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

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Abstract

The US experienced an unprecedented increase in unemployment insurance (UI) claims beginning in March 2020. State UI-benefit systems were inadequately prepared to process these claims. In states that used an antiquated programming language, COBOL, claimants experienced a larger increase in administrative difficulties, resulting in longer delays in benefit disbursement. States that used an antiquated UI-benefit system experienced a 2.8-percentage-point decline in total credit and debit card consumption relative to states with more modern UI-benefit systems. Furthermore, these states experienced a 2.1-percentage-point increase in the share of claims delayed by over 70 days.

JEL Codes: J65, I38, E21

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1 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 led to real consumption to be at least $40 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,

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

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. As a result, these states should experience a relative decline in consumption at the onset of the pandemic, followed by only a partial recovery once delayed benefits are disbursed, as those benefits are less effective at stimulating aggregate demand due to the dampened fiscal multiplier. Specifically, the attenuated fiscal multiplier implies a lasting shortfall in consumption in COBOL states relative to non-COBOL states, even after benefit payments have been disbursed.

One mechanism by which COBOL usage in UI benefits led to lower aggregate consumption is delays in the UI benefits disbursement. These delays could arise both because it took longer for claimants to successfully file a claim and because processing those claims took longer. A second, related mechanism is discouraged filing: potential claimants in COBOL states may have been less likely to file a claim, or to complete the filing process, because applying for UI was 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.[3] Delayed claims could affect aggregate consumption through a dampened UI fiscal multiplier. Ganong et al. (2024) 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 et al., 2020) from April 2020 to the end of July 2020, delaying UI benefits by months could significantly alter consumption behavior.

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

[3]I also present suggestive evidence of discouraged filers in the Appendix Section B, using UI outlays data on first benefit payments.

2 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.

2.1 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 (PUA) 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 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

[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 et al. (2020a).

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

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 median 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 differed in maximum benefit levels and durations prior to the pandemic, UI benefits were not uniform nationwide. 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 potentially 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]

[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.

2.2 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 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 frictions would have increased the administrative burden faced by claimants through reduced UI administrative capacity, contributing to longer payment delays and reducing the effectiveness of UI delivery.

Despite COBOL being capable of completing the same tasks as modern programming languages, implementing these changes were more difficult in COBOL states. One challenge is that COBOL-based UI benefit systems often rely on legacy "spaghetti code"—highly complex and difficult-to-maintain programming that makes implementing policy changes especially burdensome. 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 roughly 8.6 million lines of code, 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 rejected in 2020. In previous years the number of rejected claims varied between 50,000 to 100,000 claims. These rejected claims require

[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.

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 systems 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 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 4.2, 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.[16]

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.

[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.

[16] In the Online Appendix, I use alternative measures of COVID-19 cautiousness.

2.3 Related Literature

This paper adds to the growing literature measuring the economic impacts of the pandemic recession (Baker et al. (2020); Cajner et al. (2020b); Chetty et al. (2023); Coibion et al. (2020); Marinescu et al. (2021); Ganong et al. (2020)). I exploit a novel source of heterogeneity across states, COBOL usage in 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 et al. (2012); McKay and Reis (2016)) by being the first to directly examine the aggregate economic consequences at the state-level of delaying a fiscal stabilizer: UI. Specifically, I exploit cross-state variation in COBOL usage as a source of differential exposure to UI administrative constraints during the pandemic. I show that COBOL states experienced longer UI disbursement delays and that consumption in these states remained persistently below consumption in non-COBOL states. I also provide suggestive evidence that COBOL states had a higher share of discouraged filers, indicating that administrative burdens may have affected consumption both by delaying payments and by reducing access to benefits.

The paper in the pandemic-recession literature that most closely resembles mine is Ganong et al. (2024). 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 instead ask how administrative burdens in COBOL-based UI systems affected aggregate consumption in COBOL states by making it more difficult for potential claimants to access benefits. These burdens could operate both by discouraging potential claimants from filing or completing claims and by slowing the delivery of benefits to successful claimants. 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 $40 billion lower than it would have been if COBOL states had modernized their UI systems.[17] My estimates are limited to the effects of

[17]This $40 billion figure corresponds to a back-of-the-envelope exercise that uses the same April 2020 to July 2020

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. Potential claimants had to spend hours on the phone or online trying to file, visit UI offices during a pandemic, and face long and uncertain wait times for their claims to be processed. These administrative burdens could have discouraged potential claimants from receiving benefits. Deshpande and Li (2019) find that increased administrative burdens reduced program participation by discouraging potential disability insurance recipients. My work is the first, to my knowledge, to examine the macroeconomic consequences of administrative burdens in the context of a fiscal stabilizer.

A key distinction in my setting is that the administrative burden induced by COBOL reflects a latent administrative-capacity constraint rather than an explicit policy choice to reduce take-up. Herd and Moynihan (2018) note that administrative burdens can be a form of policymaking known as targeting, whereby states deliberately increase burdens in order to reduce a program's take-up rate. This logic could apply 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 et al., 2022). States may also impose administrative burdens in UI to reduce fraud. In contrast, COBOL was not chosen to increase the burden on claimants. Instead, COBOL-based systems became burdensome when UI systems were overwhelmed by claims and when large policy changes had to be implemented quickly, as was the case during the pandemic recession. Prior to the pandemic, COBOL-based systems did not appear to generate longer processing delays, as reflected by COBOL states having a lower share of topcoded claims—claims with processing delays greater than 70 days—in Figure 4. Thus, the pandemic revealed how latent administrative-capacity constraints can become binding precisely when fiscal stabilizers are most needed.

3 Data

I use two main sources of data for my outcome variables: the Department of Labor Employment and Training Administration (DOLETA) and Affinity Solutions, which is part of the Economic

period as Ganong et al. (2024).

Tracker (Chetty et al., 2023).

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.

3.1 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 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 (right-censors) 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 UI programs, and data from Ganong et al. (2024) 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 also have 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 the number of people whose first benefit was more than 70 days late (i.e., the number topcoded claims) 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 with processing delays is that the accuracy of measured delays could be related to COBOL usage, particularly during recessionary periods. Specifically, non-COBOL states may do a better job of measuring 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 delays between COBOL and non-COBOL states.

3.2 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. (2023). 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., 2023). Chetty et al. (2023) 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.[18] Chetty et al. (2023) 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., 2023).[19] 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 (2023) 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.

3.3 Data on Discouraged Filers

Because discouraged filers are not directly measured in traditional administrative datasets, I use a Google Survey analyzed by the Economic Policy Institute in April 2020.[20] The survey asked respondents whether they had applied for UI benefits in the previous four weeks. Respondents could select one of the following options: (1) I applied successfully, (2) I tried but I could not get through, (3) I tried but my application was rejected, (4) I did not apply because it was too difficult, (5) I did not apply because I am not eligible, and (6) I did not apply because I did not lose a job.

I restrict the sample to respondents who either applied successfully or reported a filing difficulty,

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

[19] In the Online Appendix, I find differences in aggregate PCE between COBOL and non-COBOL states starting in 2020

[20] The survey was fielded between April 14, 2020 and April 24, 2020. For details, see Zipperer and Gould (2020).

excluding respondents who did not apply because they were not eligible or because they did not lose a job. I define a respondent as a discouraged filer if they tried to apply but could not get through, tried but had their application rejected, or did not apply because the process was too difficult. Respondents who applied successfully are coded as non-discouraged filers. This measure captures filing barriers at the extensive margin of UI access, rather than processing delays among successfully filed claims. Because the survey is cross-sectional and fielded during a short window in April 2020, I use it as suggestive evidence on filing burdens rather than as a causal estimate of the effect of COBOL systems on discouraged filing.

3.4 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 five other covariates, are shown in Table 1.

To further address concerns that COBOL and non-COBOL states may have differed in ways that shaped their consumption responses during the pandemic, I also include a set of pre-pandemic state characteristics interacted with the post-emergency period indicator, $Post_t$. 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. These five control variables were selected because they may plausibly affect 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 industry (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 industry was disproportionately affected by the pandemic recession, raising the concern that COBOL states may have had a higher income share in this industry. Another concern may be that COBOL states had less generous UI benefits, so they were slower to recover from the pandemic recession. Both concerns are mitigated by the fact that COBOL states had a lower income share in the accommodation and food services sector and more generous UI benefits, as shown in Table 1. 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 Program (PPP), the Economic Impact Payments (EIP), and the Supplemental Nutrition Assistance Program (SNAP). This analysis is reflected in Table 5. 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 1. 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.

3.5 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).[21] This definition of an antiquated UI benefit system 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.[22] 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: generally, a COBOL state is also an antiquated state, and a non-COBOL state is a modernized state.

4 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.

4.1 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:

$$Y_{it} = \alpha_0 + \beta_1 \big(Post_t \times COBOL_i\big) + \big(Post_t \times X_i\big)'\beta_2 + Z_{it}'\gamma + \phi_t + \psi_i + \varepsilon_{it}. \tag{1}$$

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

[21] 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.

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

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.

4.2 Relative Consumption

My dependent variable for the consumption 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. (2023) 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. (2023). 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:

$$Rel\_Cons_{ik} = \alpha_0 + \sum_{\ell=-5}^{41} \beta_\ell \big(COBOL_i \times I_{\ell k}\big) + \delta' \big(Post_k \times X_i\big) + \gamma' Z_{ik} + \phi_k + \psi_i + \epsilon_{ik}. \quad (3)$$

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. $I_{\ell k}$ is a binary variable that takes the value 1 if observation week k corresponds to event week ℓ. The omitted event-week category is $\ell = -1$, the week before the emergency declaration. The event study allows me to trace the evolution of the effect of higher administrative burdens and lower state UI capacity on aggregate consumption. I define the relative weekly-consumption measure using total card spending from each Friday of the week.[23] This step ensures that each week's value is not mechanically related to the previous week's value through the seven-day moving average. 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 the 2016 Republican vote share. X_i is then interacted with $Post_k$, which takes the value of one for weeks after the emergency declaration.[24]

To interpret the relative consumption effect from Equations 1 and 3 as causal, I need to satisfy the conditional parallel trends assumption. Although Figure 3 and the longer aggregated PCE series show similar pretrends, the conditional parallel trends assumption could still be violated if COBOL and non-COBOL states responded differently to the COVID-19 shock. Despite experiencing similar COVID-19 case numbers and deaths as reflected in Table 1, individuals in COBOL states were more COVID-19 cautious than individuals in non-COBOL states. This difference in attitude would upward-bias the magnitude of my consumption estimates. This would violate the (conditional)

[23] I choose Friday because March 13, 2020, fell on a Friday.

[24] I have also interacted X_i, Republican vote share, with $I_{\ell k}$ in a similar fashion to COBOL. I find consistent results.

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.

4.3 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).[25] 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.

4.4 Discouraged Filers

As additional evidence on whether COBOL-based UI systems increased the burden of accessing benefits, I estimate cross-sectional regressions using the Google Survey described in Section 3.3. These regressions test whether respondents in COBOL states were more likely to report being unable to access UI benefits because of filing difficulties. The estimating equation is

$$Discouraged_{is} = \alpha + \beta COBOL_s + X_{is}'\gamma + Z_s'\delta + \eta_r + \varepsilon_{is}, \tag{4}$$

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

where $Discouraged_{is}$ is an indicator equal to one if respondent i in state s tried to apply but could not get through, tried but had their application rejected, or did not apply because the process was too difficult. Respondents who applied successfully are coded as zero. The coefficient of interest is β, which captures whether respondents in COBOL states were more likely to report a filing barrier than respondents in non-COBOL states.

I estimate a sequence of specifications. The baseline specification includes only the COBOL indicator. I then add pre-pandemic UI generosity, defined as the maximum weekly benefit amount multiplied by the maximum benefit duration, divided by 1,000. Subsequent specifications add individual characteristics for gender and age, state-level controls, and Census region fixed effects. The state-level controls are the 2016 Republican presidential vote share, the percentage of the population living in urban areas, the percentage of the population with at least a bachelor's degree, the income share in accommodation and food services, and the poverty rate. All regressions use the Google Survey weights and restrict the sample to observations with positive weights.

These regressions are intended to provide suggestive evidence on the discouraged-filing channel rather than causal estimates. Unlike the main consumption and delay analyses, the Google Survey is a cross section fielded during a short window in April 2020. As a result, the estimates do not exploit within-state changes over time or isolate quasi-random variation in filing burdens. Instead, they document whether respondents in COBOL states were more likely to report filing difficulties at the height of the initial UI claims surge.

5 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.[26]

5.1 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. (2023) only provide consumption data starting in January 2020, and I start the analysis period in February 2020 to exclude the index period.[27] 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,

[26]The DOLETA data on processing delays and UI outlays, presented in Appendix Section B, provide suggestive evidence on the administrative channel linking COBOL usage to consumption outcomes and can be interpreted as a quasi–first stage.

[27]I cannot extend the analysis earlier because the Economic Tracker data begin in January 2020, which serves as the index period and is therefore excluded. As additional context, in the Online Appendix, I use aggregated PCE data and show similar consumption patterns between COBOL and non-COBOL states over the pre-pandemic period from 2012 to 2019 (annual frequency).

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 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 estimate 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 2, I present results from Equation 1. In column 1, I estimate a baseline specification that includes only state and day fixed effects, capturing the effect of reduced UI administrative capacity in COBOL states on aggregate consumption relative to non-COBOL states. 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 and reduced administrative UI state capacity. In particular, I am concerned about a violation of the parallel trends assumption: while pre-trend differences appear minimal (both in the Economic Tracker and PCE), post-treatment dynamics could still bias the results.

As with any empirical exercise that employs a quasi-experimental design, there is a challenge for identifying causal results. To assess whether this difference in relative consumption between COBOL and non-COBOL states was driven by other state-level factors unrelated to UI administration, I explore a set of potential confounders. One key source of bias appears to be differences in attitudes to COVID-19. Although COBOL and non-COBOL states had similar COVID-19 health outcomes, as measured by death and case rates in Table 1, they differed in attitudes toward the pandemic. Specifically, households in COBOL states were relatively more cautious about COVID-19 than households in non-COBOL states.[28] This behavioral difference could upwardly bias the magnitude

[28] To determine that COBOL and non-COBOL states had different attitudes, I use Google mobility data and find that households in COBOL states spent relatively more time at home than households in non-COBOL states as shown in the Online Appendix.

of my point estimates in Table 2 column 1 because households in COBOL states would consume less after the emergency declaration for reasons unrelated to unemployment insurance.

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. The coefficient is positive and significant at the 1 percent level. This result is consistent with differences in attitudes to COVID-19 upwardly biasing the magnitude of my results in column 1. This interaction variable serves as a proxy for attitudes on COVID-19 and potentially differential policies implemented by governors implemented during this period. 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. The results stabilize after controlling for the interaction of Republican and Post.

Even though column 2 in Table 2 represents my preferred specification, concerns about omitted variable bias remains. One potential concern is that COVID-19 affected COBOL and non-COBOL states differently. In column 3, I add the daily new COVID-19 case rates and new death rates as additional controls. The coefficient on the interaction of COBOL and Post remains stable at a 2.8-percentage-point decline and statistically 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 1. To further assess robustness, column 4 includes five previously discussed confounders interacted with Post: (1) income share in accommodation and food services industry (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). The coefficient on the interaction of COBOL and Post only marginally decreases, dropping to a 2.6-percentage-point decline. Column 5 adds the monthly state unemployment rate as a control, which is positively but insignificantly associated with relative consumption. However, this variable is potentially endogenous: one mechanism through which COBOL states may experience lower consumption is precisely through delayed UI disbursements that depress measured unemployment and aggregate demand. As such, controlling for unemployment likely induces post-treatment bias, attenuating the estimated effect. The coefficient on the interaction of COBOL and Post falls to 2.4 percentage points but remains statistically significant at the 5% level, providing a conservative lower bound.

5.2 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 3, 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 3 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 distinguish whether the persistent consumption shortfall is driven by continued processing delays, delayed disbursement of approved benefits, or broader dynamic effects from earlier demand contractions. Discouraged filing may also have reduced benefit receipt, further dampening recovery. The results likely reflect a combination of these channels.

5.3 Aggregate Effects of Lack of UI Modernization

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

$$Cost = \frac{PI_{2019}}{PI_{2012}} \times Relative\_Decline \times \frac{Days}{365} \times \sum_{s=1}^{28} \sum_{c \in \mathcal{C}} PCE_{sc,2019} \tag{5}$$

where $\sum_{s=1}^{28} \sum_{c \in \mathcal{C}} PCE_{sc,2019}$ denotes total nominal personal consumption expenditures (PCE) in 2019 (in 2012 dollars) across 28 COBOL states, excluding five categories unlikely to be captured in card-based spending: (1) motor vehicles and parts, (2) housing and utilities, (3) health care, (4) financial services, and (5) nonprofit institution expenditures. These PCE categories are excluded from $\mathcal{C}$, and imply that credit and debit card consumption account for roughly 50% of PCE. To take a conservative measure, I take the period while Federal Pandemic Unemployment Compensation (FPUC) was in effect, roughly April 1, 2020 to July 31, 2020. That corresponds to 121 days or about a third of a year. To express PCE measured in 2012 dollars in 2019 dollars, I multiply by the price-index ratio, $\frac{PI_{2019}}{PI_{2012}}$. The estimated 2.8-percentage-point relative decline, $Relative\_Decline$, in aggregate consumption then translates into a real consumption decline of $40 billion in COBOL states relative to non-COBOL states; this figure corresponds to roughly 0.3% of real GDP in COBOL states. This estimate is conservative as I assume there was no consumption effect in the excluded PCE categories and I restrict to only consider the period that FPUC was in effect. However, this $40 billion estimate likely understates the true economic costs of administrative burdens in UI benefit systems, since non-COBOL states also experienced substantial administrative strain and benefit-processing delays after the emergency declaration. Thus, the estimate should be interpreted as the avoidable consumption loss associated with COBOL-related administrative constraints—i.e., the decline that could have been mitigated had COBOL states modernized their UI systems to the same extent as non-COBOL states, thereby reducing claimant burden and accelerating benefit delivery.

The cost that COBOL states would have incurred to modernize their systems is likely only a small fraction of the $40 billion relative decline in consumption 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.[29] 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 the economy.

5.4 COBOL-Induced Delays

One channel through which relatively larger administrative burdens and reduced administrative UI state capacity in COBOL states could be affecting aggregate consumption is through longer processing delays in UI disbursement. I examine this channel using the share of intrastate regular UI claims that were topcoded, meaning claims with processing delays of more than 70 days. These delays are unusually long: DOLETA does not separately track 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-month, as reflected in Figure 4. However, from July 2020 to October 2020, at least 20% of all intrastate claims were topcoded in both COBOL and non-COBOL states. Because the number of UI claimants increased sharply after the emergency declaration, measuring topcoded claims as a share may understate the overall severity of these delays.

Figure 4 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

[29] See the public contract.

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.

In Table 3, 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 implies that after March 2020, COBOL states experienced a 2.3-percentage-point larger increase in the share of topcoded claims relative to non-COBOL states.[30] This is economically meaningful given that, from June 2020 to December 2020, states delayed claims by more than 70 days for an average of 18.7 percent of claims.[31]

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 remains significant at the 1% level. The coefficient on the interaction of Republican and Post is statistically insignificant.

Despite topcoded claims 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 4. 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 four of the five previously discussed confounders interacted with Post in column 3 of Table 3: (1) income share in accommodation and food services industry (2019), (2) the percentage of the population living in urban areas (2010), (3) the percentage of the population living in poverty (2019), and (4) the percentage of the population

[30]Recall that March, April, and May 2020 are dropped from topcoding analysis.
[31]This 18.7 percent statistic is not population weighted.

with at least a bachelor's degree (2019). In column 3, once I add those four confounders interacted with Post, the coefficient on the interaction of COBOL and Post increases to 4.3 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 1. Another interpretation is that including these controls helps mitigate the non-classical measurement error introduced by topcoding, thereby yielding a larger estimated effect.

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 3. By including the unemployment rate, the coefficient on the interaction of COBOL and Post increases from 4.3 ppt. 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 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. Finally, in column 5, I add UI generosity as an additional control. The coefficient of interest decreases modestly, from 4.4 ppt. to 4.0 ppt., but remains statistically significant at the 1 percent level.[32] The covariates included in Table 3 do not meaningfully change the significance of the coefficient on the interaction of COBOL and Post, but they do meaningfully change the point estimates.

5.5 Discouraged Filers

There may be alternative channels through which consumption was suppressed in COBOL states relative to non-COBOL states that operate through unemployment insurance, but not necessarily through payment delays. The persistent pattern of relative consumption declines in Figure 3 suggests

[32] I include UI generosity separately because it is a baseline UI policy measure rather than a demographic or industry-share control.

that delayed UI disbursement may not be the only mechanism leading to lower relative consumption in COBOL states. One possibility is that filing a UI claim was more onerous in COBOL states, leading to a relatively higher share of discouraged filers.

Table 4 reports individual-level regressions of an indicator for being a discouraged filer on an indicator for residing in a COBOL state. The dependent variable equals one for respondents who tried to apply but could not get through, tried but had their application rejected, or did not apply because the process was too difficult. It equals zero for respondents who applied successfully. Column 1 shows that, without additional controls, respondents in COBOL states are about 4 percentage points more likely to be discouraged filers than respondents in non-COBOL states. Column 2 adds UI generosity, and the estimate remains similar, at about 6 percentage points. Columns 3, 4, and 5 add individual characteristics, state characteristics, and Census region fixed effects, respectively. These results provide suggestive evidence that larger relative consumption losses in COBOL states partly reflect barriers to accessing UI benefits, rather than only delayed payments among successful claimants. However, I do not interpret the estimates in Table 4 as causal. The analysis is cross-sectional, based on respondents surveyed in April 2020, and does not isolate exogenous variation in filing difficulty across states.

5.6 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 Program (PPP), Economic Impact Payment (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.[33] The data on the EIP is provided by the Internal Revenue Service's (IRS) Statistics of Income (SOI)

[33] The data can be accessed from the SBA website.

program.[34] The data for SNAP is provided by the U.S. Department of Agriculture (USDA).[35]

Panel A of Table 5 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 in Table 5, all the PPP outcomes have t-statistics that are statistically insignificant between COBOL and non-COBOL states.

Panel B of Table 5 shows 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.

Panel C of Table 5 shows 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. Specifically, COBOL states received $216.89 per capita in SNAP funds, while non-COBOL states received $219.03.

Overall, the analysis in Table 5 demonstrates that there were no systematic differences between COBOL and non-COBOL states in the disbursement of SNAP, EIP, and PPP benefits. In addition to considering whether other pandemic transfer programs drive my results, I also conduct several robustness checks in the Appendix and Online Appendix. Specifically, I conduct a randomization inference exercise in which I randomly assign COBOL status to 28 states, use a penalized synthetic control method as an alternative estimator, and consider a host of additional controls, such as whether a state had a Republican governor.[36]

[34]The data can be accessed from the IRS SOI website.

[35]The data can be accessed from the USDA website.

[36]Choi et al. (2024) conduct a balance of characteristics between COBOL and non-COBOL states and find only two significant characteristics: Republican governor and home ownership rates.

6 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' UI benefit 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 and reduced administrative UI state capacity, 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.[37] The large negative effect of UI system delays in COBOL states during the pandemic suggests that improving benefit delivery systems is essential for UI to function effectively as a countercyclical policy instrument.[38]

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.

Although lack of UI system modernization is a central problem, Lachowska et al. (2022) show that modernization alone is not necessarily sufficient to fix UI administrative issues and that federal funding to state workforce agencies should be tied to pay-for-performance schemes designed to achieve desired outcomes, such as shorter processing delays or reduced call center volumes. Some

[37] 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.

[38] 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 et al., 2020).

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, policymakers should recognize the central role of modernizing these antiquated UI systems.[39] Regardless of which approach policymakers choose to take, antiquated UI systems hamper the effectiveness of UI as a fiscal stabilizer.

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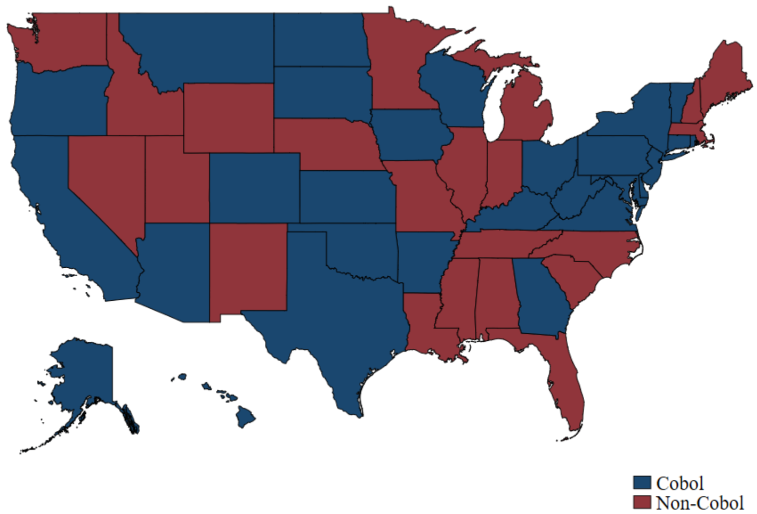
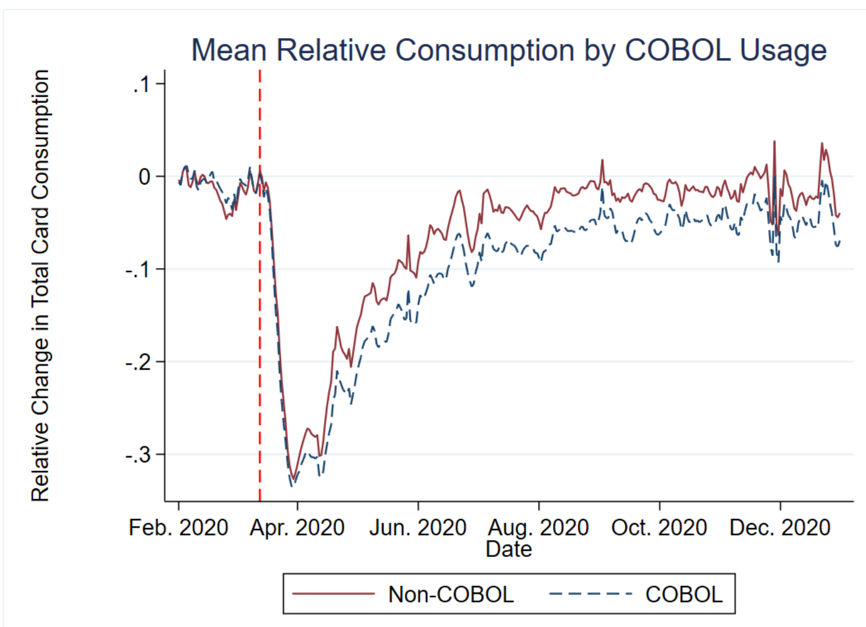
A Figures

Figure 1: Map of COBOL Status



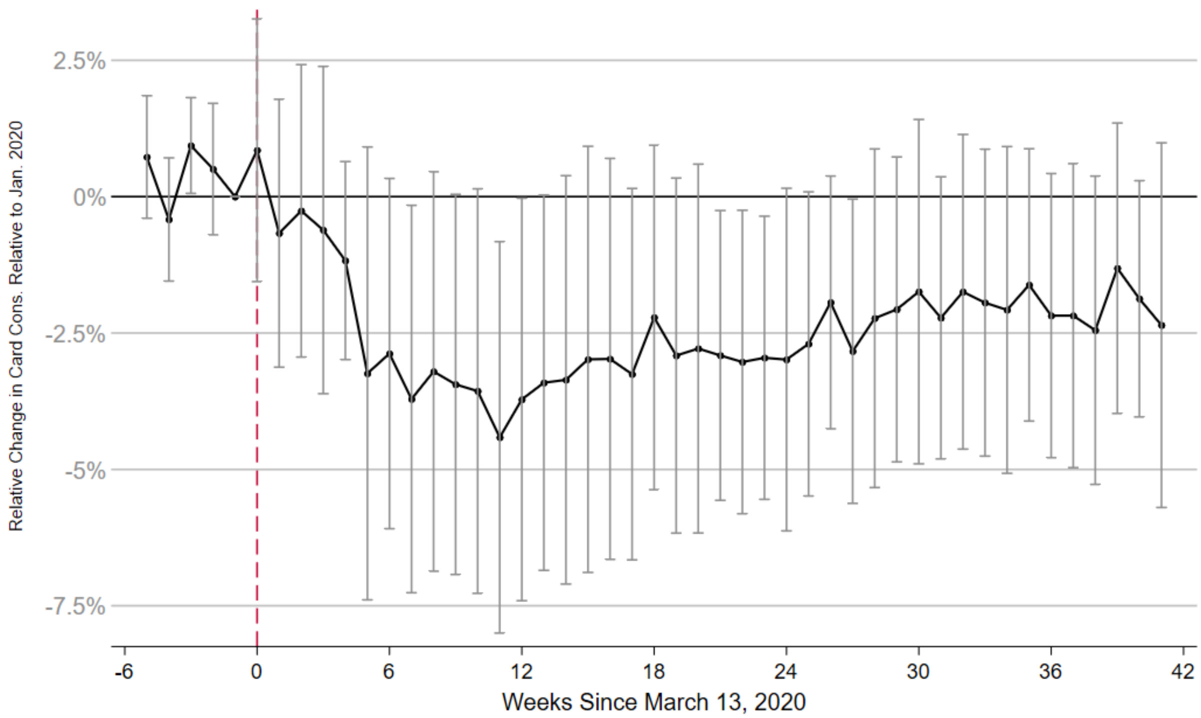
Note: The data on COBOL usage were collected by the author primarily from emails, news articles, and information provided by the UI Information Technology Support Center. Washington, DC, uses COBOL but it is excluded from the analysis due to a lack of consumption data.

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



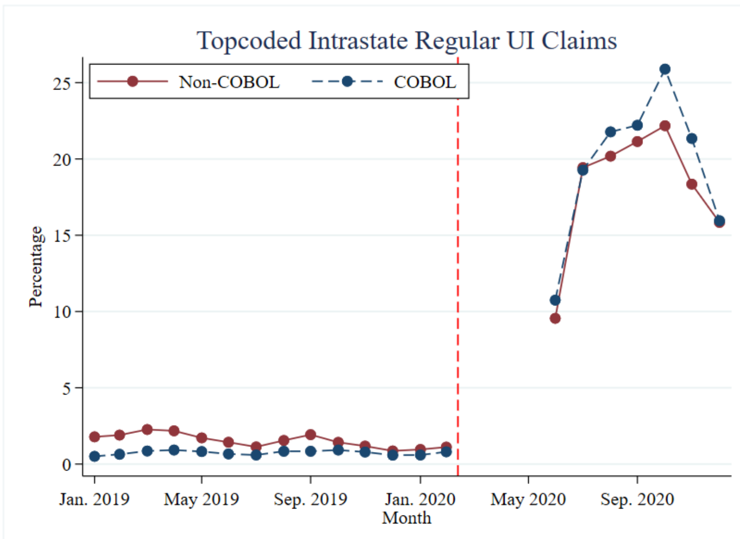
Note: This figure relies on data of all credit and debit card spending at a daily frequency for each state. These are relative consumption measures. A population-weighted average across states is computed when aggregating to COBOL and non-COBOL states. The dashed navy line corresponds to COBOL states. The solid maroon line corresponds to non-COBOL states. This figure covers the sample period from February 1, 2020, to December 31, 2020.

Figure 3: 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 4: 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.

B Tables

Table 1: 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)
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., 2023). 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., 2023). 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 2: TWFE COBOL Usage on All Card Consumption

	(1) Rel Cons	(2) Rel Cons	(3) Rel Cons	(4) Rel Cons	(5) 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) 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 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 3: TWFE Fraction of Claims Topcoded

	(1) FracIntraTop	(2) FracIntraTop	(3) FracIntraTop	(4) FracIntraTop	(5) FracIntraTop
COBOL $\times$ Post	2.29***	2.08***	4.30***	4.40***	3.97***
	[0.534]	[0.610]	[0.630]	[0.522]	[0.616]
Republican $\times$ Post		-0.05	-0.37***	-0.30***	-0.30***
		[0.063]	[0.090]	[0.083]	[0.083]
UR				0.86***	0.78***
				[0.218]	[0.223]
UI Generosity $\times$ Post					0.18*
State FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
State Char. x Post	No	No	Yes	Yes	Yes
Obs.	1,050	1,050	1,050	1,050	1,050
Depvar	0.88	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) the percentage of the population living in poverty (2019), and (4) the percentage of the population with at least a bachelor's degree (2019). Column 4 adds the unemployment rate. Column 5 adds UI generosity (Jan. 2020) interacted with Post as a control. 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 4: COBOL UI Systems and Discouraged Filing

	(1) Discouraged	(2) Discouraged	(3) Discouraged	(4) Discouraged	(5) Discouraged
COBOL	0.043*	0.059**	0.064**	0.056**	0.061**
	[0.025]	[0.027]	[0.027]	[0.026]	[0.027]
UI Generosity		-0.005	-0.005	-0.004	-0.002
		[0.003]	[0.003]	[0.003]	[0.004]
Individual Char.	No	No	Yes	Yes	Yes
State Char.	No	No	No	Yes	Yes
Region FE	No	No	No	No	Yes
Obs.	3,012	3,012	3,012	3,012	3,012
Depvar	0.42	0.42	0.42	0.42	0.42

Note: This table reports individual-level regressions of an indicator for being a discouraged unemployment insurance (UI) filer on an indicator for residing in a COBOL state. The sample is restricted to respondents who lost a job and either applied successfully, tried to apply, or did not apply because the process was too difficult or because their application was rejected. The dependent variable equals one for respondents who tried to apply but could not get through, tried but had their application rejected, or did not apply because it was too difficult, and equals zero for respondents who applied successfully. COBOL is an indicator for states using COBOL-based UI systems. UI Generosity is defined as the maximum weekly benefit amount multiplied by the maximum benefit duration, divided by 1,000. Column 1 includes no additional controls. Column 2 adds UI generosity. Column 3 adds individual controls for gender and age. Column 4 adds state-level controls for 2016 Republican vote share, urban population share, educational attainment, the income share in accommodation and food services, and the poverty rate. Column 5 adds Census region fixed effects. All regressions are weighted using the Google Survey weights and restrict the sample to observations with positive weights. Depvar reports the weighted mean of the dependent variable in the estimation sample. The survey information comes from the Google Survey analyzed in Zipperer and Gould (2020). Standard errors, reported in brackets, are clustered at the state level.

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

Table 5: Other Pandemic Transfer Programs Summary Statistics by COBOL Status

	Non-COBOL Mean	COBOL Mean	Diff	t-stat	Non-COBOL N	COBOL N
PPP						
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
EIP						
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
SNAP						
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 PPP, EIP, and SNAP by COBOL status. The first panel corresponds to PPP loan amounts, total loans, and jobs. These statistics are denominated as the total PPP amounts disbursed in 2020 once the program started in April 2020. The second panel corresponds to the first wave of EIP, which occurred in April 2020. The third panel corresponds to total SNAP payments disbursed from March 2020 to December 2020. All of these statistics are normalized by either the 2019 labor force or the 2019 population.

A 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 2, 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 use population weights. Figure A1 shows that the observed estimate is statistically significant at the 10 percent level relative to the permutation distribution. The other specifications in Table 2 are all significant at the 10 percent level.

B COBOL-Induced Discouraged Filers: DOLETA 5159

In Section 5.5, I use the Google Survey analyzed by Zipperer and Gould (2020) to provide direct survey evidence that respondents in COBOL states were more likely to report filing difficulties and becoming a discouraged filer. In this appendix section, I provide an additional, more indirect test of the discouraged-filing channel using DOLETA 5159 data on first UI payments. I cannot directly observe discouraged filers in DOLETA administrative data. Instead, I use first payments of regular UI benefits as a proxy for the number of individuals who successfully received UI benefits. First payments are preferable to initial claims because individuals may file multiple initial claims, whereas each UI recipient receives exactly one first payment during an unemployment spell. If potential claimants in COBOL states were more likely to be discouraged from filing or completing a claim, then the relative increase in first payments should be lower in COBOL states than in non-COBOL states after the emergency declaration. The dependent variable is "Relative First Payments (Ratio)," defined as the number of first UI payments in state (i) and month (t) divided by that state's average monthly number of first payments in 2019. This normalization accounts for persistent pre-pandemic differences across states in the size of their UI systems. I estimate the

TWFE specification in Equation 1, using the ratio of first payments as the dependent variable.[40] Figure A2 shows the large increase in first UI payments during the pandemic. In April 2020, first payments in non-COBOL states were about 36 times their 2019 monthly average, compared with about 32 times in COBOL states. By May 2020, the ratios were 21 and 14, respectively. If the gap reflected only longer processing delays in COBOL states, one might expect the difference to reverse as delayed claims were eventually paid. Instead, the gap remains persistent, consistent with some potential claimants in COBOL states never receiving benefits. Table A1 reports the corresponding TWFE estimates. Column 1 includes state and month fixed effects and shows that COBOL states experienced a 2.9-ratio-unit decline in relative first payments from March to September 2020. Column 2 adds the interaction of Republican vote share and Post, reducing the estimate to 2.4 ratio units. Column 3 adds the following confounders interacted with Post: (1) income share in accommodation and food services, (2) percentage of the population in urban areas, (3) percentage of the population living in poverty, and (4) percentage of the population with at least a bachelor's degree. Including these controls attenuates the coefficient of interest from -2.4 ratio units to -1.9 ratio units, but the results remain significant at the 5 percent level. Column 4 adds the unemployment rate as a control, which further attenuates the coefficient of interest to -1.7 ratio units. In column 5, I add UI generosity interacted with Post. This allows states with more generous pre-pandemic UI systems to experience different post-period changes in first payments. This control drives the coefficient of interest to -0.1 ratio units and the coefficient is statistically insignificant. These results are not robust, which is why I view these results as suggestive evidence of discouraged filers.

To translate the ratio estimates into an implied number of discouraged filers, I use the following calculation:

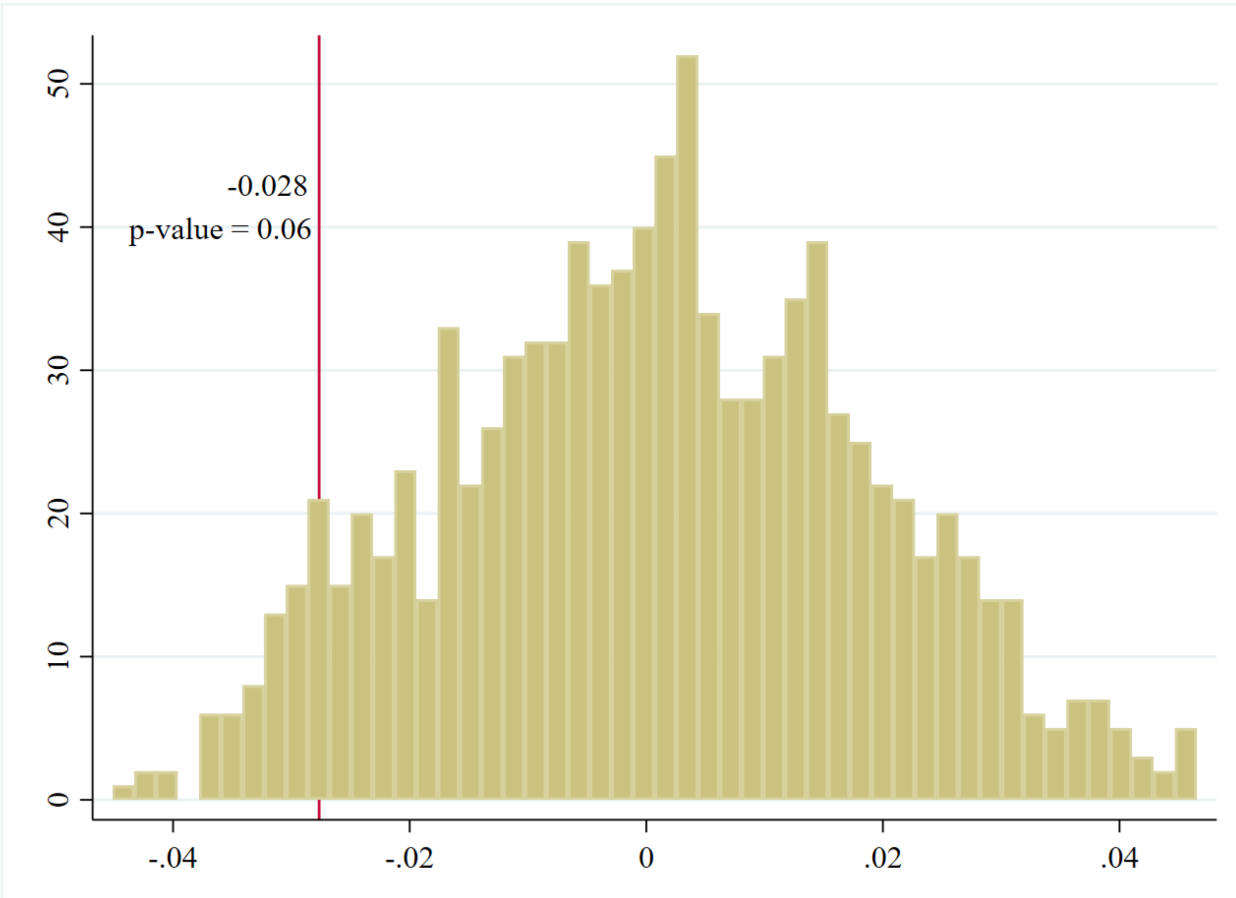
$$Discouraged\_Filers = ATE\_Ratio \times COBOL\_Payments_{2019} \times Months \times COBOL\_States. \tag{6}$$

ATE_Ratio is the coefficient on the interaction term in Equation 6. $COBOL\_Payments_{2019}$ is the average monthly number of first payments in COBOL states in 2019, equal to 9,900. $Months$ is the number of post-period months, equal to 7, and $COBOL\_States$ is the number of COBOL states

[40]In analyses involving DOLETA data, I exclude Montana, a COBOL state, and Minnesota, a non-COBOL state, because of unreliable data. These exclusions reduce the number of COBOL states from 28 to 27. A DOLETA contact confirmed that these two states provided unreliable data.

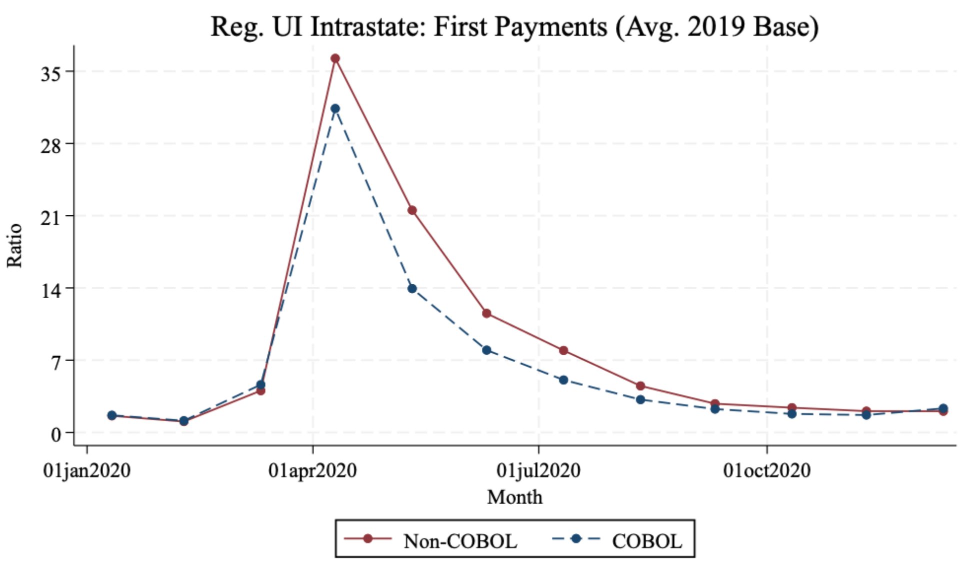
in the DOLETA sample, equal to 27. Using the 2.4-ratio-unit estimate from column 2 implies a shortfall of approximately 4.5 million first payments in COBOL states. If interpreted as discouraged filing, this corresponds to approximately 4.5 million discouraged filers. I interpret these results as suggestive rather than causal. The first-payment measure combines discouraged filing with other factors that affect benefit receipt, including differences in baseline UI generosity, eligibility rules, and application behavior. The estimates are also not robust to controlling for UI generosity. Nevertheless, the persistent shortfall in first payments in COBOL states is consistent with the Google Survey evidence in Section 5.5: administrative burdens in COBOL states appear to have affected UI access not only by delaying payments, but also by discouraging some claimants from successfully receiving benefits. Overall, the DOLETA evidence is best interpreted as complementary and suggestive: COBOL states experienced a persistent shortfall in first UI payments after the emergency declaration, but this shortfall is sensitive to controls for UI generosity and therefore should not be interpreted as definitive causal evidence that COBOL systems alone discouraged filing.

Figure A1: 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 A2: Flow of UI Payments (Discouraged Filers)



Note: The data on first-time regular intrastate UI payments come from the Department of Labor Employment and Training Administration. The data are monthly and at the state level. The sample period is January 2020 to December 2020. Values are normalized to be in terms of ratios relative to the average number of first-time regular intrastate UI payments from 2019 (before aggregation). When aggregating across states to create COBOL and non-COBOL samples, 2019 population weights are employed. Given that each person receiving UI benefits only receives one first UI payment, this figure can be interpreted as the flow of unemployed individuals into the UI system.

Table A1: TWFE Relative First Payments (Discouraged Filers)

	(1) FirstPayRatio	(2) FirstPayRatio	(3) FirstPayRatio	(4) FirstPayRatio	(5) FirstPayRatio
COBOL × Post	-2.9**	-2.4**	-1.9**	-1.7**	-0.1
	[1.02]	[0.86]	[0.82]	[0.83]	[0.66]
Republican × Post		0.1*	0.2***	0.2***	0.2***
		[0.05]	[0.07]	[0.07]	[0.08]
UR				0.2	0.4
				[0.28]	[0.32]
UI Generosity × Post					-0.6***
					[0.21]
State FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
State Char. × Post	No	No	Yes	Yes	Yes
Observations	1,008	1,008	1,008	1,008	1,008
Implied shortfall (mn)	5.4	4.5	3.6	3.2	0.2

Note: This table relies on UI payment-activity data from the Department of Labor Employment and Training Administration's 5159 reports and is at the state-month level. The dependent variable is the ratio of monthly regular UI first payments to each state's average monthly first payments in 2019. 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) the percentage of the population living in poverty (2019), and (4) the percentage of the population with at least a bachelor's degree (2019). Column 4 adds the unemployment rate. Column 5 adds one confounder interacted with post: UI generosity. UI generosity is the product of the maximum UI benefit and maximum UI duration in January 2020. The sample starts in January 2019 and ends in September 2020, with the post-period starting in March 2020. The average number of first payments in COBOL states in 2019 was about 9,900. Montana (a COBOL state) and Minnesota (a non-COBOL state) are excluded from this analysis due to unreliable data. The standard errors are clustered at the month level.

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