platform constructed here provides a new tool for empirical macroeconomics. First, the data can be used to learn rapidly from sub-national heterogeneity, as different places, sectors, and subgroups are often hit by different shocks and pursue different local policy responses. This approach can permit rapid diagnosis of the root factors underlying an economic crisis. Second, the data permit rapid policy evaluation – often within two or three weeks of implementation – opening a path to fine-tuning policy responses in an evidence-based manner.

The advantage of constructing a public platform to support such analyses rather than working directly with the underlying confidential data held by private sector firms is that it permits a much broader range of downstream work along these lines. For example, the data on the platform are now being used by local policymakers to inform local policy responses and forecast tax revenue impacts (e.g., Maine, Missouri, Kansas, and Texas). They are also being used by Congressional staff to design federal policies, e.g. predicting the impacts and costs of policies targeted based on business revenue losses or other outcomes (RESTART Act 2020). And they are being used by other researchers to analyze a broad range of issues: constructing more accurate price indices that account for changes in consumption bundles (Cavallo 2020), analyzing the effects of political views on economic outcomes (Makridis and Hartley 2020), estimating the effects of the Paycheck Protection Program on smaller firms’ employment decisions (Granja et al. 2020), and estimating the impacts of changes in unemployment benefits on aggregate spending (Casado et al. 2020).

The platform built here can be viewed as a 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 and Product accounts. Our analysis demonstrates that even this first prototype yields timely insights that are not apparent in existing data, suggesting that a broader, more refined platform that aggregates data from additional private companies has great potential for improving our understanding of economic activity and policymaking going forward.
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Online Appendix

A Automated Data Processing Pipeline

This appendix describes the automated pipeline we built to ingest raw data, process it to construct aggregate statistics, and then release those statistics publicly. This automated pipeline allows us to typically post updated statistics within one business day of receiving the raw data. By automating the data processing to the extent possible, we aim to post data as close to real-time as possible, while maintaining the quality of the data and minimizing the manual upkeep required. The primary source of lags in the posted data is therefore driven by lags in the underlying data generating processes: for example, card transactions can take up to a week to settle and employment income is typically paid in bi-weekly or monthly payrolls. We summarize our data engineering methods here for those who may be interested in setting up similar infrastructure in other contexts.

Step 1: Data Ingestion. In order to flexibly accommodate diverse data sources, with varying secure file transfer methods and update frequencies, we operate a server in the cloud that pulls updated data from each source on a regular interval. We receive data updates from private companies on a daily, weekly or monthly cadence. Many companies have unique policies and requirements for securing data transfers, so we write scripts to intake this data using a variety of secure file transfer services (e.g. Amazon S3 buckets and SFTP servers). We also download or scrape a variety of publicly available statistics from the web, such as unemployment insurance claims and COVID-19 case counts.

Three main challenges arise when handling this large volume of frequently updated data: storing, syncing, and version controlling the data we receive. We store all the raw data we receive as flat files in a data lake (an Amazon S3 bucket). We use object storage rather than a database or a more customized storage service (such as Git LFS) to minimize storage costs while maximizing our flexibility to ingest incoming data which arrives in numerous formats that may change over time. We version control each snapshot of the data we download within the same Git repository that stores our code using a tool called DVC (“Data Version Control”). DVC creates a pointer to a hash of the raw data for each data file or folder (in other words, a shortcut to the files in the data lake), which we version control in Git and update every time new data is downloaded. This associates each snapshot of data with the code that existed at the time it was processed, and allows us to easily roll back our code and data simultaneously to any prior state. DVC also facilitates syncing the raw data from the data lake by efficiently downloading the data that is associated with each
Step 2: Data Processing. For each dataset, we have an automated pipeline of programs that process and transform the raw data into the public datasets that we post online. We use an automated build tool to organize and execute this collection of programs. We mostly process the data using Stata and execute our automated builds within Stata using the `-project-` command developed by Robert Picard.

This data processing step generates two outputs: (1) a set of CSV files that contain all the data to be posted publicly and (2) a quality control report. The quality control report is a document that allows analysts to quickly assess any notable deviations in the data and determine whether the updated data require further review before being publicly released. Each report flags three types of changes that would require manual review: revisions made to previously posted data, large deviations in newly reported data, or newly missing data. The report also contains a series of tables and figures that preview the data and highlight any changes in the newly processed data.

Each time new data is ingested, the data processing step is run automatically. If it runs to completion, a Git pull request is generated with DVC pointers to the newly updated raw data alongside a link to the quality control report. If the data processing fails (for example, because the structure of the raw data has changed), an error report is generated. At this point, we pause and perform a manual review before posting the new data online. If the data processing failed or if any changes were detected in the quality control report that require further review, we manually investigate and write new code as needed, then re-process the data and inspect the updated quality control report before proceeding.

After reviewing and approving the quality control report, we merge the Git pull request containing the new data, which automatically triggers the final Data Release step. This manual review and approval is therefore the only manual step in the data processing pipeline.

Step 3: Data Release. Once the processed data is ready for release, our scripts automatically post the updated data to two public destinations. First, we sync the updated data into the database powering our online data visualization website built by DarkHorse Analytics (www.tracktherecovery.org). While doing so, we also update the “last updated” and “next expected update” dates on the website. Second, we upload the CSV files containing all the updated data to our “data downloads” page. The updated visualizations and data downloads are then both immediately available for public use.
B Consumer Spending Series Construction

This appendix provides greater detail on the construction of the consumer spending series using the Affinity Solutions data. We receive data from Affinity Solutions in cells corresponding to the intersection of (i) county by (ii) income quartile by (iii) industry by (iv) week, where cells where fewer than five unique cards transacted are masked. Income quartile is assigned based on ZIP code of residence using 2014-2018 ACS estimates of median household income along with population size. We use population weights when defining quartile thresholds so that each income quartile has the same number of individuals. ZIP code income quartile and county are both determined by the cardholder’s residence.

We adjust the raw data we receive from Affinity Solutions to address three challenges: (1) changes in the customer base over time, (2) a data quality issue which creates spurious increase in consumer spending, and (3) the fact that 2020 is a leap year.

Changing Customer Base. The raw Affinity data have discontinuous breaks caused by entry or exit of card providers from the sample. We identify these sudden changes systematically by regressing the number of transacting cards on our date variable separately for each year-by-county, and then implementing a Supremum Wald test for a structural break at an unknown break point.

950 counties have a structural break where the p-value of the test is less than $5 \times 10^{-8}$. For counties with only one break below this threshold, we correct our estimate using chain weighting. For this procedure, we first compute the state-level percent change from week-to-week excluding all counties with a structural break (using the national series for DC and states for which all counties have a structural break). If we identify a structural break in week $t$, we impute spending levels in weeks $t - 1$, $t$, and $t + 1$ levels because we don’t know the precise date when the structural break occurred (e.g. it might have occurred on the 2nd day of $t - 1$ or the 6th day of $t$). To keep our estimates conservative, when there is a change in coverage we adjust the series to be in line with the lower level of coverage. For example, say a county has $n$ active cards, up until week $t$, when the number of cards in the county increases to $3n$. In week $t - 2$, the county would have a level of $n$ cards, its reported value. In week $t - 1$, if counties in the rest of the state had a 5% increase in the number of cards, we would impute the county with a break to have a level of $1.05n$ cards in week $t - 1$. In week $t$, if counties in the rest of the state had a 10% increase in the number of cards, we would impute $t$ to have a level of $(1.10) \times (1.05n) = 1.155n$. Likewise, if counties in the rest of the state had an 8% decrease in the number of cards in week $t + 1$, we would impute $t + 1$ to have
a level of \((0.92) \times (1.155n) = 1.0626n\). At this point, state-level fluctuations no longer impact the series, and we use the reported percent change each week to adjust this number for card coverage. For counties with multiple breaks, we omit the county from our series; this happens in 98 cases. Furthermore, we do not remove any counties where the structural break occurred between March 10th and March 31st of 2020 because the consumer spending response to the coronavirus epidemic was so strong that in many places it could be mistaken as a structural break. We also impute the number of cards by averaging the preceding and succeeding week for the week of Christmas, since holiday spending spikes are also sometimes mistaken for a structural break.

We implement a structural-break correction for three counties: Philadelphia County, Pennsylvania (county fips of 42101); Washington, District of Columbia (11001); Jefferson County, Kentucky (21111). For Philadelphia and Washington, we implement a correction by estimating a regression discontinuity at the date of the break, and then adding the RD estimate to the series prior to the structural break. The structural break in Jefferson county occurs on January 7th of 2020, and so there are not enough days on the left-hand side to implement the RD correction. Consequently, we assign the January 7th value to each day between January 1st and January 6th.

*Spurious Increases in Consumer Spending.* There is an unreasonably large spike in consumer spending between January 15th, 2019 and January 17th, 2019 that is not found in other data series, and so we believe it is not representative of true economic activity. This spike in national consumer spending is not driven by specific regions nor sectors. We deal with this data quality issue by replacing each impacted day with the average spending on \(t-7\), \(t+7\), and \(t+14\), where \(t\) is the impacted day. A similar problem arises in the “Accommodations and Food Services” sector in Richmond City County, Virginia where spending increases by over 80 times on May 23rd, 2019 relative to to nearby days. We implement a similar procedure replacing the impacted day with the average spending on \(t-14\), \(t-7\), \(t+7\), and \(t+14\), where \(t\) is the impacted day.

*Treatment of February 29, 2020.* We divide the daily value for February 29, 2020 by the average value of February 28, 2019 and March 1, 2019.

**C Small Business Revenue and Small Businesses Open Series Construction**

This appendix details our methodology for constructing the Small Business Revenue and Small Businesses Open series, using data from Womply.

*Initial Construction.* We receive Womply data on total revenue and number of open businesses at the date x ZIP code x firm category level. Small businesses are defined as businesses with annual
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 more than one transaction in 2 out of the 3 months. We convert Womply’s firm categories to two-digit NAICS codes using an internally generated Womply category-NAICS crosswalk, and then aggregate to NAICS supersectors. We measure small business revenue as the sum of all credits (generally purchases) minus debits (generally returns). We define small businesses as being open on a given day if they have at least one transaction in the previous three days. We exclude counties with a total average revenue of less than $250,000 during the pre-COVID-19 period (January 4-31).

We crosswalk from ZIP codes to counties using the geographic definitions described in Appendix G to aggregate the series to the county, state and national level. We then collapse the Womply data to aggregate spending and total small businesses open within each day x NAICS Supersector x geography x ZIP income quartile, creating ZIP income quartiles as described in Appendix B. We take a seven-day lookback moving average of each series, and norm each series relative to its average level over the period January 1-28.

Masking. To preserve the privacy of firms in the data and to avoid displaying noisy estimates for small cells, we mask Womply series that report less than $250,000 in total revenue during the base period of January 4-31. In addition, Womply adds merchants and an imputed revenue quantity such that every cell with 1 or 2 merchants has no fewer than 3 merchants. This imputation has the result of dampening the effect of any declines that would otherwise place the number of merchants in a cell at 1 or 2, dampening the effect of any increase from 1 or 2 merchants to 3 merchants, and enhancing the effect of any increase from 0 merchants to 1 or 2 merchants. We minimize the impact of this masking by processing data at the highest level of aggregation available. When possible, we correct for this imputation by comparing similar datasets.

Anomalous Data. Our quality-control process checks for anomalous variations in the Womply raw data. There are several cases of single-day spikes of positive or negative revenue within a given firm category x ZIP code. We treat these cases as outliers, and replace the revenue value with the revenue for the same category x ZIP code from the previous week, unless that is also an outlier or a holiday, in which case we substitute zero revenue.48

48. More generally, negative revenue may appear in the Womply data due to returns and refunds. There are a number of cases of observed negative revenue, especially during March 2019, due to consumers seeking returns or refunds on certain products. We include these cases in the Womply series, but exclude large single-day occurrences
Delayed Processing of Payments. Due to differences in the speed at which data providers share their data with Womply, the most recent date as of a given data refresh is typically incomplete. If left unaddressed, there would appear to be a decline in small business revenue and small businesses open in the most recent data. We generally receive Womply’s data a week after the reported transactions. As a conservative approach, we exclude the four most recent days in the data we publish.

D Employment Series Construction

This appendix provides further details on how we construct various employment series analyzed in the paper.

Paychex Employment Series. We receive Paychex data at the county x industry x 2019 hourly wage quartile x 2019 firm size bin x pay frequency x week of payroll processing level. Salaried employees’ wages are translated to hourly wages by dividing weekly pay by 40 hours. Firms that are new in 2020 are assigned a size of 0 (as they had no employees in 2019). Firm size at multi-establishment firms is calculated by summing establishment-level employment at each establishment within the same Dun & Bradstreet parent firm. Since we seek to measure private sector employment, we exclude workers employed in public administration and those with an unclassified industry (which each represent 0.8% of workers as of January 2020). We restrict the sample to workers with weekly, bi-weekly, semi-monthly or monthly pay frequencies; these workers represent over 99% of employees in the Paychex data.

We begin by creating a daily series of paychecks processed on each date by linearly interpolating daily values between each week in each county x 2-digit NAICS code x 2019 hourly wage quartile x 2019 firm size bin x pay frequency cell. In order to construct a series of employment as of each date, rather than paychecks being processed as of each date, we take two steps.

First, we construct a series of pay periods ending as of each date. We take a separate approach for paychecks following regular weekly cycles (i.e. weekly and bi-weekly paychecks) and for paychecks following a cycle based on fixed calendar dates (i.e. semi-monthly and monthly paychecks). For weekly and bi-weekly payfrequencies, we use data provided by Paychex on the distribution of the number of days between a worker’s pay date and the last date in the worker’s pay period (i.e., date at which payroll is processed – last date in pay period), for weekly and bi-weekly payfrequencies.
to distribute paychecks to the last date of the corresponding pay period, treating the distribution of (date at which payroll is processed – last date in pay period) as constant across geographies and NAICS codes. For monthly and semi-monthly payfrequencies, where cycles regularly occur on fixed calendar dates (e.g. the 15th and 30th of each month for semi-monthly paycycles), we assume that the last date within each pay period is the closest preceding calendar date that is the 15th or the 30th day of the month (semi-monthly paycycles) or the 30th day of the month (monthly paycycles). In each case, we interpolate values around public holidays.

Second, to construct a series of employment as of each date, we record a worker as being employed for the full duration of the paycycle up until the last date in their pay period, under the assumption that workers are employed for each day during their pay period. We then collapse the data to the level of county x industry x 2019 hourly wage quartile x 2019 firm size x pay x date.

Finally, we take steps to prevent the introduction of new Paychex clients from artificially creating breaks in the employment series at smaller levels of geography. We begin by calculating the share of wage quartile employment in January 2020 accounted for by each industry x firm size bin within each county quartile group. Next, we calculate the change in employment relative to January for each county x quartile x industry x firm size bin, and multiply this change by the share of total employment in the respective county quartile groups, creating an employee-weighted employment series for each county x quartile x industry x firm size bin cell. We denote a county x quartile x industry x firm size bin cell as an “influential cell” if the county contains 100 or fewer than unique county x quartile x industry x firm size bin cells, and the cell accounts for over 10% of employment in the county quartile group at any date in 2020, or if the county contains greater than 100 unique county x quartile x industry x firm size bin cells, and the cell accounts for over 5% of employment in the county quartile group at any date in 2020. We drop influential cells that record a change in employment relative to January 2020 of at least +50% on any date, on the basis that such a trend likely arises due to changes in Paychex’s client base rather than true employment changes. Of the 126,595 unique county x quartile x industry x firm size bin cells in the Paychex data, fewer than 5,000 cells are affected by this procedure.

Earnin Employment Series. We obtain anonymized microdata at the worker level from Earnin. We construct our analysis sample by restricting the sample to workers who are paid on a weekly or bi-weekly paycycle; these categories account for 92% of paychecks. We also restrict the sample to workers who are active Earnin users, with non-missing earnings and hours worked over the last 28 days. Next, we exclude workers whose reported income over the prior 28 days is greater than
$50,000/13 (corresponding to an income of greater than $50,000 annually).

We then restrict the sample to workers who are in paid employment. Users may continue to use Earnin after they have been laid off; we exclude payments which Earnin classifies as unemployment payments, either based on the user’s registration with Earnin as being unemployed, or based on the string description of the transaction. Where a user has previously been unemployed, but stops receiving unemployment checks after a certain date, we treat the user as having been re-employed if they receive a payment amount of $200 within the two weeks following their last unemployment check. Using this approach, we find that 90% of Earnin users are re-employed within fourteen days of receiving their last unemployment check.

We use external data sources to gather further information on firm size and industry. To obtain information on industry, we use a custom-built crosswalk created by Digital Divide Data which contains NAICS codes for each employer in the Earnin data with more than ten Earnin users. To obtain information on firm size, we crosswalk Earnin employers to ReferenceUSA data at the firm location level by spatially matching Earnin employers to ReferenceUSA firms. We begin by geocoding Earnin addresses to obtain latitudes and longitudes for each Earnin employer. We then remove common prefixes and suffixes of firm names, such as “inc” and “associated”. Next, we compute the trigram similarities between firm names for all Earnin and ReferenceUSA firms within twenty-five miles of another. We then select one “match” for each Earnin firm within the ReferenceUSA data, among the subset of firms within one mile. We first match Earnin employers to ReferenceUSA firms if the firms are within one mile of one another, and share the same firm name. Second, where no such match is available, we choose the geographically closest firm (up to a distance of one mile) among all firms with string similarities of over 0.6. Third, where no such match is available, we match an Earnin employer to the ReferenceUSA employer within twenty-five miles with the highest trigram string similarity, provided that the employer has a trigram string similarity of 0.9. We then compute the modal parent-firm match in the ReferenceUSA data for each parent-firm grouping in Earnin. Where at least 80% of locations within a parent-firm grouping in Earnin are matched to a single parent-firm grouping in the ReferenceUSA data, we impute that parent-firm to every Earnin location. In total, we match around 70% of Earnin employers to ReferenceUSA firms.

Earnin data are observed at the ZIP code level. We crosswalk from ZIP Codes to counties using the geographic definitions described in Appendix G to aggregate the series to the county, state and national level.
We construct an employment series in the Earnin data from our analysis sample as follows. In the paycheck-level data, we observe the worker’s paycycle frequency. We use paycycle frequency to construct an employment series by assuming that workers are employed throughout the full duration of their paycycle. That is, we assume that a worker paid every two weeks has been fully employed for the two weeks prior to receiving their paycheck. To account for the delay in receipt of paychecks, we shift the Earnin series back by one week. We then take the count of employed individuals across the Earnin sample as our measure of employment. We take a 7-day moving average to form our Earnin employment series, and express the series as a change relative to January 4-31.

We also receive transaction-level data on all payments received, which we use to measure the receipt of stimulus checks. We classify a transaction as a stimulus check if the transaction (1) has a string description containing words indicating that it is a stimulus transaction, such as “IRS” or “Economic Impact Payment”, (2) is of an amount that could be received as stimulus under the CARES Act, and (3) was received after 10 April 2020.

Comparison of Construction of Earnin and Paychex Employment Series to Cajner et al. (2020) ADP Series. In both the Earnin and Paychex datasets, we construct daily employment series using data on paychecks. Our treatment of paycheck data is similar to the treatment of paycheck data in Cajner et al. (2020), who estimate employment based on paycheck deposits using firm-level data from ADP. Cajner et al. (2020) define employment within a week as the count of paychecks that are processed during that week. For businesses which do not process payroll every week (e.g. businesses whose workers are paid every two weeks), Cajner et al. (2020) impute the count of paychecks in the “missing” week using the number of paychecks in the next period in which the businesses processes payroll.

Because the Earnin data are available at the worker level, we do not observe whether a business as a whole does not process payroll every week. However, under the assumption that all workers within a business are paid on the same paycycle, our worker-level approach of distributing paychecks uniformly over the paycycle matches the approach in Cajner et al. (2020) of imputing employment based on the next week in which paychecks are observed. The two primary differences between our treatment of paycycles and the treatment in Cajner et al. (2020) are that we use a 7-day moving average, whereas Cajner et al. (2020) use a 14-day moving average, and that we treat that the last date of the employment period as seven days prior to the receipt of the paycheck, whereas Cajner et al. (2020) observe the pay period directly. The seven-day lag accounts for delays between the end of a worker’s pay period, which is the event observed in Cajner et al. (2020), and the date on
which paychecks are received by workers, which is the event observed in the Earnin data.

Because the Paychex data are not available at the firm level, we are not able to directly implement the approach in Cajner et al. (2020) of imputing employment using the count of paychecks in the “missing” week for firms that do not process payroll on a weekly basis. Instead, we make the conceptually similar assumption that workers are employed throughout the full duration of their paycycle, such that we can infer the full set of dates on which an individual worked by observing the last date of each of their pay periods and their pay frequency. Under the assumptions that all workers within a given firm are paid according to the same paycycle, our approach of inferring employment based on last date of pay period matches the approach in Cajner et al. (2020) of imputing employment based on the next week in which paychecks are observed. A further difference is that pay period is observed in Cajner et al. (2020); by contrast, in the Paychex data, pay periods are imputed using payroll processing date and the distribution of (payroll processing date – last date in pay period). Finally, Cajner et al. (2020) use a 14-day moving average, whereas we use a 7-day moving average.

**Combined Employment Series.** We combine Paychex, Earnin, and Intuit data to construct our primary employment series. Because Paychex covers all sectors and wage levels fairly comprehensively, we use it as the base for the combined employment series. We then use Earnin and Intuit to refine the series in cells represented by those datasets.

Because Earnin best represents workers in the bottom wage quartile, we combine Earnin data with Paychex data to construct employment estimates for the bottom wage quartile. To do so, we first calculate total employment levels within each two-digit NAICS code by firm size by geography cell by summing employment levels for bottom-wage-quartile Paychex workers and Earnin workers. We place the majority of the weight on Paychex, with greater weight on Earnin in geographic areas and in NAICS codes where it has greater coverage; the exact weights are undisclosed to protect privacy. These combined Paychex+Earnin values are used to assess the effects of the Paycheck Protection Program. In order to create the other analysis datasets, we then collapse across firm sizes and compute mean levels of employment for bottom-wage-quartile workers by geography by taking a weighted average of the NAICS-by-geography combined estimates, weighting by the January Paychex NAICS shares for bottom-wage-quartile workers in each geography.

Next, we combine Intuit with the Paychex+Earnin data. Intuit provides us with overall national

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49. We convert the weekly Paychex data to daily measures of employment by assuming that employment is constant within each week.
industry shares as of 2019, but does not release data broken down by wage level or industry. We therefore must effectively impute the Intuit data to wage-industry cells in order to combine it with the Paychex data. To do so, we assume that any differences in employment between Intuit and Paychex are constant (in percentage terms, relative to the January baseline) by industry and wage quartiles within a given geography and month. We reweight the Paychex data to match the national Intuit industry distribution and compute the percentage difference between the employment decline in the reweighted Paychex data and the Intuit data in each geography-month cell. We then apply this correction factor to each wage-industry cell in the Paychex data to obtain imputed values by wage and industry for the relevant industries covered by Intuit. For instance, if Intuit exhibits a 5% larger employment decline than the reweighted Paychex series in Manhattan in April, we would impute a value for each wage-by-industry cell covered in the Intuit data that 1.05 times the decline observed in Paychex for that cell. When constructing the series that we use to analyze the effects of the Paycheck Protection Program, we exclude the Intuit data, since Intuit primarily consists of small firms.

Finally, we take a weighted average of the Paychex data and the imputed Intuit data in each industry to compute the final combined series. We place the majority of the weight on Paychex, with greater weight on Intuit in sectors where it has greater coverage; the exact weights are undisclosed to protect privacy.

The preceding steps yield combined data at the industry by wage quartile for each geography (county, state, and national). We construct aggregate estimates across industries, wage quartiles, and overall by aggregating these estimates using Paychex January employment weights.\footnote{In a few cases, Earnin and Intuit data do not provide coverage for a given geographical region or industry; we suppress such cells. We also suppress cells in which Paychex records less than an average of 100 total monthly employees in the second half of 2019 at the industry by geography or income quartile by geography level. When aggregating employment series to the geographical level without breakdowns by industry or wage quartile, however, we use data from all cells, without masking.} We report seven-day moving averages of these series, expressed as a percentage change relative to January 4-31. We construct a series for average total earnings analogously, using total earnings instead of total employment.

To construct employment predictions for the most recent weeks, we regress the combined employment series for each quartile of workers on the Kronos series for the same week, the corresponding quartile of the Paychex weekly series for the same week, as well as the three prior weeks of Paychex weekly data. We then use these regression coefficients combined with the most recent Kronos and Paychex weekly data to create a series of predicted employment rates for workers in
each wage quartile.

**ZIP Code-Level Low-Income Employment Series.** As ZIP code is not observed in Paychex and Intuit, we separately construct ZIP code-level employment using the Earnin data only. We construct our analysis sample as above. To account for the noisier data at the ZIP code-level, we norm the ZIP code-level changes relative to a pre-period of January 5 - March 7. We suppress estimates for ZIP codes with fewer than 100 worker-days observed over this period.

**Assessing Mismeasurement of Firm Sizes using SBA data.** We assess the degree of misclassification of PPP eligibility in our sample by merging publicly available data on PPP recipients from the SBA to data on firm sizes from both ReferenceUSA and Dun & Bradstreet, which form our measures of firm size in the Earnin and Paychex data, respectively. To construct SBA data on PPP recipients, we restrict attention to firms receiving loans of at least $150,000, as the names and addresses of these firms are publicly available from the SBA. We first geocode addresses recorded in SBA, ReferenceUSA, and Dun & Bradstreet data to obtain a latitude and longitude for each firm. We then compute the trigram similarities between firm names for all SBA and ReferenceUSA firms, and all SBA and Dun & Bradstreet firms within twenty-five miles of another. We then select one “match” for each PPP recipient from both the ReferenceUSA and Dun & Bradstreet data, among the subset of firms within twenty-five miles, following the procedure described above in our merge of Earnin data to ReferenceUSA data. For firms with loans of above $150,000, exact loan size is not observed; we impute loan size as the midpoint of loan range.

We use the merged SBA-ReferenceUSA and SBA-Dun & Bradstreet data to estimate the first-stage of our difference-in-differences design, i.e. how much more PPP assistance firms classified as having 100-500 employees in our sample received relative to those classified as having more than 500-800 employees. To do so, we stack the datasets and use the same weights used when constructing the combined employment series. The SBA released firm names and ZIP codes of PPP recipients receiving over $150,000 in loans, which represent 72.8% of total PPP expenditure. Of the roughly 660,000 PPP recipients of these loans, we merge around 60% of firms and 62% of total expenditure to firm size data. In this matched subset, we find that mean PPP expenditure per worker is $2,303 for firms we classify as having 100-500 employees and $586 per worker for firms with 500-800 employees (excluding firms in the food services industry). Given that we match only 62% of the publicly available PPP expenditure to our data and the publicly available data covers only 73% of total PPP expenditure, this implies that firms measured as having 100-500 employees in our sample received \( \frac{2303}{0.62 \times 0.73} = 5,090 \) of PPP assistance per worker, while firms
with 500-800 employees received $5,092 in PPP assistance per worker. We calculate that PPP assistance to eligible firms with between 100 and 500 employees (excluding NAICS 72) is $5,092 per worker on average. Hence, firms with 501-800 workers in the ReferenceUSA-Dun & Bradstreet data (the control group) were effectively treated at an intensity of $1,290 = 25.3\%$, whereas firms with 100-500 workers in the ReferenceUSA-Dun & Bradstreet data (the treatment group) were treated at an intensity of $\frac{5,090}{5,092} = 100\%$.

Inflating our baseline reduced-form estimates by $\frac{1}{(1 - 0.253)} = 1.35$ yields estimates of the treatment effect of PPP eligibility adjusted for attenuation bias due to mismeasurement of firm size.

Calculating PPP Expenditures Per Worker. Using Statistics of U.S. Businesses (SUSB) data, we calculate that approximately 62.4 million workers work at firms eligible for PPP assistance (53.7 million workers excluding those in the food services industry, NAICS 72). To compute total PPP expenditure, we first use publicly released data on loan recipients to calculate that 92.1\% of total PPP expenditure was received by non-NAICS 72 firms. We then multiply this share by total PPP expenditure as of August 8 to reach an estimate of $486$ billion in non-NAICS 72 firms.

E  Zearn Data

In this appendix, we provide additional details about how we define Zearn indices of math progress and engagement.

Masking. 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 have at least 5 students using Zearn Math for at least one week from January 6 to February 7.

School Breaks. To reduce the effects of school breaks, we replace the value of any week for a

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51. This calculation assumes that the degree of misclassification of eligibility among identifiable PPP recipients matches the degree of misclassification of eligibility in the broader ReferenceUSA sample.

52. To compute this statistic, we first calculate the share of total loan amounts received by non-NAICS 72 firms in the publicly released SBA data. We begin by imputing precise loan amount as the midpoint of minimum and maximum of loan range, where precise loan amount is not released. We then calculate the share of loans in firms with firm size between 100 and 500, in NAICS codes other than NAICS 72, under the assumption that our merge rate is constant by firm size. Using this approach, we calculate that 13.1\% of PPP loan spending was allocated to non-NAICS 72 firms with between 100 and 500 employees. We then rescale the total PPP expenditure to the end of June, $521$ billion, by $0.131$ to arrive at an estimate of $69.22$ billion in PPP loan spending to non-NAICS 72 firms with 100-500 employees. Finally, we divide $69.22$ billion by the number of workers at non-NAICS 72 firms with 100-500 employees to arrive at an estimate of loan spending per worker.
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.

F Public Data Sources

This appendix provides further details on our use of public data sources on unemployment benefits, COVID-19 incidence, and mobility measures.

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.\textsuperscript{53} We also report daily state-level data on the number of tests performed per day per 100,000 people from the COVID Tracking Project.\textsuperscript{54} 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

\textsuperscript{53} 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.

\textsuperscript{54} 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.
construct measures of daily time spent at parks, retail and recreation, grocery, transit locations, and workplaces. 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 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.

G  Key Dates and Geographic Definitions

In this appendix, we provide additional details about how we define key dates and geographic units 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:

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55. 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.
"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 and verified from the New York Times reopening data, available here, and hand-collection from local news and government sources where needed.

- Stay-at-home order ends: Sourced and verified from the New York Times reopening data, available here, and hand-collection from local news and government sources where needed. Defined as the date at which the state government lifted or eased executive action or other policies instructing residents to stay home. We code “regional” and “statewide” expiry of stay-at-home orders separately. A “regional” expiration of a stay-at-home orders occurs when a stay-at-home order expires in one region within a state, but not everywhere within the state. A “statewide” expiration of a stay-at-home order occurs when a stay-at-home order first expired throughout a whole state, either due to a statewide change in policy, or due to the stay-at-home order in each county having expired.

- Partial business reopening: Sourced and verified from the New York Times reopening data, available here, and hand-collection from local news and government sources where needed. Defined as the date at which the state government allowed the first set of major industries to reopen (non-essential retail or manufacturing in nearly every case). Deviations from the New York Times reopening data are deliberate and usually involve our regional classification or our inclusion of manufacturing. A “regional” reopening occurs when businesses are allowed to reopen in one region within a state, but not everywhere within the state. A “statewide” reopening occurs when businesses are allowed to reopen throughout a whole state, either due to a statewide change in policy, or due to restrictions being eased in each individual county.

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 7. 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. We use the Uniform Data Systems (UDS) Mapper to crosswalk from ZIP codes to ZCTAs.
### Table 1
Changes in Consumer Spending by Sector and Income Quartile

<table>
<thead>
<tr>
<th>Dep. Var.: Change in Mean Consumer Spending Per Day ($ Billions) Relative to January 2020</th>
<th>Level of Mean Consumer Spending Per Day ($ Billions)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Change as of April 8-14</td>
</tr>
<tr>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Pooled, All Income Quartiles</td>
<td>-$7.8</td>
</tr>
<tr>
<td>Low-Income</td>
<td>-$1.0</td>
</tr>
<tr>
<td></td>
<td>(12.77%)</td>
</tr>
<tr>
<td>Q2</td>
<td>-$1.6</td>
</tr>
<tr>
<td></td>
<td>(20.34%)</td>
</tr>
<tr>
<td>Q3</td>
<td>-$2.1</td>
</tr>
<tr>
<td></td>
<td>(26.75%)</td>
</tr>
<tr>
<td>High-Income</td>
<td>-$3.2</td>
</tr>
<tr>
<td></td>
<td>(40.14%)</td>
</tr>
</tbody>
</table>

**Panel A: Consumer Spending by Income Quartile**

**Panel B: Consumer Spending by Sector**

<table>
<thead>
<tr>
<th>Overall Sector Decomposition</th>
</tr>
</thead>
<tbody>
<tr>
<td>Durable Goods</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Non-Durable Goods</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Remote Services</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>In-Person Services</td>
</tr>
<tr>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>In-Person Services Sub-Sector Decomposition</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hotels &amp; Food</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Transportation</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Health Care</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Recreation</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Other In-Person Services</td>
</tr>
<tr>
<td></td>
</tr>
</tbody>
</table>

**Notes:** This table shows changes in and levels of national mean daily spending between 2019 and 2020 for selected dates, ZCTA income quartiles, and categories of goods. Panel A shows changes in and levels of consumer spending by income quartile, in the Affinity spending data. Column (1) shows the change in consumer spending as of the week April 8-14. The first row shows the change in national spending on all goods. To compute this change, we begin by calculating total daily spending in the Affinity Solutions data for each day in 2019 and 2020. We then scale the 2020 (2019) values of daily total spending by multiplying by the ratio of January 2020 total spending for components of PCE that are likely captured in credit/debit card spending (shown in the last bar of Figure 1a) to the January 2020 (2019) total spending in the Affinity data. We then calculate the change in total spending in the card spending data between 2019 and 2020 for the period April 8-14 as ((Spending in April 8 through April 14 2020) - (Spending in April 8 through April 14 2019)) - ((Spending in January 4 through January 31 2020) - (Spending in January 4 - January 31 2019)). The second, third, fourth and fifth rows of Panel A replicate the first row, restricting to ZCTAs in a given income quartile. The decline in spending in each income quartile is expressed in percentage terms as a share of the national decline in brackets underneath each row. Columns (2) and (3) replicate column (1), calculating the change as of June 8-14 (column (2)) and July 8-14 (column (3)) respectively. Column (4) shows mean daily national spending over the period January 4-31 2020 for each income quartile. Panel B replicates Panel A in the Affinity data for various categories of goods instead of income quartiles. The first four rows of Panel B show the change in consumer spending across four broad categories of goods: durable goods (row 1), non-durable goods (row 2), remote services (row 3) and in-person services (row 4). For details of the definitions of these categories, see notes to Figure 2. The change in consumer spending summed across these four categories may not add to 100% because of spending on uncategorized goods. Rows 5-9 show the change in consumer spending within five components of in-person services: hotels and food (row 5), transportation (row 6), health care (row 7), recreation (row 8), and other in-person services (row 9). Data source: Panels A-B: Affinity Solutions.
### Table 2
Association Between ZIP code Rent and Changes in Business Revenue and Employment

#### Panel A: Changes in Business Revenue

<table>
<thead>
<tr>
<th>Dep. Var.</th>
<th>% Change in Small Business Revenue</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>Median 2BR Rent (per thousand dollars)</td>
<td>-13.86</td>
</tr>
<tr>
<td>Log of Density of High Wage Workers</td>
<td>-2.39</td>
</tr>
<tr>
<td>County FEs</td>
<td>X</td>
</tr>
<tr>
<td>Observations</td>
<td>18269</td>
</tr>
</tbody>
</table>

#### Panel B: Changes in Low-Wage Employment

<table>
<thead>
<tr>
<th>Dep. Var.</th>
<th>% Change in Low-Wage Employment</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>Median 2BR Rent (per thousand dollars)</td>
<td>-12.02</td>
</tr>
<tr>
<td>Log of Density of High Wage Workers</td>
<td>-1.02</td>
</tr>
<tr>
<td>County FEs</td>
<td>X</td>
</tr>
<tr>
<td>Observations</td>
<td>15685</td>
</tr>
</tbody>
</table>

Notes: This table shows OLS regressions of average percentage changes in small business revenue by ZIP code (using Womply data) and small business low-wage employment (using Earnin data) on average ZIP code median two-bedroom rent. Standard errors are reported in parentheses. The dependent variable is scaled from 0 to 100, such that, for example, the coefficient of -13.86 in Column (1), Panel A implies that a $1000 increase in monthly two-bedroom rent is associated with a 13% larger drop in total revenue. The dependent variable in Panel A is the change in small business revenue between January 1-28 and March 25-April 14. The dependent variable in Panel B is the change in low-wage employment at small businesses between January 4-31 and April 8-28. In both cases, each column is estimated at the ZIP code level. Column (1) shows the baseline regression without any controls while Columns (2) adds county fixed effects and column (3) adds county fixed effects and the log of the density of high wage workers. Data sources: Panel A: Womply; Panel B: Earnin
### Table 3
Causal Effects of Re-Openings on Economic Activity: Event Studies

<table>
<thead>
<tr>
<th>Dep. Var.:</th>
<th>Spending (%)</th>
<th>Employment (%)</th>
<th>Low-Wage Employment (%)</th>
<th>High-Wage Employment (%)</th>
<th>Merchants Open (%)</th>
<th>Time Away From Home (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Outcome:</td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>Dif-in-Dif Effect of Reopening:</td>
<td>1.45 (0.44)</td>
<td>1.47 (0.53)</td>
<td>0.62 (0.51)</td>
<td>0.53 (0.87)</td>
<td>0.51 (0.55)</td>
<td>0.74 (0.89)</td>
</tr>
<tr>
<td>State-Week</td>
<td>208</td>
<td>300</td>
<td>208</td>
<td>264</td>
<td>248</td>
<td>248</td>
</tr>
<tr>
<td>Weeks on either side of reopening included:</td>
<td>2 3</td>
<td>2 3</td>
<td>2 2</td>
<td>2 3</td>
<td>2 3</td>
<td></td>
</tr>
<tr>
<td>Decline (Peak to Trough):</td>
<td>34.6</td>
<td>23.9</td>
<td>37.7</td>
<td>14.0</td>
<td>45.3</td>
<td>23.5</td>
</tr>
</tbody>
</table>

Notes: This table displays changes to outcomes as a result of reopenings using an event study design looking at states that reopened non-essential businesses between April 20 and April 27. Each state that reopens is matched to multiple controls states that did not reopen but had similar trends of the outcome variable during the weeks preceding the reopening. Standard errors are reported in parentheses. Columns (1) - (2) look at changes in consumer spending using seasonally-adjusted data from Affinity Solutions. Consumer spending is normalized by its level over the period January 4-31, and seasonally adjusted using 2019 data, as described in Section II.A. Columns (3) - (4) look at employment data from Paychex, Intuit and Earnin (see Appendix D for details). Columns (5) and (6) restrict employment to workers in the bottom and top quartile of earnings respectively. Columns (7) and (8) look at the businesses that are open, using data from Womply. Columns (9) and (10) look at time spend away from home, using data from Google. Columns (2), (4), (8) and (10) estimate the coefficient on the reopening using the three weeks preceding and following each reopening; all other columns use two weeks instead. Columns (1), (3), and (7) correspond to the specifications displayed in Figures 12B, 12C, and 12D respectively. Data sources: Affinity Solutions, Paychex, Intuit, Earnin, Womply, Google Mobility.
### Table 4
Regression Discontinuity Estimates of Stimulus Payments on Spending

<table>
<thead>
<tr>
<th></th>
<th>Panel A: Impact of Stimulus Payments on Consumer Spending</th>
<th>Panel B: Impact of Stimulus Payments on Revenue</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) Q1 ZIP codes</td>
<td>(2) Q1 ZIP codes</td>
</tr>
<tr>
<td></td>
<td>(3) Q4 ZIP codes</td>
<td>(4) Q4 ZIP codes</td>
</tr>
<tr>
<td>Dep. Var.:</td>
<td>Spending</td>
<td>Revenue</td>
</tr>
<tr>
<td>RD Effect of</td>
<td>0.26 (0.07)</td>
<td>0.38 (0.10)</td>
</tr>
<tr>
<td>Stimulus:</td>
<td></td>
<td>0.09 (0.04)</td>
</tr>
<tr>
<td>Window:</td>
<td>April 1 - April 30</td>
<td>April 7 - April 21</td>
</tr>
<tr>
<td></td>
<td></td>
<td>April 1 - April 30</td>
</tr>
<tr>
<td></td>
<td></td>
<td>April 7 - April 21</td>
</tr>
<tr>
<td></td>
<td></td>
<td>April 1 - April 30</td>
</tr>
<tr>
<td></td>
<td></td>
<td>April 7 - April 21</td>
</tr>
</tbody>
</table>

#### Notes:
This table shows regressions of changes to outcomes as a result of stimulus payments using a regression discontinuity design around the April 14 payment date. Standard errors are reported in parentheses. Panel A looks at changes in consumer spending using seasonally-adjusted national by income quarter data from Affinity Solutions (where the seasonal adjustment consists of dividing spending on each day by the average level of spending in January and then 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. Columns (1)-(2) looks at changes to spending for cardholders living in ZIP codes in the bottom quartile of the distribution of ZIP code median household income (based on data from the 2014-2018 ACS), while Columns (3)-(4) look at cardholders living in the quartile of ZIP codes with the highest median incomes. This panel corresponds to the specifications displayed in Figures 12B and 12C. Panel B looks at changes in consumer spending using seasonally-adjusted data from Womply (cleaned the same way as the Affinity Solutions data). Columns (1)-(2) looks at changes to business revenue for ZIP codes in the bottom quartile of the distribution of ZIP code median rent for a two bedroom apartment (based on data from the 2014-2018 ACS), while Columns (3)-(4) look at businesses in the quartile of ZIP codes with the highest median two bedroom rents. This panel corresponds to the specifications displayed in Figures 13A and 13B. In both panels, Columns (1) and (3) include all of April 2020 in the regression specification, while Columns (2) and (4) restrict to only considering dates within one week of the stimulus payment date. Data sources: Panel A: Affinity Solutions; Panel B: Womply.
Table 5  
Estimated Effects of the Paychex Protection Program on Employment 

<table>
<thead>
<tr>
<th>Outcome Variable: Employment</th>
<th>Combined Paychex and Earnin Data</th>
<th>Earnin Data</th>
<th>Kronos Data</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Baseline Estimate (100-800 Employees)</td>
<td>Small Bandwidth (300-700 Employees)</td>
<td>Baseline Estimate (100-800 Employees)</td>
</tr>
<tr>
<td>DD Estimate</td>
<td>1.71 (1.98)</td>
<td>1.62 (2.68)</td>
<td>1.01 (0.94)</td>
</tr>
</tbody>
</table>

Notes: This table shows difference-in-difference estimates of the effect of PPP eligibility on employment. The outcome variable is employment at the county x 2-digit NAICS x income quartile x eligibility x week level, expressed as a percentage change relative to a pre-period of January 4-31 2020. Columns (1)-(2) present regressions in combined Paychex-Earnin data. In the baseline estimate in column (1), we begin by restricting to firms with between 100 and 800 employees. We then reweight firms on 2-digit NAICS codes such that the (worker-weighted) distribution of 2-digit NAICS codes within eligible Paychex (Earnin) firms matches the national distribution of 2-digit NAICS codes among Paychex (Earnin) firms in the period January 4-31 2020. Next, to combine the datasets, we reweight such that the (worker-weighted) share of each dataset is constant in eligible vs. ineligible firms. We then sum employment across datasets at the county x 2-digit NAICS x income quartile x eligibility x week level. We then regress change in employment on PPP eligibility, county x worker income quartile x week fixed effects, and an interaction term for PPP eligibility and the date being after April 3, clustering on county x industry. The DD estimate presents the coefficient and standard error on the interaction term for PPP eligibility and the date being after April 3. Column (2) replicates Column (1), restricting to firms with between 300 and 700 employees. Column (3) replicates Column (1) in Earnin data. As we treat all Earnin workers as belonging to the first quartile, we use county x week FEs, rather than county x worker income quartile x week FEs. Column (4) replicates Column (3) in Kronos data. Data sources: Paychex, Earnin, Kronos.
### Industry Employment Shares Across Data Sets

<table>
<thead>
<tr>
<th>NAICS Code</th>
<th>NAICS Description</th>
<th>QCEW All Establishments</th>
<th>QCEW Small Establishments</th>
<th>Homebase</th>
<th>Paychex-Earnin</th>
<th>Kronos</th>
<th>Intuit</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>11</td>
<td>Agriculture, Forestry, Fishing and Hunting</td>
<td>0.84</td>
<td>1.04</td>
<td>0.61</td>
<td>0.83</td>
<td></td>
<td></td>
</tr>
<tr>
<td>21</td>
<td>Mining, Quarrying, and Oil and Gas Extraction</td>
<td>0.55</td>
<td>0.43</td>
<td>0.21</td>
<td>0.08</td>
<td></td>
<td></td>
</tr>
<tr>
<td>22</td>
<td>Utilities</td>
<td>0.44</td>
<td>0.29</td>
<td>0.17</td>
<td>0.17</td>
<td>0.17</td>
<td></td>
</tr>
<tr>
<td>23</td>
<td>Construction</td>
<td>5.72</td>
<td>7.62</td>
<td>6.35</td>
<td>1.13</td>
<td>7.32</td>
<td></td>
</tr>
<tr>
<td>31-33</td>
<td>Manufacturing</td>
<td>10.27</td>
<td>5.16</td>
<td>8.48</td>
<td>22.14</td>
<td>2.24</td>
<td></td>
</tr>
<tr>
<td>42</td>
<td>Wholesale Trade</td>
<td>4.72</td>
<td>5.99</td>
<td>5.78</td>
<td></td>
<td>1.66</td>
<td></td>
</tr>
<tr>
<td>44-45</td>
<td>Retail Trade</td>
<td>12.48</td>
<td>14.06</td>
<td>11.28</td>
<td>8.32</td>
<td>3.72</td>
<td>4.65</td>
</tr>
<tr>
<td>48-49</td>
<td>Transportation and Warehousing</td>
<td>4.30</td>
<td>2.82</td>
<td>0.87</td>
<td>2.28</td>
<td>10.39</td>
<td>1.58</td>
</tr>
<tr>
<td>51</td>
<td>Information</td>
<td>2.29</td>
<td>1.64</td>
<td>1.63</td>
<td></td>
<td>0.94</td>
<td></td>
</tr>
<tr>
<td>52</td>
<td>Finance and Insurance</td>
<td>4.83</td>
<td>4.60</td>
<td>3.57</td>
<td>5.88</td>
<td></td>
<td>1.72</td>
</tr>
<tr>
<td>53</td>
<td>Real Estate and Rental and Leasing</td>
<td>1.71</td>
<td>2.90</td>
<td>3.08</td>
<td></td>
<td></td>
<td>1.85</td>
</tr>
<tr>
<td>54</td>
<td>Professional, Scientific, and Technical Services</td>
<td>7.63</td>
<td>8.97</td>
<td>2.78</td>
<td>12.12</td>
<td></td>
<td>11.84</td>
</tr>
<tr>
<td>55</td>
<td>Management of Companies and Enterprises</td>
<td>1.93</td>
<td>0.79</td>
<td>0.45</td>
<td></td>
<td></td>
<td>0.15</td>
</tr>
<tr>
<td>56</td>
<td>Administrative Support</td>
<td>7.25</td>
<td>5.30</td>
<td>6.61</td>
<td></td>
<td></td>
<td>5.00</td>
</tr>
<tr>
<td>61</td>
<td>Educational Services</td>
<td>2.39</td>
<td>1.53</td>
<td>3.62</td>
<td>2.43</td>
<td>1.11</td>
<td>1.18</td>
</tr>
<tr>
<td>62</td>
<td>Health Care and Social Assistance</td>
<td>16.16</td>
<td>13.16</td>
<td>5.34</td>
<td>15.21</td>
<td></td>
<td>22.71</td>
</tr>
<tr>
<td>71</td>
<td>Arts, Entertainment, and Recreation</td>
<td>1.78</td>
<td>1.64</td>
<td>2.07</td>
<td>2.17</td>
<td>1.77</td>
<td>1.30</td>
</tr>
<tr>
<td>72</td>
<td>Accommodation and Food Services</td>
<td>11.04</td>
<td>15.60</td>
<td>49.17</td>
<td>11.09</td>
<td>10.20</td>
<td>2.61</td>
</tr>
<tr>
<td>81</td>
<td>Other Services (except Public Administration)</td>
<td>3.57</td>
<td>6.21</td>
<td>1.83</td>
<td>8.74</td>
<td></td>
<td>5.96</td>
</tr>
<tr>
<td>99</td>
<td>Unclassified</td>
<td>0.11</td>
<td>0.24</td>
<td>23.04</td>
<td>8.72</td>
<td>20.15</td>
<td>43.2</td>
</tr>
</tbody>
</table>

**Notes:**
- This table reports the NAICS two-digit industry mix for four 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. Columns (1) - (6) indicate the share of employees in the given dataset who work in the specified sector. In columns (1) and (2), 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. In columns (3) - (6), we construct employment shares for the private datasets. For Homebase, Paychex-Earnin, Kronos, and Intuit we use January 2020 employment to do so. We define employment in Homebase as the number of unique individuals working a positive number of hours in the month. We define employment in Paychex-Earnin as a weighted sum of employment in Paychex (the total number of employees recorded by Paychex clients in the month) and employment in Earnin (the total number of worker-days in the month). We define employment in Kronos as the total number of employee punches in the month. We define employment in Intuit as the total number of employees recorded by Intuit clients in the month. Industries missing from any of the private data sources are left blank.
- Data sources: Homebase, Paychex, Earnin, Kronos, Intuit.
### Appendix Table 2
#### Hourly Wage Rates By Industry

<table>
<thead>
<tr>
<th>NAICS Code</th>
<th>NAICS Description</th>
<th>Mean (Pre Tax)</th>
<th>Homebase (Pre Tax)</th>
<th>Paychex - Earnin (Pre Tax)</th>
<th>Intuit (Pre Tax)</th>
</tr>
</thead>
<tbody>
<tr>
<td>11</td>
<td>Agriculture, Forestry, Fishing and Hunting</td>
<td>16.35</td>
<td></td>
<td>20.63</td>
<td></td>
</tr>
<tr>
<td>21</td>
<td>Mining, Quarrying, and Oil Gas Extraction</td>
<td>32.15</td>
<td></td>
<td>32.89</td>
<td></td>
</tr>
<tr>
<td>22</td>
<td>Utilities</td>
<td>39.80</td>
<td></td>
<td>33.18</td>
<td></td>
</tr>
<tr>
<td>23</td>
<td>Construction</td>
<td>27.87</td>
<td></td>
<td>28.74</td>
<td></td>
</tr>
<tr>
<td>31-33</td>
<td>Manufacturing</td>
<td>26.48</td>
<td></td>
<td>25.44</td>
<td></td>
</tr>
<tr>
<td>42</td>
<td>Wholesale Trade</td>
<td>28.85</td>
<td></td>
<td>27.34</td>
<td></td>
</tr>
<tr>
<td>44-45</td>
<td>Retail Trade</td>
<td>17.02</td>
<td>12.47</td>
<td>21.07</td>
<td></td>
</tr>
<tr>
<td>48-49</td>
<td>Transportation and Warehousing</td>
<td>24.33</td>
<td>14.63</td>
<td>24.62</td>
<td></td>
</tr>
<tr>
<td>51</td>
<td>Information</td>
<td>39.07</td>
<td></td>
<td>32.78</td>
<td></td>
</tr>
<tr>
<td>52</td>
<td>Finance and Insurance</td>
<td>36.73</td>
<td></td>
<td>32.82</td>
<td></td>
</tr>
<tr>
<td>53</td>
<td>Real Estate and Rental and Leasing</td>
<td>24.98</td>
<td></td>
<td>25.66</td>
<td></td>
</tr>
<tr>
<td>54</td>
<td>Professional, Scientific, and Technical Services</td>
<td>41.83</td>
<td>14.20</td>
<td>34.37</td>
<td></td>
</tr>
<tr>
<td>55</td>
<td>Management of Companies and Enterprises</td>
<td>42.59</td>
<td></td>
<td>24.06</td>
<td></td>
</tr>
<tr>
<td>56</td>
<td>Administrative Support</td>
<td>20.50</td>
<td></td>
<td>23.51</td>
<td></td>
</tr>
<tr>
<td>61</td>
<td>Educational Services</td>
<td>28.34</td>
<td>12.57</td>
<td>24.78</td>
<td></td>
</tr>
<tr>
<td>62</td>
<td>Health Care and Social Assistance</td>
<td>26.98</td>
<td>15.87</td>
<td>25.47</td>
<td></td>
</tr>
<tr>
<td>71</td>
<td>Arts, Entertainment, and Recreation</td>
<td>19.18</td>
<td></td>
<td>12.26</td>
<td>22.47</td>
</tr>
<tr>
<td>72</td>
<td>Accommodation and Food Services</td>
<td>13.65</td>
<td>11.11</td>
<td>16.62</td>
<td></td>
</tr>
<tr>
<td>81</td>
<td>Other Services (except Public Administration)</td>
<td>21.58</td>
<td>14.80</td>
<td>22.50</td>
<td></td>
</tr>
<tr>
<td>All</td>
<td></td>
<td>25.72</td>
<td>12.11</td>
<td>25.34</td>
<td>26.27</td>
</tr>
</tbody>
</table>

**Industry-Weighted Average of BLS Mean Wages**

<table>
<thead>
<tr>
<th></th>
<th>2019 BLS Wages</th>
<th>Mean in Private Datasets</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>All</td>
<td>25.72</td>
<td>12.11</td>
</tr>
</tbody>
</table>

**Notes:**
- The table reports mean wages for two-digit NAICS sectors. 2019 BLS Wages (1) come from the May 2019 Occupational Employment Statistics and are inflated to 2020 dollars using the Consumer Price Index.
- Columns (2-4) report mean wages in four private employment datasets, Earnin, Homebase, Paychex, and Intuit. In Earnin and Homebase (2), the mean wage is mean of hourly wages for workers of the given industry during the pre-COVID period (January 8th - March 10th). In Paychex, the mean wage is mean of county-level average hourly wages for the given industry during the pre-COVID period (January 8th - March 10th). In Paychex-Earnin (3), the mean wage is a weighted sum of industry-level mean wages in Paychex and Earnin. In Intuit (4), the mean wage is the mean of county-level average hourly wages during the pre-COVID period (January - February). In Earnin, wages are calculated by dividing the payment deposited in the individual’s bank account by hours worked and are thus post-tax. Homebase, Paychex, and Intuit wages are pre-tax. Industries missing from the Homebase data are left blank. The industry-weighted average of BLS mean wages are calculated using the industry shares for the relevant dataset. Data sources: Earnin, Homebase, Paychex, Intuit.
## Appendix Table 3
### Demographic Characteristics of Zearn Users

<table>
<thead>
<tr>
<th></th>
<th>Zearn Users (1)</th>
<th>U.S. Population (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Income</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>ZIP Median Household Income</td>
<td></td>
<td></td>
</tr>
<tr>
<td>25th Percentile</td>
<td>43,355</td>
<td>45,655</td>
</tr>
<tr>
<td>Median</td>
<td>54,941</td>
<td>57,869</td>
</tr>
<tr>
<td>75th Percentile</td>
<td>71,485</td>
<td>77,014</td>
</tr>
<tr>
<td>Number of ZIP codes</td>
<td>6,529</td>
<td>33,253</td>
</tr>
<tr>
<td>Number of People</td>
<td>925,978</td>
<td>322,586,624</td>
</tr>
<tr>
<td><strong>Panel B: School Demographics</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share of Black Students</td>
<td></td>
<td></td>
</tr>
<tr>
<td>25th Percentile</td>
<td>1.2%</td>
<td>1.5%</td>
</tr>
<tr>
<td>Median</td>
<td>5.2%</td>
<td>5.8%</td>
</tr>
<tr>
<td>75th Percentile</td>
<td>21.3%</td>
<td>19.1%</td>
</tr>
<tr>
<td>Share of Hispanic Students</td>
<td></td>
<td></td>
</tr>
<tr>
<td>25th Percentile</td>
<td>4.3%</td>
<td>5.6%</td>
</tr>
<tr>
<td>Median</td>
<td>11.4%</td>
<td>15.0%</td>
</tr>
<tr>
<td>75th Percentile</td>
<td>33.5%</td>
<td>40.6%</td>
</tr>
<tr>
<td>Share of Students Receiving FRPL</td>
<td></td>
<td></td>
</tr>
<tr>
<td>25th Percentile</td>
<td>35.7%</td>
<td>28.2%</td>
</tr>
<tr>
<td>Median</td>
<td>56.9%</td>
<td>50.1%</td>
</tr>
<tr>
<td>75th Percentile</td>
<td>80.4%</td>
<td>74.8%</td>
</tr>
<tr>
<td>Number of Schools</td>
<td>11,400</td>
<td>88,459</td>
</tr>
<tr>
<td>Number of Students</td>
<td>887,592</td>
<td>49,038,524</td>
</tr>
</tbody>
</table>

**Notes:** This table reports demographic characteristics for Zearn schools vs. the U.S. population. Household income percentiles are calculated using the 2018 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. Data source: Zearn
### Appendix Table 4

Cities with Largest Small Business Revenue Losses Following COVID Shock

<table>
<thead>
<tr>
<th>City</th>
<th>State</th>
<th>% Change in Small Bus. Revenue (Womply)</th>
</tr>
</thead>
<tbody>
<tr>
<td>New Orleans</td>
<td>Louisiana</td>
<td>-77.0%</td>
</tr>
<tr>
<td>Washington</td>
<td>District of Columbia</td>
<td>-74.0%</td>
</tr>
<tr>
<td>San Francisco</td>
<td>California</td>
<td>-69.0%</td>
</tr>
<tr>
<td>New York City</td>
<td>New York</td>
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<td>Baltimore</td>
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*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.). Columns (1) and (2) display the name of the city and the state in which it is located. Column (3) shows the decline in small business revenue, computed as the change in net small business revenue in Womply data between January 1-28 2020 and March 25 2020-April 14 2020, normalized against 2019 values of net revenue. Data source: Womply.
### Appendix Table 5

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

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<tr>
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<td>Median Home 2BR Rent</td>
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*Controls:*

- County Fixed Effects
  - X

| Observations           | 9052 | 6716 | 9052 |

*Notes:* This table shows OLS regressions of average percentage changes in consumer spending by ZIP code code (using data from Affinity Solutions) on average workplace ZIP code median two-bedroom rent. Standard errors are reported in parentheses. Workplace ZIP 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 and the independent variable is expressed in thousands such that, for example, the coefficient of -13.71 in Column (1) implies that a $1000 increase in monthly workplace rent is associated with a 13.71% 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. Data source: Affinity Solutions.
## Appendix Table 6

### List of Partial Re-Openings and Control States for Event Study

<table>
<thead>
<tr>
<th>Date</th>
<th>States that Re-Opened</th>
<th>Affinity Controls</th>
<th>Employment Controls</th>
<th>Womply Controls</th>
<th>Google Controls</th>
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Notes: This table lists the treatment and control states for each opening date in Figures 12B-12D.
## Appendix Table 7
City to County Crosswalk

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**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: Consumer Spending in National Accounts vs. Credit and Debit Card Data

A. National Accounts: Changes in GDP and its Components

B. Retail and Food Services in Affinity Solutions Data vs. Monthly Retail Trade Survey

C. Consumer Spending in Affinity Data vs. Monthly Retail Trade Survey Estimates in April 2020, by Industry

Notes: This figure compares changes in consumer spending in national income and product accounts (NIPA) data to measures of consumer spending recorded on debit and credit cards. Panel A summarizes NIPA data (Tables 1.1.2, 1.1.6 and 2.3.2), comparing Q1-2020 to Q2-2020. The first bar shows the seasonally adjusted change from Q1-2020 to Q2-2020 in real GDP in chained (2012) dollars (-$1.73T). In parentheses under the first bar is the compound annual growth rate corresponding to this one-quarter change in real GDP (-31.7%). 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 (-$1.73T) 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 (-$1.03T). 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 (-$1.35T) by the contributions to the percent change in real GDP given in Table NIPA 2.3.2 (excluding the aforementioned subcategories). We calculate the decline in GDP using the Second Estimate of 2nd Quarter 2020 Gross Domestic Product, which was released on August 27 2020. Panels B and C report average daily spending for each month in the Affinity Solutions credit and debit card data and the Monthly Retail Trade Survey (MRTS), a government survey providing estimates of sales at retail and food services stores across the United States. The retail series for panel B restricts to retail trade sectors (NAICS codes 44-45) excluding motor vehicles (NAICS code 441) and gas (NAICS code 447). The MRTS series is constructed by dividing the total spending in each category by the number of days in that month, and then indexing the average daily spending to January of the corresponding year. The Affinity series is constructed by taking the average of the seven-day moving average series indexed to January of the respective year for each month. The top left corner displays the root mean squared error (RMSE) corresponding to the difference between indexed MRTS monthly spending and indexed Affinity Solutions monthly spending separately for retail and food and accommodation services. Panel C displays a scatter plot of changes in spending at the three-digit NAICS code level between January and April 2020 in the Affinity Solutions data vs. the MRTS data, restricting to industries where the industry definitions in the Affinity Solutions data align closely with a three-digit NAICS code surveyed in the MRTS. Data sources: Panels A-C: Affinity Solutions
Notes: This figure disaggregates spending changes by income and sector in the COVID crisis using debit and credit card data from Affinity Solutions. Panel A plots a weekly series of consumer spending in the Affinity Solutions data for cardholders residing in ZIP codes in the top and bottom quartiles of the distribution of ZIP code median household income (defined using population-weighted data from the 2014-2018 ACS). We scale the 2020 (2019) series by multiplying by the ratio of January 2020 (2019) total spending for components of PCE that are likely captured in credit/debit card spending (shown in the last bar of Figure 1a) to the January 2020 (2019) total spending in the Affinity Solutions data. We impute the value plotted for February 29, 2019 with the average of February 22, 2019 and March 7, 2019. Panels B and C show industry level spending changes using the Affinity Solutions data. Panel B disaggregates spending changes (left bar) and pre-COVID levels (right bar) by sector. The left bar in Panel B shows the share of the total 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 through January 28 2019)). The second bar shows the share of spending of each sector over the period January 8-28 2020. 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 shown in the left bar and 23.0% of January spending as shown in the right bar. 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 for six specific categories of goods. Consumer spending for each good is normalized by its level over the period January 4-31, and seasonally adjusted using 2019 data, as described in Section II.A. Panel D decomposes the change in personal consumption expenditures (PCE) in the COVID-19 Recession 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 the COVID-19 Recession (Great Recession), the peak is defined as January 2020 (December 2007) and the trough is April 2020 (June 2009). Data source: Panels A-C: Affinity Solutions.
FIGURE 3: Association Between COVID-19 Incidence, Spending, and Time Outside Home

A. Change in Consumer Spending vs. COVID Case Rate, by County

- Low-Income Counties (Q1)
  - Slope = -2.08 (s.e. = 0.47)

- High-Income Counties (Q4)
  - Slope = -1.17 (s.e. = 0.58)

B. Change in Time Spent Away From Home vs. COVID Case Rate, by County

- Low-Income Counties (Q1)
  - Slope = -2.24 (s.e. = 0.29)

- High-Income Counties (Q4)
  - Slope = -1.87 (s.e. = 0.23)

C. Change in Time Spent Outside Home vs. County Median Income

- Slope = -0.21%/$1000 (s.e. = 0.01)

Notes: This figure presents three county-level binned scatter plots. To construct each binned scatter plot, we divide the data into a number of equal-sized bins, ranking by the x-axis variable and 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 from the base period (January 8-January 28) to the three-week period March 25-April 14 vs. the logarithm of the county’s mean daily cumulative COVID case rate per capita over the period March 25-April 14. The panel plots values separately for low-income and high-income counties, defining low-income and high-income counties as those with median household income in the bottom 25% and top 25% of all counties respectively, as measured in the 2014-2018 ACS (weighted by county population). The annotation in the panel displays the slope coefficient and standard error in a regression of changes in consumer spending on the logarithm of COVID-19 cases, computed separately for low-income and high-income counties. Panel B replicates Panel A, changing the y-axis variable to the change in time spent outside home. Panel C presents a binned scatter plot of the change in time spent outside home in each county, as constructed in Panel B, vs. the county’s median household income, as measured in the 2014-2018 ACS. The bottom left corner of the panel displays the slope coefficient and standard error in a regression of changes in time spent outside on county median household income, expressed as the change in time spent outside home per $1,000 increase in county median household income. Data sources: Panel A: Affinity Solutions; Panels B-C: Google Mobility
FIGURE 4: Changes in Small Business Revenues by ZIP code

A. New York

B. Chicago

C. San Francisco

Notes: This figure plots percentage small business revenue declines during the COVID crisis by ZIP code from March 25 through April 14 in the MSAs corresponding to New York-Newark-Jersey City, NY-NJ-PA MSA (Panel A), Chicago-Naperville-Elgin, IL-IN-WI MSA (Panel B), and San Francisco-Oakland-Hayward, CA MSA (Panel C). The change in revenue is calculated in each ZIP code using data from Womply. We first create a weekly series in each ZIP code by calculating total revenue in each ZIP in each week of 2019 and 2020, and then dividing weekly revenue by average weekly revenue over the period January 1-28 in the respective year. Next, we calculate seasonally-adjusted normalized revenue in each week by dividing the indexed value relative to January for that week in 2020 by the corresponding indexed value from 2019. We then calculate the change in each ZIP code as the mean seasonally-adjusted normalized revenue in each ZIP code over the weeks March 25-April 14. We calculate the signal-to-noise ratio by regressing seasonally-adjusted normalized weekly revenue on an indicator variable for whether the week is after March 9, 2020, within each ZIP code, denoting the coefficient and standard error on this indicator variable in each ZIP code as $\beta_z$ and $SE_z$, respectively. We then calculate the signal-to-noise ratio as $1 - \frac{\sum SE_z^2}{\sum \beta_z^2}$. The signal variance to total variance ratios for the panels are 0.79 (New York), 0.78 (Chicago), and 0.78 (San Francisco). These maps must be printed in color to be interpretable; dark red colors represent areas with larger revenue declines, while dark blue colors represent areas with smaller declines. Data source: Panels A-C: Womply
FIGURE 5: Changes in Small Business Revenues vs. ZIP code Characteristics

A. Median Income

\[ \text{Slope} = -0.13\% / \$1000 \ (\text{s.e.} = 0.01) \]

B. Population Density

\[ \text{Slope} = -2.95\% / \log \text{Population Density} \ (\text{s.e.} = 0.08) \]

C. Median Two Bedroom Rent

\[ \text{Slope} = -13.86\% / \$1000 \ (\text{s.e.} = 0.39) \]

D. Median Two Bedroom Rent: In-Person vs. Teleworkable

\[ \text{Finance and Professional Services} \]

\[ \text{Slope} = 1.46\% / \$1000 \ (\text{s.e.} = 1.27) \]

\[ \text{Food and Accommodation Services and Retail Trade} \]

\[ \text{Slope} = -13.44\% / \$1000 \ (\text{s.e.} = 0.52) \]

Notes: This figure presents binned scatter plots showing the relationship between changes in small business revenue in Womply data vs. various ZIP code-level characteristics. Binned scatter plots are constructed as described in Figure 3. In each panel, we calculate changes in business revenue in each ZIP code as mean seasonally-adjusted normalized weekly revenue, as defined in Figure 4, from March 25 to April 14. We exclude data from ZIP codes in which changes are larger than 200% or where variance of normalized revenue exceeds 900%. To preserve the privacy of firms included in the data, ZIP code-by-industry cells with average weekly revenue of less than $4,250 during the base period are excluded. Panel A shows a binned scatter plot of changes in small business revenue vs. median household income at the ZIP code level, from the 2014-2018 ACS. Panel B replicates Panel A, changing the x-axis variable to the logarithm of the number of inhabitants per square mile in each ZIP code in the 2014-18 ACS. Panel C replicates Panel A, changing the x-axis variable to the median rent for a two-bedroom apartment in each ZIP code in the 2014-2018 ACS. Panel D replicates Panel C for two sectors: in person services, 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). The annotations in each panel display the slope coefficient and standard error from a regression of changes in small business revenues on the x-axis variable. In Panel B, the annotation displays the slope coefficient and standard error from a regression of changes in small business revenues on the logarithm of the number of inhabitants in the ZIP code per square mile. In Panel D, the annotations display the slope coefficients and standard errors from separate regressions restricted to businesses in in-person vs. teleworkable sectors. Data source: Panels A-D: Womply
**FIGURE 6: Changes in Employment Rates Over Time**

### A. Pooling All Industries

- **RMSE CES:** 3.66
- **RMSE CPS:** 1.88

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</tbody>
</table>

Notes: This figure compares employment changes relative to January 2020 within various datasets. In Panel A, we combine Paychex, Intuit, and Earnin data to construct a daily private employment series for all industries. We then construct an employment index by averaging employment over the prior seven days and then norming to the average value of employment over the period January 4-January 31, 2020. The Current Employment Statistics (CES) data, as well as the Current Population Survey (CPS) are available monthly, so we plot changes in each month relative to January 2020. The CES is a monthly survey of firms at the parent level. The CPS is a monthly survey of households, which we then adjust to match a payroll definition of employment by accounting for multiple jobholders. The CES reports employment for the pay period including the 12th of each month, and the CPS is fielded during the week of the 19th of each month, so we plot these monthly series on the 15th of the month. Panel B replicates the combined private employment series and the CES series from figure A, but instead restricts to employment in the two-digit NAICS sector 72, Accommodations and Food Services and NAICS supersector 60, Professional and Business Services. In addition, we plot a series for NAICS 72 firms in the Homebase data. Data sources: Panel A: Paychex, Intuit, Earnin; Panel B: Paychex, Intuit, Earnin, Homebase
FIGURE 7: Changes in Employment by Income Quartile and Consumer Spending

A. Changes in Employment by Wage Quartile

![Chart showing changes in employment by wage quartile]

- Top Wage Quartile (>$60K)
- Third Quartile (<$60K)
- Second Quartile (<$37K)
- Bottom Wage Quartile (<$27K)

B. Changes in Employment by Income Quartile and Consumer Spending, All Industries

![Chart showing changes in employment and consumer spending]

- Employment: Top Wage Quartile
- Employment: Bottom Wage Quartile
- Total Consumer Spending

C. Changes in Employment by Income Quartile and Consumer Spending, Retail Trade

![Chart showing changes in employment and consumer spending]

- Employment: Top Wage Quartile
- Employment: Bottom Wage Quartile
- Total Consumer Spending

Notes: This figure shows changes in employment by income quartile and consumer spending relative to January 2020. In each panel, we present a daily employment series constructed by combining Paychex, Intuit and Earnin data; for details, see notes to Figure 6. Panel A shows changes in employment at the national level since January 2020, split by wage quartile. The values for annualized earnings corresponding to the thresholds for each wage quartile are displayed in the bottom left of the panel. The solid series shows changes in combined Paychex-Intuit-Earnin data, and the dotted series from August 22 to September 25 is forecasted using Kronos data and Paychex data from firms with weekly paycycles. To construct this forecast, we regress the combined Paychex-Intuit-Earnin series on the de-seasonalized Kronos series for the same date \((t)\), the Paychex weekly series for the same date \((t)\), and the Paychex weekly series for three weeks prior \((t - 7), (t - 14), (t - 21)\). We then use the resulting coefficients to predict values of combined Paychex-Intuit-Earnin employment. Panels B and C compare changes in employment by income quartile to changes in consumer spending. In both panels, the spending series is constructed using card spending data. Panel B shows changes in spending and employment across all industries. To construct the employment series in Panel B, we begin with combined Paychex-Intuit-Earnin employment at the county x 2-digit NAICS code x income quartile x date level. We restrict the sample to county x 2-digit NAICS code cells which have positive first-quartile and fourth-quartile employment in the period January 4-31 2020; this sample restriction excludes 0.6% of worker-days from the sample. We then calculate the change in employment since January 4-31 2020 in each county x 2-digit NAICS code x income quartile x date level. We restrict the sample to county x 2-digit NAICS code cells which have positive first-quartile and fourth-quartile employment in the period January 4-31 2020; this sample restriction excludes 0.6% of worker-days from the sample. We then calculate the change in employment since January 4-31 2020 in each county x 2-digit NAICS code x income quartile cell as of each date, winsorizing at the 99th percentile (weighted by total employment in the period January 4-31 2020). To construct first-quartile (fourth-quartile) employment, we take the mean change in first-quartile employment as of each date, weighting by first-quartile (fourth-quartile) employment in January 4-31 in each cell. To construct first-quartile employment reweighted to match fourth-quartile employment on NAICS code x county, we take the mean change in first-quartile employment as of each date, weighting by fourth-quartile employment in January 4-31 in each cell. Panel C shows changes in spending and employment in NAICS 44-45 (Retail Trade) since January 4-31 2020. Data sources: Panel A: Paychex, Intuit, Earnin, Kronos; Panels B-C: Paychex, Intuit, Earnin, Affinity Solutions.
FIGURE 8: Changes in Employment Rates by ZIP code

A. New York

B. Chicago

C. San Francisco

Notes: This figure replicates Figure 4 using the changes in employment from Earnin. We focus on small and medium-sized businesses, defined as firms with at most 500 employees (as measured in the ReferenceUSA data). For users whose employer cannot be matched to ReferenceUSA data on firm sizes, we restrict to users whose employer is in the fourth decile or below of firms in the Earnin data, in terms of number of Earnin users working for the firm; the median firm size for the fourth decile of Earnin employers is roughly 300 employees, among employers matched to ReferenceUSA data on firm sizes. We calculate normalized weekly employment as total employment in each week for the weeks covering the period April 15 to May 19 divided by total average weekly employment in the period January 4-31, 2020. We calculate the signal-to-noise ratios as in Figure 4; these ratios are 0.79 in New York, 0.59 in Chicago, and 0.67 in San Francisco. These maps must be printed in color to be interpretable; dark red colors represent areas with larger employment declines, while dark blue colors represent areas with smaller declines. Data source: Panels A-C: Earnin
FIGURE 9: Changes in Employment Rates and Job Postings vs. Rent

A. Employment vs. Median Rent, by ZIP

- Medium Businesses (500-9,999 Employees)
  - Slope = -12.80%/$1000 (s.e. = 0.75)
- Small Businesses (<500 Employees)
  - Slope = -12.02%/$1000 (s.e. = 0.43)
- Large Businesses (10,000+ Employees)
  - Slope = -8.01%/$1000 (s.e. = 0.54)

B. Job Postings for Low-Education Workers vs. Median Rent, by County

- Slope = -24.72%/$1000 (s.e. = 1.15)

C. Job Postings for High-Education Workers vs. Median Rent, by County

- Slope = -2.34%/$1000 (s.e. = 0.79)

**Notes:** This figure shows binned scatter plots of the relationship between changes in employment rates and median rents (Panel A) and job postings and median rent (Panels B-C). Binned scatter plots are constructed as indicated in Figure 3. In each panel, rent is computed as median monthly two bedroom from the 2014-2018 ACS. Panel A shows the ZIP-level relationship between the change in employment rates between January 4-31 2020 to April 8-28 2020 in the Earnin data and median rents, separately for small firms (less than or equal to 500 employees), medium-sized firms (between 500 and 10,000 employees), and large firms (more than 10,000 employees). Panel B shows the county-level relationship between the percentage change in job postings for workers with minimal or some education and median rent. Panel C replicates Panel B, changing the y-axis variable to the change in job postings for workers with moderate, considerable, or extensive education. Changes in job postings are computed using Burning Glass data. Solid lines are best-fit lines estimates using OLS regression, except in Panel B, where we use a lowess fit. The annotation in each panel displays the slope coefficient and standard error in an OLS regression of change in employment (Panel A), job postings for low-education workers (Panel B) or job postings for high-education workers (Panel C), on rent. In Panel A, this regression is computed separately for workers in small, medium and large firms. Data sources: Panel A: Earnin; Panels B-C: Burning Glass Technologies.
Notes: This figure displays the share of job losses occurring in low vs. high income counties in 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 into (population-weighted) quartiles 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 January 2020 and April 2020. The third set of bars reports the share of total initial UI claims between March 15 and April 12, 2020 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. In the third bar, we only include counties that are in states which report county-level UI claims data, which comprise 53% of the U.S. population.
FIGURE 11: Changes in Employment and Consumer Spending vs. Workplace Rent for Low-Income Households

A. Change in Low-Income Employment vs. Workplace Rent

Slope = -17.10%/$1000 (s.e. = 1.73)

B. Trends in Low-Income Employment by Workplace Rent Quartile

(Q1 - Q4) Gap at 15 July: 15.59 p.p.

C. Change in Spending Among Low-Income Households vs. Workplace Rent

Slope = -13.71%/$1000 (s.e. = 0.63)

Notes: This figure compares changes in employment (Panels A-B) and consumer spending (Panel C) by ZIP code to average rent in the workplace ZIP codes of individuals who live in a given ZIP code. We construct the average workplace rent variable by combining data on the matrix of home residence by workplace ZIP codes taken from Census’ LEHD Origin-Destination Employment Statistics (LODES) for low-income workers (workers earning below $1,250 per month) with data on median two bedroom monthly rents from the 2014-2018 ACS. In particular, we assign median rents from the ACS to each ZIP code of workplace in the LODES data and then compute mean workplace rent in each home ZIP code, weighting by the number of low-wage jobs in each workplace ZIP code. Panel A presents a binned scatter plot of change in employment in Earnin data vs. workplace rent, at the ZIP code level. The change in employment variable is based on payroll data from Earnin mapped to workers’ home ZIP codes, and is computed as the percentage change in total employment from Jan 5-Mar 7 to April 8-28, 2020. Panel B shows trends in employment by workplace rent quartile. The annotation in the bottom left of the panel displays the slope coefficient and standard error in a ZIP code-level regression of change in employment from January to April on workplace rent. To construct Panel B, we first assign home ZIP codes to quartiles based on workplace rent (weighting by population), and then calculate total weekly employment in each workplace rent quartile. We then express total weekly employment in each workplace rent quartile as a percentage change relative to January 4-31 2020. Panel C replicates Panel A using change in consumer spending based on data from Affinity Solutions, mapped to the cardholder’s residential ZIP as the y-axis variable, rather than change in employment. The change is spending is computed from the period of January 5-March 7 to March 22-April 14 2020. In Panel C, the sample is restricted to individuals who live in ZIP codes in the bottom quartile of the household income distribution. Data sources: Panels A-B: Earnin; Panel C: Affinity Solutions.
FIGURE 12: Causal Effects of Re-Openings on Economic Activity: Event Studies

A. Case Study on Business Re-Openings: Colorado vs New Mexico

Colorado
Closing
New Mexico
Closing
Colorado Begins
Re-Opening
New Mexico Begins
Re-Opening

B. Re-Opened States vs. Control States: Consumer Spending

Diff-in-diff Estimate: +1.62p.p. (s.e. = 0.53)

C. Re-Opened States vs. Control States: Employment

Diff-in-diff Estimate: +0.62p.p. (s.e. = 0.51)

D. Re-Opened States vs. Control States: Merchants Open

Diff-in-diff Estimate: +3.52p.p. (s.e. = 1.21)

E. Variance Explained by Re-Openings

Notes: This figure shows event studies on the causal effects of re-opening on consumer spending, employment, and the number of small business open. Panels A and B show percent change in consumer spending in the Affinity Solutions data. Consumer spending is normalized by its level over the period January 4-31, and seasonally adjusted using 2019 data. Panel A shows the consumer spending 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 20 and April 27, compared to a matched control group. We construct the control group separately for re-opening states on each re-opening day and then stack the resulting event studies to align the events. Panel C replicates Panel B but instead plots the percent change in employment of workers using combined Paychex-Intuit-Earnin data. Panel D replicates Panel B but instead plots the percent change in open small businesses using Womply data. In Panels B-D, we report the coefficient from a difference-in-differences regression comparing treated vs. untreated states in the two weeks following vs. the two weeks prior to the partial re-opening. Panel E reports the share of variance in outcomes explained by re-openings as of May 18. To estimate this, we first calculate the variance of outcome levels across states on May 18, 2020. Then, we add the estimated effect of reopening for a given outcome to all states not open on May 18 (adding only half of the effect if the state opened between May 11 and May 18). This effect is the difference-in-difference estimate reported in Panels B-D. We then recalculate the variance in this counterfactual in which all states had reopened. The share of variance explained by reopenings for each outcome is defined as 1-(counterfactual variance/actual variance). Data sources: Panels A-B: Affinity Solutions; Panel C: Paychex, Intuit, Earnin; Panel D: Womply; Panel E: Affinity Solutions, Paychex, Intuit, Earnin, Womply.
FIGURE 13: Impact of Stimulus Payments on Consumer Spending

A. Seasonally Adjusted Spending Changes by Income Quartile

B. Regression Discontinuity Plot for Lowest Income Quartile ZIP codes

C. Regression Discontinuity Plot for Highest Income Quartile ZIP codes

D. Regression Discontinuity Plot for Durable Goods

E. Regression Discontinuity Plot for In-Person Services

Notes: This figure studies the effect of the stimulus payments made on April 15, 2020 on credit and debit card spending. Panel A plots the percent change in seasonally-adjusted consumer spending in Affinity Solutions data for cardholders living in ZIP codes in the bottom and top quartiles of the distribution of ZIP code median household income (based on data from the 2014-2018 ACS). Estimates are seasonally adjusted relative to 2019 values as described in notes to Figure 12. In Panels B-E, each point is the national level of spending on that day divided by the average level of spending in January 2020 in the Affinity Solutions data. The points are residualized by day of week and first of the month fixed effects, which we estimate using data from January 1, 2019, to May 10, 2019. In each panel, we also report regression discontinuity estimates of the jump in spending on April 15, using a linear control function before and after April 15 (shown by the solid best fit lines), excluding the partially treated day of April 14, shown by the hollow point and demarcated by the dashed vertical line. Panel B restricts the sample to cardholders living in ZIP codes in the lowest median household income quartile. Panel C replicates B, restricting the sample cardholders in highest income quartile ZIP codes. Panel D pools all cardholders and restricts spending to spending on durable goods, as defined in the notes for Figure 2. Panel E pools all cardholders and restricts spending to spending on in-person services, also defined in the notes for Figure 2. Data sources: Panels A-E: Affinity Solutions
FIGURE 14: Impact of Stimulus Payments on Business Revenue and Employment

A. Regression Discontinuity Plot for Lowest Rent Quartile ZIP codes

B. Regression Discontinuity Plot for Highest Rent Quartile ZIP Codes

C. Revenue and Worker Earnings Changes Among Small Businesses, by ZIP code Rent Quartile

Notes: This figure shows trends in small business revenue and employment around stimulus payments made on April 15 2020. Panels A and B study the effect of stimulus payments on small business revenues using data from Womply. These panels are constructed in exactly the same way as Panels B and C of Figure 13 except that (1) we use revenue instead of spending as the outcome variable and (2) we split ZIP codes into quartiles based on median rent for a two-bedroom apartment from the 2014-2018 ACS instead of median household income. Panel C plots the percent change in seven-day moving averages of small-business revenue using Womply data and changes in employment rates using Earnin data, by ZIP code rent quartile. The employment series restricts to smaller businesses in the Earnin sample, defined by parent employer size being at most 500 employees. The employment series is plotted as a raw change relative to the mean value in January 2020, as described in notes to Figure 6. The revenue series is seasonally-adjusted by dividing the percentage change from January to each calendar date in 2020 by the corresponding change in 2019, as described in notes to Figure 4. Data sources: Panels A-B: Womply; Panel C: Womply, Earnin
FIGURE 15: Impact of Paycheck Protection Program on Employment

A. Change in Employment by PPP Eligibility, All Industries Excl. NAICS 72 (Combined Paychex and Earnin Data)

B. Change in Employment by Firm Size, All Industries Excl. NAICS 72 (Combined Paychex and Earnin Data)

Notes: This figure studies the effects of the Paycheck Protection Program on employment. Each panel excludes workers in the Accommodation and Food Services sector (NAICS 72). In both panels, we construct employment series using combined Paychex and Earnin data. Panel A compares employment trends among firms with 100-500 employees, which were eligible for PPP loans, vs. firms with 501-800 employees, which were ineligible for PPP loans. To construct employment trends, we begin by collapsing each dataset to the county x 2-digit NAICS code x income quartile x firm size bin x date level. We then reweight on 2-digit NAICS code and data source such that the industry and data source composition in each firm size bin matches the pooled distribution of industry composition and data source over the period January 4-January 31 2020. We then express employment in each county x 2-digit NAICS code x income quartile x eligible cell at each date as a change since January 4-31 2020, winsorizing at the 99th percentile. To construct the “control” series for ineligible firms with between 501 and 800 employees, we take the mean value of employment in each week, expressed as a change relative to January 4-31 2020, and weighting by employment over the period January 4-31 2020 in each county x 2-digit NAICS code x income quartile x firm size bin cell. To construct the “treatment” series, for each week, we regress change in employment at the county x 2-digit NAICS code x income quartile x firm size bin level on county x income quartile fixed effects and a dummy variable for the firm being eligible for the PPP. We then add the point estimate for the coefficient on eligibility to the control series in order to form the treatment series. Finally, we recenter each series so that mean change in employment since January 4-31 is 0 percentage points over the period 12 February to 18 March. The gray dashed line corresponds to April 3, 2020, the first day for enrollment in the Paycheck Protection Program (PPP). The annotation in the right of the panel presents the coefficient and standard error on the interaction between eligibility and the date being after April 3 2020, in a regression of change in employment since January on eligibility for the PPP, the date being on or after 3 April 2020, the interaction between eligibility and the date being after April 3 2020, and quartile x county x week fixed effects. Panel B presents a binned scatterplot of changes in employment between the period January 4-31 and the period June 1-23 vs. firm size. To construct changes in employment by firm size, we first classify firms in bins of size 50 according to their parent employer size. Next, we reweight on 2-digit NAICS code and data source, as in Panel A. Finally, we calculate the mean change in employment among firms in each bin. We plot this mean change in employment against the midpoint in each bin. Data sources: Panels A-B: Paychex, Earnin
Notes: This figure plots educational progress on the Zearn online math platform for schools located in ZIP codes in the bottom, middle (quartiles 2 and 3), and top quartile of the distribution of median household income. When assigning ZIP codes to income quartiles, we include all ZIP codes, whether they contain Zearn users or not, and weight by by the population in each ZIP code. Student progress is defined as the number of accomplishment badges earned in Zearn in each week, and the figure plots changes in this metric relative to the mean value over the period January 6-February 7 2020. The sample includes all classes with more than 10 students using Zearn during the base period and at least five users in every week during the reference period. We aggregate data to the ZIP code level weighting by the average number of students using the platform at each school during the period January 6-February 7 2020. Data source: Zearn
APPENDIX FIGURE 1: Industry Shares of Consumer Spending and Business Revenues Across Datasets

Notes: This figure compares the industry composition of spending in private sector datasets to the industry composition of spending in representative survey datasets. Panel A shows the NAICS two-digit industry mix for transactions in the Affinity Solutions and Womply 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 Affinity Solutions and Womply datasets (respectively) by aggregating card transactions in Q1 2020, using the merchant to classify the purchase by sector. 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: Panels A-B: Affinity Solutions, Womply.
APPENDIX FIGURE 2: 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 the Job Openings and Labor Turnover Survey (JOLTS) data from the U.S. Bureau of Labor Statistics, in January 2020. Data source: Burning Glass
Notes: This figure compares trends cash transactions in CoinOut data vs. card spending on groceries in Affinity Solutions data. Panel A plots 7-day moving averages of these two series at the national level. The signal correlation between the two datasets at the national level is 0.90 at the weekly level between January 1st and June 9th. We compute this correlation by collapsing both datasets to the national weekly level, where values in each week are expressed as a percentage change from the January average. To adjust for measurement error at the weekly level, we calculate the series-specific reliability as the week-on-week correlation within each dataset. We then divide the raw weekly correlation between datasets by the square root of the product of the reliabilities to get the signal correlation. Panels B and C plot total expenditure in CoinOut (Panel B) and total grocery expenditure in Affinity Solutions (Panel C) by county rent quartile, compared to January averages. We assign counties to rent quartiles according to median two bedroom monthly rent from the 2014-2018 ACS, weighting by population. Data sources: Panel A: CoinOut, Affinity Solutions; Panel B: CoinOut; Panel C: Affinity Solutions
APPENDIX FIGURE 4: Small Business Revenue Changes vs. Consumer Spending Changes

A. Retail Services (Excluding Auto and Gas)

B. Food Services and Accommodations

Notes: This figure compares the seven-day moving average of total consumer spending (from Affinity Solutions data) and small business revenue (from Womply) normalized to the average January and February levels of each year. 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). The bottom right corner of each panel displays the root mean squared error (RMSE) corresponding to the difference between the two lines. Data sources: Panels A-B: Affinity Solutions, Womply
APPENDIX FIGURE 5: 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 replicates Figure 4 within Food and Accommodation Service (NAICS 72) small businesses, showing the change in revenue levels by ZIP Code from January 1-28 to March 25-April 14. For details, see notes to Figure 4. The signal variance to total variance ratios for the panels are 0.83 (New York), 0.88 (Chicago), and 0.69 (San Francisco). Data source: Panels A-C: Womply

A. Changes in Small Business Revenues, by County

B. Changes in Low-Income Employment, by CZ

Notes: This figure presents national maps of changes in small business revenues (Panel A) and low-income employment (Panel B). Panel A replicates Figure 4 for the entire United States instead of a single MSA, showing the change in small business revenue from January 1-28 to March 25-April 14 in each county (rather than ZIP code, as in Figure 4). See notes to Figure 4 for details. Panel B replicates Figure 8 at the commuting zone (CZ) level for the entire United States instead of a single city and its surrounding area, showing the change in employment from Jan 4-31 to April 15-May 19 in the Paychex-Intuit-Earnin combined data on first-quartile employment (rather than Earnin data on first-quartile employment, as in Figure 8) in each CZ (rather than ZIP code, as in Figure 8). See notes to Figure 8 for details. Data sources: Panel A: Womply; Panel B: Paychex, Intuit, Earnin
APPENDIX FIGURE 7: Changes in Small Business Outcomes vs. ZIP and County Characteristics

A. Changes in Small Business Revenue vs. Gini Index, County Level

B. Changes in Small Business Revenue vs. Income Share of Top 1% of Income Distribution, County Level

C. Changes in Small Business Revenue vs. Share of Population Below Poverty Line, County Level

D. Changes in Small Businesses Open vs. Rent, ZIP Level

Notes: This figure shows the association between ZIP- or county-level characteristics and changes in small business outcomes between January 4-31 and March 25-April 14 2020, as measured in Womply data. Panels A-C replicate Figure 5 but compare the declines in small business revenue with various measures of inequality at the county level. Panel A presents a binned scatter plot of changes in small business revenue vs. the Gini index within each county from the 2014-2018 ACS. Panel B presents a binned scatter plot of changes in small business revenue vs. the income share of the top 1% of the income distribution within each county, as constructed using the distribution of parent incomes in Chetty et al. (2014). The top 1% of the income distribution is defined using the distribution of incomes within each county, rather than the national income distribution. Panel C presents a binned scatter plot of changes in small business revenue vs. the share of the county population with incomes below the poverty line in the 2014-2018 ACS. Panel D replicates Figure 5c using the change in the number of small businesses open, rather than the change in small business revenue, as the outcome variable. See notes to Figure 5 for details. Data source: Panels A-D: Womply
APPENDIX FIGURE 8: Changes in Small Business Revenue, Employment and Job Postings to July vs. Rent

A. Change in Small Business Revenue vs. Median Rent, by ZIP

B. Change in Employment in Earnin Data vs. Median Rent, by ZIP

C. Change in Job Postings for Low-Education Workers vs. Median Rent, by County

D. Change in Employment vs. Workplace Rent Among Low-Income Households, by ZIP

Notes: This figure presents binned scatter plots showing the association between changes in various economic measures vs. ZIP- and county-level median rent levels, contrasting the patterns in April vs. July 2020. See the notes to Figure 3 for more details on the construction of binned scatter plots. Panel A replicates Figure 5c, adding a second series showing ZIP-level changes in small business revenue from January to July 4-31 2020 vs. ZIP median rent. Panel B replicates Figure 9a, pooling all firm sizes, and then adding a second series showing ZIP-level changes in low-income employment (from Earnin) from January to July 4-31 2020 vs. ZIP median rent. Panel C replicates Figure 9b, adding a second series showing county-level changes in job postings from January to July 4-31 2020 vs. county median rent. Panel D replicates Figure 11a, adding a second series showing ZIP-level changes in low-income employment (from Earnin) from January to July 4-31 vs. workplace rent in ZIP. See the notes to Figure 11 for more details on the construction of workplace rent in ZIP. The annotation in each panel displays the slope coefficients and standard errors from OLS regressions of the change in the outcome variable against rent, separately for changes to April and changes to July. Data sources: Panel A: Womply; Panel B: Earnin; Panel C: Burning Glass; Panel D: Earnin
APPENDIX FIGURE 9: Paychex-Intuit-Earnin Combined Employment vs. ADP, CPS and CES Employment

Notes: This figure benchmarks the Paychex-Intuit-Earnin combined employment series to the Current Population Survey (CPS), the Current Employment Statistics (CES), and estimates based on ADP data in Cajner et al. (2020). Panel A shows employment trends in the Paychex-Intuit-Earnin combined data (solid series) and ADP data (dotted series), cut by income quartile. The Paychex-Intuit-Earnin series is expressed as a percentage change relative to January 4-31 2020. The ADP series (from Cajner et al. 2020) is expressed as a percentage change relative to February 15 2020. Panel B shows changes in employment from January to April 2020, cut by income quartile, in the Paychex-Intuit-Earnin combined, ADP, and CPS datasets. In the combined Paychex-Intuit-Earnin data, we express the change in employment relative to January 4-31 2020. The ADP series in Cajner et al. (2020) is expressed as a percentage change relative to February 15 2020. The CPS series is expressed as a percentage change relative to January 2020. Panel C shows a scatterplot of changes in employment in Paychex-Intuit-Earnin combined data between January 4-31 and April 15 vs. changes in CES employment between January and April, by state. We exclude Hawaii and North Dakota, where Paychex-Intuit-Earnin data have poor coverage. Panel D shows a scatterplot of changes in employment in Paychex-Intuit-Earnin combined data between January 4-31 and April 15 vs. changes in CES employment between January and April, by two-digit NAICS code. In Panels C and D, the bottom right corner displays the correlation between the data points in each graph, weighted by state population (Panel C) and CES employment in each NAICS code (Panel D), respectively. Data sources: Panels A-D: Paychex, Intuit, Earnin
Notes: This figure compares trends in wages and employment in Earnin data, relative to the period January 4-31 2020. We construct employment series as described in notes to Figure 7. We construct wages in Earnin as a chain-weighted series, calculating the change between wages in week \( w \) and wages in week \( (w + 1) \), for workers who are employed in both weeks. We then express our wage series by chaining together changes in wages since January 2020. Data source: Earnin
Notes: This figure displays a binned scatterplot of average percent declines in employment in the Earnin data at firms of different sizes. We calculate the change in employment from the period January 4-31 2020 to the period June 1-23 2020, as described in notes to Figure 7. We estimate the size of firms by matching Earnin employer names and locations to employer names and locations in ReferenceUSA data. Data source: Earnin
Notes: This figure displays trends in seasonally-adjusted consumer spending in the Affinity Solutions data, pooling states by the date on which a state-wide order closed non-essential businesses. The consumer spending series is constructed as in Figure 3. States are aggregated into “Early” (state-wide closure order issued between March 19 and March 24), “Late” (state-wide closure order issued between March 30 and April 6), and “Never”. The left (right) dotted gray line indicates the date March 19 (March 30), the date at which “Early Closers” (“Late Closers”) began issuing state-wide orders closing non-essential businesses. Data source: Affinity Solutions
APPENDIX FIGURE 13: Impact of Stimulus on the Composition of Consumer Spending

Notes: This figure presents the distribution of spending changes among categories of goods and services in the Affinity Solutions data. The first bar replicates the right bar in Figure 2b, showing the composition of spending for the period January 8-28 2020. The second and third bars repeat this distribution for the post-COVID, pre-stimulus period (March 25-April 14) and the post-COVID, post-stimulus period (April 29-May 5), respectively. The fourth bar replicates the left bar in Figure 2b, except decomposing the recovery (change from 2nd to 3rd bars in this graph) rather than the decline. We define each spending category using Merchant Category Codes (MCCs), as in Figure 2; see notes to Figure 2 for details. Data source: Affinity Solutions.
APPENDIX FIGURE 14: Effect of Paycheck Protection Program on Employment (Earnin and Kronos Data)

A. Change in Employment by PPP Eligibility, All Industries Excl. NAICS 72 (Earnin Data)

B. Change in Employment by Firm Size, All Industries Excl. NAICS 72 (Earnin Data)

C. Change in Employment by PPP Eligibility, All Industries Excl. NAICS 72 (Kronos Data)

D. Change in Employment by Firm Size, All Industries Excl. NAICS 72 (Kronos Data)

Notes: This figure replicates Figure 15, using Earnin data (Panels A and B) and Kronos data (Panels C and D) (rather than combined Paychex-Earnin data, as in Figure 15) to study the effect of the Paycheck Protection Program by comparing firms above and below the 500-employee eligibility threshold. For details, see notes to Figure 15. Data sources: Panels A-B: Earnin; Panels C-D: Kronos.
APPENDIX FIGURE 15: Out-Of-Sample Fit of Advance Series

A. Testing Period: May 26 - June 25

B. Testing Period: June 16 - July 15

Notes: This figure compares out-of-sample predictions for employment to realized employed series. We construct predicted values for Paychex-Intuit-Earnin employment using Kronos data and Paychex data for firms with weekly paycycles; see notes to Figure 7 for details. Panel A compares out-of-sample predictions to realized values in the testing period of May 26-June 25 2020. Panel B compares the out-of-sample prediction to realized values in the testing period of June 16-July 15 2020. The Root Mean Square Error (RMSE) for the difference between the prediction model and the true values across the top, middle, and bottom quartiles in the first testing period is 1.030 percentage points, while the RMSE across the top, middle, and bottom quartiles in the second testing period is 0.893 percentage points. Data sources: Panels A-B: Paychex, Intuit, Earnin, Kronos