

Tracking the Pandemic in Real Time: Administrative Micro Data in Business Cycles Enters the Spotlight

Joseph Vavra

Looking back, we now know that the US unemployment rate at the start of the COVID-19 pandemic rose from 3.2 percent in February 2020 to 4.1 percent in March and 13.1 percent in April. However, the April unemployment rate was not reported by the Bureau of Labor Statistics until early May. Preliminary data on April retail sales was not released by the Census Bureau until mid-May, and the first release of gross domestic product data by the Department of Commerce covering April did not occur until the end of July. Thus, a number of economists turned to private-sector micro data to try to understand the recession while it was still unfolding: for example, data on employment patterns from the payroll processing firm ADP and the scheduling firm Homebase, data on bank accounts and credit card payments from sources like the JPMorgan Chase Institute and firms that provide financial planning services like mint.com and SaverLife, and even data on locations of cell phone users from firms like PlaceIQ and SafeGraph. The use of administrative micro data from these and other sources allowed pandemic-related research to be produced in nearly real-time and the scope for analysis of individual behavior, which would be impossible using traditional aggregate data.

In this essay, I loosely define administrative data as that arising as a by-product of some non-research activity, which contrasts with traditional data sources that are primarily collected for research purposes, like the Panel Survey of Income Dynamics, the American Community Survey, or the Consumer Expenditure Survey. The applications I discuss in this paper use administrative data collected by

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mortgage servicers, cell phone apps, credit bureaus, financial services firms, and payroll processing firms. These companies collect vast amounts of data in the course of their regular business, but this data is not collected with academic research as an end-goal. While this definition of administrative data could also include data produced by government administrations as a product of non-research activity, such as micro data on taxes and households from the Internal Revenue Service or data based on information from state-level unemployment insurance agencies like the Longitudinal Household-Employment Dynamics data, I focus my discussion on privately collected data.

Over the last decade, there has been an explosion in the availability and use of administrative micro data for economic research—this trend has cut across most empirical subfields of economics. But in this paper, I discuss ways in which this data has shaped macroeconomic research on recessions and stimulus policy. The Great Recession was the first business cycle to occur in this new age of administrative data availability, and although the research was using this data retrospectively, I begin the paper with a brief discussion of some macro lessons from this research. However, I focus mostly on the pandemic, because administrative data has played a crucial, early role in shaping our understanding of this period. For example, the massive and incredibly rapid increases in unemployment at the start of the pandemic were particularly concentrated in low income, service-sector jobs, while high income workers were largely insulated from job loss and saw only modest cuts in nominal wages. Expanded unemployment benefits had a substantial and immediate effect on spending, but did little to discourage job search.

In some cases, the patterns uncovered with studies of administrative data can also be seen with traditional data sources, but they were apparent weeks or months sooner because administrative data is often available in nearly real-time. This opens up the possibility for faster policy reactions. In other cases, administrative data leads to insights which cannot be obtained with traditional data sources. Traditional survey-based data typically either have small sample sizes or a limited panel element, they often have non-trivial measurement error, and they are often released with substantial lags. Administrative data can provide novel insights by measuring variables with more precision than traditional data methods or by measuring variables that are not captured by any traditional data sources. In addition, vast sample sizes can enable very detailed data cuts and statistical precision and can sometimes allow for new sources of variation and identification strategies.

Administrative data also raises challenges and concerns. The raw data itself was collected for other purposes, and for researchers, it can often be messy and difficult to interpret. The nature of administrative data means it often has a narrow lens of focus, with great depth but more limited breadth. Representativeness and external validity of administrative data are often big concerns. The greater statistical precision does not necessarily mean that estimates are unbiased, because large sample sizes do not themselves solve identification challenges and resolve issues of causality.

I do not attempt to provide a comprehensive review of all research that uses administrative micro data to understand recessions; indeed, given the explosion of

work in this area and the many administrative datasets now in use, that task would be enormous indeed. Instead, I intentionally choose a small number of applications, and only a few papers within each application to illustrate some of the breadth of administrative micro data available as well as some common challenges. I focus on applications to the US macroeconomy, but there is clearly also a vast amount of administrative micro data, policy variation, and applications of interest in other countries.

Administrative Data in the Great Recession

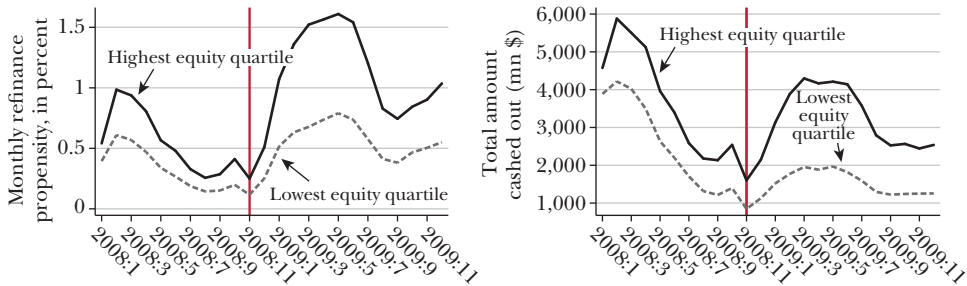
The Great Recession from 2007 to 2009 offered the first widespread application of administrative micro data to understanding recessions. The key role of house price declines and mortgage market disruptions in the Great Recession is widely established, most prominently with the work of Mian and Sufi (for an overview, see their 2018 essay in this journal). The fact that the recession itself originated in mortgage markets made data on mortgages (and credit more broadly) vital for understanding this period, and the simultaneous rise in “big data” information technology infrastructure meant that credit providers had such data. Many lenders were willing to make this data available to researchers, and macroeconomists took advantage. Here, I focus on some applications from my own work to illustrate some of the insights and the limits of such data.

In Beraja et al. (2019), we use administrative micro data to show that in addition to this direct effect of house prices on the economy, the house price boom–bust also substantially constrained the monetary policy response to the Great Recession. Collateralized borrowing in the housing market is an important part of the monetary transmission mechanism, because interest rate cuts encourage households to refinance their mortgage and extract home equity to fund current consumption. However, using the Credit Risk Insight Servicing McDash (CRISM) dataset produced by merging mortgage servicing records from Black Knight Financial Services (BKFS) with credit bureau data from Equifax, we show that this transmission mechanism was substantially dampened during the Great Recession.

This monthly panel data includes detailed loan-level characteristics including loan balances, interest rates, and origination characteristics for tens of millions of loans which are serviced through BKFS. These loans are, in turn, linked to Equifax borrower credit records, allowing us to measure consolidated borrowing positions, creditworthiness, and, most importantly, letting us link successive loans for a specific borrower across time. This monthly data ultimately allows us to measure refinancing and home equity extraction after the large declines in mortgage rates induced by the first round of the Federal Reserve’s quantitative easing program in November 2008.

During the Great Recession, house prices fell substantially on average, but declines varied greatly across space. Using the CRISM micro data, we show that interest rate declines during the Great Recession had the smallest effects on

Figure 1

Mortgage Refinancing and Cash-Outs during the Great Recession

Source: This figure is reproduced from Beraja et al. (2019, Figures 3a and 4a).

Note: Left panel shows refinancing propensities in the lowest equity (largest house price decline) compared to highest equity metropolitan statistical areas, and the right panel shows cash-out volumes.

refinancing in the locations with the largest house price declines and increases in unemployment. For example, both refinancing in general and cash-out activity, in particular, rose much less after quantitative easing (marked by the red lines in Figure 1) in these locations with little housing equity. An obvious explanation is that many households were underwater in these locations, making it difficult or impossible to refinance, and for households who could refinance, they had more limited equity to extract.

Translating this cross-region evidence through the lens of a macro model, we conclude that the house-price bust substantially constrained the stimulative power of monetary policy during the Great Recession. In closely related work, Berger et al. (forthcoming) uses this same CRISM data over a much longer period of time to show that the strength of this refinancing channel of monetary policy is influenced not just by house price movements but also by the past behavior of monetary policy itself. Keeping interest rates low for an extended period of time, as has been done since the Great Recession, means that many households lock in low interest rates and become less sensitive to future rate stimulus—even after rates return to more normal levels.

This research using administrative data provides a new basis for thinking about the aggregate strength of this refinancing channel. First, while refinancing has been studied empirically for almost 40 years, older research tended to focus on data with relatively small samples, which may or may not be representative when trying to draw inference for the economy as a whole. In contrast, the CRISM data covers around 60 percent of the entire mortgage market, with a broad cross-section of loan types and characteristics, so it is much more likely to be representative. Second, prior research typically studies loan-level datasets without links across time, which means earlier studies cannot separately distinguish loan prepayment arising from rate refinancing, cash-out refinancing, moving, and default.

However, this data is not a panacea, and it faces some limitations and pitfalls in answering some important questions. The data provides a very detailed lens into mortgage characteristics and household liabilities, but it has essentially no useful information on other important aspects of the household balance sheet like spending, assets, or income. Because this data has greater depth than breadth in its lens of coverage, it is difficult to answer important questions like how spending responds to mortgage refinancing or how refinancing responds to income shocks. While some papers (like Bartlett et al. 2019) have linked this data to other administrative datasets to expand the set of demographic information available, to my knowledge, no links to many key covariates like income exist.

On this particular point, it is useful to highlight a potential pitfall that arises frequently in the use of administrative micro data: imputed data. Casual users of Equifax CRISM data may misleadingly think that Equifax *does* collect information on income because their data reports such information. However, these variables are entirely imputed rather than coming from any actual data on income, so they are of little practical use. This particular data is well-documented and the imputation points are clear from inspection of data codebooks. However, the point illustrates a broader practical concern with the recent rise of various “big data” insight providers that mark micro data products. These products often draw from many different sources with little transparency and market the breadth of data that they provide—even though some of this data is imputed or predicted using machine learning. It is important, but not always possible, to distinguish actual from imputed data in these sources when commercial motives mean there is little transparency about underlying sources or methods. While this concern is clearly data-set specific, I recommend that researchers invest time to really understand the collection and construction of the data that they use in order to limit these potential problems.

While a lot of research is focused on mortgage markets, many papers also used administrative micro data to explore more typical macroeconomic questions during the Great Recession. For example, Stroebel and Vavra (2019) and Grigsby, Hurst, and Yildirmaz (2021) study the cyclicity of prices and nominal wages, respectively. Stroebel and Vavra (2019) use weekly store-UPC pricing data from the marketing firm IRI to construct local price indices and use these indices in a cross-zip-code-based identification strategy to argue for procyclical price markups. This type of analysis would be impossible with traditional price indices, which are only available at much higher levels of spatial aggregation. Grigsby, Hurst, and Yildirmaz (2021) use ADP data (discussed in more detail in the following section) to show that nominal wage cuts were much less common during the Great Recession than implied by traditional datasets. In traditional datasets, it is very difficult to measure the frequency and size of nominal wage adjustment because even tiny measurement errors can contaminate results. ADP data measures the actual wages paid using administrative data from those paying the wage and can thus eliminate wage “cuts” arising from measurement error.

All of the Great Recession research highlighted in this section exploits administrative micro data for research, which could not be performed with traditional

datasets. In general, this type of unique analysis is where administrative data has the highest value added. However, even in these research applications which would be impossible with traditional sources, it is important to highlight that administrative data does not supplant traditional data. This research still relies on traditional data sources for crucial benchmarking steps and validation of representativeness, which are pervasive concerns with administrative data.

Great Recession research demonstrated the fundamental value of administrative micro data for macroeconomics. Furthermore, the relationships established between academic researchers and data providers through this research, in turn, played a crucial role in speeding analysis of the pandemic when it arrived.

Administrative Data in the Pandemic

Economists have responded to the worldwide health crisis with an unusually rapid and focused outpouring of research on its economic effects. This analysis has been produced much more rapidly than in the Great Recession, often being released weeks or even just days after relevant events. This pace of research opens new opportunities for influencing policy as it unfolds rather than later analyzing the consequences of policy, but it also introduces a number of novel challenges. There are obvious trade-offs between producing deep and careful research that will stand the test of time and producing research quickly. Indeed, the fast pace of this research means that more findings will likely eventually need to be revised or clarified relative to research conducted at a more typical academic pace.

In my discussion here, I choose applications with two goals in mind: 1) I want to highlight several broadly different types of administrative data used to understand the pandemic recession; and 2) I want to highlight results and conclusions that have received some amount of support in multiple administrative data sources or with traditional data. Most of the research I mention here focuses on the period from March to September 2020. We know that underlying health and economic conditions have changed rapidly across time, so it is important to note that conclusions from research looking at this early stage of the pandemic may differ from research examining the current stage of the pandemic or the eventual recovery over the coming months or years.

Labor Market Data

Some of the first empirical research on the pandemic focused on measuring labor market disruptions using administrative micro data. Cajner et al. (2020) and Grigsby et al. (2021) use data from the payroll processing firm ADP, and Bartik et al. (2020) use data from the scheduling firm Homebase to document a number of labor market facts in the early stages of the pandemic.

ADP is a large human resources firm providing payroll processing for around 26 million US workers each month. This data is broadly representative of private sector employment using a variety of external benchmarks, although it modestly

overweights medium-large firms (Cajner et al. 2020; Grigsby, Hurst, and Yildirmaz 2021). Homebase is a scheduling firm providing services to tens of thousands of small businesses that employ hundreds of thousands of workers. This dataset is much less representative because it is skewed towards small firms in sectors like restaurants and retail that disproportionately employ hourly workers. However, these firms were among those most disrupted by the pandemic and they are otherwise somewhat underrepresented in the ADP data. Furthermore, Bartik et al. (2020) complement the raw data with an additional survey of Homebase users, allowing for some additional insights using this data.

Both papers show the striking distributional effects of the pandemic: lower-wage workers were much more likely to lose their jobs than higher-wage workers during this time period. Figure 2 illustrates these findings. In part, this pattern arises because low-wage workers tend to be concentrated in sectors like food service, which were particularly hard hit by the pandemic. Furthermore, this specific low-wage segment of the population is particularly vulnerable because these workers are also less likely to have substantial savings.

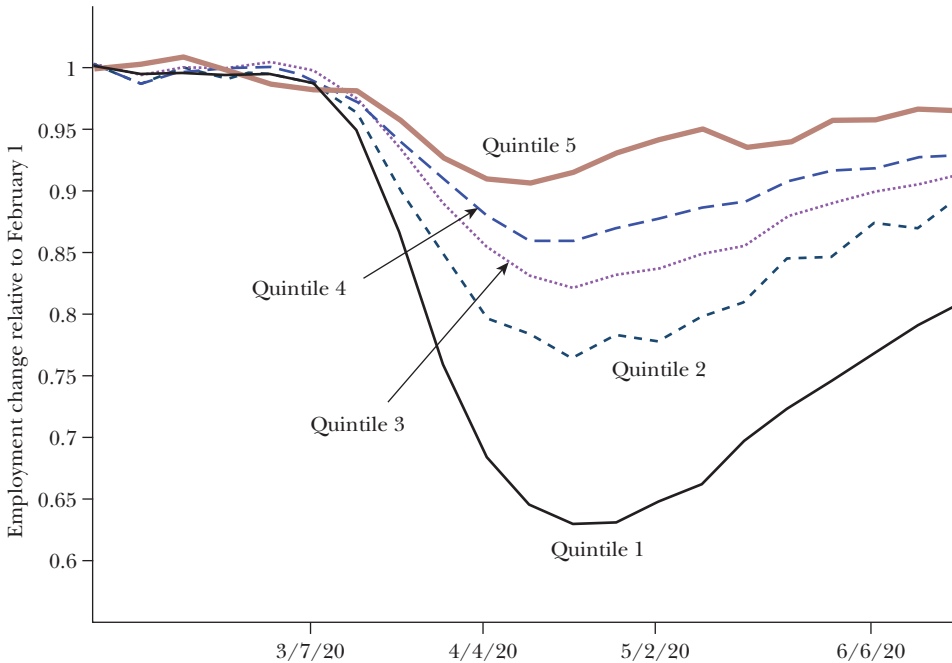
These broad distributional patterns are masked when focusing on the aggregate unemployment rate, and they were first established in these administrative datasets. Several papers have now documented this same pattern of greater unemployment for low-wage workers using traditional public datasets (for example, Ganong, Noel, and Vavra 2020; Cortes and Forsyth 2020). Thus, a main advantage of administrative data in this context was its speed, rather than a unique lens. Using administrative data to understand labor market trends 4-6 weeks earlier is of great use for policy-making decisions, but is arguably less crucial at the typical horizons of academic research.¹

However, these administrative data studies also offered some more unique insights. Bartik et al. (2020) decompose the total reduction of worker hours and find it was primarily driven by extensive margin effects with firms shutting down entirely or reducing the size of their workforce, rather than intensive margin effects where hours were reduced but workers remained employed.

As discussed in the previous section, ADP data has a unique ability to measure nominal wage adjustment because it measures actual payments made to workers and thus does not suffer from measurement error, which contaminates traditional survey-based data. Following Grigsby, Hurst, and Yildirmaz (2021), Grigsby et al. (2021) find that 6 percent of workers (mostly at the top of the income distribution) received nominal wage cuts early in the pandemic, but that 30 percent of these wage cuts were reversed by November. The pace of this nominal wage adjustment

¹Indeed, publicly available labor market data is itself already available quite rapidly. Thus, speed will generally be a greater comparative advantage for administrative data on spending, rather than for labor market data, since public data on spending is produced with moderately longer lags. See Chetty et al. (2020) for an effort to produce and publicly distribute daily statistics on consumer spending, business revenues, employment rates, and other key indicators disaggregated by ZIP code, industry, income group, and business size, based on anonymized data from a group of companies. For details, see <https://tracktherecovery.org/>.

Figure 2
Employment Change Relative to February 1, 2020, by Income Quintile



Note: Computed with ADP data in Cajner et al. (2020, their Figure 4a). The lines show pre-pandemic wage quintiles.

is substantially greater than during the Great Recession, but it still implies modest effects on overall earnings relative to the layoffs at the bottom end of the distribution. Overall, this data shows that the main labor market effect of the pandemic has been a large increase in unemployment at the bottom of the distribution, and that there is a more modest decline in wages but with continuing employment at the top of the distribution.

Moving forward, there will undoubtedly be much more research using this administrative data to understand labor markets. Two of the biggest advantages of these administrative data relative to traditional data sources are the ability to link individual workers together with firms so that workers can be tracked over time, and the fact that pay and hours can be measured exactly without the measurement error from self-reported data. On the other side, a potentially significant concern is that this data captures small employers (Homebase) or broader private-sector employment (ADP), but it has essentially no information on public-sector employment. If state and local budget cuts (early in the pandemic) or surpluses (later in the pandemic) lead to public sector employment changes, this data will largely miss these trends.

Financial Accounts Data

Data from individual bank accounts can be used to study various household-level outcomes. The first and most direct source of account-level data are banks themselves. The primary source of such data in the United States is the JPMorgan Chase Institute (JPMCI), a think tank within JPMorgan Chase & Co., which has developed a strictly controlled process to use anonymized account-level data on the universe of Chase customers directly for academic and policy research. The second common source of bank account data comes from financial service companies, which often require the user to first provide bank account log-in information to obtain some service; once the company obtains this data, they then make anonymized versions available for research purposes. For example, users of *mint.com* and *SaverLife* users enter all of their various account information, and these companies then provide financial planning services and budgeting information to their users. Users of *Earnin* can sign up to receive free payday loans, but they must first link to a bank account in order to do so.²

Bank account information provides a detailed and high-frequency lens into individual economic behavior. It typically contains transaction-level information on both account inflows (like direct deposits) and account outflows (like debit card transactions), allowing researchers to measure the connection among high frequency income, spending, and savings. Other datasets have detailed information on individual components (for example, the ADP data described in the previous section for income, or data produced by Visa or credit card processing companies for spending), but cannot link these components at a household level. Such links turn out to be crucial for some of the insights using this data to study the pandemic.

The JPMCI data has the further advantage of large sample sizes: as of 2015, it includes 27 million checking accounts (Farrell and Greig 2015). In addition, Cox et al. (2020) shows that the distribution of income in this data is generally similar to that of the population as a whole (although by construction it does not include any “unbanked” households). While the JPMCI data has essentially a complete, transaction-level accounting of everything that occurs within Chase accounts, a corresponding disadvantage of this data is that it has a limited lens for anything that occurs outside of Chase accounts, such as activities in second bank accounts or on non-Chase credit cards. For this reason, most research using JPMCI introduces various screens so that inactive or barely active accounts are not included in the analysis, but it is nevertheless possible that some important non-Chase activity is missed.

An advantage of bank account data from financial aggregators like *SaverLife* is that users have strong incentives to include all of their active accounts in order to obtain reliable planning information. These datasets do tend to have much smaller sample sizes, and selection is more of a concern because users choosing to

²Facteus also provides some similar information from card processing, combining individuals using debit cards, payroll cards, and load cards at an account level, although I have not worked with this data and am less clear on the underlying nature of the sample.

use financial planning apps may not be representative of the broader population. However, Baker (2018) provides substantial benchmarking and validation exercises to argue that these selection concerns are of more limited import in his applications and that data from these types of platforms are indeed informative about broader behavior. While few studies have used Earnin data, these selection issues are likely to be an even greater concern for that data because this sample is built on those seeking payday loans. However, that data may be useful for understanding the behavior of particular vulnerable populations of interest that might be under-represented in other datasets.

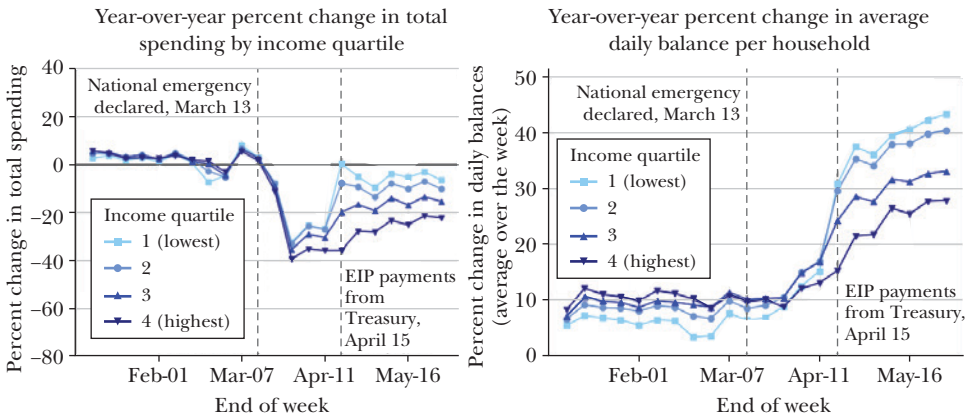
What was learned from administrative bank account data during the pandemic? Cox et al. (2020) complement the analysis of labor income losses discussed earlier by using JPMCI data to show how spending and savings have changed across the income distribution over the same period. This analysis requires linking individual income, spending, and savings, which is possible only as a result of the unique lens offered by administrative financial account data.³ Using a sample of around five million active account-holders for which they can measure pre-pandemic income using direct deposit information, Cox et al. (2020) find dramatic declines in spending across the income distribution at the end of March 2020. These spending declines are strongest in certain categories like entertainment and hotel accommodations, which require in-person activity.⁴ However, starting in mid-April, spending recovers much more rapidly for *low-income* households. At the same time, these low-income households also see the largest growth in checking account balances. Figure 3 illustrates these patterns. This finding seems surprising in light of the evidence from Cajner et al. (2020) and Bartik et al. (2020) that these households had the largest declines in labor income over this same period in time.

How can the households experiencing the most job loss during the pandemic fare best in terms of spending and savings growth? The timing suggests an important role for government support programs. The divergence in spending patterns occurred shortly after the passage of the Coronavirus Aid, Relief, and Economic Security (CARES) Act, which created large transfers that disproportionately benefited low-income households. In particular, the CARES Act provided one-time broad-based Economic Impact Payments of \$1,200 for most adults and created a Federal Pandemic Unemployment Compensation program that added \$600 per week on top of regular state unemployment insurance benefits from April through July. The Economic Impact Payments were the same absolute size for all but the

³Some of these spending patterns could potentially be explored in the publicly available consumer expenditure data once it becomes available covering this period. In addition, although Chetty et al. (2020) do not have individual income data, they find similar zip code-level spending patterns across zip codes with high and low income, suggesting that these spending patterns over the income distribution are not unique to JPMCI data. Finally, it is useful to note that the large run-up in savings observed in JPMCI is consistent with the spike in savings observed in aggregate data once it was released.

⁴These category spending patterns do not require account-level links and have been observed in a variety of credit card data sources including Affinity (Chetty et al. 2020) and Womply (Alexander and Karger 2020). These papers include a much more extensive discussion of these category patterns.

Figure 3

Changed in Total Spending and Average Daily Balances during the Pandemic

Source: Cox et al. (2020, Figure 5a and 11b).

highest-income individuals, so they resulted in larger income growth for low-income households. The \$600 unemployment insurance supplements even more disproportionately benefited low-income households, in part because low-income households were more likely to be unemployed, and in part because a flat weekly \$600 supplement represents a larger share of pre-job loss income for low income individuals. Figure 3 uses vertical lines to show the date when the national pandemic emergency was declared and when Economic Impact Payments were first distributed by the US Treasury.

These correlations with policy timing are merely suggestive, but several papers have used administrative data to make an argument for causal effects of these expanded transfers. Baker et al. (2020) use the SaverLife data described above to analyze high frequency spending responses to the one-time Economic Impact Payments. Using daily data on 38,000 active account users from December 2019 through May 2020 they identify just over 23,000 Economic Impact Payments by looking for direct and check deposits in certain categories with sizes corresponding to common amounts of these payments, like \$1,200 and \$2,400. They then estimate a marginal propensity to consume from 7 days before to 23 days after the receipt of the payments, using a distributed lag regression controlling for individual and time fixed effects to estimate responses. Overall, they find a cumulative marginal propensity to consume of around \$0.37 in their raw sample, or \$0.27 when they reweight their relatively low-income sample to instead match the distribution of income and several other observables in Current Population Survey data. Users with earnings under \$1,000 per month had a marginal propensity to consume roughly twice as large as users earning \$5,000 a month or more. There is an even steeper gradient with liquidity, as users with the highest account balances have marginal propensities to consume around 0.1 while those with balances under \$100 have marginal

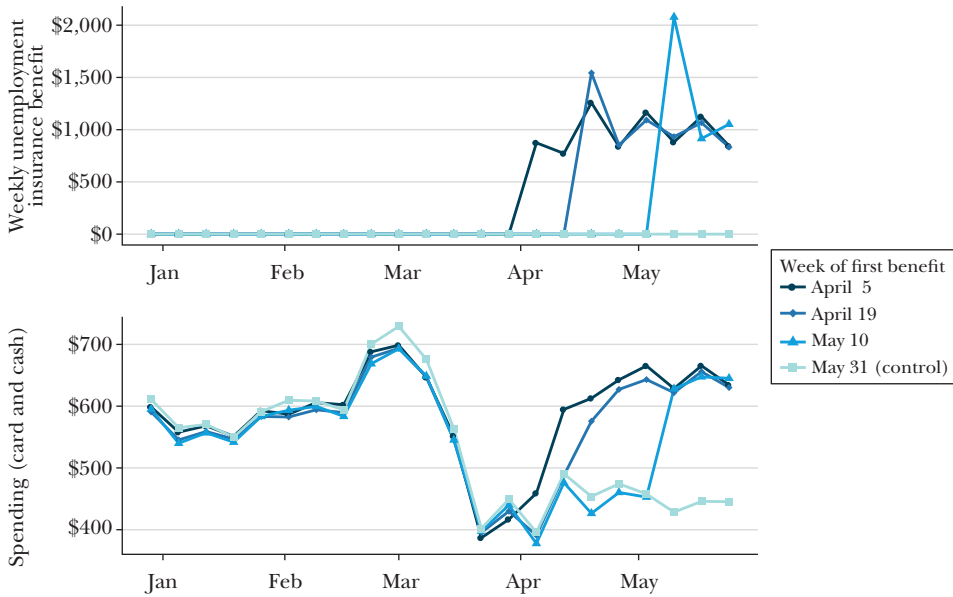
propensities to consume of 0.4 or more. Overall, these results are consistent with evidence from prior regressions in Johnson, Parker, and Souleles (2006) and Parker et al. (2013) that stimulus checks significantly boosted spending.

However, even though the SaverLife data is vastly superior in frequency, detail, and accuracy to the Consumer Expenditure Survey data used in studies of earlier recessions, the policy variation itself during the pandemic is harder to interpret from a causal identification perspective. Unlike in the 2001 and 2008 recessions, the timing of stimulus checks during the pandemic was not random and was concentrated over a shorter period. Paper checks during the pandemic were prioritized by income, and direct deposit timing depended on past tax filing status and differed for those on Social Security and other government support programs, all of which are correlated with income. The non-random timing of payments along with the differential trends in spending by income shown in Cox et al. (2020) introduce some concerns about interpreting their estimates of marginal propensity to consume, especially at longer horizons, as pure causal effects. I note this not as a critique of their general conclusion that stimulus checks increased spending. Given the very high frequency of sharp breaks in spending observed in Baker et al. (2020), there is little doubt that the stimulus checks did boost spending. But I do want to highlight the broader point that using administrative data, even when it might have enormous sample sizes and more precise measurement, does not in-and-of-itself solve identification challenges.

Overall, Economic Impact Payments were a large part of the initial US government response to the pandemic, totaling \$270 billion by the end of May 2020, and the evidence in Baker et al. (2020) implies they had an important role in increasing spending, especially for lower-income households as shown in Figure 4. In addition to these broad-based payments, the more targeted \$600 weekly supplements to unemployment insurance also played a particularly important role in increasing spending for low-income households. Overall, the aggregate scale of these expanded unemployment benefits was similar to the Economic Impact Payments, with roughly \$260 billion in expanded benefits paid out from April through the end of July 2020. However, unlike Economic Impact Payments, these \$600 payments were targeted at households with declines in labor market income. These \$600 supplements nearly tripled typical benefit levels and resulted in benefits that replaced about 145 percent of lost income for the median worker.

Ganong et al. (2021) use JPMCI data to show that expanded unemployment benefits substantially boosted the spending of unemployed households, while having a comparatively muted effect on job search at the time. They show that while the \$600 supplements were available, the net income and spending of unemployed households actually rose rather than declined after job loss, both in absolute terms and relative to that of employed households. Using a variety of identification strategies, they estimate causal spending responses to the start of the \$600 unemployment insurance supplement in April 2020, to its expiration in August, as well as to additional short-term supplements of \$300 that were paid out in September. For example, they estimate causal responses to the start of expanded unemployment

Figure 4

The Timing of Unemployment Insurance and Increased Spending

Source: Ganong et al. (2021)

benefits by comparing the spending and income of households who all become unemployed at the end of March 2020 but begin receiving unemployment benefits at different dates.

The spending of these different cohorts is nearly identical up to the start of unemployment benefits and then jumps immediately when benefits begin. Exploiting these differences across cohorts, they estimate a marginal propensity to consume out of benefits of 0.43. Strikingly, they also find high marginal propensity to consume at the time of benefit expiration in August as well as in response to an additional short-lived \$300 benefit increase paid out in September 2020, even though unemployed households had built up substantial liquidity by then through prior receipt of expanded benefits.

In contrast to these large spending responses, Ganong et al. (2021) find small effects of the \$600 on job finding. Simple job finding models calibrated to pre-pandemic evidence predict a very large and sustained increase in job finding after the expiration of unemployment benefits—the job finding rate was quite stable from May through October. Moreover, they find an important role for recall to previous employers, rather than transitions to new employers, in explaining what fluctuations in job finding rates do exist over this period. Fitting a job search model with various elements to match the patterns in job finding and recalls, they estimate that employment distortions induced by the \$600 unemployment insurance supplements were much lower than implied by pre-pandemic distortion estimates. They

also use this data to document a number of novel labor market facts that traditional data sources do not measure: for example, traditional unemployment data does not track individuals, but they show that repeat unemployment is particularly important during the pandemic.

Overall, this research leverages administrative account-level data in crucial ways, which cannot be done using other datasets currently available, by linking account-level income measures to account-level measures of spending and saving. However, this account-level data is ill-suited for answering certain other questions of great interest. For example, what fraction of households who lost jobs received unemployment insurance and how long do they have to wait to receive benefits? It might seem that bank account data could be used to answer this question, but in fact a large fraction of individuals now receive their unemployment benefits via pre-paid debit cards. These cards are unobserved in JPMCI data, and given their transitory nature, they are also unlikely to be linked in financial account aggregators like SaverLife. As a result, this financial account data cannot distinguish a worker who is waiting-for/denied/never-filed-for unemployment benefits from one is currently receiving unemployment benefits via a prepaid card.

Cell Phone Data

Cell phones produce near-constant streams of data that allow for detailed information on geographic location at very high temporal frequencies in near real-time. During the pandemic, movement and social interaction was of even more direct interest than usual. This data can also be used to proxy for shopping activity in narrow geographic areas, which can be used to identify the effects of government shutdown and reopening policies.

Couture et al. (2021) use data on roughly 75 million unique cell phones from PlaceIQ to construct a “daily location exposure index” that captures county-to-county movements together with a “device exposure index” that measures the exposure of devices to each other within venues. These exposure indices, which they post publicly every weekday, have been used in a variety of papers. In addition to providing this public good, Couture et al. (2021) provide an extensive discussion of representativeness and advantages and disadvantages of this cell phone data in addition to documenting several interesting patterns of movement during the pandemic.

I will not repeat that discussion here but will highlight a few key observations. First, the data is broadly representative of general population distributions and flows across space when benchmarked against various external data sources, but it is more representative when studying broader geographic areas like counties or states than when studying vary narrow geographies. Second, the PlaceIQ and most other US-based cell phone data is collected through smartphone apps with location-tracking services rather than directly from cell-service providers. Because older adults are less likely to own smartphones, older households are less represented in this data. This can, in turn, be important in studies of the pandemic, given that COVID-19 exhibits a sharp age-gradient in disease outcomes. They also caution

that cross-location level comparisons are likely to be less reliable than time-series variation within locations across time due to differences in coverage and representativeness across space. Furthermore, device IDs turn over frequently, which means that the panel element at the level of individual devices is typically limited to around six months.

Couture et al. (2021) is primarily focused on the development and validation of their exposure indices rather than on particular applications, but they do demonstrate a number of interesting results. During the pandemic, for example, the indices show that a sharp decline in movement in and out of Manhattan is detectable in near real-time in the early stages of the pandemic. More generally, they also explore the relationship between cell phone visit data and credit card spending data, which track each other very closely in some categories like arts and entertainment but sharply diverge in other categories like grocery spending. This divergence in grocery spending likely reflects a substitution towards online purchases together with the consolidation of multiple trips with smaller expenditure into single trips with larger expenditure per trip. Thus, while visits and spending generally track each other, this is not uniformly true. As a result, questions focused on physical presence, like in-person shopping time, are likely to be more reliably answered with cell phone data than questions about ultimate expenditures.

Goolsbee and Syverson (2021) use similar cell phone data from 45 million cell phone users produced by SafeGraph to try to understand the factors driving declines in consumer traffic from March 1 to May 16.⁵ In particular, they seek to differentiate the role of government-imposed restrictions from households voluntarily staying home in driving changes in consumer behavior. To do this, they combine local store visit data from SafeGraph with county- and city-level shutdown policies and implement a cross-border identification strategy, which compares weekly shopping visits across counties with different restrictions within commuting zones. In particular, commuting-zone fixed effects should help to control for unobserved factors, like health fear, that are common to the commuting zone within that week. Thus, the effects of government restrictions will be identified only from variation in consumer behavior across counties with different policies all within the same commuting zone. This identification strategy reduces the concern that correlations between government restrictions and declines in consumer activity reflect a common response to rising health risk rather than a causal effect of the restrictions themselves.

Overall, they find that while consumer traffic fell by 60 percentage points, legal restrictions explain only 7 percentage points of this decline, which means that declining economic activity was predominantly driven by direct consumer responses to the virus rather than by government shutdown policies. Of course, this result needs to be interpreted with the usual caveat that cross-sectional causal effects may differ from aggregate effects; for example, any restrictions in one county that have

⁵Updates after publication of the original paper, available at (<https://bfi.uchicago.edu/insight/research-update-drivers-of-economic-decline/>), extend the analysis to the period of re-opening in the summer and to the second round of closures in the fall and find nearly identical results.

spillover effects on the commuting zone as a whole will be missed by this empirical strategy. Nevertheless, such spillovers would need to be implausibly large to undo the main message. Also, it is important to reiterate the point raised above that cell phone data captures store visits, not expenditures. However, Alexander and Karger (2020) use a similar identification strategy with store-level credit card spending data from Womply and arrive at very similar conclusions.

While the applications discussed in this section used a variety of different data sources, several lessons emerge. First, the pandemic caused substantial declines in spending which were very concentrated in certain service sectors. Second, these sectors employ many low-income households, so the pandemic led to much larger unemployment for low-income households. Third, these declines in spending and increases in unemployment were largely unavoidable in the sense that they were caused directly by health fears rather than by mandated government shutdowns. Fourth, US government transfers, in the form of broad-based stimulus checks and expanded unemployment benefit checks, led to substantial increases in spending for many of those otherwise hardest hit by the recession.

Additional Discussion and Conclusions

Some of the research results from using administrative micro data rely crucially on the particular lenses offered by this data. In other cases, similar results can eventually be obtained using traditional datasets, but the use of administrative data can allow research to proceed more quickly in a way that can offer more timely input for policy decisions. I close with a few additional reflections on the challenges facing researchers who are contemplating the use of administrative micro data.

First, administrative data can be difficult to interpret because samples are often unrepresentative and because they may contain a limited set of covariates of interest. For this reason, I stress that administrative micro data should be viewed as a complement rather than substitute for traditional data sources. Without representative data sources for benchmarking and validation, it is very difficult to interpret results from particular administrative data sources. Indeed, there has also been a dramatic expansion of traditional data surveys themselves during the pandemic. For example, the Census now conducts rapid household and business pulse surveys. Some of this data has, in turn, been used for similar near real-time analysis (as in Dube 2021).

Second, administrative datasets are often very large and thus will require substantial computational resources and time to analyze. Their size can also exacerbate the first concern about interpretation because it can be harder to notice data anomalies and other issues when the data itself is cumbersome to analyze.

Third, administrative datasets often have significant barriers to access: they may be expensive to purchase or might depend on personal connections for access. Further, continued access and data availability is often not assured. For example, data access can disappear because the provider goes out of business, changes

business models, becomes subject to new legal restrictions, changes licensing terms, or for many other unanticipated reasons. Even public-spirited firms quail at the prospect of making a commitment to maintaining these data-sharing arrangements over a period of years for each research project they approve.

These issues with access and the institutional risk of private-sector administrative data raise some concerns for the economics profession. Research directions may be overly influenced by the interests of a small number of individuals privileged with access. Other researchers may have limited or no ability to test the reproducibility and robustness of findings and to carry out extensions of the analysis. Journals are increasingly requiring detailed replication code and detailed data access instructions in online data repositories as conditions of publication. These repositories and associated access information are often a useful starting point for those interested in using the same data for follow-up work, but they do not themselves eliminate access barriers.

However, other kinds of administrative data are becoming more publicly available. For example, some academic institutions have been taking on a role as data intermediaries. The Kilts Center at the University of Chicago Booth School of Business acquires data from AC Nielsen and other private providers and then administers widely available academic licenses for this data. During the pandemic, Opportunity Insights began publicly publishing data that they obtain from a very large variety of private data providers (Chetty et al. 2020), although confidentiality agreements mean that this is not micro data and is instead aggregated to zip code or higher levels of aggregation. Finally, in response to the pandemic, many data providers have reduced the barriers to entry for acquiring micro data. For example, the SafeGraph data discussed above is now widely available for academic use.

Some government institutions have also embraced the use of administrative micro data and might play a role in this process. The Federal Reserve, the Consumer Financial Protection Bureau, the Office of the Comptroller of the Currency, and other regulators have access to a wide variety of administrative micro data, and they increasingly allow the use of this data to enrich their internal research: for example, Aladangady et al. (2019) discusses the potential of high-frequency administrative data for informing public statistics. These institutions have also increasingly provided paths for external researchers to access this data. By acting as an intermediary between private data providers and the research community as a whole, this model can potentially break down some barriers and democratize access to this data.

For individual researchers, working with administrative data often requires a significant investment of time and a degree of risk. Nevertheless, this comes with the opportunity for transformative research insights, which could not be made with other sources.

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