Hutchins Center Working Paper #98

September 2024

COBOLing Together UI Benefits:
How Delays in Fiscal Stabilizers Affect Aggregate Consumption

Michael A. Navarrete†

NBER

Formerly of the Hutchins Center on Fiscal and Monetary Policy at Brookings

Abstract

The United States experienced an unprecedented increase in unemployment insurance (UI) claims starting in March 2020. State UI-benefit systems were inadequately prepared to process these claims. In states that used an antiquated programming language, COBOL, to process claims, potential claimants experienced a larger increase in administrative difficulties, which led to longer delays in benefit disbursement. Using daily debit and credit card consumption data from Affinity Solutions, I employ a two-way fixed-effects estimator to measure the causal impact of having an antiquated UI benefit system on aggregate consumption. Such systems led to a 2.8-percentage-point decline in total credit and debit card consumption relative to card consumption in states with more modern systems. I estimate that the share of claims whose processing was delayed by over 70 days rose by at least 2.1 percentage points more in COBOL states relative to non-COBOL states. Based on a back-of-the-envelope calculation using 2019 data, my results suggest that the decline in consumption in COBOL states in 2020 after the pandemic-emergency declaration corresponds to a real-GDP decline of at least $105 billion (in 2019 dollars).

Acknowledgments: I am deeply indebted to Judy Hellerstein, John Haltiwanger, Ethan Kaplan, Katharine Abraham, Tara Wat-son, and Louise Sheiner for their invaluable guidance and support. I am grateful to John Shea, Melissa Kearney, Rachel Nesbit, Guan Wang, Annelies Goger, and seminar participants at the University of Maryland for their helpful comments and discussions. Eshna Ghosh, Yonathan Melamed, Isabel Roberts, and Emily Zhou provided excellent research assistance. All errors are my own.

Disclosure: The Brookings Institution is financed through the support of a diverse array of foundations, corporations, governments, individuals, as well as an endowment. A list of donors can be found in our annual reports published online here. The findings, interpretations, and conclusions in this report are solely those of its author(s) and are not influenced by any donation.

†Contact: navarrem@nber.org; Website: https://mnav-umd.github.io/

I Introduction

The COVID-19 pandemic caused a severe contraction in US economic activity, and the fiscal policy response was unprecedented. The federal government spent over $5 trillion on subsidies, transfers, grants, and tax cuts. But a lack of administrative capacity hindered the policy response, with implementation issues affecting both the types of programs enacted and the effectiveness of those programs. The massive spike in unemployment insurance (UI) claims at the beginning of the pandemic, combined with the creation of new UI programs, led to long delays in the disbursement of benefits and even outright crashes of UI systems, particularly in states with antiquated UI benefit systems.[1]

In this paper, I examine how problems administering unemployment insurance during the pandemic reduced the effectiveness of UI as a fiscal stabilizer. Specifically, I compare consumption changes during the pandemic in states that had not modernized their UI systems with those that had. I proxy for a lack of UI modernization with the use of COBOL (Common Business Oriented Language) in a state's UI benefit system. COBOL is an antiquated programming language developed in 1959 that was once used by all state UI programs. As of 2020, COBOL had been abandoned in the UI benefit systems of 22 states through modernization of their UI system.

I find that while aggregate consumption (as measured by credit and debit card purchases) fell precipitously in all states at the start of the pandemic and remained below pre-pandemic levels for several months, it was slower to recover in states with antiquated UI systems. Using a two-way fixed-effects (TWFE) estimator, I find that the relative decline in consumption from March 13, 2020 to December 31, 2020 was 2.8-percentage-points larger in COBOL states than in non-COBOL states. Using this estimate in a back-of-the-envelope calculation, I find that the failure to invest in updating UI-benefit systems in COBOL states caused real GDP to be at least $105 billion (in 2019 dollars) lower during this period.

UI serves as not only a safety net for laid-off workers during recessions but also as a macroeconomic buffer. UI benefits increase income for households with unemployed workers, which in turn increases household consumption. Because the fiscal-multiplier effect of a dollar of UI benefits during a recession is likely greater than 1, UI has positive general equilibrium effects, including effects on household consumption.[2] My estimates, therefore, likely reflect a combination of the direct effect on UI-eligible households in COBOL states and indirect effects in the form of a dampened fiscal multiplier. In other words, the consumption effect that I estimate is driven by claimants in COBOL states experiencing a relatively higher administrative burden, which led to UI functioning as a less effective fiscal stabilizer in COBOL states.

The primary mechanism by which COBOL usage in UI benefits led to lower aggregate consumption is delays in the UI benefits disbursement. COBOL states could have experienced delays in UI

[1] For anecdotal evidence on the problems in COBOL states, see news articles about COBOL usage in New Jersey, Wisconsin, and Connecticut.

[2] Kekre (2022) find that UI benefit extensions have a contemporaneous output multiplier of around 1 using data from 2008 to 2014. Di Maggio & Kermani (2016) estimate a local fiscal multiplier of unemployment insurance expenditures of around 1.9 using data from 1999 to 2013.

disbursements, both because it took longer for claimants to successfully file a claim and because processing those claims took longer.[3] Delayed claims could affect aggregate consumption through a dampened UI fiscal multiplier. Ganong, Greig, Noel, Sullivan & Vavra (2022) find that the one-month marginal propensity to consume (MPC) for claimants who received their benefits was highest in April 2020 compared to later periods when UI benefits changed, such as the expiration of the $600 supplement in July 2020. When households save a larger share of their UI benefits due to delayed disbursement, they will purchase fewer goods and services, directly lowering consumption. That household's consumption decisions will have spillovers, leading to a lower UI fiscal multiplier, since the UI fiscal multiplier is directly related to the MPC. Given that UI relative replacement rates were on average over 100% (Ganong, Noel & Vavra, 2020) from April 2020 to the end of July 2020, delaying UI benefits by months could significantly alter consumption behavior.

II Background

In this section, I describe the changes to UI benefits in 2020, provide more justification for the use of COBOL as a proxy for administrative capacity, and relate this paper to the broader literature.

A Changes to UI Benefits in 2020

To understand the effects of administrative capacity on UI disbursements in 2020, it is helpful to review what changed in UI benefits during the pandemic and the difficulties that potential claimants faced. Before the pandemic, potential claimants filed an initial claim with their state's UI office online, over the phone, or (least commonly) in person. After the emergency declaration on March 13, 2020, states faced an unprecedented increase in claims. This led many UI websites to crash, which further overwhelmed call centers.[4] Before and after the CARES Act, claimants had to demonstrate eligibility by having worked in covered employment during their base period. Prior to the CARES Act, a large share of jobs counted as covered employment, but self-employment, gig work, and contract work did not. However, these workers were made eligible for benefits by the CARES Act through its Pandemic Unemployment Assistance provision.[5] COBOL states disproportionately struggled to process claims, both with the unprecedented increase in initial claims filed and with implementing changes to UI benefit systems such as eligibility rules.

During recessions, the federal government often extends the duration of eligibility for UI benefits and rarely increases the benefit amount.[6] However, the magnitude of the benefit enhancement in

[3]Second, there may have been more discouraged filers in COBOL states—potential claimants who did not file a claim (or who did not complete the filing process) because they viewed applying for UI as too complicated or laborious. A survey conducted by the Economic Policy Institute shows that some claimants chose not to file for benefits because it was too difficult.

[4]Some states opened pop-up UI offices to process claims. Kentucky, a COBOL state, opened multiple pop-up offices.

[5]For a more complete discussion of the typical claim process during the pandemic, refer to Cajner, Figura, Price, Ratner & Weingarden (2020b).

[6]The Federal Additional Compensation (FAC) program, part of the American and Reinvestment Act of 2009, provided an additional $25 per week to UI recipients.

the CARES Act was unprecedented—an additional \$600 per week from April 2020 until July 31, 2020. The CARES Act also increased the maximum duration of benefits by 13 weeks.

When designing the CARES Act specifics, many policymakers were aware that UI benefit systems were antiquated and slow. They would have liked to increase the UI replacement rate (the share of base period earnings replaced by UI benefits), but making that change to state UI systems would have been difficult and would have greatly delayed the disbursement of the enhanced benefits.[7] Instead, policymakers increased benefits by \$600 per week for every beneficiary,[8] even though such a change led to replacement rates of over 100% for the majority of UI recipients. In other words, this supplement led to the average UI recipient receiving more from UI benefits than from their previous employer (Ganong et al., 2020).

Although initial claims spiked just after the emergency declaration and then declined, they remained elevated throughout 2020. Initial claims in every week in 2020 after the emergency declaration surpassed the previous recorded maximum initial claims between 1967 (when the data began to be collected) and the emergency declaration.[9] This persistent state of elevated initial claims meant that states had to process not only a large stock of existing claims from the first weeks after the emergency declaration but also a large flow of initial claims. (Initial claims may represent people who newly become unemployed but also new repeat claims by filers whose claims have not been processed and who may be unsure if they filed correctly.)

Across all states, the CARES Act provided the same additional benefits for claimants, in terms of both benefit amounts and maximum weeks to receive benefits. However, because states had different initial levels of maximum benefits and maximum duration, UI benefits during the pandemic were not uniform across the United States; these increases were different relative to baseline levels across states. For example, prior to the pandemic, Florida (a non-COBOL state) offered a maximum weekly benefit of \$275 for a maximum of 12 weeks, while New Jersey (a COBOL state) offered a maximum weekly benefit of \$713 for a maximum of 26 weeks.[10] However, variation in which states used COBOL affected both when and whether claimants received UI benefits. For example, Wisconsin, a COBOL state, experienced delays so significant that in June 2020, the Wisconsin Department of Workforce Development was still processing initial claims filed in March.[11]

B The Effect of COBOL on Administrative Capacity

COBOL can perform the same tasks as any modern programming language, but systems running on it were differentially overloaded by both the sudden influx of claims and the changes to UI benefits. COBOL states likely struggled more to implement the changes introduced by the CARES

[7] Personal communication with Wendell Primus, senior policy advisor to House Speaker Nancy Pelosi.

[8] After \$600-a-week UI supplements expired at the end of July 2020, claimants received \$300-a-week UI supplements until summer 2021.

[9] To be precise, the week ending on March 21, 2020 was the first week to surpass the previous recorded peak of 695,000 claims.

[10] For a complete list of how states varied in maximum UI-benefit allocation prior to the pandemic, see this table from a research brief by the Brookings Institution.

[11] See Wisconsin news report.

Act because of the difficulty of changing COBOL-based systems. Other UI-system failures were also related to COBOL. When the pandemic started, several COBOL states needed COBOL programmers. Because demand for COBOL programmers exceeded supply, some programmers came out of retirement to work in UI offices in COBOL states, but there were insufficient COBOL programmers.[12] Furthermore, COBOL states may have experienced issues not specific to COBOL but symptomatic of an antiquated system, including having a less user-friendly website, the absence of a mobile version of their website, or legacy platforms such as mainframes. These additional issues would have increased the administrative burden that potential claimants would have faced, which would affect delays in disbursement and increase the administrative burden for claimants.

Despite COBOL being capable of completing the same tasks as modern programming languages, implementing these changes were more difficult in COBOL states. One of the issues is that UI benefit systems using COBOL have spaghetti code; this programming code is complex, difficult to read, and highly complex. For example, code written using a fourth generation programming language (most modern programming languages) has half the number of lines of code as the same program written in COBOL (third generation programming language). For example, as of 2020 Wisconsin's (COBOL state) UI benefit system comprises of roughly 8.6 million lines of does, which has been updated and extended numerous times over the previous 50 years.[13] There is usually a lack of automation that requires frequent human intervention resulting in redundant and inefficient processing workflow. For example, Wisconsin experienced a surge of over 250,000 claims in 2020 rejected by the UI benefit system relative to 50,000 to 100,000 claims per year pre-pandemic. These claims that were rejected required human intervention, from the Adjustment and Special Programs (ASP) unit in Wisconsin's case. Wisconsin's ASP unit went from a staff of 16 to 140 in 2020. The staff increased disproportionately because the complexity of these claims increased by the introduction of new federal programs such as Pandemic Emergency Unemployment Compensation (PEUC) program and the PUA program. The increased complexity results in each rejected claim taking longer to process for adjudicators.[14]

All states once used COBOL in their UI-benefit system. Some states have modernized their systems, in part by switching to a more modern programming language, such as C# or Java, as shown in Figure 1. The decision to modernize could systematically differ between states. As noted by the Government Accountability Office (GAO) in a report prior to the pandemic, some states had trouble modernizing their UI benefit systems for reasons ranging from a lack of funding to difficulties with operating legacy stems in tandem with new systems.[15] Perhaps surprisingly, the states with more generous UI benefits and a more generous social safety net such as California and

[12] A group of retired COBOL programmers called the COBOL Cowboys exists solely to aid during crises. See NPR news article.

[13] For an in depth discussion on how an antiquated UI benefit system including COBOL usage affected the functioning of Wisconsin Department of Workforce Development please refer to their informational briefing on unemployment modernization that can be streamed here.

[14] For an in depth discussion on how costly the adjudication process (rejected claims) was for Wisconsin in 2020 please refer to their informational briefing on unemployment modernization that can be streamed here.

[15] See GAO report from 2013.

New Jersey were not more likely to have modernized their UI systems. In fact, the states with less-generous UI benefits during nonrecessionary times, such as Florida and North Carolina, were more likely to have modernized. As I discuss in Section B, the fact that COBOL is not randomly distributed across states is a potential source of bias, because the COBOL states are more likely to be Democratic states, and these states were more cautious about COVID-19. In order to account for these differences, I use the Republican vote share in 2016 as a proxy control variable in my empirical analyses.

These longer delays in UI disbursement in COBOL states could have affected aggregate consumption. Figure 2 shows the means in relative consumption without controlling for potential confounders. The figure plots the mean population-weighted values of total credit and debit card consumption for COBOL and non-COBOL states. After the emergency declaration, COBOL states recovered more slowly than non-COBOL states. Figure 2 provides suggestive evidence that delays in UI benefit disbursement affected aggregate consumption.

C Related Literature

This paper adds to the growing literature measuring the economic impacts of the pandemic recession (Faulkender, Jackman & Miran (2023); Cajner, Crane, Decker, Grigsby, Hamins-Puertolas, Hurst, Kurz & Yildirmaz (2020a); Chetty, Friedman, Hendren, Stepner & Team (2020); Coibion, Gorodnichenko & Weber (2020); Marinescu, Skandalis & Zhao (2021); Ganong et al. (2020)). I exploit a novel source of heterogeneity across states, COBOL usage for UI benefit systems, to measure the relative decline in aggregate consumption caused by the increased administrative burden faced by potential UI claimants in COBOL states. My work also contributes to the literature on fiscal stabilizers (Eilbott (1966); Dolls, Fuest & Peichl (2012); McKay & Reis (2016)) by being the first to directly look at the aggregate economic consequences of delaying a fiscal stabilizer: UI.

The paper in the pandemic-recession literature that most closely resembles mine is Ganong et al. (2022). The authors exploit delays in UI benefit payments using micro data to calculate the consumption response to UI benefits at the individual level, and then they use those estimates to calculate the effect on aggregate consumption. They find that the UI-benefit enhancements of $600 and $300 led to a 2.7% and 1.5% increase in aggregate spending, respectively, and that the $600 UI supplements increased aggregate consumption by $430 billion (in 2019 dollars) nationally from April 2020 to July 2020. Because the variation they exploit is at the individual level, they cannot directly estimate multiplier effects.

I ask a different question: how did the higher administrative burden in COBOL states affect aggregate consumption in those states? I find that aggregate spending from March to December 2020 was 2.8 percentage points lower in COBOL states relative to non-COBOL states, leading aggregate consumption to be at least $105 billion lower than it would have been if COBOL states had modernized their UI systems. My estimates are limited to the effects of using an antiquated UI-benefit system but do include multiplier effects (at least to the extent that these effects differentially affected the local economy).

This paper also contributes to the literature on the effects of administrative burdens on program effectiveness. The delays in COBOL states are a form of administrative burden: people had to spend hours on the phone or online trying to file, or go in person to the UI office during a pandemic, and they experienced long and uncertain wait times for their claims to be processed. These administrative burdens could have discouraged potential claimants from receiving benefits.[16] My work is the first to look at the macroeconomic consequences of administrative burdens in the context of a fiscal stabilizer. Herd & Moynihan (2018) note that administrative burdens can be a form of policymaking known as targeting, whereby states deliberately increase the administrative burden in order to reduce a program's take-up rate. This could be true with respect to UI before the pandemic: states with less-generous UI benefits, which tended to be more Republican-leaning states, also had stricter eligibility rules (Skandalis, Marinescu & Massenkoff, 2022). An alternative explanation for the increase in administrative burden in UI is to reduce fraud. But unlike other administrative burdens where policymakers may be making deliberate choices to weaken a government program (Herd & Moynihan, 2018) or reduce fraud, COBOL is not chosen to increase the administrative burden on claimants. COBOL was not problematic in processing claims prior to the pandemic during nonrecessionary periods, as reflected by COBOL states having a lower share of topcoded claims prior to the pandemic recession in Figure 3. Topcoded claims are claims that experience a processing delay greater than 70 days. COBOL usage in UI benefit systems becomes a binding constraint when UI systems are overwhelmed with claims or when large changes are made to UI benefits, as was the case during the pandemic recession.

III Data

I use two main sources of data for my outcomes variables: the Department of Labor Employment and Training Administration (DOLETA) and Affinity Solutions, which is part of the Economic Tracker (Chetty et al., 2020).

To analyze the impact of UI disbursement delays, I use the 9050 report from DOLETA. The 9050 report contains information on disbursement delays. To measure my main outcome variable—relative aggregate consumption—I use the Economic Tracker, which provides daily consumption data from a set of debit and credit cards by state.

A Data on Delays in Processing UI Claims (9050 Report)

DOLETA's 9050 report contains monthly information on how long after receiving a claim each state takes to make the first regular UI benefit payment. These data are reported by states to DOLETA and are used for multiple purposes such as measuring state performance and allocating UI administrative funding. The report only imperfectly captures the difference in delays between COBOL and non-COBOL states. First, it only captures delays in processing time and not in filing. Delays in filing are the time between when a claimant starts filing a claim and when that claim is

[16] Using the DOLETA 5159 report, I only find weak evidence of discouraged filers.

successfully filed, while delays in processing are the time between when a claimant successfully files a claim and when they receive their first UI payment. It is likely that it took longer for claimants in COBOL states to successfully file a claim because the UI systems were more overwhelmed in those states. Second, the report topcodes delays greater than 70 days. COBOL states had more topcoded claims, making it difficult to get an accurate comparison of delays in COBOL and non-COBOL states. Third, even for non-topcoded delays, the report does not include the number of days of delay but instead assigns delays to buckets of discrete weeks (e.g., delays between 1 and 10 weeks). Finally, the report only covers regular UI, with no information available on claims processing for the Pandemic Unemployment Assistance program (the program through which people ineligible for regular UI, like gig workers, received benefits during the pandemic). However, it seems likely that delays in regular UI would be a reasonable proxy for delays in other programs, and data from Ganong et al. (2022) suggest that the majority of claims processed were regular UI claims. PUA claims could have caused larger issues for UI benefit systems given that this program added new complexity to claims. However, the PUA program could have also affected regular UI by drawing resources such as staff away from processing regular UI.

Whether a claim is topcoded is a lagging indicator of when a claim was originally filed, because claims are only reported as delayed once benefits have been paid out. For example, if a claimant files for UI benefits in March 2020 and gets benefits starting in June 2020, then the claim will be reported as topcoded in June 2020. However, if that same claimant starts to receive UI benefits in July 2020, then the claim would not be reported as topcoded until July 2020. Given that I cannot observe when claims are initially filed, I use as my measure of delay the number of people whose first benefit was more than 70 days late (i.e., the number topcoded) as a share of all the people receiving their first benefit in a given month. Ideally, I would be able to determine when a topcoded claim was initially filed and then calculate the share of topcoded claims with the month filed instead of the month paid. This would fix the lagging indicator issue in the numerator. As an alternative indicator of delays in UI benefit disbursement, I measure the share of claims that are delayed at least 5 weeks. This measure will suffer from being a lagging indicator similar to the topcoded claims, but the lag will be mechanically shorter for these non-topcoded claims.

The limitations of the data also give rise to nonclassical measurement error. Processing delays may be a relatively poorer measure of total delays in COBOL states than in non-COBOL states. Total delays are the sum of processing delays and delays in filing (the time between when the claimant starts filing a claim and when it is successfully submitted). Given the relatively larger administrative burden in COBOL states, delays in filing could be longer in COBOL states than non-COBOL states. Another concern within processing delay is that the accuracy of measuring processing delays could be related to COBOL usage particularly during recessionary periods. Specifically, non-COBOL states may do a better job of measuring in processing delays given their more modernized UI benefit system. As a result, the estimated effect of COBOL on the timeliness of claim processing should be viewed as a lower bound of the true difference in total delays between COBOL and non-COBOL states.

B Data on Consumption

I use Opportunity Insights' Economic Tracker to track consumption patterns at the state level. The main advantage of using these data is that they are available at the daily frequency. The consumer-spending data are credit and debit card spending information provided by Affinity Solutions, which is then transformed and aggregated by Chetty et al. (2020). Seasonally-adjusted daily consumption data—measured relative to consumption in January 2020—are available from January 13, 2020, through the present, although I only use data through the end of 2020.

The data cover about 10% of all debit and credit card consumption in the US (Chetty et al., 2020). Chetty et al. (2020) find that the Affinity Solutions data has broad coverage across industries as shown in their comparisons to Quarterly Services Survey and Advance Monthly Retail Trade Survey, but over-represent categories in which credit and debit card transactions are used. The exclusion of cash consumption would only be problematic if different trends in cash usage emerged between COBOL and non-COBOL states after the emergency declaration.[17] Chetty et al. (2020) compare cash transactions captured in CoinOut grocery data with the Affinity Solutions data on total card consumption of groceries and find a signal correlation of 0.9 for the period between January 1, 2020, and June 1, 2020. This high correlation suggests that cash transactions are similar to card transactions. Furthermore, credit and debit card transactions accounted for roughly half of all PCE recorded in national accounts (Chetty et al., 2020). Throughout this paper, I use the terms "consumption," "total card consumption," and "credit and debit card consumption" interchangeably.

Not having access to Chetty et al.'s (2020) raw individual data, I limit my analysis to changes in consumption relative to January 2020 by state. The data that I use are at the state level, but county-level data are also available. I focus on the state-level analysis because the variation I exploit, COBOL usage, is at the state level. The data are at the daily frequency with a seven-day moving average.

C Other Control Variables

In addition to using DOLETA and the Economic Tracker as data sources, I also use data by state from a variety of sources for building a robust set of covariates. The covariates that I control for when estimating the impact of delays on aggregate consumption are new COVID-19 death rates, new COVID-19 case rates, and the interaction of 2016 presidential Republican vote share (vote share for candidate Donald Trump) and the period after the emergency declaration. The case and death rates are provided by the *New York Times*' COVID-19 repository. The COVID-19 data are also available at a daily frequency and are measured using a seven-day moving average. The 2016 election data are cross-sectional and come from the MIT Election Data and Science Lab. Finally, the 2019 population estimates come from the US Census Bureau and are used for weighting purposes. The means of these variables by COBOL usage, along with other covariates that I will

[17] In 2019, the San Francisco Fed found that consumers used cash in about 26% of transactions.

discuss later, are shown in Table I.

Additionally, I select five covariates that represent state characteristics from before the emergency declaration and interact each of them with the binary variable $Post_t$. These five covariates have statistically insignificant differences between COBOL and non-COBOL states, but are selected due to potential concerns one may have a priori. All five control variables were selected for concerns for the main outcome of interest: consumption. However, in order to be consistent, I apply these same controls and the Republican vote share interacted with $Post_t$ in the same order across regression tables even when these controls are less relevant (such as for processing delay outcomes). The five state characteristics are (1) income share in accommodation and food services (2019), (2) the percentage of the population living in urban areas (2010), (3) UI generosity (Jan. 2020), (4) the percentage of the population living in poverty (2019), and (5) the percentage of the population with at least a bachelor's degree (2019). Data on these covariates come from (1) BEA, (2) U.S. Census Bureau, (3) Brookings Institution and Department of Labor, (4) Small Area Income and Poverty Estimates program (U.S. Census Bureau), and (5) ACS (U.S. Census Bureau), respectively.

These five confounders do not meaningfully affect my consumption results, but I will briefly list what concerns one may have. Accommodation and food services were disproportionately affected by the pandemic recession, so one may be concerned that COBOL states had a higher income share in accommodation and food services. Another concern may be that COBOL states had less generous UI benefits, so they were slower to recover from the pandemic recession. Both of these concerns are assuaged given that COBOL states had lower income share in accommodation and food services and more generous UI benefits as shown in Table I. In terms of poverty, there was expansion of the safety net during the pandemic recession, so poorer households may have received more transfers from other government transfer programs. COBOL states have a lower poverty rate, but I do not find statistically significant differences in transfers between COBOL and non-COBOL states across the Paycheck Protection Plan (PPP), the Economic Impact Payments (EIP), and the Supplemental Nutrition Assistance Program (SNAP). This analysis is reflected in Tables F.3, F.4, and F.5, respectively. Another concern could be that lower levels of education may lead to lower take up rates for the same administrative burden. I find that COBOL states had higher bachelor's degree completion rates as shown in Table I. Finally, one may be concerned that rural areas may have recovered faster from the pandemic given their lower population density and potentially different response to COVID-19 relative to urban areas.

D COBOL Status

I identify COBOL usage for all 50 state UI systems primarily through emails to state officials, news articles, and information from the National Association of State Workforce Agencies (NASWA) Information Technology Support Center (ITSC).[18] This definition of an antiquated UI benefit sys-

[18] Seven states were identified via direct email, 26 via news articles, and 13 via the ITSC's definition of "modernized" to rule out COBOL states. Secondary sources were used for the remaining states: one state was identified via a Freedom of Information Act request, two states via UI-office reports, and one state through a UI-office job posting requesting COBOL skills.

tem closely follows NASWA's definition of an antiquated UI benefit system, but my definition of COBOL usage is more clear given that UI benefit systems are always undergoing a modernization effort.[19] Figure 1 is a map of the United States showing which states use COBOL as of 2020. COBOL is the most common language used by state workforce agencies, with 28 states categorized as COBOL and 22 as non-COBOL. There is a scattered distribution of COBOL states throughout the United States, with no one region accounting for the majority of COBOL states. NASWA provides an overall description of state UI systems, describing them either as antiquated or modernized: typically, a COBOL state is also an antiquated state, and a non-COBOL state is a modernized state.

IV Empirical Strategy

I perform two empirical exercises. First, I estimate the relative decline in consumption in COBOL states versus non-COBOL states. Second, I determine whether COBOL states systematically experienced longer delays in UI disbursement.

A TWFE Estimator

I use the same empirical strategy to address both questions. The main specification is a two-way fixed-effects (TWFE) estimator in which the treatment group is states that used COBOL in their UI-benefit systems in 2020. The specification is as follows:

$$Y_{it} = \alpha_0 + \beta_1 Post_t * Cobol_i + \beta_2 Post_t * X_i + \gamma Z_{it} + \phi_t + \psi_i + \varepsilon_{it}. \tag{1}$$

States are denoted by i and time by t. For analysis using DOLETA (Economic Tracker) data for the outcome variable, t corresponds to month (day). Y_{it} is the outcome variable, which differs for each exercise. For the relative consumption analysis, Y_{it} corresponds to the relative change in consumption in state i on day t. For the processing-delay analysis, Y_{it} corresponds to either the share of topcoded claims or the share of claims delayed at least 5 weeks in state i in month t. $Post_t$ is a binary variable taking the value of 1 for the post-period, which also differs across exercises. In the relative consumption exercise, $Post_t$ takes the value 1 starting on March 13, 2020, while $Post_t$ takes the value 1 starting March 2020 in the delay analysis. $Cobol_i$ is a binary variable taking the value of 1 for states that use COBOL in their UI-benefit system in 2020. X_i are state characteristics from before the emergency declaration, such as the 2016 Republican presidential vote share in state i. Z_{it} is a set of time-varying control variables such as the unemployment rate. ϕ_t is a month or day fixed effect, ψ_i is a state fixed effect, and ε_{it} is the error term.

[19]My results are robust to using NASWA's definition of antiquated. Only two states would change following NASWA's definition: Alabama and Nebraska.

B Relative Consumption

My dependent variable for this exercise is Rel_Cons_{it}, a continuous variable that measures the relative change in relative total card spending for day t in state i. I define the start of the post-period as the date of the emergency declaration, March 13, 2020. Prior to the pandemic, the slower and less-efficient UI systems (in COBOL states) did not cause noticeable delays in benefit disbursement relative to systems in non-COBOL states. This constraint was not binding until the massive spike in UI claims after the emergency declaration, which overwhelmed UI systems. Therefore, my treatment is COBOL states after the emergency declaration.

The Rel_Cons_{it} measure of relative total card consumption incorporates seasonal adjustment and normalization components. The measure is given by

$$Rel\_Cons_{it} = \frac{\frac{C_{i,t2020}}{C_{i,t2019}}}{\frac{C_{i,index2020}}{C_{i,index2019}}} - 1. \tag{2}$$

Chetty et al. (2020) publicly share the consumption data expressed in these relative percentage-point changes instead of levels. For more details on the construction of Rel_Cons_{it}, see Chetty et al. (2020). In Equation 2, $C_{i,index2020}$ corresponds to the index period in 2020 (the first four complete weeks of January). $C_{i,t2019}$ and $C_{i,index2019}$ represent the same period in 2019. Dividing by the 2019 value seasonally adjusts the data, while dividing by the index period normalizes the data as changes relative to January 2020. In other words, the consumption measure can be thought of as the percentage-point change in seasonally-adjusted consumption relative to seasonally-adjusted mean consumption during the baseline period of January 2020. For example, a value of -0.419 on March 30, 2020, in Wisconsin represents a 41.9-percentage-point seasonally-adjusted decline in average total card consumption in the week ending on March 30, 2020, relative to average total card consumption in Wisconsin in January 2020.

To measure the dynamic effect of COBOL usage on aggregate consumption, I estimate a weekly event study:

$$Rel\_Cons_{ik} = \alpha_0 + \sum_{k=-5}^{41} \beta_k (COBOL_i \times I_k) + \beta_{42} Post_t * X_i + \gamma Z_{ik} + \phi_k + \psi_i + \epsilon_{ik}. \tag{3}$$

In Equation 3, k denotes the number of weeks since March 13, 2020. $Cobol_i$ is a binary variable that takes the value 1 if state i uses COBOL in its UI benefit system. I_k is binary variable that takes the value 1 if the week is week k. The event study allows me to track the evolution of the impact of higher administrative burdens in UI benefits on aggregate consumption. I define the relative weekly-consumption measure using total card spending from each Friday of the week; I choose Friday because March 13, 2020, fell on a Friday. Similarly, for the state controls, Z_{ik}, I use the Friday value of new COVID-19 death rates and new COVID-19 case rates. X_i denotes state characteristics from before the emergency declaration such as the 2016 Republican vote share.

These state characteristics are then interacted with $Post_t$, which takes the value of one for weeks after the emergency declaration.[20] This step ensures that each week's value is not mechanically related to the previous week's value through the seven-day moving average.

To interpret the relative consumption effect from Equations 1 and 3 as causal, I need to satisfy the conditional parallel trends assumption. A potential violation of the (conditional) parallel trends assumption would be if COBOL states responded differently to COVID-19 than non-COBOL states. Despite experiencing similar COVID-19 case numbers and deaths as reflected in Table I, individuals in COBOL states were more COVID-19 cautious than individuals in non-COBOL states. This difference in attitude would upward-bias my consumption estimates. This would violate the (conditional) parallel trends assumption because COBOL states would have had lower consumption after the emergency declaration relative to non-COBOL states for reasons unrelated to UI benefits. To address this concern, I include the interaction of the 2016 Republican presidential vote share and post-period (after the emergency declaration), which serves as a proxy for COVID-19 cautiousness. This variable also helps control for differential policies between Democratic and Republican states. Specifically, Republican states could have had more relaxed policies in general toward COVID-19 transmission such as fewer stay-at-home orders and fewer school closures.

C Delays in UI-Benefit Disbursement

As described above, my primary measure of disbursement delay is the share of processed claims that were paid 70 or more days after filing, i.e., the share of claims that are topcoded. I focus on intrastate claims, which form the majority of claims for all states (Washington, DC, is omitted from the sample).[21] This level of analysis circumvents issues with workers living in one state but working in another, which is particularly pronounced in counties on state borders.

I use data starting in January 2019. I drop March, April, and May of 2020 from the topcoded analysis because the share of claims that are topcoded during this period is mechanically decreasing for both COBOL and non-COBOL states. The numerator is not increasing because topcoded claims are a lagging indicator, but the denominator, UI recipients, is contemporaneously increasing. The share of topcoded claims first increases for both COBOL and non-COBOL states in June 2020.

As a second outcome variable highlighting that COBOL states experienced longer delays in the disbursement of benefits, I also compare the share of claims delayed at least 5 weeks between COBOL and non-COBOL states. I only exclude March 2020 from this analysis, but this becomes a less clean exercise since the variation of lags from when a claimant files to when they receive their benefit is by construction larger.

[20] In Figure F.5, I interact X_i, Republican vote share, with I_k in a similar fashion to COBOL. I find consistent results.

[21] The Economic Tracker does not provide consumption data for Washington, D.C.

V Main Results

In this section, I document the effect of antiquated UI benefit systems on aggregate consumption. After presenting the main finding on consumption, I provide evidence that these antiquated UI benefits contributed to the increased processing delays in the disbursement of benefits. For my main outcome of interest, consumption, I estimate the effect of these antiquated UI benefit systems by comparing relative changes in consumption between COBOL and non-COBOL states in a static TWFE setting, an event study difference-in-differences setting, and through a back-of-the-envelope exercise. Finally, I compare processing delays between COBOL and non-COBOL states in a static TWFE setting.

A Effects of Antiquated UI Systems on Consumption

For the TWFE estimator to be a valid approach for identifying the causal effect of COBOL usage on consumption, the conditional parallel-trends assumption must hold. This means that, absent the surge in claims and changes to UI rules during the pandemic recession, relative consumption trends between states with and without antiquated UI benefit systems would have been the same conditional on the covariates included in my regression equation.

As preliminary evidence of relative consumption differences between COBOL and non-COBOL states after the emergency declaration, I compare their mean daily consumption relative to January 2020 without any controls. Chetty et al. (2020) only provide consumption data starting in January 2020, and I start the analysis period in February 2020 to exclude the index period.[22] Both consumption series hover around zero prior to the emergency declaration on March 13, 2020, which is reflective of the lack of change in consumption patterns. This similarity in consumption patterns between COBOL and non-COBOL states suggests that the assumption of common pre-trends holds from February 1, 2020, to March 12, 2020. The relative consumption patterns of COBOL and non-COBOL states are similar prior to the emergency declaration.

The consumption patterns in COBOL and non-COBOL states begin to diverge after the emergency declaration. As seen in Figure 2, both sets of states reach a trough in relative consumption slightly below 30-percentage-points lower than their base period consumption on the same day: March 30, 2020. In addition, consumption did not fully recover by the end of the sample on December 31, 2020. The average relative consumption decline for all states from March 13, 2020 to December 31, 2020, is 7.3-percentage-points (not population weighted). The largest relative declines were immediately after the emergency declaration, but neither COBOL nor non-COBOL states had fully recovered as reflected in Figure 2. Figure 2 shows that after the emergency declaration, COBOL states consistently had lower relative consumption than non-COBOL states, as reflected in the gap between the two series that appears after March 13, 2020, marked by the red vertical dashed line. Specifically, the gap starts to form in early April, after the spike in initial UI

[22] I cannot go further back in time due to data limitations. The data start in January 2020, and I exclude January 2020 given that it is the index period.

claims that occurred at the end of March 2020. By the end of 2020, non-COBOL states' consumption is almost back to baseline period levels. COBOL states' consumption also recovered, but more slowly.

To formally test whether relative consumption was lower in COBOL states than in non-COBOL states after the emergency declaration, as suggested in Figure 2, I run a TWFE estimator. The dependent variable is relative consumption, as defined in Equation 2, with the sample period starting on February 1, 2020, and ending on December 31, 2020. The coefficient of interest is the interaction of COBOL and Post, where Post is a binary variable that takes the value 1 after March 13, 2020. In Table II, I present results from Equation 1. In column 1, I only add state and day fixed effects when estimating the macroeconomic impact of increased administrative burdens to a fiscal stabilizer in COBOL states relative to non-COBOL states on aggregate consumption. In this naive specification, I find a 4.1-percentage-point larger decline in relative consumption in COBOL states relative to non-COBOL states. However, these consumption differences between COBOL and non-COBOL states could have emerged after the emergency declaration for reasons unrelated to an increased administrative burden in the UI benefit system. In column 2, I address this concern by using the interaction of the 2016 Republican vote share and Post, the coefficient of which is positive and significant. This result shows a positive association between consumption after the emergency declaration and the 2016 Republican vote share. One possible explanation for this result is that more-Republican states had policies that stimulated consumption more, such as shorter and fewer stay-at-home orders. Another possibility is that the more-Republican states were less cautious about COVID-19, with residents being more likely to go outside and consume goods and services that they might not have consumed if they had stayed at home. After controlling for the interaction of Republican and Post, the coefficient on the interaction of COBOL and Post decreases from a 4.1-percentage-point relative decline to a 2.8-percentage-point decline.

Even though column 2 in Table II is my preferred specification, the concern remains that other omitted variables could be driving the relative drop in consumption between COBOL and non-COBOL states. One potential concern is that COVID-19 affected COBOL and non-COBOL states differently.[23] In column 3, I add the daily new COVID-19 case rates and new death rates as additional controls. The coefficient on the interaction on COBOL and Post holds at a 2.8-percentage-point decline and significant at the 5% level. This result is unsurprising given that COBOL and non-COBOL states experienced similar COVID-19 new case rates and new death rates, as reflected by Table I. There could still be other omitted variables that could be driving the differences in relative consumption between COBOL and non-COBOL states. In column 4, I add the five previously discussed confounders interacted with Post as additional controls. The coefficient on the interaction of COBOL and Post only marginally decreases, dropping to a 2.6-percentage-point decline. In column 5, I control for the monthly unemployment rate and find an insignificant positive association between the unemployment rate and relative consumption. However, the monthly unemployment

[23] Table I suggests that COBOL and non-COBOL states had similar COVID-19 new case rates and new death rates.

rate is problematic because one channel through which these antiquated UI benefit systems could affect consumption is through the unemployment rate. Specifically, UI is an automatic stabilizer and if COBOL states are relatively slower at disbursing funds, then UI will be a less effective fiscal tool at increasing aggregate demand. The post-treatment bias from using the unemployment rate as a control variable downward biases my coefficient on the interaction of COBOL and Post. This conservative estimate decreases the coefficient to a 2.4-percentage-point decline and remains significant at the 5% level.

B Consumption Results by Week

The estimated 2.8-percentage-point decline in total card consumption in COBOL states relative to non-COBOL states represents the average effect in the post-period. There could be larger consumption differences earlier in the sample and potential convergence in consumption patterns between COBOL and non-COBOL states by the end of 2020. In Figure 5, I plot the results from Equation 3 to show how the effect varies by week. The x-axis denotes weeks relative to the emergency declaration, and the y-axis denotes the percentage-point decline in relative consumption in COBOL states. The dashed red line corresponds to the week starting on March 13, 2020, which marks the beginning of the post-period. Figure 5 highlights that the lower relative consumption in COBOL states was persistent given that relative consumption in COBOL states remained lower than relative consumption in non-COBOL states for every week in the post-period. Even though COBOL states were still experiencing a decline in relative consumption relative to non-COBOL states, both groups of states were recovering in the second half of 2020, as reflected in Figure 2.

Not only was relative consumption in COBOL states lower than in non-COBOL states every week in the post-period, but the recovery in COBOL states was tepid. Relative consumption in COBOL states fell from week 0 until it reached its trough 11 weeks after the emergency declaration: -4.4 percentage points. One might expect a similar speed of recovery for COBOL states, but COBOL states experienced low relative-consumption growth. At the end of my sample, 41 weeks after the emergency declaration, COBOL states still had 2.4-percentage-point lower consumption relative to non-COBOL states. This protracted recovery suggests UI state agencies struggled not only with the initial inflow of UI claims soon after the emergency declaration but with the continued flow of claims in the subsequent weeks. However, I cannot determine whether consumption fell because states continued to experience delays in processing older claims or because states were suffering dynamic general equilibrium effects through the multiplier effects of previous consumption delays. It could be that the recovery after the trough corresponds to the eventual disbursal of delayed UI claims. The persistence could be, in part, driven by the discouraged filers who never received UI benefits. My results are likely the sum of these effects.

C Aggregate Effects of Lack of UI Modernization

To convert the 2.8-percentage-point relative decline in consumption from column 2 of Table II into a dollar amount, I perform the following back-of-the-envelope calculation:

$$Cost = \frac{PI_{2019}}{PI_{2012}} * Relative\_Decline * 0.8 * \sum_{i=1}^{28} PCE\_Cat_{i,2019}, \tag{4}$$

where $\sum_{i=1}^{28} PCE\_Cat_{i,2019}$ represents the total nominal personal consumption expenditures (PCE) (denominated in 2012 dollars) in certain categories of all 28 COBOL states in 2019. I exclude five PCE categories because they may not be reflected in total card consumption: (1) motor vehicles and parts, (2) housing and utilities, (3) health care, (4) financial services, and (5) final consumption expenditures of nonprofit institutions serving households.These PCE category exclusion results in credit and debit card consumption accounting for roughly 50% of PCE. I multiply by 0.8 because the post period, March 13, 2020 to December 31, 2020, corresponds to four-fifths of a year. To convert real GDP denominated from 2012 dollars into real GDP denominated in 2019 dollars, I divide the 2019 implicit price deflator for GDP by the 2012 implicit price deflator for GDP: $\frac{PI_{2019}}{PI_{2012}}$. The estimated 2.8-percentage-point relative decline, $Relative\_Decline$, in aggregate consumption then translates into a real-GDP decline of $105 billion in COBOL states relative to non-COBOL states; this figure corresponds to roughly 0.9% of real GDP in COBOL states. This estimate is conservative as I assume there was no consumption effect in the excluded PCE categories.[24] However, the $105 billion cost estimate likely underestimates the true overall economic costs of administrative burdens in UI-benefit systems given that non-COBOL states also experienced increased administrative burdens after the emergency declaration that resulted in delays. The estimate instead represents the GDP decline that could have been avoided if COBOL states had modernized their UI benefit systems to the same extent as non-COBOL states, which would have resulted in a lower administrative burden for claimants in COBOL states.

The cost that COBOL states would have incurred to modernize their systems is likely only a small fraction of the $105 billion relative decline in GDP that they incurred. The problems with the COBOL systems were apparent after the Great Recession, but issues that arose from the pandemic recession provided renewed interest in COBOL states to modernize their UI benefit systems. For example, the Wisconsin Department Workforce Development, a COBOL UI state agency, signed a contract with Flexion Inc. in 2021 to modernize legacy IT systems that are largely written in COBOL.[25] The initial contract lasts one year, with three optional one-year renewals. According to the contract, the total proposed cost if the contract were renewed all three times is $16.5 million. If we assumed that Wisconsin's contract stays within budget and other states have similar modernization costs, then total modernization costs of all 28 COBOL states would be less than $500 million. This amount reflects the large discrepancy between the costs of modernization and the costs of having an antiquated UI benefit system to real economy.

[24] If I were to include all PCE categories, then the real-GDP decline would be closer to $210 billion.

[25] See the public contract.

D COBOL-Induced Delays

One way that relatively larger administrative burdens in COBOL states could be affecting aggregate consumption is through longer processing delays in UI disbursement. One approach to determining whether COBOL states experienced longer processing delays in UI disbursement than non-COBOL states is to ascertain whether COBOL states had a relatively higher share of claims that were topcoded, meaning they experienced a processing delay of over 70 days. These delays are out of the norm; DOLETA does not keep track of delays beyond 70 days. Under normal circumstances, these topcoded claims only account for a small share of claims. In 2019, less than 5% of all intrastate regular UI claims were topcoded in each state in each month, as reflected in Figure 3. However, from July 2020 to October 2020, at least 20% of all intrastate claims were topcoded in both COBOL and non-COBOL states. Representing topcoding as a share might understate the severity of the issue given the drastic increase in the number of claimants receiving UI after the emergency declaration in 2020.

Figure 3 shows both that topcoding was more common in COBOL states and how common it was for claimants to have to wait over 70 days in both COBOL and non-COBOL states. In both COBOL and non-COBOL states, fewer than 2.5% of intrastate claims were delayed over 70 days in every month from January 2019 to February 2020 for the aggregated COBOL and non-COBOL states. In each of the last six months of 2020, for both COBOL and non-COBOL states, over 15% of intrastate claims were delayed by more than 70 days. Given that topcoded claims are a lagging indicator, I would not expect to see a spike in topcoded claims until 70 days after the emergency declaration. COBOL states experienced a higher share of topcoded claims than non-COBOL states starting in June 2020, when the March 2020 claims would have first been topcoded. Importantly, weekly initial claims for every week in 2020 after the emergency declaration were higher than the maximum number of claims prior to the emergency declaration, so both the numerator and denominator of the fraction of intrastate regular claims that are topcoded drastically increased in June 2020. Also, March, April, and May 2020 mechanically saw the fraction of topcoded claims fall because of the surge of new claims being processed, which is reflected in the denominator; however, topcoded claims, the numerator, cannot appear until 70 days after the spike in claims.

Figure 4 focuses on a larger set of claims that were delayed: claims that experienced at least a 5 week processing delay. It is harder to see the pattern in Figure 4 relative to Figure 3, but COBOL states experience a larger share of claims that experience at least a 5 week delay. Given that these delays encompass a larger set of claims, it is unsurprising that the values are by construction are higher relative to the topcoded case. For example the maximum value in Figure 4 is around 40%, while the maximum value in Figure 3 is around 25%. One should note that when analyzing claims that are at least delayed by 5 weeks that the range in delays mechanically increases relative to claims that are topcoded. Delays now range from 5 weeks to over 10 weeks. Unlike the topcoding analysis, I only exclude March 2020 from the sample given that claims made early in the pandemic that are delayed by at least 5 weeks could appear in April 2020 (unlike topcoded claims). Given this heterogeneity in lags in conjunction with COBOL states experiencing more topcoded claims, non-COBOL states peak sooner than COBOL states (June 2020 peak for non-COBOL states and August 2020 peak for COBOL states).

In Table III, I show results estimated using the TWFE model described in Equation 1. Column 1 presents results with only state and month fixed effects as controls. The parameter of interest is the coefficient on the interaction of COBOL and Post. The coefficient of 2.3 corresponds to COBOL states experiencing a 2.3-percentage-point increase in the share of topcoded claims after the emergency declaration relative to non-COBOL states. This is a large increase in the share of topcoded claims given that on average states only delayed claims by over 70 days for about 18.7 percent of claims from June 2020 to December 2020.[26]

As discussed above, one concern with comparing COBOL and non-COBOL states is that they differ politically, which could affect the results. In column 2, I add the interaction of Republican and Post, and the coefficient on the interaction of COBOL and Post does not meaningfully change, going from 2.3 to 2.1 and remaining significant at the 1% level. The coefficient on the interaction of Republican and Post is insignificant.

Despite topcoded claims only accounting for a small fraction of regular UI intrastate claims prior to 2020, there were differences between COBOL and non-COBOL states. Specifically, COBOL states had a lower share of topcoded claims prior to the emergency declaration, as reflected in Figure 2. Not only are the average shares different between COBOL and non-COBOL states, but there appears to be convergence in the average shares right before the emergency declaration. To address the concern that confounders are driving the differences in topcoding, I add five previously discussed confounders interacted with Post in column 3 of Table III.[27] In column 3, once I add those five confounders interacted with Post, the coefficient on the interaction of COBOL and Post increases to 3.8 ppt. These interaction terms meaningfully change the point estimate on the coefficient of interest. A potential reason for this change could be that laid-off workers in accommodation and food services have more complicated work histories that could lead to longer processing delays than a typical claim, and non-COBOL states have more workers in accommodation and food services, as reflected by Table I. Another interpretation of this increase is that by adding these confounders, I can partially address the non-classical measurement error previously described with the introduction of these control variables.

Although COBOL and non-COBOL states had similar unemployment rates on average during the pandemic, as shown in Table 1, as a robustness check, I include the unemployment rate as an additional control in column 4 of Table III. By including the unemployment rate, the coefficient on the interaction of COBOL and Post increases to 4.4 ppt and remains significant at the 1 percent level. I find that higher unemployment rates are associated with a higher share of topcoded claims, but the coefficient on the interaction term of COBOL and Post does not meaningfully change. Even though the unemployment rate is potentially endogenously affected by UI processing delays, longer processing delays in COBOL states could lead claimants to return to work sooner or they could

[26]Note that this 18.7 percent statistic is not population weighted.

[27]I use these same five confounders throughout my analyses for both delay and consumption outcomes.

increase the unemployment rate through the multiplier channel. However, it is reassuring to see that the larger share of topcoded claims in COBOL states is not exclusively being driven by the unemployment rate.

I perform a similar analysis in Table IV with the share of claims that were delayed at least 5 weeks as the dependent variable. I use the same confounders and I find that COBOL states experienced between a 3.1 ppt. increase and a 4.4 ppt. increase in the share of claims with at least a 5 week delay relative to non-COBOL states after the emergency declaration. These results are significant at the 5 percent level across all specifications.

In sum, I find that COBOL states experienced longer delays in the form of higher share of topcoded claims and higher share of claims delayed by at least 5 weeks relative to non-COBOL states after the emergency declaration. The covariates included in Table III do not meaningfully change the significance of the coefficient on the interaction of COBOL and Post, but they do meaningfully change the point estimates.

E Robustness Check on Consumption Results

As a robustness check of the results in Table II, I use the penalized synthetic control method developed by Abadie & L'Hour (2021) to measure the decline in relative consumption for COBOL states relative to non-COBOL states after the emergency declaration.

This method uses covariates in the pre-intervention period and the donor pool to create a synthetic control for each treated unit. In my setting, I have 28 treated units, COBOL states, and 22 control units in the donor pool, non-COBOL states. The innovation of the penalized synthetic control method over the traditional synthetic control method is that there is a tuning parameter, λ, that puts additional weight on pairwise comparisons instead of the aggregate comparison. The higher the value of this tuning parameter, the more sparse the synthetic controls will be, and fewer non-COBOL states will be selected from the donor pool. As the tuning parameter approaches 0, the penalized synthetic control method becomes the traditional synthetic control method that minimizes the sum of pairwise discrepancies. As the tuning parameter approaches ∞, the penalized synthetic control method becomes the nearest-neighbor matching with replacement estimator.

The penalized synthetic control method is similar to the traditional synthetic control method in that they are both heavily dependent on the controls selected from the pre-intervention period. These controls affect which non-COBOL states are selected in the synthetic control in addition to the weight assigned in the synthetic control. Typically, more controls are used in a synthetic control setting than in a TWFE setting given the lack of fixed effects. I select 15 covariates to match on: (1) Republican vote share (2016), (2) income share in accommodation and food services (2019), (3) the percentage of the population living in urban areas (2010), (4) UI generosity (Jan. 2020), (5) the percentage of the population living in poverty (2019), (6) the percentage of the population with at least a bachelor's degree (2019), (7) the employment-to-population ratio (2019), (8) the log of income per capita (2019), (9) median age (2019), (10) the African American population share (2019), (11) the relative replacement rate (2020), (12) teleworkable employment (2019), (13) a Republican governor indicator (2019), (14) labor force population, and (15) real GDP (2019).

Table V reports the relative consumption decline for COBOL states using the penalized synthetic control method. The last three columns report results using this method. The column labeled PSC fixed λ corresponds to a fixed value for the tuning parameter of 0.1. The other two penalized synthetic control estimator columns choose the tuning parameter in a data-driven manner. The column labeled PSC MSE λ uses a leave-one-out cross-validation procedure to select λ by minimizing the mean squared prediction error in the post-intervention period (after the emergency declaration). The column labeled PSC Bias λ uses validation over the outcomes (relative consumption) in the pre-intervention period (prior to the emergency declaration) to select the tuning parameter. The average treatment effects across these three specifications with different tuning parameters yield a relative decline in consumption for COBOL states of between 3.7 and 4.8 percentage points. The results from the penalized synthetic control method are not meaningfully different than the results from Table II.

To conduct inference with the penalized synthetic control method, permutation tests are typically conducted. I randomly assign treatment across 28 of the 50 states 10,000 times and estimate a relative consumption decline using a tuning parameter identical to the one from the column labeled PSC MSE λ in Table V (0.01) in each iteration. To be consistent with the results from Table V, I aggregate the 28 cohort treatment effects using population weights. Figure 6 shows the distribution of these 10,000 simulations. The red dashed line corresponds to the average treatment effect for the 28 COBOL states with a tuning parameter of 0.01. This permutation test yields an effect that is significant at the 10% level.

F Heterogeneity Analysis

Throughout this analysis, I have focused on relative consumption for all consumption categories among all consumers. However, one of the benefits of using the Affinity Solutions data is that consumption at the state level can be decomposed by type of goods purchased, by type of services purchased, or by income quartile.[28] I focus on each income quartile and the four mutually exclusive aggregated consumption types defined by Chetty et al. (2020): durable goods, nondurable goods, remote services, and in-person services. If discouraged filers are playing a role in the relative consumption decline, then I expect durable-goods consumption to be negatively affected by the large UI benefit transfers during the pandemic recession. Parker, Souleles, Johnson & McClelland (2013) found a shift towards durable goods under the Economic Stimulus Act of 2008 where the typical single household received \$300 to \$600. Unemployment Insurance benefits between April 2020 and July 2020 were much larger because of the provision granting unemployed workers an extra \$600 per week in benefits. I also decompose consumption by income quartiles because income groups were differentially exposed to the COVID-19 shock. I expect consumption of in-person services to have decreased more in COBOL states than non-COBOL states because when households receive a

[28] Due to data limitations, I cannot decompose results by goods or services within an income quartile at the state level using the Economic Tracker.

negative income shock, they reduce their consumption at restaurants and on entertainment, which count as in-person services. In addition, studies such as Amburgey, Birinci et al. (2020) find that the top quintile had the smallest shift in its unemployment rate during the pandemic recession. If one was not unemployed, they would have been ineligible for UI benefits and could not have suffered from delayed UI benefits nor become a discouraged filer. I therefore expect the richest income quartile, quartile 4, to experience at most a small drop in relative consumption in COBOL states relative to non-COBOL states.

I use the Affinity Solutions data to see whether durable-goods consumption was affected by having an antiquated UI system. Durable-goods consumption is defined as consumption in the following merchant category codes: (1) building materials, gardening equipment, and supplies; (2) electronics and appliances; (3) furniture and home furnishings; (4) sporting goods, hobbies, musical instruments, and bookstores; (5) telecommunications; and (6) vehicles and parts. Nondurable-goods consumption is defined as consumption in the following codes: (1) clothing and clothing accessories; (2) food and beverage stores; (3) general merchandise; (4) health and personal care stores; and (5) wholesale trade. Remote-services consumption is defined as consumption in the following codes: (1) administrative and support and waste management and remediation services; (2) education; (3) finance and insurance; (4) information; (5) professional, scientific, and technical; (6) public administration; and (7) utilities, construction, and manufacturing. In-person-services consumption is defined as consumption in the following codes: (1) accommodation and food services; (2) healthcare and social assistance; (3) arts, entertainment, and recreation; (4) transportation and warehousing; (5) rental and leasing; (6) repair and maintenance; and (7) personal and laundry services.

To formally estimate the effect of increased administrative burden by type of good or service consumed, I use a TWFE estimator. Specifically, I use a TWFE estimator similar to the one in Equation 1 to estimate heterogeneous relative consumption differences between COBOL and non-COBOL states. The only difference is that the dependent variable changes from relative consumption in all categories to relative consumption in one of these four aggregated categories. In all four specifications, I match the controls used in column 2 of Table II where I include state and day fixed effects in addition to controlling for the interaction of Republican and Post. Column 1 of Table VI provides suggestive evidence that increased administrative burden from antiquated systems reduced durable-goods consumption. The coefficient on the interaction of *COBOL* and *Post* is marginally insignificant at the 10% level (significant at 10.2%level). The coefficient corresponds to a 2.4-percentage-point decline in durable-goods consumption in COBOL states relative to non-COBOL states. The patterns in durable goods consumption differ from those of aggregate consumption in that durable goods consumption declined sharply at the start of the pandemic but quickly recovered, topping pre-pandemic levels soon afterward. By the end of May, durable-goods consumption was above baseline values and remained elevated for the remainder of the sample period for both COBOL and non-COBOL states. This suggestive finding of a relative decline in consumption of durable goods may suggest more discouraged filers in COBOL states. Durable goods are large purchases that arguably are less sensitive to delays in the disbursement of UI benefits.[29]

Unlike durable goods, nondurable goods should be affected by both discouraged filers and delayed payments. In column 2 of Table VI, I estimate the effect of the increased administrative burden in UI benefit systems on nondurable goods consumption. The table shows a relative decline of 3.5 percentage points more in COBOL states than non-COBOL states, which is significant at the 5% level. If households are not able to perfectly smooth consumption, then they will reduce their consumption of nondurable goods prior to the receipt of their delayed UI benefits. Column 3 estimates the impact on in-person services, showing a 2.8-percentage-point relative decline, which is significant at the 10% level. Consumers typically reduce their consumption of in-person services such as dining in restaurants when they receive a negative income shock. In column 4, I estimate the impact on remote services and find no effect.

Instead of looking at the goods or services purchased, as in Table VI, I next examine which individuals experienced the largest declines in relative consumption. Specifically, I sort the individuals into income quartiles.[30] I rerun the analysis from Table VI using income quartiles as the dependent variable. I report the results in Table VII. Column 1 corresponds to consumption in the bottom quartile, column 2 corresponds to consumption in the second income quartile, and so forth. All the specifications include state and day fixed effects as well as the interaction of Republican and Post as an additional control. The results show that as the income quartiles increase, the standard errors shrink. Even though the top income quartile has the smallest standard error, I find an insignificant result, as expected. The richest income quartile was the least likely to become unemployed during the pandemic recession and thus the least likely to receive UI benefits, independent of administrative burdens. The strongest effects are for the second- and third-income quartiles. There are two possible reasons for not finding a strong result for the bottom quartile: (1) larger standard errors or (2) fewer discouraged filers. It could be that unemployed individuals in the bottom quartile were less susceptible to an increase in administrative burden because they were more likely to depend on the benefits to cover necessities such as rent payments and food expenses. Administrative burdens in programs like UI may be a form of targeting (Nichols & Zeckhauser, 1982) in which only the most motivated claimants overcome all the hurdles. In columns 2 and 3, I find a 3.2- and 2.7-percentage-point decline for the second and third income quartiles, respectively, both of which are significant at the 10% level.

[29]Note that delays may also partially encourage durable goods consumption because the first payment of delayed UI payments will be larger than UI payments that are not delayed. For example, if an individual is entitled to 8 weeks of UI, but their claim is delayed over 10 weeks (topcoded), then the recipient will receive all their UI benefits in one lump sum transfer.

[30]Two states, Alaska and Hawaii, are omitted from the sample because consumption data for the bottom quartile are unavailable.

VI Conclusion

Using a TWFE estimator, I find that problems with UI-benefit systems in states whose systems ran on COBOL resulted in a decline in relative consumption after the emergency declaration that was 2.8 percentage points larger than in non-COBOL states. I cannot definitively attribute all the difference to the use of COBOL, since COBOL states' systems may have also been antiquated in other ways; however, my results do clearly show that not having a well-functioning UI benefit system during the pandemic meaningfully harmed Americans. My results illustrate the economic consequences of only the increased administrative burden on potential claimants, but my results do not capture the nonpecuniary costs that potential claimants faced in COBOL states by repeatedly being disconnected, losing time in filing a claim, and experiencing added uncertainty regarding whether and when they would receive their benefits.[31] The large negative effect of UI system delays in COBOL states during the pandemic strongly suggests that the effectiveness of UI benefit systems as a countercyclical tool.[32]

Given data limitations, I cannot decompose how much processing delays contributed to the relative decline in consumption in COBOL states (relative to non-COBOL states) during the pandemic. COBOL states experienced longer delays in disbursing benefits and could have also had relatively more discouraged filers due to increased administrative difficulties. In particular, relative consumption declines among UI-eligible claimants in COBOL states may have led to a lower UI fiscal multiplier relative to non-COBOL states. Such an effect would dampen consumption even among households that remained employed and households that received benefits promptly. Future work could potentially decompose the overall decline in consumption into these components.

One potential policy approach is to create federal incentives for states to update their UI systems. With the American Recovery and Reinvestment Act of 2009, the federal government made $7 billion available for states who chose to modernize.[33] Thirty-nine states chose to implement at least some of the changes required to receive funding. These changes included expanding the definition of eligible workers to include part-time workers. Only $4.4 billion was allocated, but the program shows that the financial cost of enticing states to change their UI-benefit system is only a fraction of the cost incurred during the pandemic recession by COBOL states. Instead of focusing first on transitioning away from COBOL, states could focus on other less costly issues that likely contribute to delays, such as not having all UI information available in commonly spoken languages, not making it easy to reset passwords, and not making it possible to complete the entire online filing process on mobile devices. These other issues are less costly to fix and can cause large

[31] NJ Labor Commissioner Asaro-Angelo discussed how his team received death threats from claimants frustrated with issues in filing their claims during the pandemic recession in a panel discussion held by the Heldrich Center for Workforce Development on UI systems in New Jersey.

[32] How a UI benefit system should modernize lies outside the scope of this paper. Other work has shown that states should allocate enough time for modernization and incorporate extensive user testing throughout the process (Simon-Mishel, Emsellem, Evermore, Leclere & Coven, 2020).

[33] States had until August 22, 2011, to submit their applications to the Department of Labor to receive funding. See the briefing paper from the National Employment Law Project on how federal incentives after the Great Recession helped states modernize their UI benefit systems.

problems such as locking out a claimant from the state UI website, which leads to delays in filing.

Another approach is for states to join a consortium in which they share the same UI system and split the maintenance costs. A few states have taken this approach. For example, Mississippi, Rhode Island, Maine, Connecticut, and Oklahoma have joined together in the ReEmploy USA Consortium.[34] A more radical approach is to form a federal UI system, which might—or might not—improve efficiency. States might oppose this idea, especially if they are trying to decrease their UI uptake rate by discouraging eligible claimants.

Although lack of UI system modernization is a central problem, Lachowska, Mas & Woodbury (2022) show that modernization by itself is not necessarily sufficient to fix UI administrative issues. It is important that federal funding to state workforce agencies be tied to a pay-for-performance scheme to achieve the outcomes desired, such as shorter processing delays or reduced call center volumes. Some of these approaches are already being implemented through a grant of up to $600 million to support state UI information technology modernization under the American Rescue Plan Act (Parton, 2023). However, it is important that policymakers understand the importance of modernizing these antiquated UI systems.[35] Regardless of which approach policymakers choose to take, antiquated UI systems hamper the effectiveness of UI as a fiscal stabilizer.

[34] Though five states joined the consortium, only Maine and Mississippi had fully implemented the software program to modernize their UI benefit system away from COBOL by the start of the pandemic.

[35] For example, the initial support of $600 million available in grant opportunities was reduced to $200 million through the Fiscal Responsibility Act of 2023.

Table I: Unweighted Summary Statistics by COBOL Usage

	Non-COBOL	COBOL
Relative Consumption	-5.44	-7.51
	(9.82)	(10.08)
Fraction Topcoded	10.74	13.23
	(12.93)	(16.27)
Relative First Payments (Ratio)	7.79	6.74
	(11.95)	(10.48)
New COVID-19 Death Rate	0.29	0.29
	(0.33)	(0.46)
New COVID-19 Case Rate	18.81	18.10
	(23.75)	(25.40)
Unemployment Rate	7.76	7.73
	(4.04)	(3.69)
Population (Thous.)	5,800.01	7,143.62
	(4,650.14)	(8,809.51)
Republican (2016)	50.59	48.11
	(9.06)	(10.89)
Urban (2010)	72.56	74.38
	(13.72)	(14.90)
UI Generosity (Jan. 2020)	10154.82	12470.57
	(4,710.13)	(3,378.89)
Acc. and Food Services Inc. Share (2019)	4.14	3.70
	(2.40)	(1.46)
Bachelor's Degree (2019)	31.23	32.90
	(5.09)	(5.32)
Poverty (2019)	12.43	11.88
	(3.14)	(2.11)

Note: The table provides summary statistics for the variables used in my main specification and covariates used as controls. Relative consumption, the new COVID-19 death rate, and the new COVID-19 case rate come from the Economic Tracker (Chetty et al., 2020). Fraction of intrastate claims that are topcoded and relative first payments come from DOLETA. Relative consumption is measured in percentage point changes from the index period of January 2020. State population estimates for 2019 come from the US Census Bureau. The relative consumption variable is identical to the all-spending variable in (Chetty et al., 2020). The monthly unemployment rate estimates come from the Bureau of Labor Statistics. The remaining covariates are cross-sectional data from a point in time prior to the emergency declaration. The reported statistics are the means of the corresponding group, with their standard deviations in parentheses. These summary statistics cover the sample period of February 1, 2020, to December 31, 2020.

Table II: TWFE COBOL Usage on All Card Consumption

	(1)	(2)	(3)	(4)	(5)
	Rel Cons	Rel Cons	Rel Cons	Rel Cons	Rel Cons
COBOL × Post	-0.041**	-0.028**	-0.028**	-0.026**	-0.024**
	[0.020]	[0.013]	[0.013]	[0.011]	[0.011]
Republican × Post		0.003***	0.003***	0.003***	0.004***
		[0.001]	[0.001]	[0.001]	[0.001]
UR					0.002
					[0.002]
State FE	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	No	Yes	Yes	Yes
State Char. × Post	No	No	No	Yes	Yes
Days	335	335	335	335	335
States	50	50	50	50	50
Observations	16,750	16,750	16,750	16,750	16,750

Note: The table presents results from a TWFE estimator with day and state fixed effects. The dependent variable is the percentage-point change relative to the base period in credit and debit card consumption measured at the daily frequency. *Post* is a binary variable that takes the value 1 if the date is on or after March 13, 2020. *COBOL* is a binary variable that takes the value 1 if a state uses COBOL in its UI benefits system. The interaction term of interest is the product of *COBOL* and *Post*. *Republican* is the Republican vote share in the 2016 presidential election. COVID-19 controls include new COVID-19 death rates and new COVID-19 case rates. Column 1 only includes state and day fixed effects. Column 2 adds the interaction of *Republican* and *Post*. Column 3 adds the COVID-19 controls. Column 4 adds five terms of *Post* interacted with another confounder: (1) income share in accommodation and food services (2019), (2) mask mandates in July 2020 (2020), (3) the percentage of the population living in poverty (2019), (4) the percentage of the population with at least a bachelor's degree (2019), and (5) UI generosity (Jan. 2020). Column 5 adds the monthly unemployment rate. These estimates cover the sample period of February 1, 2020, to December 31, 2020. State populations in 2019 are applied as analytic weights. Standard errors are clustered at the state level.

Standard errors: *** p<0.01, ** p<0.05, * p<0.1

Table III: TWFE Fraction of Claims Topcoded

	(1) Frac Intra Top	(2) Frac Intra Top	(3) Frac Intra Top	(4) Frac Intra Top
COBOL × Post	2.3***	2.1***	3.8***	4.0***
	(0.53)	(0.61)	(0.72)	(0.62)
Republican × Post		-0.0	-0.4***	-0.3***
		(0.06)	(0.09)	(0.08)
UR				0.8***
				(0.22)
State FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
State Char. x Post	No	No	Yes	Yes
Obs.	1050	1050	1050	1050
Depvar	0.88	0.88	0.88	0.88

Note: This table relies on first-payment time-lapse data from the Department of Labor Employment and Training Administration's 9050 reports. The dependent variable is the fraction of intrastate claims that are topcoded, that is, delayed by over 70 days. All specifications correspond to a TWFE estimator with state and month fixed effects. Column 1 does not include any additional controls. Column 2 includes the interaction of 2016 presidential Republican vote share and Post. Column 3 adds multiple interaction terms of post and another confounder: (1) income share in accommodation and food services (2019), (2) the percentage of the population living in urban areas (2010), (3) UI generosity (Jan. 2020), (4) the percentage of the population living in poverty (2019), and (5) the percentage of the population with at least a bachelor's degree (2019). Column 4 adds the unemployment rate. The sample starts in January 2019 and ends in December 2020, with the post-period starting in June 2020. Given the spurious nature of topcoding being a lagging indicator, for 2020, I drop March, April, and May from the sample. Depvar corresponds to the average value of the fraction of topcoded claims from January 2019 to February 2020 across all 50 states (unweighted). The standard errors are clustered at the month level.

Standard errors: *** p<0.01, ** p<0.05, * p<0.1

Table IV: TWFE Fraction of Claims Delayed at Least 5 Weeks

	(1) Frac Intra Top	(2) Frac Intra Top	(3) Frac Intra Top	(4) Frac Intra Top
COBOL × Post	3.1**	3.3**	4.3***	4.4***
	(1.42)	(1.54)	(1.13)	(1.08)
Republican × Post		0.0	-0.3**	-0.3**
		(0.07)	(0.11)	(0.12)
UR				0.2
				(0.65)
State FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
State Char. x Post	No	No	Yes	Yes
Obs.	1150	1150	1150	1150
Depvar	4.83	4.83	4.83	4.83

Note: This table relies on first-payment time-lapse data from the Department of Labor Employment and Training Administration's 9050 reports. The dependent variable is the fraction of intrastate claims that are delayed at least 5 weeks. All specifications correspond to a TWFE estimator with state and month fixed effects. Column 1 does not include any additional controls. Column 2 includes the interaction of 2016 presidential Republican vote share and Post. Column 3 adds multiple interaction terms of post and another confounder: (1) income share in accommodation and food services (2019), (2) the percentage of the population living in urban areas (2010), (3) UI generosity (Jan. 2020), (4) the percentage of the population living in poverty (2019), and (5) the percentage of the population with at least a bachelor's degree (2019). Column 4 adds the unemployment rate. The sample starts in January 2019 and ends in December 2020, with the post-period starting in April 2020. Given the spurious nature of topcoding being a lagging indicator, I drop March 2020 from the sample. Depvar corresponds to the average value of the fraction of claims delayed at least 5 weeks from January 2019 to February 2020 across all 50 states (unweighted). The standard errors are clustered at the month level.

Standard errors: *** p<0.01, ** p<0.05, * p<0.1

Table V: Penalized Synthetic Control Method, Results

	Treated	Control	PSC fixed λ	PSC MSE λ	PSC Bias λ
Sample Size	28	22	18	22	20
Republican	44.43	48.80	48.88	49	48.42
Urban	82.95	77.45	81.61	81.46	82.31
UI Generosity	12.3	9.11	7.02	7.55	7.31
ACF Incshare	3.37	3.930	4.290	4.360	4.220
Poverty	12.20	12.540	12.650	12.470	12.400
Education	33.81	31.810	31.330	31.380	31.620
EPOP	46.01	45.540	44.340	44.230	44.670
Income Per Cap.	10.96	10.87	10.89	10.9	10.89
Median Age	37.72	38.92	39.73	40.14	39.72
AA Pop. Share	0.11	0.140	0.150	0.140	0.150
Rel. Rep. Rate	1.01	1.09	1.13	1.12	1.12
Teleworkable Emp.	0.37	0.350	0.350	0.350	0.350
Republican Governor	0.46	0.780	0.750	0.900	0.800
Labor Force Pop.	8,903,412	4,695,989	6,589,242	6,579,186	6,578,733
Real GDP	1,179,523	484,750	652,204	647,904	650,749
Treatment Effect	NA	-0.041	-0.039	-0.048	-0.037
λ	NA	NA	0.100	0.000	0.010
Min. Density	NA	NA	1	1	1
Median Density	NA	NA	2	22	2
Max. Density	NA	NA	3	22	4

Note: The table presents results from the penalized synthetic control method (Abadie & L'Hour, 2021) in comparison and the traditional TWFE. The sample size corresponds to how many states are used to create synthetic control groups. In the TWFE setting, the control group is all 22 non-COBOL states. With the penalized synthetic control method, not all states from the donor pool may get selected. For both the traditional TWFE estimator and the penalized synthetic control method, the 28 treated states are the COBOL states. Fifteen covariates are used for creating synthetic controls that are measured prior to the emergency declaration. The one parameter changing across the three penalized synthetic control methods is the penalization parameter: λ. This parameter makes a trade-off between the component-wise fit (to a COBOL state) and aggregate fit (to all COBOL states). The column labeled "PSC fixed λ" corresponds to a fixed value for λ of 0.1. The other penalized synthetic control estimator columns choose lambda in a data-driven manner. One uses a leave-one-out cross-validation procedure to select λ by minimizing the mean squared prediction error in the post-intervention period (after the emergency declaration). The other method chooses λ on validation over the outcomes (relative consumption) in the pre-intervention period (prior to the emergency declaration). All five confounders as well as the 2016 Republican vote share are included in this analysis. The density refers to the number of non-COBOL states used for creating the synthetic control of the COBOL states. For example, a maximum density of 22 refers to at least one COBOL state using all 22 non-COBOL states in its synthetic control. All results use 2019 population weights.

Table VI: TWFE COBOL Usage by Consumption Type

	(1)	(2)	(3)	(4)
	Durables	Nondurables	Inperson_serv	Remote_serv
COBOL × Post	-0.024	-0.034**	-0.028*	-0.014
	[0.014]	[0.014]	[0.015]	[0.016]
Republican × Post	0.002***	0.001	0.006***	0.001
	[0.001]	[0.001]	[0.001]	[0.001]
State FE	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes
Days	335	335	335	335
States	50	50	50	50
Observations	16,750	16,750	16,750	16,750

Note: The table provides results from a two-way fixed-effects (TWFE) estimator with day and state fixed effects, where consumption is broken down by consumption type. The dependent variable is the percentage-point change in a type of credit and debit card consumption (measured at a daily frequency) relative to the base period (January 2020). Column 1 corresponds to durable-goods consumption, column 2 to nondurable-goods consumption, column 3 to in-person services consumption, and column 4 to remote-services consumption. *Post* is a binary variable that takes the value 1 if the date is on or after March 13, 2020. *COBOL* is a binary variable that takes the value 1 if a state uses COBOL in its UI benefits system. The main interaction term is the product of *COBOL* and *Post*. As an additional control in all specifications, I interact *Post* and the 2016 Republican presidential election vote share. These estimates cover the sample period of February 1, 2020, to December 31, 2020. State populations in 2019 are applied as analytic weights. Standard errors are clustered at the state level.

Standard errors: *** p<0.01, ** p<0.05, * p<0.1

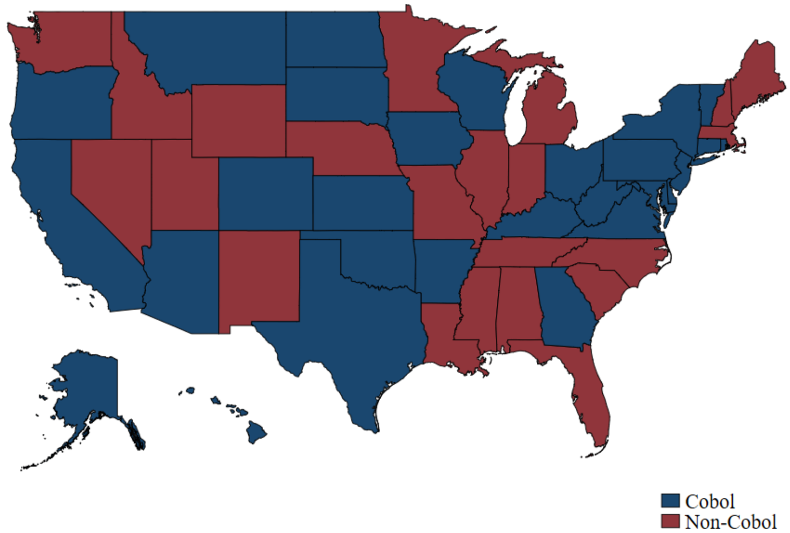
Table VII: TWFE COBOL Usage on All Card Consumption by Income Quartiles

	(1)	(2)	(3)	(4)
	Rel Cons (Q1)	Rel Cons (Q2)	Rel Cons (Q3)	Rel Cons (Q4)
COBOL × Post	-0.014	-0.032*	-0.027*	-0.011
	[0.020]	[0.019]	[0.015]	[0.010]
Republican × Post	0.004***	0.002	0.002	0.002**
	[0.001]	[0.001]	[0.001]	[0.001]
State FE	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes
Days	335	335	335	335
States	48	50	50	50
Observations	16,080	16,750	16,750	16,750

Note: The table provides results from a two-way fixed-effects (TWFE) estimator with day and state fixed effects, where consumption is broken down by income quartiles. Column 1 corresponds to the bottom quartile, column 2 to the second quartile, column 3 to the third quartile, and column 4 to the top quartile. The dependent variable is the percentage-point change in credit and debit card consumption (measured at a daily frequency) for the relevant income quartile relative to the base period. *Post* is a binary variable that takes the value 1 if the date is on or after March 13, 2020. *COBOL* is a binary variable that takes the value 1 if a state uses COBOL in its UI benefits system. The main interaction variable is the product of *COBOL* and *Post*. As an additional control in all specifications, I interact *Post* with the 2016 Republican presidential election vote share. Alaska and Hawaii are omitted in column 1 because their consumption data are missing. These estimates cover the sample period of February 1, 2020, to December 31, 2020. State populations in 2019 are applied as analytic weights. Standard errors are clustered at the state level.

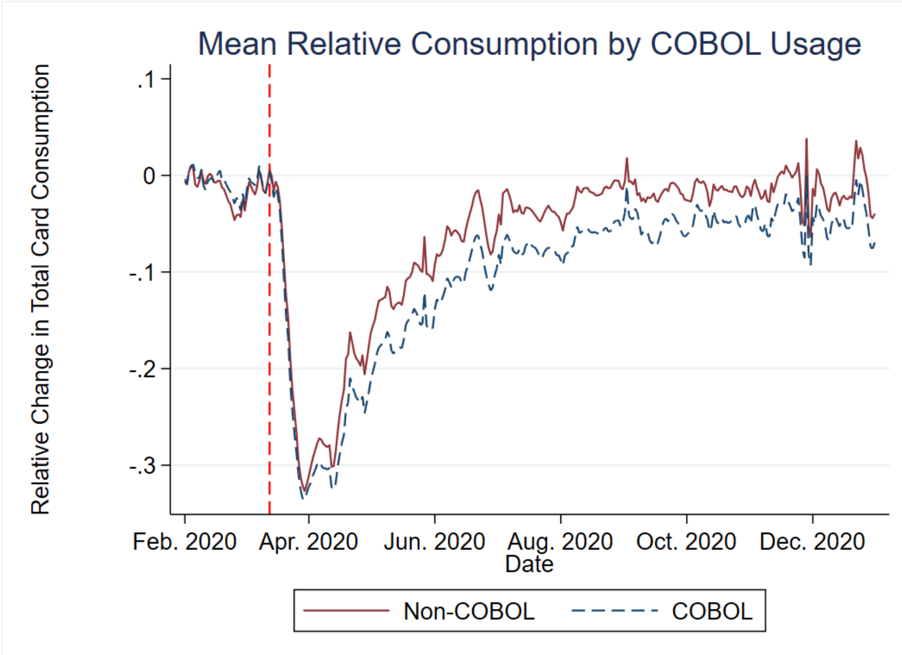
Standard errors: *** p<0.01, ** p<0.05, * p<0.1

Figure 1: Map of COBOL Status



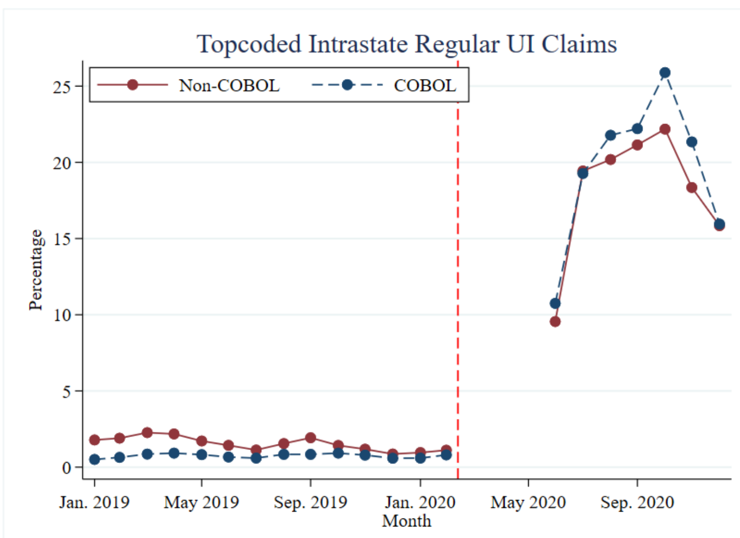
Note: The data on COBOL usage were collected by the author primarily from emails, news articles, and information from the UI Information Technology Support Center. Washington, DC, uses COBOL, but it is excluded from the analysis because of lack of consumption data.

Figure 2: Relative Credit and Debit Card Consumption for All Consumers



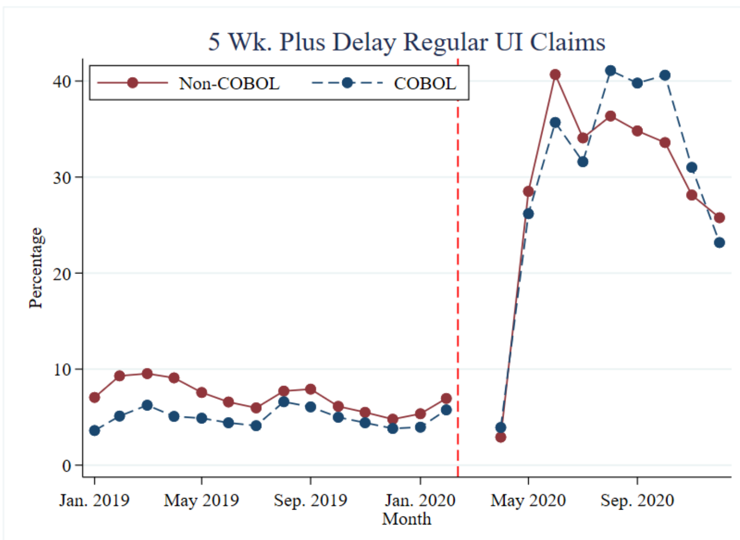
Note: This figure relies on data of all credit and debit card spending at a daily frequency for each state. A population-weighted average across states is computed when aggregating to COBOL and non-COBOL states. The dashed maroon line denotes states expected to experience longer delays and higher shares of discouraged filers: COBOL states. This figure covers the sample period from February 1, 2020, to December 31, 2020.

Figure 3: Percentage of Topcoded Claims (Processing Delays)



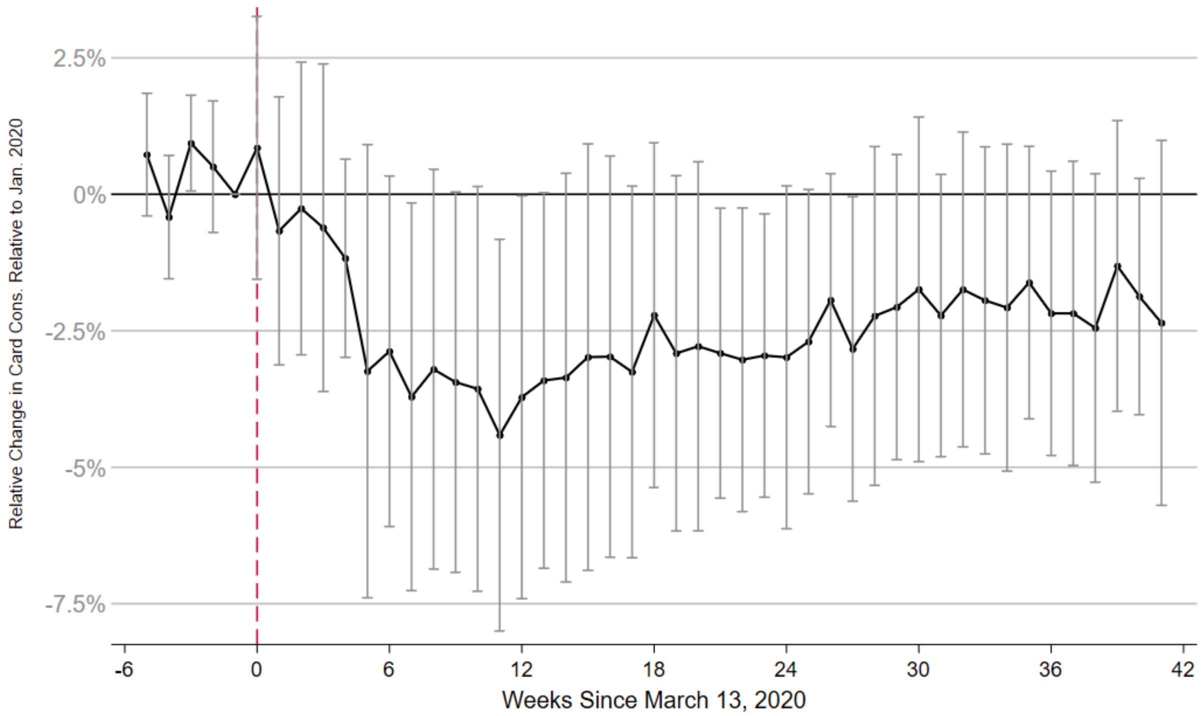
Note: This figure is based on first-payment time-lapse data from the Department of Labor Employment and Training Administration's 9050 reports. The groups are population weighted using 2019 Census estimates. The figure depicts the percentage of intrastate regular UI claims reported as having over a 70-day delay between January 2019 and December 2020 for COBOL and non-COBOL states. The vertical red dashed line corresponds to March 13, 2020. Because topcoding is a lagging indicator, for 2020, I drop March, April, and May from the sample.

Figure 4: Percentage of Claims Delayed at Least 5 Weeks (Processing Delays)



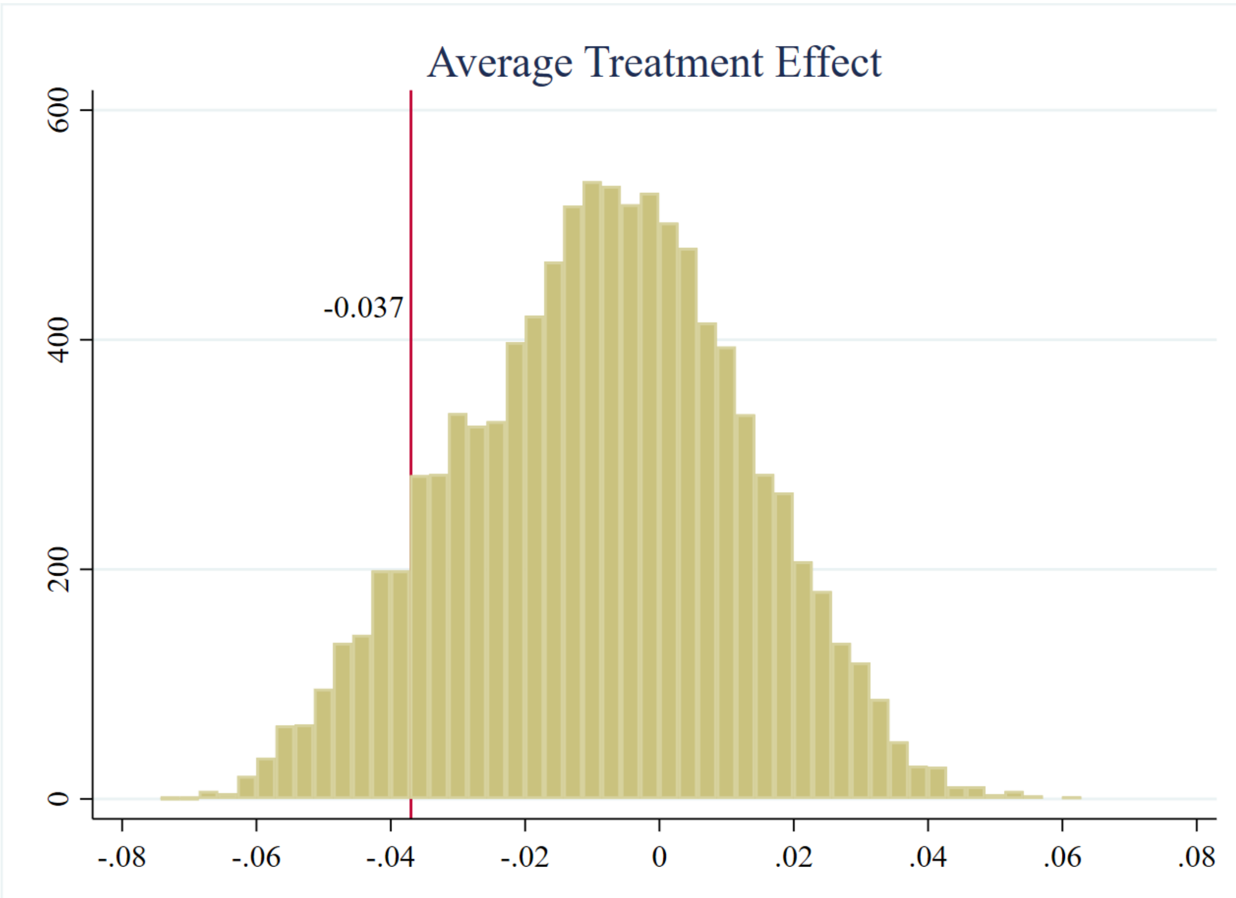
Note: This figure is based on first-payment time-lapse data from the Department of Labor Employment and Training Administration's 9050 reports. The groups are population weighted using 2019 Census estimates. The figure depicts the percentage of intrastate regular UI claims reported as having at least a 5 week processing delay between January 2019 and December 2020 for COBOL and non-COBOL states. The vertical red dashed line corresponds to March 13, 2020. Because topcoding is a lagging indicator, I drop March 2020 from the sample.

Figure 5: Relative-Consumption Weekly Event Study:
Relative Difference between COBOL and Non-COBOL States



Note: The graph is a coefficient plot showing the coefficient on β_k from Equation 3. State and week fixed effects are used in conjunction with an interaction term of *Post* and Republican presidential election vote share in 2016. The red dashed line that goes through week zero corresponds to March 13, 2020. This figure shows that in each week after the week of the emergency declaration, COBOL states saw a larger decline in relative consumption than non-COBOL states. The bounds on each point estimate correspond to a 95% percent confidence interval. State populations from 2019 are applied as analytic weights. Standard errors are clustered at the state level.

Figure 6: Permutation Test for Penalized Synthetic Control Method (10,000 Simulations)



Note: The histogram shows the distribution of average treatment effects when treatment is randomly assigned across 28 of the 50 states 10,000 times using the penalized synthetic control method. The tuning parameter is identical to the one from the column labeled PSC MSE λ in Table V (0.01) in each iteration. To be consistent with the results from Table V, I aggregate the 22 cohort treatment effects using population weights. The red dashed line corresponds to actual treatment effect with the 28 COBOL states: a 3.7-percentage-point decline. This permutation test yields an effect that is significant at the 10% level.

References

Abadie, Alberto & Jeremy L'Hour (2021), "A Penalized Synthetic Control Estimator for Disaggregated Data," *Journal of the American Statistical Association* 116(536): 1817–1834, doi: 10.1080/01621459.2021.1971535, URL https://doi.org/10.1080/01621459.2021.1971535.

Amburgey, Aaron, Serdar Birinci et al. (2020), "Which Earnings Groups Have Been Most Affected by the COVID-19 Crisis," *Economic Synopses* (37).

Cajner, Tomaz, Leland D. Crane, Ryan A. Decker, John Grigsby, Adrian Hamins-Puertolas, Erik Hurst, Christopher Kurz & Ahu Yildirmaz (2020a), "The U.S. Labor Market during the Beginning of the Pandemic Recession," Working Paper 27159, National Bureau of Economic Research, doi:10.3386/w27159, URL http://www.nber.org/papers/w27159.

Cajner, Tomaz, Andrew Figura, Brendan Price, David Ratner & Alison Weingarden (2020b), "Reconciling unemployment claims with job losses in the first months of the COVID-19 crisis," Working paper, Board of Governors of the Federal Reserve System, URL https://doi.org/10.17016/FEDS.2020.055.

Chetty, Raj, John N Friedman, Nathaniel Hendren, Michael Stepner & The Opportunity Insights Team (2020), "How Did COVID-19 and Stabilization Policies Affect Spending and Employment? A New Real-Time Economic Tracker Based on Private Sector Data," Working Paper 27431, National Bureau of Economic Research, doi:10.3386/w27431, URL http://www.nber.org/papers/w27431.

Coibion, Olivier, Yuriy Gorodnichenko & Michael Weber (2020), "The Cost of the Covid-19 Crisis: Lockdowns, Macroeconomic Expectations, and Consumer Spending," Working Paper 27141, National Bureau of Economic Research, doi:10.3386/w27141, URL http://www.nber.org/papers/w27141.

Di Maggio, Marco & Amir Kermani (2016), "The Importance of Unemployment Insurance as an Automatic Stabilizer," Tech. rep., National Bureau of Economic Research.

Dolls, Mathias, Clemens Fuest & Andreas Peichl (2012), "Automatic stabilizers and economic crisis: US vs. Europe," *Journal of Public Economics* 96(3): 279–294, ISSN 0047-2727, doi:https://doi.org/10.1016/j.jpubeco.2011.11.001, URL https://www.sciencedirect.com/science/article/pii/S0047272711001642.

Eilbott, Peter (1966), "The Effectiveness of Automatic Stabilizers," *American Economic Review* 56(3): 450–465, ISSN 00028282, URL http://www.jstor.org/stable/1823778.

Faulkender, Michael W, Robert Jackman & Stephen Miran (2023), "The job preservation effects of paycheck protection program loans," *Available at SSRN 3767509* .

Ganong, Peter, Fiona E. Greig, Pascal J. Noel, Daniel M. Sullivan & Joseph S. Vavra (2022), "Spending and Job-Finding Impacts of Expanded Unemployment Benefits: Evidence from Administrative Micro Data," NBER Working Papers 30315, National Bureau of Economic Research, Inc, URL https://ideas.repec.org/p/nbr/nberwo/30315.html.

Ganong, Peter, Pascal Noel & Joseph Vavra (2020), "US Unemployment Insurance Replacement Rates during the Pandemic," *Journal of Public Economics* 191: 104273, ISSN 0047-2727, doi:https://doi.org/10.1016/j.jpubeco.2020.104273, URL https://www.sciencedirect.com/science/article/pii/S0047272720301377.

Herd, Pamela & Donald P. Moynihan (2018), *Administrative Burden: Policymaking by Other Means*, Russell Sage Foundation, ISBN 9780871544445, URL http://www.jstor.org/stable/10.7758/9781610448789.

Kekre, Rohan (2022), "Unemployment Insurance in Macroeconomic Stabilization," *Review of Economic Studies* 90(5): 2439–2480, ISSN 0034-6527, doi:10.1093/restud/rdac080, URL https://doi.org/10.1093/restud/rdac080.

Lachowska, Marta, Alexandre Mas & Stephen A. Woodbury (2022), "Poor Performance as a Predictable Outcome: Financing the Administration of Unemployment Insurance," *AEA Papers and Proceedings* 112: 102–06, doi:10.1257/pandp.20221073, URL https://www.aeaweb.org/articles?id=10.1257/pandp.20221073.

Marinescu, Ioana, Daphné Skandalis & Daniel Zhao (2021), "The Impact of the Federal Pandemic Unemployment Compensation on Job Search and Vacancy Creation," *Journal of Public Economics* 200: 104471, ISSN 0047-2727, doi:10.1016/j.jpubeco.2021.104471, URL https://www.sciencedirect.com/science/article/pii/S0047272721001079.

McKay, Alisdair & Ricardo Reis (2016), "The Role of Automatic Stabilizers in the U.S. Business Cycle," *Econometrica* 84(1): 141–194, doi:https://doi.org/10.3982/ECTA11574, URL https://onlinelibrary.wiley.com/doi/abs/10.3982/ECTA11574.

Nichols, Albert L. & Richard J. Zeckhauser (1982), "Targeting Transfers through Restrictions on Recipients," *American Economic Review* 72(2): 372–377, ISSN 00028282, URL http://www.jstor.org/stable/1802361.

Parker, Jonathan A., Nicholas S. Souleles, David S. Johnson & Robert McClelland (2013), "Consumer Spending and the Economic Stimulus Payments of 2008," *American Economic Review* 103(6): 2530–53, doi:10.1257/aer.103.6.2530, URL https://www.aeaweb.org/articles?id=10.1257/aer.103.6.2530.

Parton, Brent (2023), "Announcement of a Grant to Support State Unemployment Insurance (UI) Information Technology (IT) Modernization Activities under the American Rescue Plan Act (ARPA)," Online source, Brookings Institution, URL https://www.dol.gov/sites/dolgov/files/ETA/advisories/UIPL/2023/UIPL%2007-23/UIPL%2007-23.pdf.

Simon-Mishel, Julia, Maurice Emsellem, Michele Evermore, Andrew Leclere, Ellen Stettner & Martha Coven (2020), "Centering Workers–How to Modernize Unemployment Insurance Technology," Tech. rep., The Century Foundation, URL https://tcf.org/content/report/centering-workers-how-to-modernize-unemployment-insurance-technology/.

Skandalis, Daphné, Ioana Marinescu & Maxim N. Massenkoff (2022), "Racial Inequality in the US Unemployment Insurance System," Tech. rep., National Bureau of Economic Research.

A Appendix

A Additional Robustness Checks

A.1 Republican Governor and Republican Vote Share

In my preferred specification for my consumption analysis, I have state fixed, day fixed effects, and the 2016 presidential Republican vote share interacted with Post as controls. In Table F.1, instead of using the 2016 presidential Republican vote share as a control, I use the Republican governor. Unlike the 2016 Republican vote share, there are statistically significant differences in Republican governor between COBOL and non-COBOL states. Columns 2 and 3 are significant at the 5 percent level and have a marginally higher point estimate, 3.0 ppt. relative decrease, than the baseline case with the Republican vote share. Columns 4 and 5 that introduce the state characteristics interacted with post noticeably decrease the point estimates to 2.1 ppt. and 2.0 ppt., respectively. These results are significant at the 10 percent level. I prefer using Republican vote share over Republican governor given that Republican vote share is a continuous variable while Republican governor is a binary variable.

However, one could view the Republican vote share and Republican governor as picking up different sources of variation. Republican vote share could capture COVID-19 cautiousness, while the Republican governor could capture different policies implemented during the pandemic. In table F.2 I use the Republican governor and Republican vote share as controls simultaneously. My results are robust across all specifications. The point estimates range from a 2.1 ppt decline to a 2.6 ppt. decline in specifications with controls other than fixed effects. All specifications are all significant at the 10 percent level and have have similar point estimates to the version without Republican governor. These new point estimates are slightly lower than the baseline, but are not statistically different than the baseline.

A.2 Randomization Inference

As another robustness check, I perform randomization inference where I conduct a permutation test. Specifically, I randomly assign COBOL status to 28 states 1,000 times and then run my preferred specification, column 2 of Table II, with this random assignment of treatment. The specification has relative card consumption as the dependent variable, day fixed effects, state fixed effects, and 2016 Republican vote share interacted with Post. The treatment is the product of Post and the simulated COBOL variable. Post is a binary variable that takes the value of 1 after March 13 independent of the iteration. The simulated COBOL variable assigns COBOL status to 28 out of the 50 states and which 28 states are selected varies depending on the iteration. Similar to my regression results, I applied population weights. Figure F.1 shows that the permutation test yields significant results at the 10 percent level. The other specifications in Table II are all significant at the 10 percent level.[36]

[36] Available upon request.

A.3 Other Pandemic Transfer Programs

One concern one may have is that COBOL usage in UI benefit systems is correlated with other pandemic transfer programs, and that these transfer programs may well have had an independent effect on consumption. I focus on three pandemic transfer programs that were in effect in 2020: Paycheck Protection Plan (PPP), Economic Impact Payments (EIP), and Supplemental Nutrition Assistance Program (SNAP). As Faulkender et al. (2023) show PPP was intricately connected to UI given that PPP was partially implemented to help alleviate the stress that UI benefit systems were undergoing during the pandemic recession. SNAP and EIP were large pandemic transfer programs that were income based where high income earners were less likely to be eligible or at least eligible for lower amounts. EIP and PPP were federal programs, while SNAP is a state administered program. The data for the PPP loans is provided by the U.S. Small Business Administration.[37] The data on the EIP is provided by the Internal Revenue Service's (IRS) Statistics of Income (SOI) program. [38] The data for SNAP is provided by the U.S. Department of Agriculture (USDA).[39]

Table F.3 shows summary statistics for COBOL and non-COBOL states for the following PPP measures, reported in per capita and per worker terms: initial loan amount, jobs reported, and total loans after dividing by either the 2019 labor force or by the 2019 population. These summary statistics correspond to PPP loans with approval dates before January 1, 2021. Specifically, COBOL states received \$1,635.65 per capita in PPP funds, while non-COBOL states received \$1,535.67. As reflected Table F.3, all the PPP outcomes have t-statistics that are statistically insignificant between COBOL and non-COBOL states.

In Table F.4, I created summary statistics for the first round of EIP that was disbursed in April 2020. Similar to PPP, there is no statistically significant difference between COBOL and non-COBOL states regardless of which of the two normalizations that I apply: (1) labor force (LF) or (2) population (Pop). Specifically, COBOL states received \$851.76 per capita of EIP funds, while non-COBOL states received \$841.79.

In Table F.5, I created summary statistics for SNAP benefits that were disbursed between March 2020 and December 2020. Similar to PPP and EIP, there is no statistically significant difference between COBOL and non-COBOL states regardless of which of the two normalizations that I apply: (1) labor force (LF) or (2) population (Pop). Specifically, COBOL states received \$216.89 per capita in SNAP funds, while non-COBOL states received \$219.03.

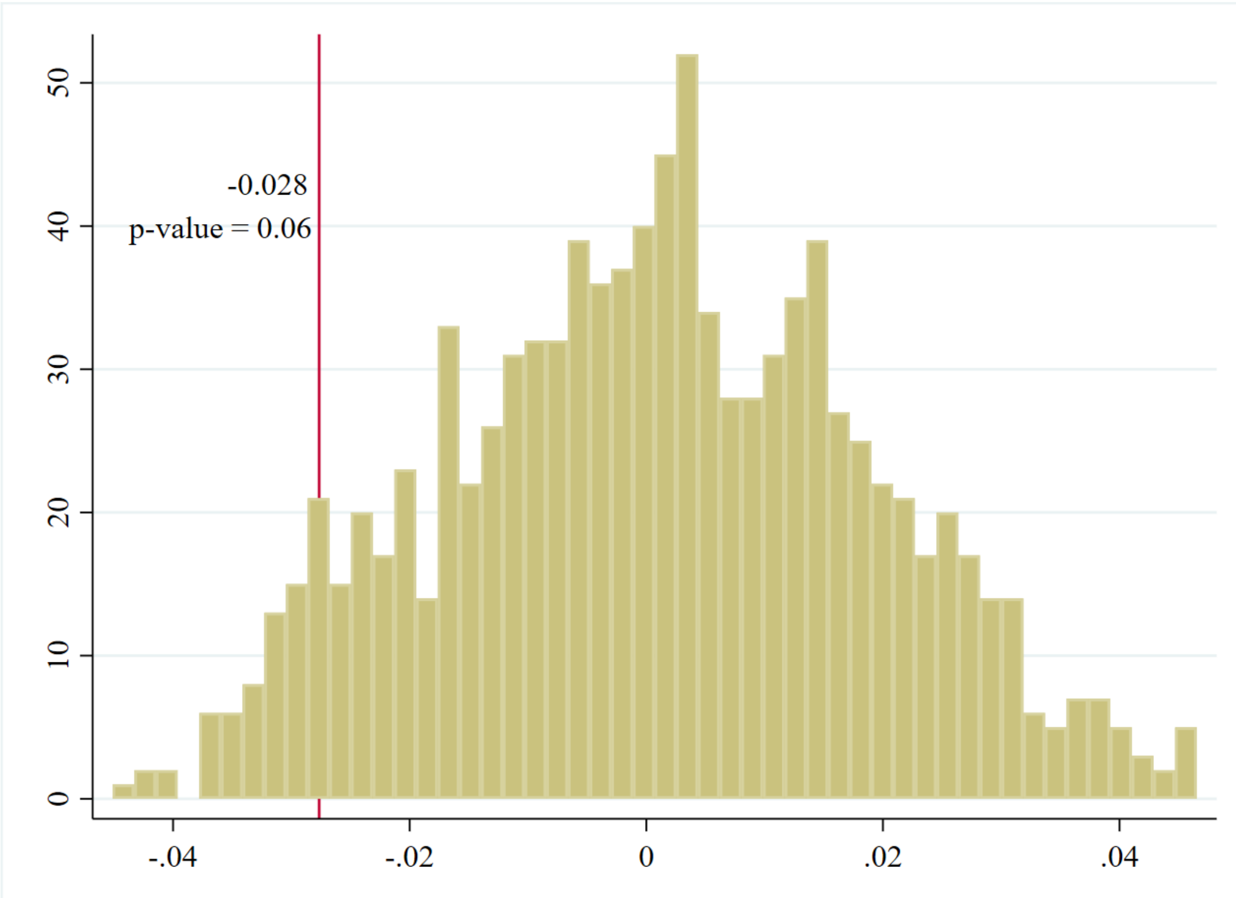
Overall, the analysis in Tables F.3, F.4, and F.5 demonstrates that there were not systematic differences in the disbursement of benefits between COBOL and non-COBOL states in regards to SNAP, EIP, and PPP. A full analysis of the impact of COBOL-induced UI issues on consumption with the inclusion of the transfer program benefit amounts would require a complex analysis across states and timing of disbursement. For example, Chetty et al. (2020) look at EIP affecting consumption only for 31 days starting from April 15, 2020. This complex analysis goes beyond the

[37]The data can be accessed from the SBA website.
[38]The data can be accessed from the IRS SOI website.
[39]The data can be accessed from the USDA website.

scope of this dissertation paper.

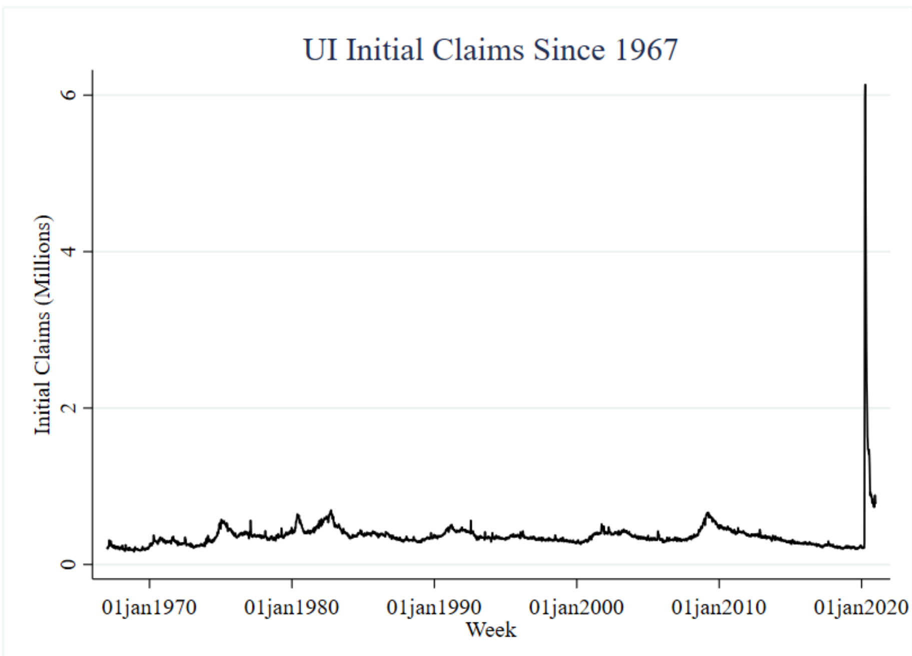
B Additional Tables and Figures

Figure F.1: Permutation Test for TWFE Estimator (1,000 Simulations)



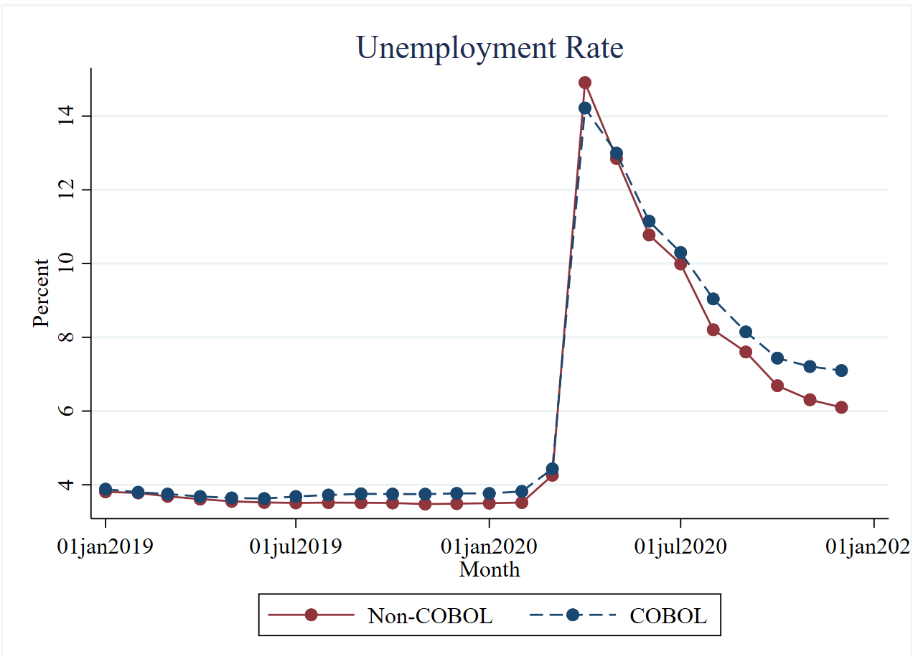
Note: The histogram shows the distribution of average treatment effects when treatment is randomly assigned across 28 of the 50 states 1,000 times using the TWFE specification with the 2016 Republican vote share interacted with Post. To be consistent, I apply population weights as analytic weights. The red line corresponds to actual treatment effect with the 28 COBOL states: a 2.8-percentage-point decline. This permutation test yields an effect that is significant at the 10% level.

Figure F.2: National UI Initial Claims



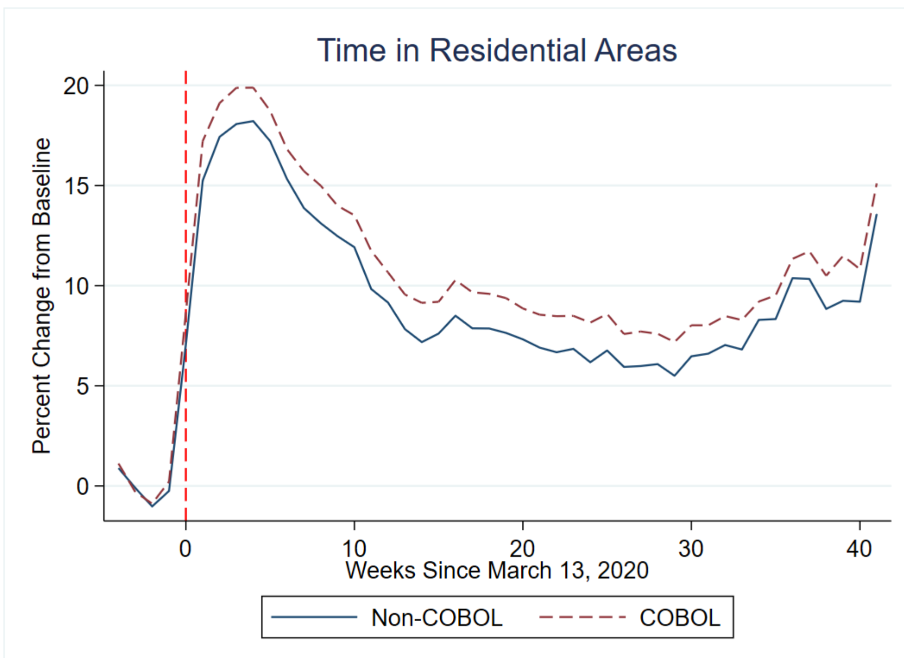
Note: This figure uses weekly initial claims data from 1967 to the end of 2020. The data used are from the Department of Labor Employment Training Administration (DOLETA). The peak in initial claims corresponds to the first week of April 2020 where there was just north of 6 million initial claims filed that week at the national level.

Figure F.3: Unemployment Rate by COBOL Usage



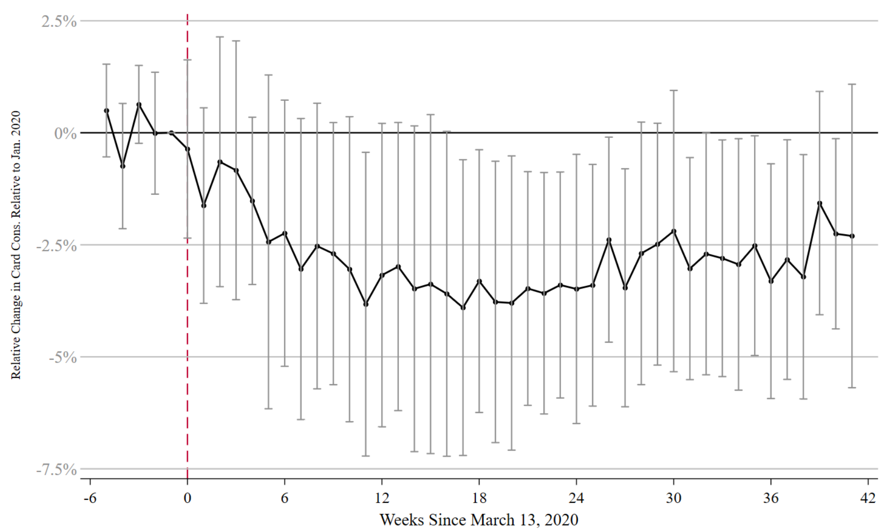
Note: This figure uses monthly seasonally adjusted state unemployment rate from the BLS. The data range from January 2019 to December 2020. The data is relative time spent in residential area relative to that's baseline period of time in residential areas. The baseline period corresponds to the first six weeks of 2020. The maroon line corresponds to non-COBOL states and the navy line corresponds to COBOL states. These two groups of states are aggregated using 2019 population weights.

Figure F.4: Potential COVID-19 Cautiousness



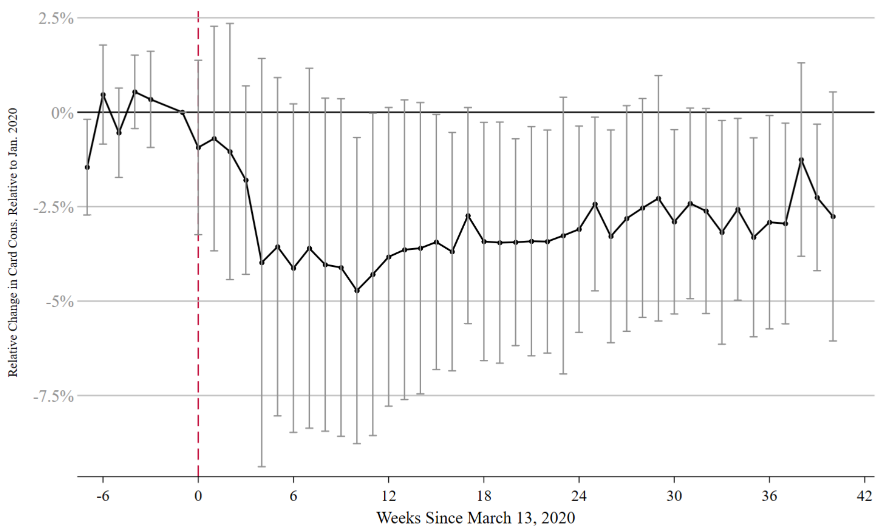
Note: This figure uses daily Google Mobility data from February 2020 to December 2020. I aggregate the data to the weekly frequency. Weeks a are determined relative to the week ending on March 13, 2020, which corresponds to the red vertical dashed line. The maroon line corresponds to non-COBOL states and the navy line corresponds to COBOL states. These two groups of states are aggregated using 2019 population weights.

Figure F.5: Relative-Consumption Weekly Event Study:
Relative Difference between COBOL and Non-COBOL States with Dynamic Rep. Share



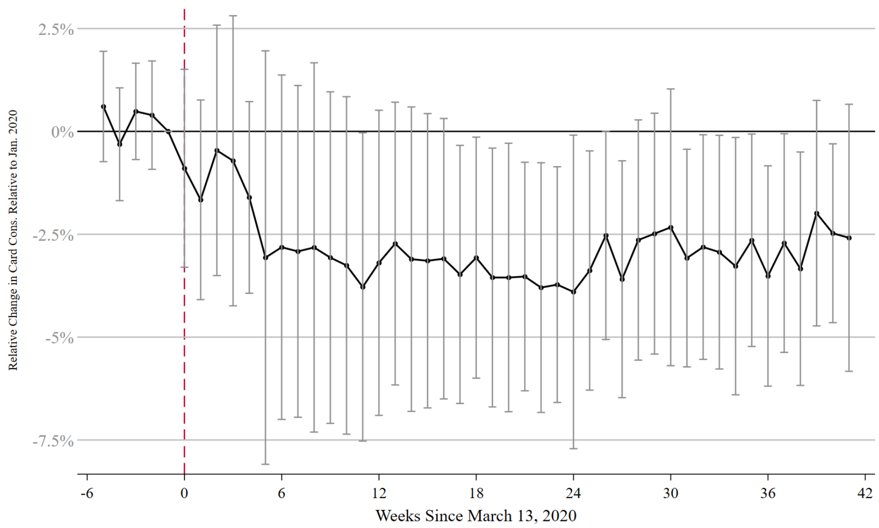
Note: The graph is a coefficient plot showing the coefficient on β_k from Equation 3. State and week fixed effects are included as well as COVID-19 controls. However, instead of using 2016 Republican vote share interacted with $Post_t$, I use the 2016 Republican vote share interacted with I_k. The red dashed line that goes through week zero corresponds to March 13, 2020. This figure shows that in each week after the week of the emergency declaration, COBOL states saw a larger decline in relative consumption than non-COBOL states even when dynamically interacting relative time spent at home. Relative time spent at home should be interpreted as a proxy for COVID-19 cautiousness. The bounds on each point estimate correspond to a 95% percent confidence interval. State populations from 2019 are applied as analytic weights. Standard errors are clustered at the state level.

Figure F.6: Relative-Consumption Weekly Event Study:
Relative Difference between COBOL and Non-COBOL States with Google Mobility (Control)



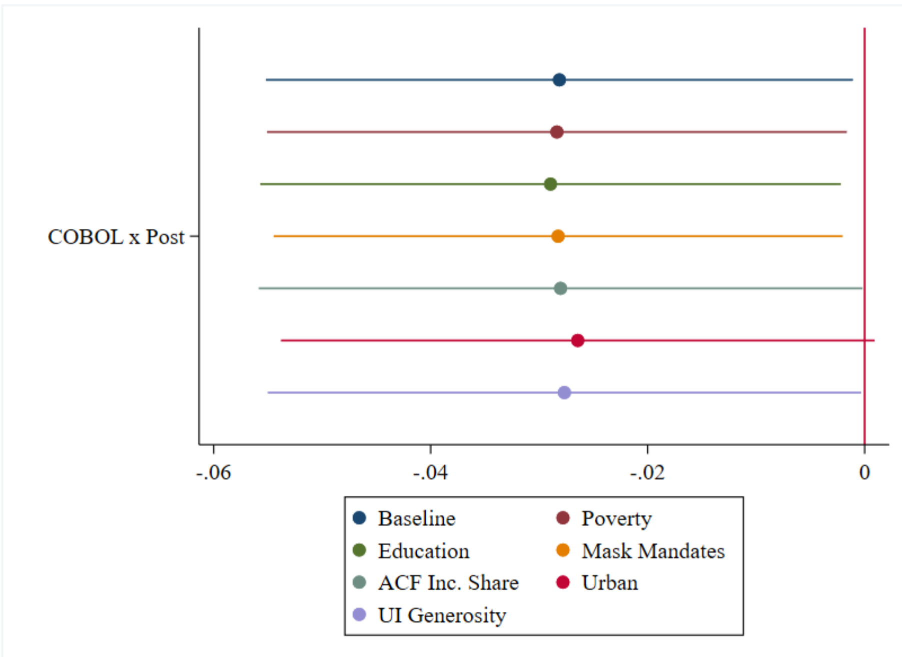
Note: The graph is a coefficient plot showing the coefficient on β_k from Equation 3. State and week fixed effects are included as well as COVID-19 controls. However, there instead of controlling for the 2016 Republican vote share, I control for relative time at home in state i in week k using Google Mobility data. The red dashed line that goes through week zero corresponds to March 13, 2020. This figure shows that in each week after the week of the emergency declaration, COBOL states saw a larger decline in relative consumption than non-COBOL states even when dynamically interacting relative time spent at home. Relative time spent at home should be interpreted as a proxy for COVID-19 cautiousness. The bounds on each point estimate correspond to a 95% percent confidence interval. State populations from 2019 are applied as analytic weights. Standard errors are clustered at the state level.

Figure F.7: Relative-Consumption Weekly Event Study:
Relative Difference between COBOL and Non-COBOL States with Google Mobility (Interacted Control)



Note: The graph is a coefficient plot showing the coefficient on β_k from Equation 3. State and week fixed effects are included as well as COVID-19 controls. However, there instead of controlling for the 2016 Republican vote share, I control for relative time at home in state i in week k and interact it with I_k. The red dashed line that goes through week zero corresponds to March 13, 2020. This figure shows that in each week after the week of the emergency declaration, COBOL states saw a larger decline in relative consumption than non-COBOL states even when dynamically interacting relative time spent at home. Relative time spent at home should be interpreted as a proxy for COVID-19 cautiousness. The bounds on each point estimate correspond to a 95% percent confidence interval. State populations from 2019 are applied as analytic weights. Standard errors are clustered at the state level.

Figure F.8: Coefficient Plot Interacting Potential Confounders Individually



Note: This figure plots the coefficient on $COBOL \times Post$ from the TWFE estimator (state fixed effects and day fixed effects). The baseline definition includes the COVID-19 controls and the interaction of COBOL state and the 2016 Republican presidential vote share. The remaining five coefficients build upon the baseline specification by adding one confounder, $Confounder_i$, and interacting it with $Post_t$. The figure plots coefficients on $COBOL \times Post$ ranging from a 2.6 percentage point decline in relative consumption to a 2.9 percentage point decline. The effect is significant at the 5 percent level in all specifications except for the one that adds percentage of the population living in an urban area (still significant at the 10 percent level). Standard errors are clustered at the state level.

Table F.1: TWFE COBOL Usage on All Card Consumption (Republican Governor, Replacement)

	(1) Rel Cons	(2) Rel Cons	(3) Rel Cons	(4) Rel Cons	(5) Rel Cons
COBOL × Post	-0.041**	-0.030**	-0.030**	-0.021*	-0.020*
	[0.020]	[0.015]	[0.015]	[0.011]	[0.011]
RepGov × Post		0.037*	0.038*	0.031	0.032
		[0.021]	[0.021]	[0.020]	[0.019]
UR					0.002
					[0.002]
State FE	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	No	Yes	Yes	Yes
State Char. × Post	No	No	No	Yes	Yes
Days	335	335	335	335	335
States	50	50	50	50	50
Observations	16,750	16,750	16,750	16,750	16,750

Note: The table presents results from a TWFE estimator with day and state fixed effects. The dependent variable is the percentage-point change relative to the base period in credit and debit card consumption measured at the daily frequency. *Post* is a binary variable that takes the value 1 if the date is on or after March 13, 2020. *COBOL* is a binary variable that takes the value 1 if a state uses COBOL in its UI benefits system. The interaction term of interest is the product of *COBOL* and *Post*. *RepGov* is a binary variable corresponding to whether a state had a Republican governor in 2019. COVID-19 controls include new COVID-19 death rates and new COVID-19 case rates. Column 1 only includes state and day fixed effects. Column 2 adds the interaction of *Republican* and *Post*. Column 3 adds the COVID-19 controls. Column 4 adds five terms of *Post* interacted with another confounder: (1) income share in accommodation and food services (2019), (2) mask mandates in July 2020 (2020), (3) the percentage of the population living in poverty (2019), (4) the percentage of the population with at least a bachelor's degree (2019), and (5) UI generosity (Jan. 2020). Column 5 adds the monthly unemployment rate. These estimates cover the sample period of February 1, 2020, to December 31, 2020. State populations in 2019 are applied as analytic weights. Standard errors are clustered at the state level.

Table F.2: TWFE COBOL Usage on All Card Consumption (Republican Governor, Inclusion)

	(1) Rel Cons	(2) Rel Cons	(3) Rel Cons	(4) Rel Cons	(5) Rel Cons
COBOL × Post	-0.041**	-0.026*	-0.027*	-0.022**	-0.021*
	[0.020]	[0.014]	[0.014]	[0.011]	[0.011]
Republican Gov. × Post		0.006	0.007	0.018	0.019
		[0.016]	[0.016]	[0.018]	[0.017]
Republican × Post		0.003***	0.003***	0.003**	0.003***
		[0.001]	[0.001]	[0.001]	[0.001]
UR					0.002
					[0.002]
State FE	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes
COVID-19 Controls	No	No	Yes	Yes	Yes
State Char. × Post	No	No	No	Yes	Yes
Days	335	335	335	335	335
States	50	50	50	50	50
Observations	16,750	16,750	16,750	16,750	16,750

Note: The table presents results from a TWFE estimator with day and state fixed effects. The dependent variable is the percentage-point change relative to the base period in credit and debit card consumption measured at the daily frequency. *Post* is a binary variable that takes the value 1 if the date is on or after March 13, 2020. *COBOL* is a binary variable that takes the value 1 if a state uses COBOL in its UI benefits system. The interaction term of interest is the product of *COBOL* and *Post*. *Republican* is the Republican vote share in the 2016 presidential election. COVID-19 controls include new COVID-19 death rates and new COVID-19 case rates. Column 1 only includes state and day fixed effects. Column 2 adds the interaction of *Republican* and *Post* as well as interacting a binary variable indicating whether a state had a Republican governor and *Post*. Column 3 adds the COVID-19 controls. Column 4 adds five terms of *Post* interacted with another confounder: (1) income share in accommodation and food services (2019), (2) mask mandates in July 2020 (2020), (3) the percentage of the population living in poverty (2019), (4) the percentage of the population with at least a bachelor's degree (2019), and (5) UI generosity (Jan. 2020). Column 5 adds the monthly unemployment rate. These estimates cover the sample period of February 1, 2020, to December 31, 2020. State populations in 2019 are applied as analytic weights. Standard errors are clustered at the state level.

Standard errors: *** p<0.01, ** p<0.05, * p<0.1

Table F.3: PPP Summary Statistics by COBOL Status (PPP 2020)

	Non-COBOL Mean	COBOL Mean	Diff	t-stat	Non-COBOL N	COBOL N
Loan Amount per LF	3,056.37	3,212.00	-155.64	-1.25	22	28
Jobs Reported per LF	0.36	0.37	-0.00	-0.42	22	28
Total Loans per LF	0.03	0.03	-0.00	-0.03	22	28
Loan Amount per Pop	1,535.67	1,635.65	-99.99	-1.20	22	28
Jobs Reported per Pop	0.18	0.19	-0.01	-0.71	22	28
Total Loans per Pop	0.02	0.02	-0.00	-0.30	22	28

Note: The summary statistics table shows the differences in PPP loan amounts, total loans, and jobs reported between COBOL and non-COBOL states. These statistics are normalized by either the 2019 labor force or the 2019 population. This covers the period of PPP loans disbursed in 2020. These statistics are denominated as the total PPP amount disbursed in 2020 once the program started in April 2020. All statistics are in per capita or per worker terms.

Table F.4: EIP Summary Statistics by COBOL Status (First Round)

	Non-COBOL Mean	COBOL Mean	Diff	t-stat	Non-COBOL N	COBOL N
No. Pay per LF	1.01	0.99	0.02	0.75	22	28
No. Pay per Pop	0.50	0.50	0.00	0.26	22	28
Pay Amount per LF	1,715.88	1,669.02	46.87	1.02	22	28
Pay Amount per Pop	851.76	841.79	9.98	0.77	22	28

Note: The summary statistics table shows the differences in EIP payment amounts and number of EIP recipients. These statistics are either normalized by the 2019 labor force (LF) or the 2019 population (Pop). These statistics only correspond to the first wave of EIP, which occurred in April 2020. These statistics are denominated in total EIP disbursed in 2020 (one observation per state). All statistics are in per capita or per worker terms.

Table F.5: SNAP Summary Statistics by COBOL Status (Mar. 2020 to Dec. 2020)

	Non-COBOL Mean	COBOL Mean	Diff	t-stat	Non-COBOL N	COBOL N
Pay Amount per LF	448.33	433.65	14.68	0.31	22	28
Pay Amount per Pop	219.03	216.89	2.14	0.10	22	28
HH Par. per LF	1.26	1.21	0.05	0.39	22	28
HH Par. per Pop	0.62	0.61	0.01	0.17	22	28
Ind. Par. per LF	2.49	2.36	0.13	0.53	22	28
Ind Par. per Pop	1.22	1.18	0.04	0.34	22	28

Note: The summary statistics table shows the differences in SNAP payment amounts, household participation, and individual participation. These statistics are normalized by either the 2019 labor force (LF) or the 2019 population (Pop). These statistics are denominated as the total SNAP amount disbursed from March 2020 to December 2020. All statistics are in per capita or per worker terms.

Hutchins Center
on Fiscal & Monetary Policy
at BROOKINGS

The mission of the Hutchins Center on Fiscal and Monetary Policy is to improve the quality and efficacy of fiscal and monetary policies and public understanding of them.

Questions about the research? Email ESmedia@brookings.edu.
Be sure to include the title of this paper in your inquiry.