The Effect of Unemployment Benefit Supplements:

Evidence from the Lost Wages Assistance Program

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September 15, 2025

Abstract

This article studies the effects of large, temporary unemployment insurance supplements. Using administrative data from California, I implement a regression discontinuity design around an eligibility threshold for the Lost Wages Assistance Program, a 2020 program providing 6 weeks of $300/week supplements to claimants with weekly benefit amounts over $100. The supplements had a minimal effect on labor supply, increasing unemployment durations by just 1.5 weeks. This response was mostly driven by liquidity effects rather than distorting incentives, suggesting the supplements were welfare-enhancing. The supplements did not affect job quality and had a limited effect on the state's unemployment rate.

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I thank Till von Wachter, Yotam Shem-Tov, Daniel Haanwinckel, Geoff Schnorr, Sree Kancherla, and Jesse Rothstein for helpful comments, as well as UCLA Proseminar and SOCAE participants. I gratefully acknowledge the California Employment Development Department's Labor Market Information Division and the California Policy Lab for their partnership in producing this analysis. I am also grateful for financial support from the California Policy Lab while producing this analysis. I benefited from facilities and resources provided by the California Center for Population Research at UCLA (CCPR), which receives core support (P2C-HD041022) from the Eunice Kennedy Shriver National Institute of Child Health and Human Development (NICHD). The content is solely the responsibility of the author and does not necessarily represent the official views of the Eunice Kennedy Shriver National Institute of Child Health & Human Development or the National Institutes of Health. Any views expressed here are solely those of the author and not of the California Employment Development Department or funding agencies.

1 Introduction

What are the effects of large, temporary unemployment insurance (UI) benefit supplements? During the COVID-19 pandemic, the U.S. government spent over 400 billion dollars on such supplement programs (Pandemic Oversight Response, 2023). These supplements were so generous that many workers earned more on UI than they did at their previous job, prompting a heated debate among economists and policymakers about the supplements' disincentive effects.

On one hand, prior research has shown that permanent increases in weekly benefit amounts tend to increase unemployment duration (Card et al. (2015); Landais (2015); Bell et al. (2024)). On the other hand, these benefit supplements were explicitly *temporary*, and provided in the context of exceptionally high unemployment. Thus, even if one could make more money from UI benefits than they would in a single week at work, a forward-looking claimant may realize that those supplements wouldn't last forever, and still find it prudent to return to work quickly.

Understanding how UI supplements affect the labor market is crucial for policymakers tasked with crafting economic policy during recessions. If benefit supplements have large distortionary effects, providing relief through other means may be preferable. However, if the distortionary effects are minimal, the UI program's inherent targeting of displaced workers could make temporary supplements an attractive option for delivering relief.

Despite the importance of understanding these supplement programs, for several reasons it has proven challenging for researchers to estimate their causal effects. First, administrative data on UI claimants is rarely made available for researchers. While some studies use survey data, research shows surveys such as the CPS can dramatically understate both the number of UI recipients and the level of benefits (Larrimore, Mortenson and Splinter (2023)). Another major challenge is identification. While benefit extension programs often have a staggered turn-off which can be exploited for identifying variation (Farber, Rothstein and Valletta (2015); Coombs et al. (2022)), UI supplements are typically provided to *all* UI claimants, regardless of state. Thus, there is rarely a control group with which to compare treated claimants.

This paper overcomes both the data and identification challenges by using administrative UI data from California to study the Lost Wages Assistance (LWA) Program, a temporary supplement program enacted during the summer of 2020. LWA provided some UI claimants with $300 weekly supplements for 6 calendar weeks. It was also the first UI supplement program in the history of the United States to include an eligibility restriction, so that not all UI claimants received them. This restriction – only claimants with a weekly benefit amount of $100 or more were eligible – provides a setting ripe for a regression discontinuity design, which I use to estimate the causal effects of the supplements. I show that the relationship between claimants' average unemployment durations and weekly benefit amounts (WBAs) evolves smoothly as the WBA approaches $100, but unemployment durations jump discontinuously at the eligibility threshold. By measuring the size of this jump, I am able to obtain an estimate of the causal effect of the supplements on labor supply.

I offer three sets of results. First, I show that the supplements had only a small effect on unemployment durations, increasing the average time on UI (past the start of the LWA program) from 21.9 weeks to 23.4 weeks for claimants near the eligibility threshold. After converting this raw effect into an elasticity, I show that the behavioral response to LWA supplements was much lower than the responsiveness seen in other pre-pandemic contexts, and slightly lower than what has been estimated for other pandemic-era supplements. I then highlight heterogeneous responses between demographic groups - with the largest effects seen among older and more-educated workers. I do a back-of-the-envelope calculation to show that the $300 LWA supplements were not a key driver of aggregate unemployment, only increasing the state's unemployment rate from 8.2% to 8.8% during the peak effect.

Second, I show that the majority of the increase in unemployment duration was driven by the supplements relaxing liquidity constraints, as opposed to the benefits distorting the net return to finding work. The unique timing of the LWA program allows me to disentangle these "liquidity" and "moral hazard" effects. Crucially, LWA supplements were only applicable to 6 calendar weeks of unemployment (July 26th-September 5th), yet supplements were not distributed until after the 6 weeks ended. This allows me to estimate a lower bound on the liquidity effect by assuming that the liquidity effect only affects job finding rates after September 5th. In essence, I assume that the entire effect on behavior during the weeks in which the supplements can be applied to is due to moral hazard (i.e., changing the relative return to working vs remaining unemployed in a given week), and the effect on behavior *after* these 6 weeks is due to the liquidity these supplements provide. I demonstrate there are substantial behavioral effects for LWA-eligible claimants *after* the 6 weeks end - in fact this liquidity effect drives a larger share of the change in total UI duration than the moral hazard effect – which has key implications for the welfare effects of the LWA supplements.

Third, I show that the increase in unemployment duration was not accompanied by an increase in job quality. Given that claimants saw an increase in liquidity, which allowed them to slightly prolong their unemployment spell, one might expect that claimants used this additional time in unemployment to search for a higher-quality job. However, I find precisely estimated null effects on multiple measures of job quality.

My paper makes three key contributions to the literature. First, I provide well-identified causal estimates of the effect of large, temporary UI supplements on labor supply. While prior research has estimated the labor supply effects of UI benefit extensions and permanent changes to weekly benefit amounts in various settings,[1] much less is known about how temporary supplements affect labor supply, despite hundreds of billions of federal dollars being spent on these programs throughout the pandemic (Pandemic Oversight Response, 2023). These pandemic-era supplements were among the most expensive labor-market policy experiments in U.S. history, yet until now, it has been difficult to measure their effects on workers.

The distinction between the temporary supplements that I study and the permanent changes to benefit amounts more often studied in the literature is economically meaningful: the "path" of weekly benefit amounts alters the incentive structure in a way that forward-looking, potentially liquidity-constrained claimants might respond to. Permanent benefit changes shift the return to continued unemployment throughout the entirety of the spell, whereas temporary supplements—especially when brief and very large—create concentrated, short-term incentives,

[1]Research on benefit extensions: Chodorow-Reich, Coglianese and Karabarbounis (2019); Farber and Valletta (2015); Farber, Rothstein and Valletta (2015); Johnston and Mas (2018); Schmieder, Von Wachter and Bender (2012); Schmieder, von Wachter and Bender (2016)
Research on benefit levels: Card et al. (2015); Jäger et al. (2020); Bell et al. (2024)

and may provide a sharp increase in liquidity to recipients.[2] As Shimer and Werning (2008) show, when workers face liquidity constraints, the timing of benefits can significantly affect job search behavior, even if total benefit amounts are unchanged. Lindner and Reizer (2020) demonstrate empirically that front-loading benefits in Hungary reduced nonemployment duration and increased reemployment wages—despite no change in total generosity. While my setting differs in policy design and institutional context (the supplements here do increase total generosity), my results reinforce the insight that the *structure* of UI policies can have substantial effects on claimants' behavioral response.

I am not the first to study the effects of UI supplements. However, because UI supplements were rarely used in the U.S. before the pandemic, most prior research (Ganong et al., 2024; Marinescu, Skandalis and Zhao, 2021) has focused on the Federal Pandemic Unemployment Compensation (FPUC) program, which provided \$600 supplements to *all* UI claimants between March and July 2020. Because these FPUC supplements were available to all UI claimants (thus making it difficult to find a plausible control group), researchers have been forced to rely on fairly strong identifying assumptions to estimate their causal effects.

In contrast to the FPUC supplements, the LWA program which immediately followed it had a strict eligibility restriction, which allows me to implement a regression discontinuity design, requiring a relatively simple and transparent continuity assumption for causal estimates. Furthermore, while Ganong et al. (2024) include both regular UI claimants and Pandemic Unemployment Assistance (PUA) claimants throughout their analysis of FPUC supplements, the administrative UI data that I use allows me to identify and focus my analysis on "regular" UI claimants. PUA claimants are workers who were ineligible for regular UI – either because they did not work in a UI-eligible job (e.g., independent contractors and self-employed workers), or because they were unable to work as a result of the COVID-19 pandemic (i.e., because they were caring for a family member), but were nevertheless provided benefits through a temporary federal program. To the extent that PUA claimants' response to supplements was different than that of regular UI claimants (due to the fact that their relationship with the labor market is fundamentally different), my results may be more generalizable to contexts outside of the pandemic.

My second contribution is to provide insight into the welfare implications of temporary UI supplements. After estimating the total effect of LWA supplements on unemployment durations, I decompose this effect into two parts in the style of Chetty (2008) - a moral hazard effect and a liquidity effect. The moral hazard effect represents the duration response to the price distortion of UI supplements – which effectively subsidize unemployment while in effect. The liquidity effect represents the change in duration resulting from the supplements increasing a worker's cash-on-hand, which, in the presence of liquidity constraints, allows them to maintain a higher level of consumption while unemployed. Chetty (2008) shows that the relative size of these two effects is a sufficient statistic for understanding the welfare implications of UI. A higher

[2] Because of this distinction, the findings of Bell et al. (2024) – who use a regression kink design with the same administrative data as this paper to document dampened responses to weekly benefit amounts during the pandemic – may not translate to settings where supplements are explicitly short-lived. Beyond studying an inherently different policy (permanent WBA changes versus temporary supplements), this paper differs from Bell et al. (2024) by identifying effects relevant for claimants near the \$100 WBA threshold —a substantially lower-earning group than the claimants near the benefit cap leveraged there.

liquidity effect implies that that claimants are far from the "full insurance" benchmark, and supplements are a welfare-improving correction of a market failure. Conversely, a relatively higher moral hazard effect implies that distorted incentives are driving the change in behavior, and supplements are welfare-decreasing.

Prior work (Card, Chetty and Weber, 2007; Chetty, 2008; Landais, 2015) has estimated the relative size of these effects by comparing estimates from two separate quasi-experiments involving different samples.[3] One estimate focuses solely on the liquidity effect (e.g., the duration response to severance payments), while the other includes both the moral hazard and liquidity effects (e.g., the effect of state-level changes to UI benefit levels). The difference in these effects allows them to back out the moral hazard effect, which can then be compared to the size of the liquidity effect. In contrast to combining estimates from two separate policies and populations, I provide a simple method to separate the liquidity and moral hazard effects for the same policy change, by leveraging the fact that the supplements I study were temporary and payments were not distributed until after the program had ended (and the moral hazard effect was no longer in effect). I show that the liquidity effects of LWA supplements were relatively large, implying the program was welfare-increasing for workers near the eligibility threshold. Understanding the labor supply responses and welfare implications of LWA supplements allows policy-makers in the next recession to better weigh the trade-offs between providing economic relief through temporary UI supplements and other relief programs.

Thirdly, I contribute to the literature on UI benefits and job quality. Previous work on the job-quality effects of unemployment benefits has focused on how benefit extension programs, permanent benefit increases, or, in the case of Card, Chetty and Weber (2007), severance payments, affect future earnings, match quality, and/or employment duration. The job-quality literature has thus far found conflicting results.[4] To my knowledge, this paper is the first to estimate the effect of temporary UI supplements (which include both moral hazard and liquidity effects, unlike the severance payments studied by Card, Chetty and Weber (2007) and Chetty (2008)) on job quality. I find that these supplements had no measurable effect on long-run earnings, firm size, job tenure, or the probability the claimant found work in a new industry.

My results should be interpreted with two contextual factors in mind. First, the pandemic may have altered the economic and behavioral environment in ways that affect the external validity of the estimates. Elevated health risks from in-person work, widespread closures of schools and care facilities, and greater labor market uncertainty may have shaped job search behavior in ways that differ from other downturns. Second, the RD design identifies a local

[3]Others have studied liquidity effects by taking a structural approach, where each of the model primitives are estimated, and then the model is numerically simulated under different policy frameworks, as in Wolpin (1987). Ganong et al. (2024) use the fact that they can observe bank account balances, and implement a triple difference design (comparing high and low-liquidity workers), finding suggestive evidence that the effect of FPUC supplements on job finding does not appear to be driven by liquidity - a result which aligns with their structural model.

[4]Nekoei and Weber (2017) find that extensions increase wages in Austria, while Schmieder et al. (2016) find the opposite in Germany, and Van Ours and Vodopivec (2008) find no effect on wages in Slovenia. They also estimate a null effect on the duration of the next job, a proxy for job quality. In America, Centeno et al. (2004) provide some evidence that UI extensions lead to longer durations at the next job, while Farooq, Kugler and Muratori (2020) find benefit extensions lead to increased earnings, higher match quality, and longer spells of employment. To my knowledge, Jager et al. (2020) is the only paper to estimate the effect of benefit *levels* (permanent changes to the weekly benefit amount) on wages, and finds no measurable effect.

effect for a specific group of UI claimants—those near the $100 WBA threshold—who tend to have low prior earnings and relatively weak labor force attachment.

Taken together, these findings offer substantial contributions to our understanding of how temporary UI supplements affect the labor market trajectories of displaced workers. The policy setting provides a unique opportunity to estimate causal effects using a transparent and internally valid research design. UI supplements have only been offered twice in U.S. history, and in both prior instances—including the FPUC supplements directly preceding this program—they were extended universally, precluding the most credible identification strategies. As a result, there is remarkably little direct evidence on how UI supplements affect labor supply in *any* context. This paper provides new evidence on that question - with important implications for policymakers tasked with supporting displaced workers during a recession. I show that large, temporary UI supplements appear to be an effective way to provide targeted relief to displaced workers while minimizing the economic distortion that is typically seen in policies that increase the generosity of the UI system.

2 Institutions, Data, and Methods

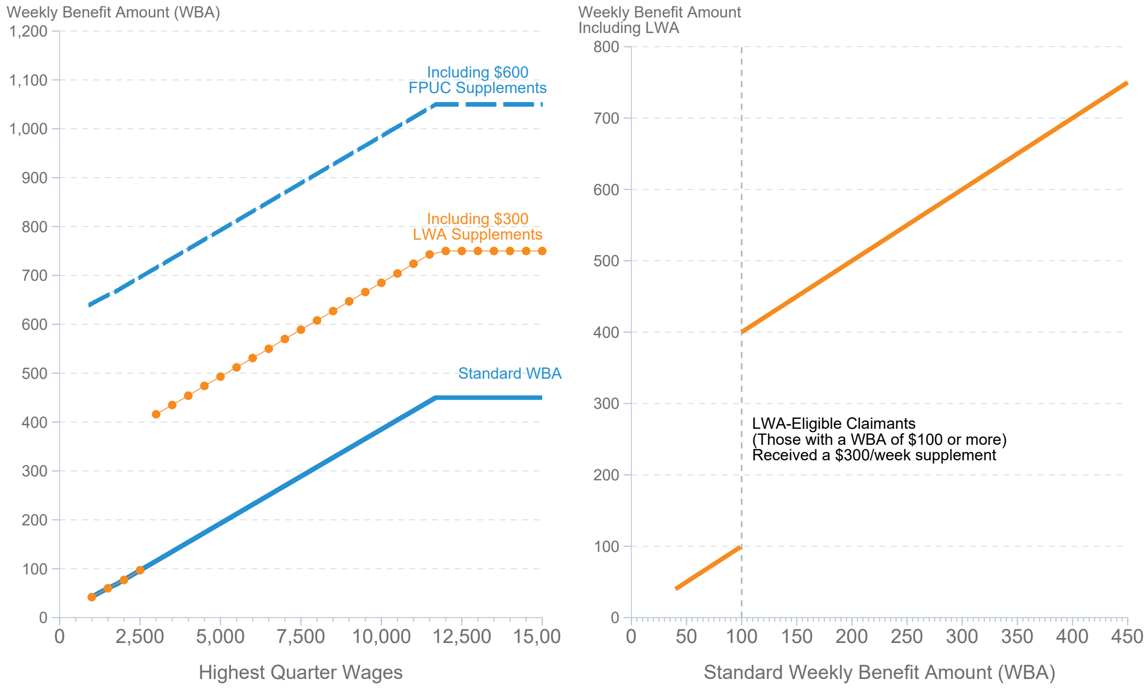
2.1 Institutions

The Unemployment Insurance Program in California

In California, a displaced worker who is eligible for unemployment insurance must file an initial claim in order to receive benefits. After filing an initial claim, they are informed of their weekly benefit amount (WBA), which is based on a claimant's prior earnings. In most cases, the Weekly Benefit Amount is based on the highest earning quarter out of the first four of the last five completed calendar quarters prior to their UI claim.[5] The left panel of Figure (1) illustrates the relationship between a claimant's Highest Quarter Wages (HQW) and their WBA. In general, a claimant's WBA is equivalent to approximately 50% of their average weekly earnings, up to a maximum WBA of $450 (dark blue line). Most claimants are eligible to receive 26 weeks' worth of UI payments, though a small number of claimants with low earnings histories have a lower maximum benefit amount, and during the COVID-19 pandemic, multiple extension programs were put into effect.

[5] If a claimant does not have sufficient wages in the Standard Base Period to establish a claim, the EDD automatically considers whether they have enough wages to file a claim using the *Alternate Base Period* — the last four completed calendar quarters prior to the beginning date of the claim.

Figure 1: Weekly UI Benefit Schedules



2.1.1 Pandemic-Era Changes to the UI System

In March of 2020, the COVID-19 pandemic shocked the U.S. labor market. Claims for unemployment insurance spiked to record levels, and as a result, Congress passed the CARES act at the end of March. The CARES act made 3 dramatic changes to America's UI system. First, it created the Pandemic Unemployment Assistance (PUA) program, which expanded the UI system to cover a variety of workers affected by the pandemic who would otherwise be ineligible for regular UI benefits. Second, it extended the potential benefit duration (PBD) of UI benefits, allowing claimants to receive an additional 13 weeks' of benefits (and eventually much more than that).[6] Third, it created the Federal Pandemic Unemployment Compensation (FPUC) supplements, which provided an additional $600 of supplements for weeks of unemployment between March 29 and July 25, 2020 (blue dashed line in left panel of Figure 1). These supplements were later renewed (at a lower rate of $300/week) for weeks of unemployment between December 27, 2020-September 5th, 2021.

As the first expiration of FPUC benefits approached in July of 2020, the fate of the supplements was unclear. After a contentious debate, Congress abstained from renewing the supplements, and they officially expired on July 25th. Nevertheless, two weeks later, and without Congressional consent, (and essentially no warning), President Trump signed an executive order creating the "Lost Wages Assistance" (LWA) program. The LWA program provided $300 UI supplements for 6 calendar weeks, applied to unemployment from July 26th to September 5th,

[6] These Pandemic Extended Unemployment Compensation (PEUC) extensions were renewed/extended multiple times: adding an additional 11 months starting in December 2020, then an additional 24 weeks beginning in March 2021. https://edd.ca.gov/en/about_edd/coronavirus-2019/cares-act/

2020 (Orange lines in Figure (1)).[7]

2.1.2 Policy Setting: The Lost Wages Assistance Program

The LWA program was unique in two ways. First, there was a sharp eligibility threshold, which allows me to implement a regression discontinuity design. Unlike all other UI supplement programs in the United States, the supplements were not available to all UI claimants – only those with a weekly benefit amount of $100 or more were eligible. The placement of this discontinuity in the earnings distribution is such that the treatment effect my RD design identifies is relevant for workers with particularly low levels of earnings and/or weak labor force attachment. I provide more detail on these affected workers in Section (2.2.3).

Second, the program was implemented in such a way that the timing of the supplements is uniquely suited for an analysis of liquidity effects. First, as FPUC supplements had already expired, and Congress had recessed for the summer, the supplements were not preceded by a policy announcement, so it was unanticipated by claimants (which I demonstrate empirically with a lack of pre-trends in Figure (3)). Second, the policy was very short-lived. LWA lasted for just 6 weeks, whereas the first round of FPUC supplements were available for 17 weeks. This short timing meant that supplements were not actually distributed until after the entire period in which the supplements were applied to had ended.[8] As I discuss in section (4.2), this allows me to separate the liquidity effects (which occur once the payments are disbursed) from the moral hazard effects (which are only relevant during those 6 weeks). Figure (2) provides a timeline of key events regarding the LWA supplements and pandemic-era UI programs more broadly.

In order to receive LWA supplements, a claimant must have had a weekly benefit amount of $100 or more, and be unemployed as a result of the COVID-19 pandemic. California Employment Development Department, the agency tasked with administration of the UI program, sent a certification form to all claimants filing for benefits at that time, which asked them to certify that they "were unemployed or partially unemployed due to disruptions by COVID-19."[9] I show in Appendix B that for claimants with a WBA over the $100 threshold, take-up was effectively universal.

2.2 Data

2.2.1 Raw Data

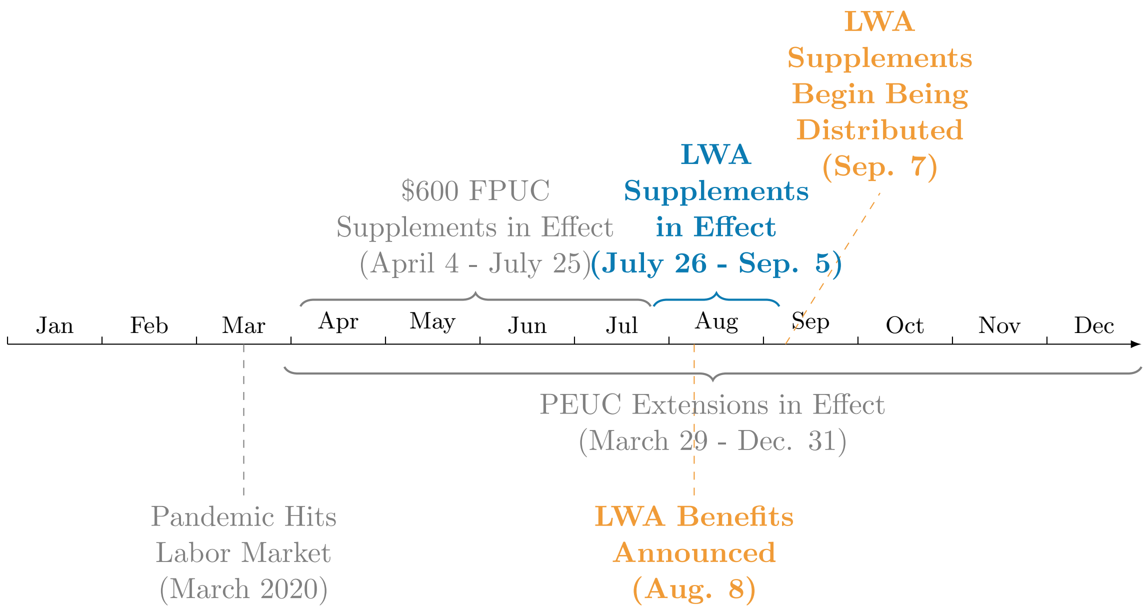
I combine three administrative datasets maintained by California's Employment Development Department (EDD) They include the state's Quarterly Earnings records (1995-2021), the Quarterly Census of Employment and Wages (QCEW, 2000-2021), and UI claims microdata

[7]Throughout the pandemic, these supplement programs were controversial among economists and the public more broadly. A survey of leading economists in 2021 asked if the $300 FPUC supplements constituted a "major disincentive to work for lower wage workers" found 28% agreed, 49% were uncertain, and 16% disagreed (7% did not answer). When asked if those same supplements would lead to meaningful gains in re-employment wages, there was similar levels of disagreement/uncertainty. https://www.kentclarkcenter.org/surveys/unemployment-benefits

[8]I provide additional details on the timing of the payments in Appendix section C.

[9]Appendix Figure A5 shows an example of the LWA Self-certification form in California.

Figure 2: Timeline of COVID-19 UI Policies, 2020



(2000-2021). These raw datasets are the same as those used in Bell et al. (2024), as well as a series of policy briefs on UI in CA during the pandemic (Bell et al., 2020*a*).

The UI claims data allows me to observe the start date of a claim (the "benefit year begin" date (BYB)), as well as the dates corresponding to when each UI payment was processed, and which week of unemployment every certification corresponds to. This allows me to separately observe 2 key dates associated with each UI payment and "continued claim" certification: when the payment was certified, and which week of unemployment the certification corresponds to. In normal times, this distinction is largely trivial, as each certification typically corresponds to unemployment experienced in the 2 preceding weeks. However, during the pandemic, there were widespread delays in claim processing, leading to a large number of retroactive payments (Bell et al., 2020*a*). This administrative data allows me to distinguish if a gap in payments is due to a claimant exiting the UI system, or if their claims were simply processed later. Following Bell et al., 2023), I define a UI exit date as the first of two consecutive weeks where a claimant does not have a corresponding UI payment. I define unemployment duration as the number of weeks between the start of a claim (the first paid week of unemployment) and the exit week.[10]

The UI claims data also provides claimant demographics (date of birth, gender, and self-reported race/ethnicity), which I use to analyze heterogeneity in section 4.1.3.

The quarterly earnings records include total UI-covered earnings in the relevant quarter for each employer-employee pair (employers are observed at the firm level). I use this data to construct quarterly earnings and employment measures, which are used as outcome variables. Each UI claim is linked to the relevant base period quarterly earnings amounts in order to calculate the "Highest Quarter Wage" (HQW), which is the determinant of the Weekly Benefit Amount (WBA), the running variable of my RD design, and will determine if a claimant is

[10] In normal times, there is a mandatory waiting week between the date the initial claim is filed and the first paid week of unemployment. During the COVID-19 pandemic, this waiting week was waived. https://edd.ca.gov/en/about_edd/disaster_related_services

eligible for LWA supplements. The QCEW data contains earnings, employment, and industry information at the establishment-quarter level, which is aggregated to the firm level before linking to the earnings data.

2.2.2 Sample Restrictions

I construct my sample with the aim of estimating the causal effect of LWA supplements for regular UI claimants. As such, I restrict my analysis sample to exclude all claims for the Pandemic Unemployment Assistance (PUA) program as well as claims for other specialized UI programs, such as Disaster Unemployment Assistance (DUA).[11] My primary analysis sample also restricts the sample to only include claimants whose claim started after March 15 (so as not to include claimants who were laid off prior to the pandemic), but before July 26th (the effective start-date of the supplement program, so as not to include claimants who filed claims after learning about the supplement program). I also restrict the sample to exclude any claimants who exited the UI program prior start of the LWA program (and then returned to it after the program was announced), so as not to include any workers who may have re-opened their claim in response to the supplements. As done in Bell et al. (2024), I drop claimants who had a prior UI claim within 2 years of the claim in question, or had HQW values within \$1 of a \$1,000 multiple (e.g. \$999 < HQW < \$1,001; \$1,999 < HQW < \$2,001; etc.) This HQW restriction is made because substantial "heaping" is known to occur at these values, which can induce bias in related research design (Barreca, Lindo and Waddell, 2016).

2.2.3 Descriptive Statistics

Table (1) shows summary statistics for my sample. Column 1 shows the demographic and economic information for my primary analysis sample, focusing on the 91,640 claimants with a WBA within \$10 of the LWA eligibility threshold. Column 2 removes the WBA restriction to include all claimants in the sample period. Comparing the two, we see that on average, claimants near the LWA eligibility threshold are younger, more likely to be female, less likely to be college educated and slightly more likely to identify as Black. These claimants earned less than \$7,000 in the year preceding the quarter of their UI claim, and had at least 1 quarter with no reported earnings, implying a weak labor force attachment. Columns 3 and 4 repeat columns 1 and 2, but use data from 2019, prior to the pandemic. Columns 5 and 6 use claims filed during the Great Recession (2009-2012).

Looking at the full distribution of claimants (All WBAs), we see that pandemic claimants were younger, less likely to have a college degree, and more likely to be Black or Asian (and less likely to be Hispanic) than claimants in 2019 or the Great Recession. Pandemic claimants earned substantially less on average than 2019 claimants, and worked in different types of jobs. Appendix Table (A1) highlights the industry composition of claimants. Nearly 1 in 5 claimants in my sample worked in the Accommodation and Food Services sector, with another 15% coming from Retail Trade – two industries especially affected by the Pandemic and government shutdowns. In 2019, the share of claimants from the Accommodation and Food Services sector

[11] During the week ending July 25th, 37% of all claimants receiving benefits were PUA claimants (Bell et al., 2020*b*).

was less than half as large, highlighting the distinct industry composition of the pandemic-era unemployed. These sharp shifts are likely driven by the unique nature of the pandemic, which disproportionately impacted in-person service jobs. The observed differences in labor force attachment (relative to the Great Recession claimants) may also be influenced by the FPUC supplement program substantially increasing the economic incentive to take-up UI early in the pandemic. Indeed, Bell et al. (2021*a*) document a dramatic rise in UI recipiency rates throughout 2020, even when excluding PUA claimants.

2.2.4 Descriptive Patterns in Exit Rates During the Pandemic

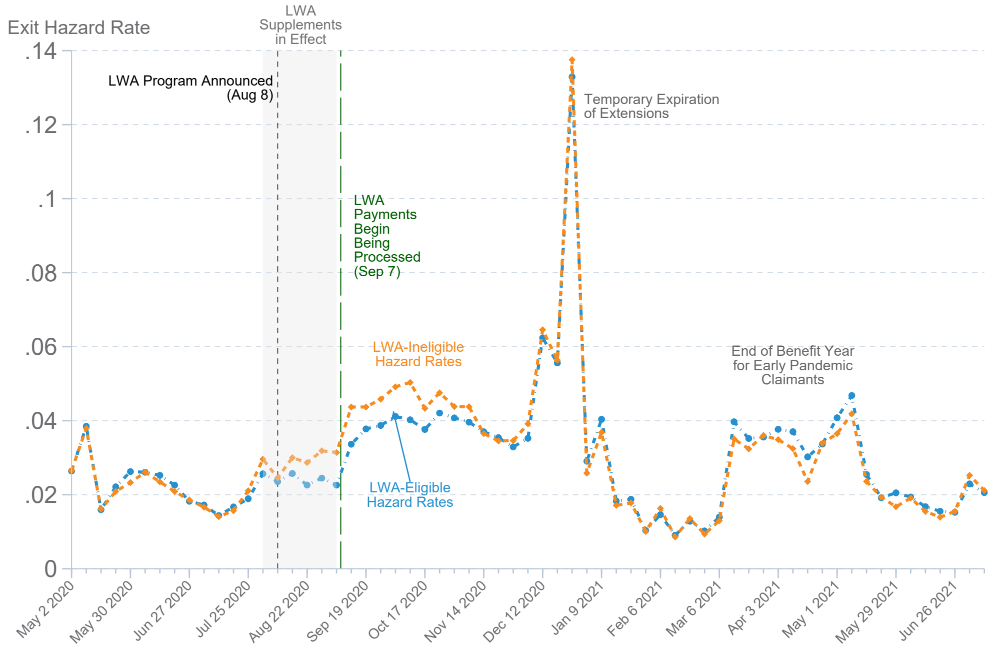
Figure 3 illustrates the hazard rate of UI claimants during pandemic. The orange dashed line corresponds to claimants with a WBA just below the LWA eligibility threshold, and the blue line corresponds to claimants just above the eligibility threshold. From May to July 2020, when $600 FPUC supplements were in effect, about 2-3% of claimants exited the UI system each week, and the two hazard rates are indistinguishable. After FPUC expires, LWA supplements are announced shortly thereafter. The expiration in FPUC supplements leads to an increase in hazard rates, while the corresponding announcement of the LWA program causes a sudden divergence between the two groups, which continues well into October 2020 (despite the supplements ending in early September). The formal regression discontinuity design will use this divergence to shed light on how LWA supplements affected worker's job search behavior.

Table 1: Summary Statistics

	Early 2020 Claimants		2019 Claimants		GR Claimants	
	WBAs within $10 BW	All WBAs	WBAs within $10 BW	All WBAs	WBAs within $10 BW	All WBAs
Age	33	37	37	41	35	39
Female	0.59	0.53	0.59	0.45	0.56	0.42
H.S. or Less	0.55	0.55	0.60	0.49	0.61	0.48
Some College/Associate's Deg.	0.30	0.27	0.29	0.28	0.31	0.31
Bachelor's or More	0.11	0.16	0.09	0.21	0.07	0.19
Not Reported	0.03	0.02	0.01	0.01	0.02	0.02
Asian	0.18	0.16	0.06	0.09	0.08	0.10
Black	0.10	0.09	0.14	0.09	0.10	0.07
Hispanic	0.34	0.37	0.46	0.43	0.41	0.36
Native American/Alaskan	0.01	0.01	0.01	0.01	0.01	0.01
White	0.30	0.30	0.24	0.30	0.31	0.36
Not Reported	0.07	0.07	0.08	0.08	0.10	0.11
Earnings in qtr prior to qtr of claim	2,092	8,041	2,956	11,523	2,175	8,827
Earnings in year prior to qtr of claim	6,866	31,343	6,637	42,115	6,756	35,794
Share with $\leq$ 2 qtrs with earnings in prior year	0.32	0.15	0.45	0.17	0.31	0.13
Number of qtrs in prior year with positive earnings	2.99	3.50	2.66	3.43	3.00	3.54
Earnings in highest qtr of base period (HQW)	2,822	10,892	2,783	15,107	2,838	13,142
Potential Benefit Duration (weeks) (Excl. extentions)	21	24	20	23	21	24
Weekly Benefit Amount (WBA)	99.6	276.8	99.7	323.9	99.6	299.3
% with job tenure $\geq$ 1 year	0.57	0.72	0.35	0.61	0.43	0.68
UI duration (weeks)	31.10	32.14	11.15	14.47	20.59	28.57
N	91,640	2,636,822	15,909	887,722	47,399	1,611,949

Notes: Column 1 includes claimants with a WBA within 10 dollars of the LWA eligibility threshold, and a benefit year beginning between 3/15/2020-8/8/2020. (The start of the pandemic and prior to the LWA announcement) Column 2 includes all claimants with a benefit year beginning between 3/15/2020-8/8/2020. Column 3 includes claimants with a WBA within 10 dollars of the LWA eligibility threshold, and a benefit year beginning in 2019. Column 4 includes all claimants with a benefit year beginning in 2019. Columns 5 and 6 include all claimants with a benefit year beginning in 2009-2012.

Figure 3: Exit Hazards for Claimants near the LWA Eligibility Threshold During the Pandemic



Notes: The orange line illustrates the share of claimants with a WBA $\in [90, 99]$ who received a UI payment for unemployment experienced in the prior week, and then exited the UI system (missed the first of at least 2 consecutive payments) in that week. The blue line shows the analogous exit rate for claimants with a WBA $\in [100, 109]$ (the treated group). The shaded region indicates the 6 weeks that eligible claimants could apply LWA supplements to: the week ending August 1st through the week ending September 5th. In late December 2020, the PEUC supplements temporary expired, but were soon (retroactively) renewed by Congress. From March-May 2021, claimants who filed their initial claims a year prior (at the start of the pandemic) reached the end of their benefit year. Despite remaining eligible to continue receiving UI benefits, some claimants stopped receiving benefits, as documented in Bell et al. (2021*b*).

3 Empirical Strategy

3.1 Identifying the Effects of LWA

In order to identify the causal effect of LWA supplements, I implement a sharp regression discontinuity design, where the running variable is a claimant's weekly benefit amount (WBA), and claimants assigned a WBA above the \$100 eligibility threshold receive the treatment.[12] The identifying assumption of my RD design is that all factors other than the treatment variable (LWA receipt) vary *continuously* at the \$100 WBA threshold (Hahn, Todd and Van der Klaauw, 2001). Under this assumption, any discontinuity in the average outcome at the threshold can be attributed to the LWA supplements, and interpreted as the causal average effect of the supplements for claimants with a \$100 WBA.

To implement the RD design, I estimate a local polynomial regression on each side of the

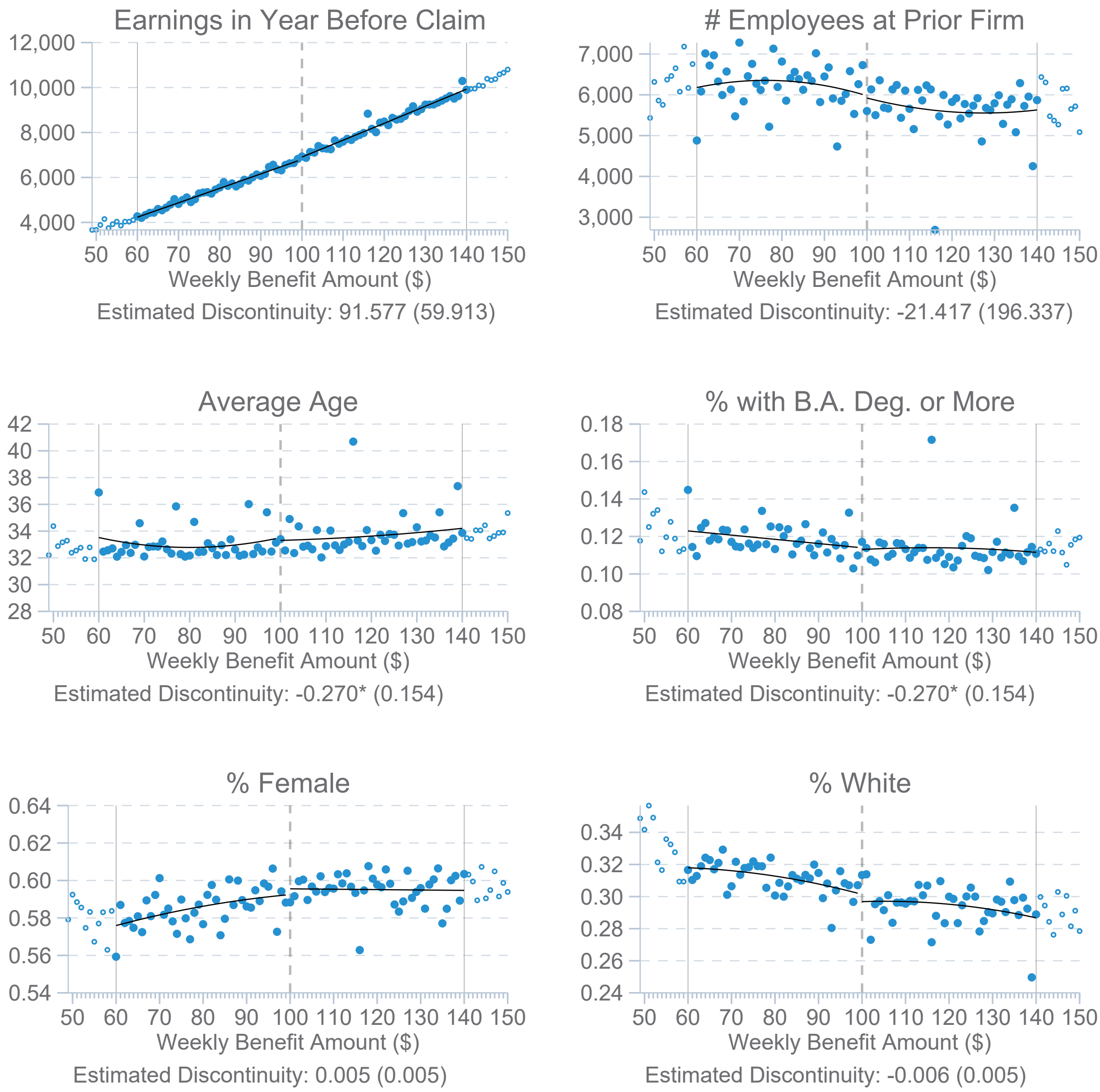
[12]I don't technically observe LWA supplements in my data, but in Appendix B I show that take-up appears to be 100% for claimants above the threshold. Thus, I assume all claimants with a WBA of \$100 or more are treated, and claimants with a WBA of \$99 or less are not.

eligibility threshold, using a $40 bandwidth and quadratic specification in my preferred estimates. I show in Figure A1 that my results are not sensitive to either bandwidth choice or the polynomial, but the $40 bandwidth allows for more precision than the MSE-optimal bandwidth, while the quadratic polynomial assuages concerns of bias. Following Cattaneo, Idrobo and Titiunik (2019), I report conventional point estimates with bias-corrected standard errors.

3.2 Evaluating the Identifying Assumption

One potential threat to my identification strategy is if claimants can precisely manipulate their WBA so that they can receive LWA supplements. If this were feasible, it could introduce a discontinuity in potential outcomes at the threshold, which would violate the identifying assumption and and introduce bias in my estimates. While I cannot observe potential outcomes, one approach for assessing the identification assumption is to test for discontinuities in the observable, pre-determined characteristics of UI claimants at the LWA eligibility threshold. Figure (4) presents the results of this exercise, and shows that a variety of economic and demographic variables ares smooth throughout the eligibility threshold. There is a marginally significant discontinuity in the share of claimants with a Bachelor's degree, but it is economically small and in the opposite direction as one would expect if higher-educated claimants were sorting around the eligibility threshold. Overall, the exercise provides strong evidence that the research design is valid.

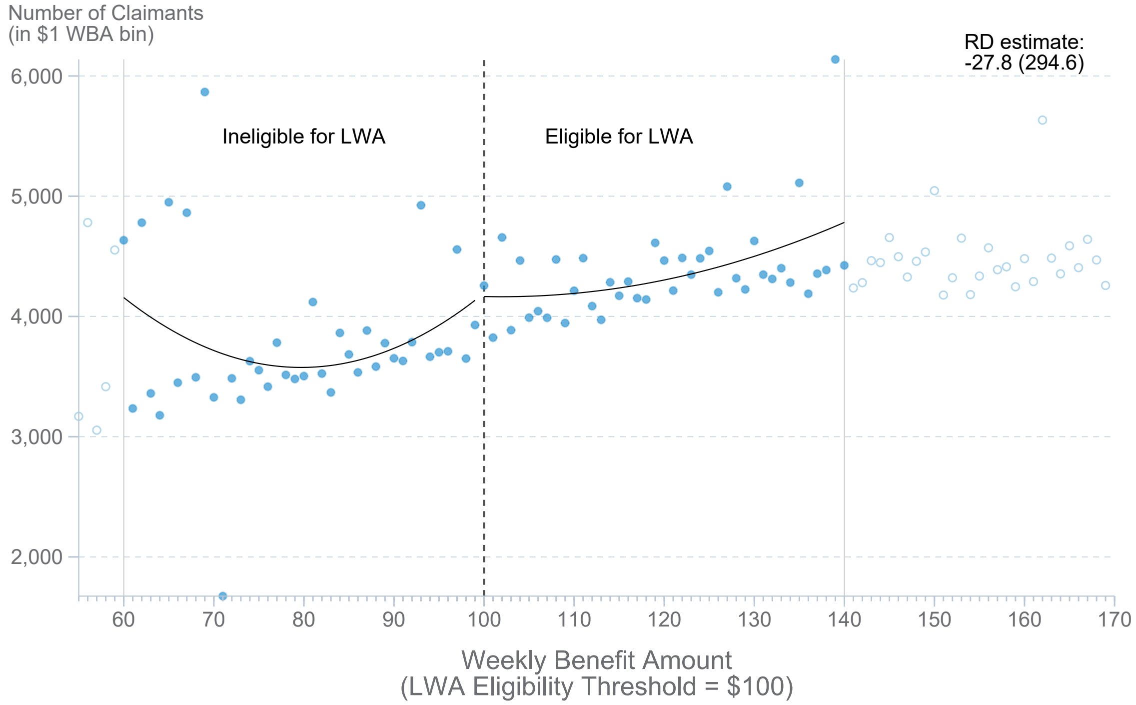
Figure 4: Evaluating Continuity of Pre-Determined Covariates



Notes: For consistency, all estimates for the percent change at the eligibility threshold are estimated using a $40 WBA bandwidth and a quadratic polynomial on each side of the discontinuity. The sample includes claimants who filed their initial claim between 3/15/2020 and 7/25/2020 (the first week of LWA), were still receiving payments until 7/25/2020, and were not flagged for fraud, had a disqualified claim, or had a HQW in a $1,000 increment.

Another way of testing the identifying assumption is to look at the smoothness of the density of UI claimants around the cutoffs. If claimants are able to precisely manipulate the running variable, we would expect their to be bunching just to the right of the eligibility threshold, or equivalently, a discontinuity in the density. Figure (5) shows the density of claimants near the eligibility cutoff, grouped into $1 WBA bins. We see there is no discontinuity in the density or heaping to the right of the eligibility threshold, suggesting that claimants in the sample did not have the ability to precisely manipulate their WBA in order to receive LWA supplements.

Figure 5: Density of Claimants Near the LWA Eligibility Threshold



Notes: Sample includes claimants who filed their initial claim between 3/15/2020 and 7/25/2020 (the first week of LWA), were still receiving payments until 7/25/2020, and were not flagged for fraud, had a disqualified claim, or had a HQW in a $1,000 increment. The RD estimate uses a $40 WBA bandwidth, estimated with quadratic polynomials on each side of the threshold. I report conventional standard errors, as these are more conservative than bias-adjusted.

While these empirical tests suggest that the continuity assumption is satisfied, it is perhaps more convincing to consider how the sample restrictions relate to the policy timeline. When a claimant files their initial claim, they are assigned a WBA by EDD, which is based on the earnings reported to EDD in the quarters leading up to their UI claim. I have restricted my analysis sample to claimants who filed their initial claims between March 15, 2020 and August 25, 2020, so that everyone in the sample has already been assigned their WBA well before the announcement of the LWA program (on August 8th). While one might be concerned about LWA affecting the propensity to file a UI claim after the policy announcement, such claims are not included in my sample. Overall, since the predetermined covariates appear to be mostly smooth at the eligibility threshold, the density at the eligibility threshold is relatively smooth, and because the sample was constructed in such a way to limit the potential for claimants to sort themselves around the threshold, I am not concerned about any threats to my identification strategy.

4 The Effect of LWA Supplements on Labor Supply

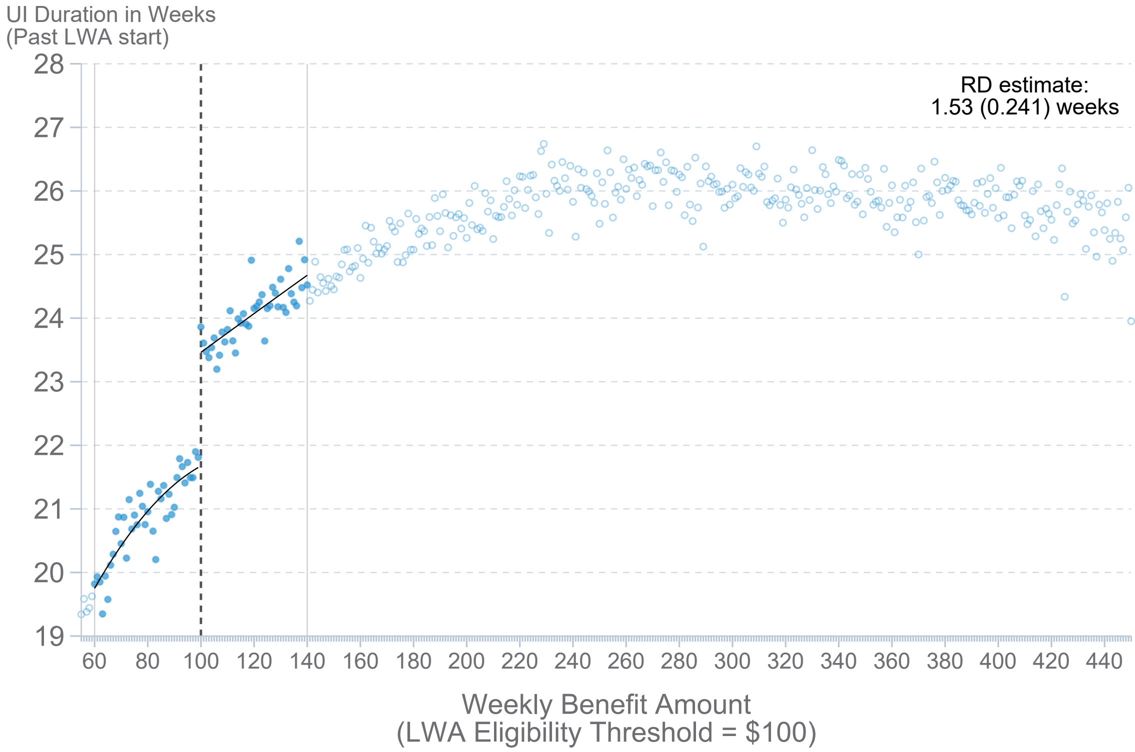
This section presents estimates of the effect of LWA supplements on labor supply. I first estimate the effect on unemployment durations, and then transform this result into an elasticity so that it can be compared to other settings. I do a similar exercise with two other outcomes: non-employment duration and the probability of exiting the labor force. Next, I decompose the total effect on unemployment duration into two parts: a liquidity effect and a moral hazard effect, and compare these to help understand the welfare effects of the supplements. Finally, I estimate the effect of LWA supplements on various measures of job quality.

4.1 Effect on Unemployment Duration

4.1.1 Raw Effects

My primary measure of unemployment is the total number of weeks a claimant receives UI before exiting. Since I am only considering claimants who were still unemployed when LWA started, I count the number of weeks past the start of the LWA program. Figure 6 illustrates the effect of LWA supplements on unemployment. We see that UI duration is increasing in the weekly benefit amount in the region around the LWA threshold. Claimants just to the left of the threshold were unemployed for 21.9 weeks after the start of LWA, while claimants just to the right of the threshold were unemployed for about 23.4 weeks . Column 1 of Table 2 shows formally that LWA increased total UI duration by a statistically significant 1.53 weeks.

Figure 6: LWA Supplements Increased Unemployment Duration by About 1 Week



Notes: The RD Estimate in the upper right corresponds to a conventional point estimate with bias-corrected standard errors, using claims within a $40 bandwidth on each side of the eligibility threshold. Each point represents the average unemployment duration (past the start of the LWA program) for claimants with a given weekly benefit amount. The solid vertical lines indicate the bandwidth used in the RD estimation. Sample definition is described in Section 2.2.2.

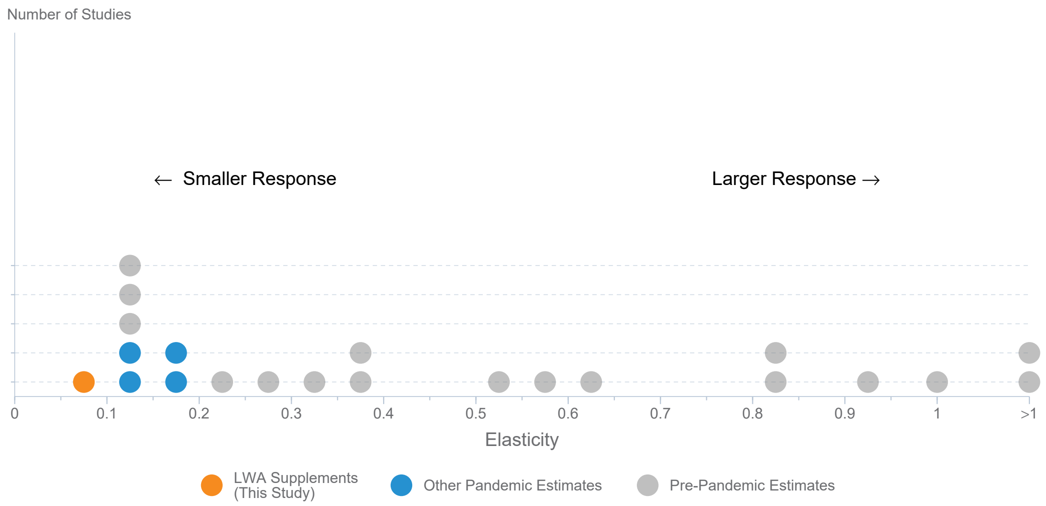
4.1.2 Comparing Elasticities

In order to make my estimates comparable to what has been found in other contexts, it is helpful to transform the raw effect of LWA receipt into an elasticity, taking the percent change in weekly benefits that LWA provided over the percent change in the outcome (unemployment duration). In the UI literature, elasticities are typically used to study permanent changes in weekly benefit amounts, whereas I am focusing on a temporary supplement. As such, how one translates the temporary supplements into a percent change in weekly benefit amounts is somewhat subjective. I estimate the elasticities under two alternative assumptions about the percent change in benefits, and show they yield qualitatively similar results. The first approach is most simple: since the supplements were $300 per week, and I am studying the effect on claimants with a $100 LWA, I assume a 300% increase in benefits. While this approach is simple, it ignores the fact that the supplements were only temporary, and not applied to the entire spell. To explicitly take into account the temporary nature of the supplements, my second approach takes the $1800 of total LWA supplements and spreads them out over the entire remaining UI spell. Table 2 shows that the average duration at the LWA threshold (for the untreated claimants, past the start of the LWA program) was 21.9 weeks. Thus, the 6-week, \$1800 supplements were similar to an \$82 supplement lasting the rest of the spell – equivalent to an 82% increase over the standard \$100 WBA.

Table 2 converts the raw effects, estimated for the full sample and separately for different demographic groups, into elasticities.

Looking at the full sample, we find a duration discontinuity of 1.53 weeks, or 6.9% relative to the 21.9 base duration. Under method 1 (a 300% WBA increase) the elasticity is $\frac{1.53/21.9}{3} = .023$, while under method 2 (82% increase in WBA) the elasticity is $\frac{.0698}{.82} = .086$. Both of these suggest the duration response to LWA supplements was very small. This is in line with other research based on UI responsiveness during the pandemic: Ganong et al. (2024) estimate the FPUC supplements had similarly small results (0.06 for the \$600 FPUC supplements and 0.10 for the \$300 supplements in 2021), while Bell et al. (2024) estimate elasticities to regular UI benefits both during the pandemic and in the years prior using a regression kink design, and find that the responsiveness to marginal changes in the WBA dropped dramatically during the pandemic. Taken together, these results suggest that throughout the pandemic, the labor supply response to UI benefits was quite small.

Figure 7: Comparing Elasticity for LWA Supplements to Findings in Other Settings



Notes: Pre-pandemic estimates from Schmieder and Von Wachter (2016). Other pandemic estimates include Ganong et al. (2024); Bell et al. (2024); Coombs et al. (2022); Petrosky-Nadeau and Valletta (2021).

Figure 7 places these results in context by comparing them to those summarized by Schmieder and von Wachter (2016) along with more recent pandemic-era estimates. We see that estimates focused on the pandemic-era are typically much smaller than those found in the pre-pandemic literature - but the LWA estimates are even smaller than these other pandemic-era estimates. This is likely due to the partially retroactive implementation and short-term nature of the LWA supplements, which mechanically reduced the potential for moral hazard responses in the first two weeks of the program. I discuss this finding further in section 6.2.

Table 2: The Effect of LWA on UI Duration (Past LWA Start)

	(1)	(2)	(3)	(4)
Group	Duration Elas. (Method 1)	Duration Elas. (Method 2)	Discontinuity	Avg. Duration at Threshold
Full Sample	0.0857***	0.0235***	1.53***	21.9
	(0.0100)	(0.00275)	(0.241)	(0.183)
Gender				
Male	0.0913***	0.0256***	1.65***	21.4
	(0.0153)	(0.00428)	(0.373)	(0.282)
Female	0.0820***	0.0222***	1.45***	22.2
	(0.0133)	(0.00360)	(0.317)	(0.241)
Age Group				
16–20	0.0778***	0.0253***	1.38***	18.5
	(0.0216)	(0.00702)	(0.516)	(0.388)
20–24	0.0651***	0.0187***	1.15***	20.9
	(0.0182)	(0.00523)	(0.436)	(0.331)
25–34	0.0829***	0.0220***	1.48***	22.6
	(0.0226)	(0.00601)	(0.546)	(0.411)
35–44	0.109***	0.0297***	1.95***	22.1
	(0.0306)	(0.00831)	(0.736)	(0.567)
45–54	0.0634*	0.0170*	1.16	22.3
	(0.0326)	(0.00875)	(0.806)	(0.616)
55+	0.141***	0.0333***	2.54***	25.5
	(0.0285)	(0.00670)	(0.692)	(0.525)
Race/Ethnicity				
White	0.0930***	0.0266***	1.66***	21.0
	(0.0180)	(0.00516)	(0.434)	(0.330)
Black	0.0419	0.00957	0.738	26.3
	(0.0328)	(0.00749)	(0.776)	(0.587)
Hispanic	0.0688***	0.0193***	1.22***	21.3
	(0.0170)	(0.00479)	(0.407)	(0.310)
Asian	0.108***	0.0299***	1.97***	21.8
	(0.0233)	(0.00642)	(0.578)	(0.436)
Education				
H.S. or Less	0.0774***	0.0207***	1.38***	22.5
	(0.0135)	(0.00362)	(0.325)	(0.248)
Some College/Associate's Deg.	0.102***	0.0283***	1.84***	21.6
	(0.0183)	(0.00508)	(0.444)	(0.335)
B.A.+	0.0929***	0.0282***	1.63**	19.8
	(0.0295)	(0.00897)	(0.696)	(0.526)
Job Tenure				
Tenure $<$ 1 yr	0.0636***	0.0185***	1.12***	20.7
	(0.0151)	(0.00439)	(0.360)	(0.274)
Tenure $\geq$ 1 yr	0.107***	0.0281***	1.92***	22.8
	(0.0138)	(0.00363)	(0.334)	(0.253)

Notes: Column 1 shows the estimated effect of LWA receipt on the number of weeks a claimant continues to claim benefits after the start of the program. Columns 2 and 3 transform that effect into an elasticity. When calculating the percent change in WBA, Column 2 assumes the $1800 in total LWA supplements are distributed equally across N weeks, where N is the average continued duration observed for claimants just to the left of the eligibility threshold (Column 4). Column 3 instead uses the $300 increase in WBA (relative to a $100 base) and estimates the elasticity using a 300% increase in WBA. *, **, and *** indicate significance at 10, 5, and 1% levels, respectively. Discontinuity estimates use a local quadratic model, with a $40 bandwidth, conventional point estimates and robust standard errors.

4.1.3 Heterogeneous Job-Finding Effects

The summary estimates at the top of Table 2 can mask substantial heterogeneity in the response between different demographic groups. The lower portion of the table examines how labor supply responses to LWA supplements vary by gender, age group, race/ethnicity, educational attainment, and job tenure. I find that the labor supply response was larger for men, older workers (55+), Asian workers, higher educated workers, and workers who had worked at their separating employer for at least a year. The response was especially low for Black workers (for whom it was not statistically different from 0), those with a high school degree or less, and workers aged 45-54. This heterogeneity could be a result of different health risks associated with returning to work for these groups (e.g. if Black claimants tended to work in occupations where the risk of contracting COVID-19 was exceptionally high, LWA supplements may have had little effect on their decision to return to work), or a result of different economic circumstances. Future research should more rigorously examine the factors undrlying this heterogeneity, and examine the extent to which it exists in other (non-pandemic) contexts.

Table 3: The Effect of LWA on Nonemployment Duration and Labor Force Exit Rates

	(1)	(2)	(3)	(4)
	Raw Effect of LWA Supplements	Avg. Outcome Just Left of Threshold	Elasticity (Method 1)	Elasticity (Method 2)
Pr(Exit Labor Force)	0.00027	0.56	0.00059	0.00016
	(0.0095)		(0.015)	(0.0042)
Nonemployment Duration (Qtrs)	0.035	1.99	0.021	0.0059
	(0.029)		(0.013)	(0.0036)

Notes: Column 1 shows the estimated effect of LWA supplements on each outcome. These are estimated via a regression discontinuity design. Column 1 reports conventional point estimates with Bias-Corrected standard errors. Column 2 reports the average outcome for each variable as the WBA approaches the eligibility threshold from the left. Columns 3 and 4 transform the raw effect into an elasticity. When calculating the percent change in WBA, Column 3 assumes the $1800 in total LWA supplements are distributed equally across N weeks, where N is the average continued duration observed for claimants just to the left of the eligibility threshold. Column 4 instead uses the $300 increase in WBA (relative to a $100 base) and estimates the elasticity using a 300% increase in WBA. *, **, and *** indicate significance at 10, 5, and 1% levels, respectively. Discontinuity estimates use a local linear model, MSE-optimal bandwidth (as outlined in Cattaneo et al. 2017), conventional point estimates and bias-corrected standard errors.

4.1.4 Effects on Non-Employment and Labor Force Exit

While UI duration is a timely measure of unemployment, there are two caveats one should keep in mind when interpreting results. First, claimants can continue to receive UI benefits while working part time, so long as their earnings do not exceed a given threshold. Secondly, claimants may exhaust their benefits and exit the UI system despite remaining unemployed. Thus, I complement my measure of UI duration with two other measures of labor supply responses: the nonemployment duration and the probability a claimant exits the labor force. I define nonemployment duration as the number of quarters (past the start of LWA) in which the claimant had $0 in reported earnings (from any employer). Thus, workers who find part time work but continue collecting UI will not be considered non-employed. I define a claimant has having exited the labor force if they report \$0 in earnings in each quarter from 2020q4-2021q4. I then implement the same regression discontinuity design as before to estimate the effect of LWA supplements on each outcome. Table 3 shows the results. LWA supplements had no statistically significant effect on nonemployment duration or labor force exit rates.

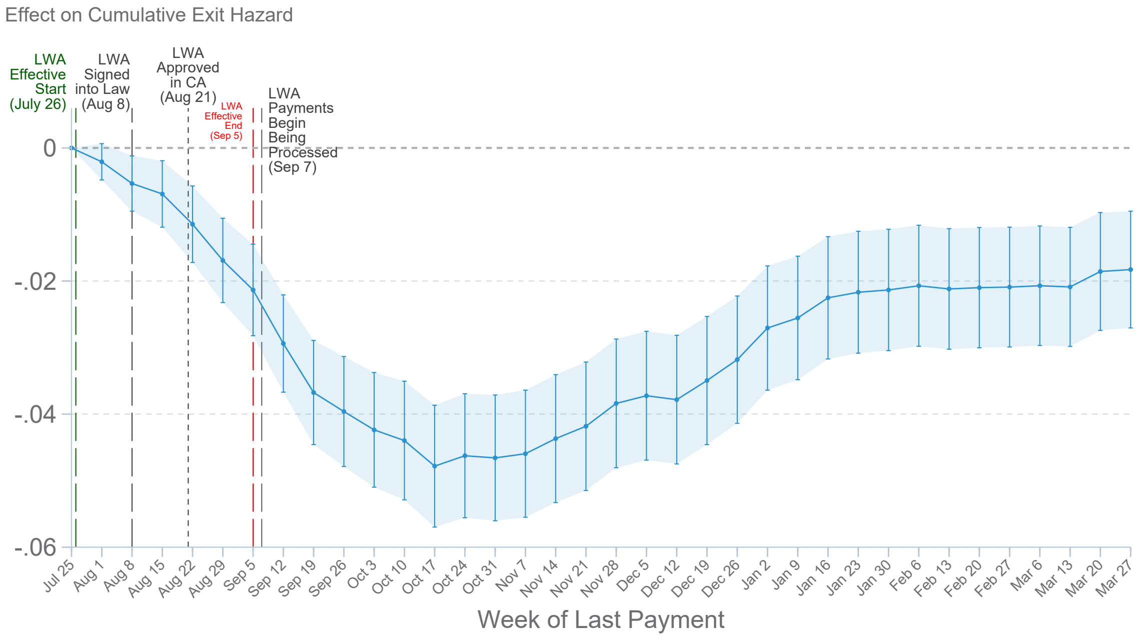
4.1.5 Hazard Rates

Another way to understand the effect of supplements on labor supply is to look at their effect on exit hazard rates. Figure 3 shows the hazard rates for those just above and just below the eligibility threshold. We see that before the announcement of LWA on August 8th, the hazard rates move in tandem, with 2-3% of claimants exiting UI each week. After the LWA announcement, the hazard rates quickly diverge, as the claimants ineligible for LWA begin to exit the UI system (relatively) more rapidly.

There are two things to note about the hazard rates in this figure. First, while the supplements were announced on August 8th, they were retroactively applied to the 2 weeks ending August 1st and August 8th. However, the divergence in hazard rates did not become apparent until the third (effective) week of the program. This is unsurprising, as the supplements hadn't been announced yet, and claimants had no reason to differentially adjust their job search behavior during those two weeks. This suggests that the retroactive nature of the program's implementation may have *mechanically* reduced the moral hazard effects associated with the supplements. Second, the gap in exit hazards persists well past September 5th, the last day the supplements were in effect. As I discuss in section 4.2, this suggests liquidity effects are playing a substantial part in driving the effects on unemployment duration.

Figure 8 formalizes this analysis by estimating a series of RD designs, where each outcome is a cumulative hazard rate – the probability of exiting UI between the start of LWA and a given calendar date. As before, we see that LWA made claimants less likely to exit the UI system in the period the supplements were in effect (July 26 - September 5). We also see that this trend continued until October 17th, at which point the cumulative hazards began to converge again. This prolonged effect suggests liquidity effects may be playing a substantial role in the labor supply response to LWA supplements.

Figure 8: Effect of LWA Supplements on Having Exited UI



Notes: Each estimate represent the effect of receiving LWA supplements on the probability a claimant has exited the UI system by the corresponding date on the x axis. The sample is constant across all estimates, only the observed outcomes (exited or not exited) change over time. The sample is defined in Section 2.2.2.

4.2 Moral Hazard and Liquidity Effects of LWA Supplements

Chetty (2008) showed that UI benefits (or supplements, in this case) can affect unemployment duration through two conceptually distinct channels: a "moral hazard" effect and a "liquidity" effect. The moral hazard effect is a result of the supplements effectively lowering the agent's net benefit from working (weekly wages - taxes - weekly benefits received while unemployed) during the weeks the supplements are in effect. The liquidity effect refers to the fact that the supplements increase the agent's cash on hand, which, in the presence of liquidity constraints, allows them to maintain a higher level of consumption while unemployed. (Recall that in a complete market with full insurance, agents will choose to smooth their consumption across time and states.) To the extent that LWA supplements relax this liquidity constraint, they can reduce the pressure to find a new job quickly, leading to longer unemployment spells.

The relative size of these two distinct effects has direct implications for the welfare consequences of UI supplements – Chetty (2008) demonstrates that the ratio of the moral hazard effect to the average liquidity effect is a sufficient statistic for the optimal level of UI benefits, even in the presence of heterogeneity. Intuitively, Chetty's formula for optimal UI quantifies the extent to which the full insurance benchmark is violated. By estimating the effect of transitory income shocks on the consumption of leisure (instead of goods or services), we can assess the agent's capacity to smooth marginal utilities. (Cochrane, 1991; Gruber, 1997). The relative size of the liquidity effect of LWA supplements to the total effect on unemployment duration thereby reveals the extent to which the supplements enable the worker to attain a more socially desirable allocation (Chetty, 2008).

With this framework in mind, Card, Chetty and Weber (2007) implement a regression discontinuity design around a threshold in the eligibility for severance payments in Austria during the 1980s-90s, showing empirically that an increase in cash-on-hand can lead to substantial increases in unemployment duration. Combining this result with another estimate of the effect of UI *extensions* on unemployment duration, they demonstrate that a representative job seeker from their sample is 70% of the way between the permanent income benchmark and credit constrained behavior. Relative to their work, I focus on large UI *supplements*, rather than extensions, (which can have different effects on both liquidity and moral hazard), use much more recent data, and have a substantially lower-earning sample. (The average earnings in his estimation sample is over 17,033 euros/year (in year 2000 euros), while my sample earns just $6,807/year in 2020 dollars.) Furthermore, my approach analyzes the liquidity effects of a *single policy*, as opposed to comparing the effects of two separate policies/quasi-experiments – which may have different sets of compliers.

Prior work has estimated the liquidity effects of UI in other ways. Some have used a structural approach, where each of the model primitives are estimated, and then the model is numerically simulated under different policy frameworks, as in Wolpin (1987). More recent work has focused on a reduced-form approach - Landais (2015) uses two regression kink designs (a kink the weekly benefit amount schedule and a kink in the benefit duration schedule) to back out the liquidity effect of UI benefits, finding that the total total duration response is driven as much by liquidity effects as by moral hazard. Ganong et al. (2024) implement a triple difference design (comparing high and low-liquidity workers), showing that the effect of FPUC supplements on job-finding does not appear to be driven by liquidity - a result which aligns with their structural model. While I cannot observe the cash balances of UI claimants in my sample, I can isolate liquidity effects using the sudden change in incentives at the expiration of LWA.

Figure 2 highlights a key feature of the LWA program that allows me to separate liquidity and moral hazard effects. While the supplements could be applied to each of the 6 weeks from July 26 - September 5th, none of the payments were actually distributed until September 7th. Thus, I impose a relatively mild assumption that the moral hazard effects associated with the LWA supplements only changed claimants behavior in the 6 weeks the supplements could be applied to. Then, aiming for an upper bound on this moral hazard effect (and a corresponding lower bound on the liquidity effect), I assume that the *entire* effect on behavior during those 6 weeks was due to moral hazard effects – another mild assumption given that no money was distributed during that period. Any change in behavior (differences in UI exit rates) *after* September 5th I attribute to the liquidity effect of LWA supplements.[13]

[13]This implies that there are no "lagged" moral hazard effects. Given the nature of my sample (low-earning workers concentrated in service industries), one would expect the job finding process to be relatively quick, making this a relatively mild assumption. Another implicit assumption is that the changing macroeconomic environment does not substantially alter the relative importance of liquidity and moral hazard effects over the analysis period. While UI exit hazards were rising during this period, this likely reflects both an improving labor market and the expiration of the $600/week FPUC supplements, which created a sharp drop in incentives to

4.2.1 Empirical Implementation

Let $t = 0$ denote the first effective week of LWA supplements: July 26th-August 1st. Let s_j denote the search effort for a claimant in week j. I normalize search efforts to equal the probability of finding employment, which I measure using the weekly exit hazard rate. Using potential outcomes notation, let $s_j(1)$ denote the exit hazard in week j for a claimant who receives LWA supplements, and $s_j(0)$ denote the exit hazard for a claimant who is ineligible for LWA supplements

In general, one can write a claimant's expected unemployment duration (past the start of LWA) as:

$$D = \sum_{t=0}^{T-1} \Pi_{j=0}^{t}(1 - s_j) \tag{1}$$

and the claimant's expected compensated duration (the number of weeks which they receive UI benefits) as

$$D_B = \sum_{t=0}^{B-1} \Pi_{j=0}^{t}(1 - s_j) \tag{2}$$

Since, in my setting, benefits were extended for over a year, I abstract away from any differences between (1) and (2). I estimate $s_j(1)$ and $s_j(0)$ each week for claimants on each side of the eligibility threshold (within a narrow bandwidth) from the start of the LWA program through August 2021. In each week, $s_j(1) - s_j(0)$ is analogous to the effect of LWA supplements on the exit hazard in week j. Using equation (1), I can estimate 4 different expected durations for claimants near the LWA eligibility threshold: 2 directly observed in the data, and two hypothetical durations. The first two are straightforward: the expected duration for claimants just left of the eligibility threshold, who do not receive supplements ($D_{\text{Untreated}}$) (eqn. 3), and the expected duration for claimants who just right of the threshold, who did receive supplements (D_{Treated}) (eqn. 4).

Furthermore, I can estimate the expected total duration if there were only a moral hazard effect (and no liquidity effect) by using a series of hazard rates for the treated group $s_j(1)$ for $j \in [0, 5]$ (the 6 weeks of unemployment that LWA supplements can be applied to), and $s_j(0)$ for $j \in [6, T]$ (each week after LWA expires) (eqn. 5). This uses the hazard rates for the LWA-eligible claimants while the program is in effect (and they have an additional incentive to remain unemployed and continue collecting benefits/supplements), then switches to the hazard rate for the LWA-ineligible group as soon as the supplements end. Since the LWA-ineligible group never received any supplements, their hazard rates will have neither the liquidity effect or the moral hazard effect. Piecing together the two sets of hazard rates, one which captures the entire moral hazard effect and the other which acts as a control during the period the liquidity

remain on UI. Moreover, prior work (Schmieder, Von Wachter and Bender, 2012) suggests that moral hazard effects are relatively stable across the business cycle, and that cyclical changes in liquidity effects are largely driven by exhaustion dynamics—which are less relevant here given the extended federal benefits already in place throughout 2020.

effect would occur, we can estimate the expected duration as if there were *only* a moral hazard effect.

Similarly, I can estimate the total duration as if there were *only* a liquidity effect (and no moral hazard effect) by using the hazard rates of the untreated claimants in the 6 weeks that LWA is in effect (since this group, being ineligible, experiences no moral hazard effect in those weeks), and then the hazard rates of the treated group in the weeks after the program expires (since the treated claimants will experience income effects during this period). This gives us:

$$D_{\text{Untreated}} = \sum_{t=0}^{B-1} \Pi_{j=0}^{t}(1 - s_j(0)) \tag{3}$$

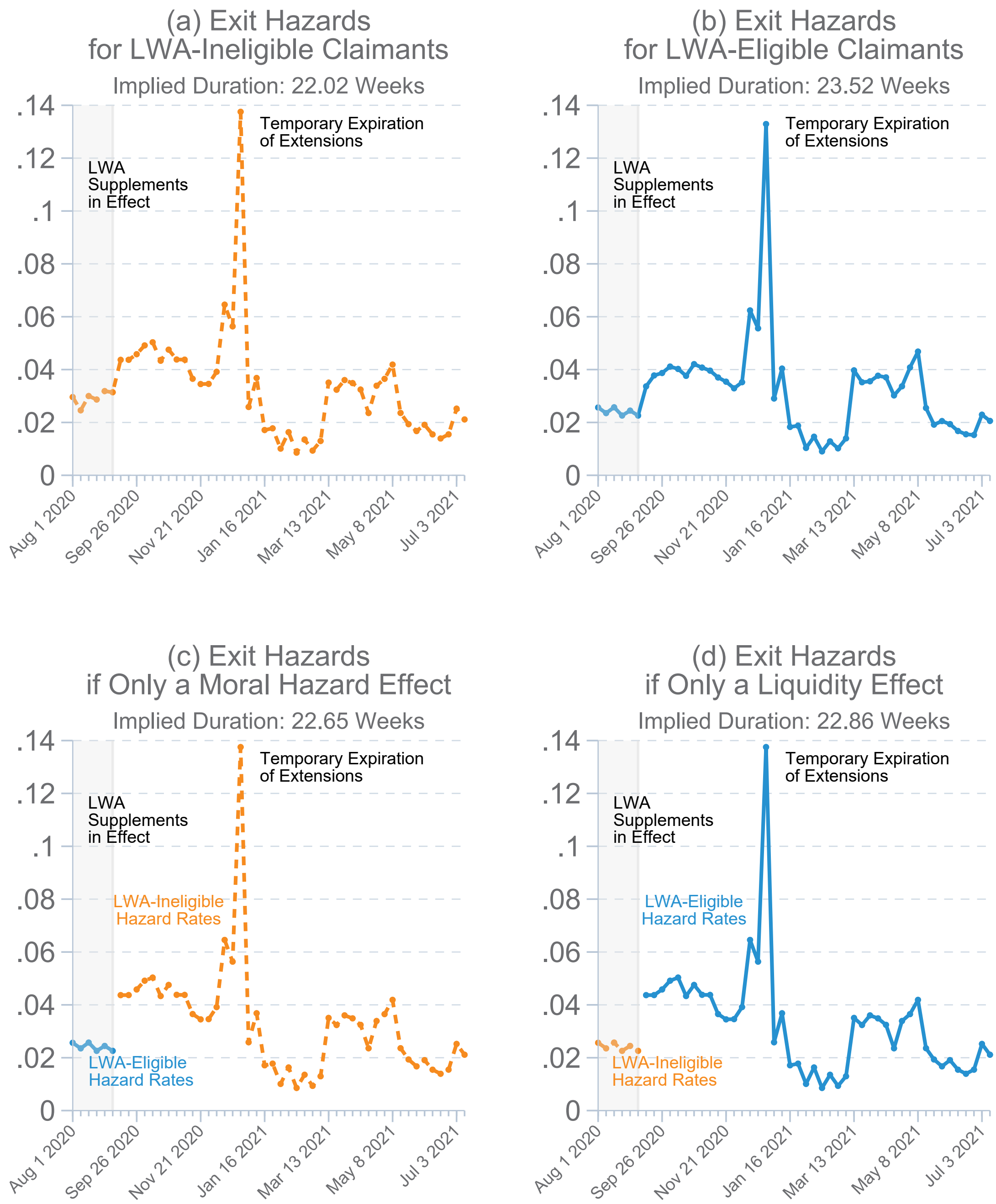
$$D_{\text{Treated}} = \sum_{t=0}^{B-1} \Pi_{j=0}^{t}(1 - s_j(1)) \tag{4}$$

$$D_{\text{MH only}} = \sum_{t=0}^{B-1} \Pi_{j=0}^{t}(1 - \tilde{s}_j) \quad \text{where } \tilde{s}_j = \begin{cases} s_j(0) & j > 5 \\ s_j(1) & j \leq 5 \end{cases} \tag{5}$$

$$D_{\text{Liq. only}} = \sum_{t=0}^{B-1} \Pi_{j=0}^{t}(1 - \tilde{s}'_j) \quad \text{where } \tilde{s}'_j = \begin{cases} s_j(1) & j > 5 \\ s_j(0) & j \leq 5 \end{cases} \tag{6}$$

Figure (9) illustrates the hazard rates used to estimate equations (3) - (6) and shows the corresponding implied UI durations. The total effect of the LWA supplements on UI duration is given by $D_{\text{Treated}} - D_{\text{Untreated}} = 1.5$ weeks. I can isolate the moral hazard effect as $D_{\text{MH only}} - D_{\text{Untreated}} = 22.65 - 22.02 = 0.63$ weeks. Similarly, I can isolate the liquidity effect as $D_{\text{Liq. only}} - D_{\text{Untreated}} = 22.86 - 22.02 = 0.84$ weeks. The liquidity effect as a share of the total effect of LWA supplements on UI duration is given by $\frac{D_{\text{Liq. only}} - D_{\text{Untreated}}}{D_{\text{Treated}} - D_{\text{Untreated}}}$. We see that the ratio of liquidity effect to the total effect is 56%. This means that workers were choosing a longer duration *primarily* because they had more cash on hand, as opposed to having distorted incentives – and the LWA supplements brought these workers closer to the social optimum.

Figure 9: Computing Moral Hazard and Liquidity Effects



Notes: Figure (3) overlays panels (a) and (b) over a longer time frame and provides more details on the hazard rates. See sec. (4.2) for an explanation of the calculation of implied UI durations. The sample for these figures are describe in Section 2.2.2

4.2.2 Dynamic Selection and Lagged Moral Hazard Effects

A potential concern with this analysis is that a difference in the hazard rates between the treated and untreated groups in week $j > 0$ could be driven by dynamic selection in addition to the LWA supplements. Prior to the start of LWA, we can safely assume that the composition of the treated and untreated groups are similar near the eligibility threshold (Section 3.2). After LWA takes effect, this assumption becomes more tenuous.

In particular, if LWA has heterogeneous effects on search behavior, it could lead to the composition of claimants on each side of the threshold differing in periods $j > 0$, since only the treated group receives the supplement. If the treatment effect is correlated with potential outcomes (how long someone might stay on UI regardless of the supplements), then the differential hazard rates between the two groups in periods $j > 0$ would be a combination of the causal effect of the supplements and the mechanical difference driven by composition effects.

To better understand the extent to which dynamic selection is present, I test for discontinuities in the observable covariates at different dates past the start of LWA. Figure A2 suggests the composition of the 2 samples (treated and untreated) does not change differentially over the spell, and there are no substantive discontinuities in economic and demographic characteristics by October 2020.

Nevertheless, to understand the extent to which dynamic selection may bias my estimates of liquidity effects (and to correct for any selection effects that may arise) I repeat the liquidity/moral hazard effect exercise described above after re-weighting the sample of surviving claimants in each week so that the claimants on each side of the threshold are comparable on observables. To the extent that the unobservable characteristics affecting UI duration are correlated with observable characteristics (age, gender, education, and various characteristics of the prior employer), then re-weighting the sample each week can address concerns of differential dynamic selection.

Appendix figure A3 shows these re-weighted hazard rates (analogous to figure (9)) and the corresponding implied durations. After re-weighting, the estimated liquidity effect is still 56% of the total effect, suggesting that dynamic selection does not seem to have a substantial effect on the results..

We can also relax the assumption that the post-program behavioral response is entirely due to liquidity by allowing for lagged moral hazard effects. This would arise if the supplements reduced job-finding effort while in effect, but those behavioral changes did not translate into lower exit rates until after the program ended – due to administrative delays associated with starting a new job or returning to work. To account for this possibility, I model a scenario in which part of the post-program response is attributed to delayed moral hazard rather than liquidity. Specifically, I assume that 50% of the total change in exit hazard during the two weeks immediately following the end of the LWA program is due to lagged moral hazard effects (with the other 50% still being due to liquidity effects). I then split those two post-program weeks evenly between the two channels—using the average of the treated and untreated hazard rates in those weeks when calculating the durations attributable to each. This adjustment mechanically reduces the estimated liquidity effect, but does not substantively change the takeaway: even under this conservative assumption, liquidity effects account for 47% of the total effect.[14]

[14]Formally, I re-calculate equation (6) so that $\tilde{s}'_j = \frac{1}{2}s_j(0) + \frac{1}{2}s_j(1)$ for the two weeks j ending September 12th and September 19th, then calculate a new liquidity effect using this revised $D_{\text{Liq. only}}$.

5 The Effect of LWA Supplements on Job Quality

In addition to allowing workers to smooth their consumption, UI supplements could potentially increase welfare if claimants are able to use the additional time unemployed the supplements allow them in order to find a better job. For instance, if UI benefits/supplements allow liquidity-constrained individuals to undertake a more thorough job search, they may be able to find work in an occupation more suited to their skill-set. Earlier work on the job-quality effects of UI has found mixed results (see section 1), while recent work focused on the liquidity effects of UI benefits suggest that the associated increase in unemployment durations leads to persistent declines in earnings (Flamang and Kancherla, 2024). I test the effect of LWA supplements on future job quality by implementing my regression discontinuity design with a variety of measures of job quality. I find that LWA supplements had little effect on future earnings, tenure at the next job, the incidence of industry switching, firm size, or the average earnings of other employees at the firm. Table 4 presents the results, showing that none of the elasticities for these outcomes are economically or statistically significant (except for recall, which has ambiguous welfare implications), and the 90% confidence intervals can rule out elasticities greater than 0.15 or less than -0.15 for all outcomes.

Given the effects on unemployment duration were relatively small, it may not be surprising that the job quality effects are small as well – if it takes time to find a higher-quality job, then a program which has little effect on unemployment duration is unlikely to dramatically affect job quality.

6 Discussion

6.1 Aggregate Effects on Unemployment

To what extent did LWA supplements contribute to the elevated unemployment rate in the latter half of 2020? Answering this question requires extrapolating the estimates from my regression discontinuity design (which applies to claimants near the eligibility threshold), and assuming they are relevant for the rest of the sample.

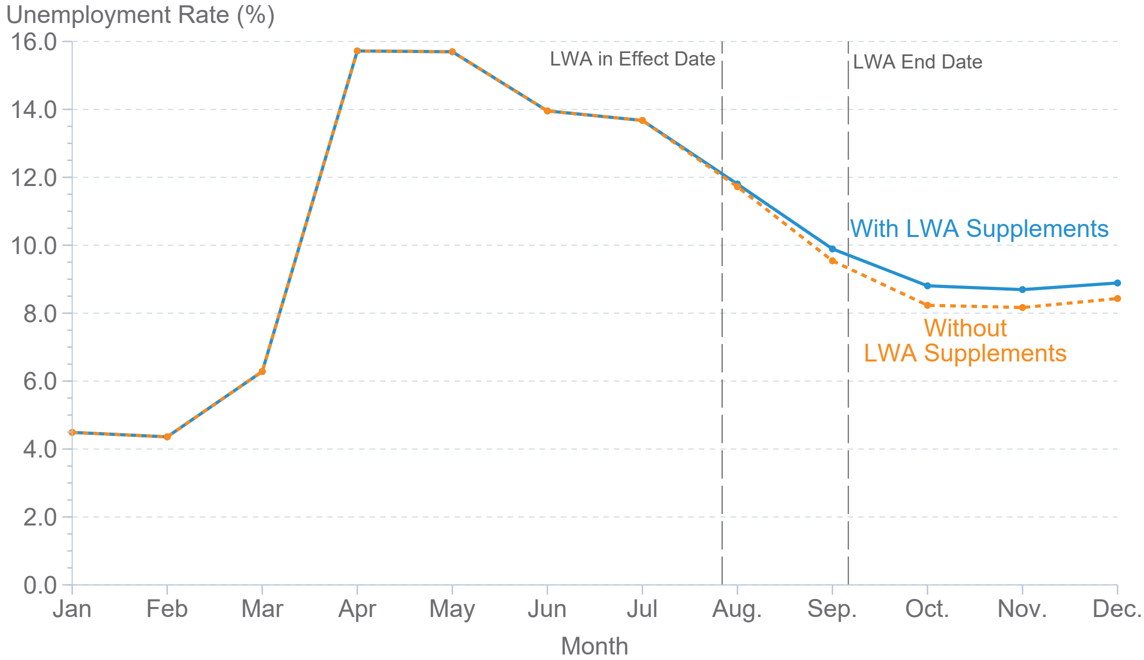
To shed light on this question, I estimate the counterfactual employment and labor force participation patterns of UI claimants above the LWA threshold, had they not received LWA supplements. I do this by estimating two types of RD estimates - one series of estimates for the effect of LWA supplements on the probability a UI claimant remains unemployed in each month, and another estimate for the probability a UI claimant is induced to remain in the labor force because of the supplements. I then use these estimated probabilities to calculate the number of claimants above the eligibility threshold who changed their employment status in a given month or their labor force status. I then construct a hypothetical unemployment rate for the scenario in which LWA was not implemented, where I adjust both the size of the labor force in each month and the number of unemployed to remove the effects of the LWA program.

Table 4: The Effect of LWA on Job Quality

	(1)	(2)	(3)	(4)
	Raw Effect of LWA Supplements	Avg. Outcome Just Left of Threshold	Elasticity (Method 1)	Elasticity (Method 2)
Avg Qtrly Earnings of Employees at 1st Job	15	10,931	0.0016	0.00045
	(478)		(0.039)	(0.011)
Avg Size of 1st Employer	188	6,868	0.033	0.0091
	(329)		(0.043)	(0.012)
Pr(Industry Change)	0.0045	0.63	0.0088	0.0024
	(0.0064)		(0.0093)	(0.0025)
Tenure (in Qtrs) at 1st Job	–0.023	2.6	–0.011	–0.0029
	(0.028)		(0.0096)	(0.0026)
Pr(Recalled to Prior Employer)	–0.0061	0.42	–0.018	–0.0048
	(0.0066)		(0.014)	(0.0039)
Total Earnings Within 2 Years	–190	12,561	–0.018	–0.005
	(184)		(0.018)	(0.0049)

Notes: Column 1 shows the estimated effect of LWA supplements on each outcome. These are estimated via a regression discontinuity design. Column 1 reports conventional point estimates with Bias-Corrected standard errors. Column 2 reports the average outcome for each variable as the WBA approaches the eligibility threshold from the left. Columns 3 and 4 transform the raw effect into an elasticity. When calculating the percent change in WBA, Column 3 assumes the $1800 in total LWA supplements are distributed equally across N weeks, where N is the average continued duration observed for claimants just to the left of the eligibility threshold. Column 4 instead uses the $300 increase in WBA (relative to a $100 base) and estimates the elasticity using a 300% increase in WBA. *, **, and *** indicate significance at 10, 5, and 1% levels, respectively. Discontinuity estimates use a local linear model, MSE-optimal bandwidth (as outlined in Cattaneo et al. 2017), conventional point estimates and bias-corrected standard errors.

Figure 10: Effect of LWA Supplements on California's Unemployment Rate (2020)



Notes: The unemployment rate with LWA supplements (blue solid line) is the non-seasonally adjusted UE rate available on the California EDD website. The hypothetical unemployment rate without LWA supplements (orange dashed line) is estimated by adjusting the total number of unemployed workers in the state upwards to account for the UI claimants who remained unemployed through that month as a result of LWA, as well as the UI claimants who remained in the labor force because of the supplements.

I find that the LWA supplements had only a marginal effect on unemployment rates in the months following the program. In October of 2020, when the effect was largest, the state's unemployment rate would have been 8.2% had LWA supplements not taken effect, instead of the 8.8% observed. The size of this estimated effect should be interpreted as an upper bound, as it only accounts for the "micro" effect of the LWA supplements, and ignores any potential spillovers (often called "macro" effects), which are likely to push in the opposite direction. In addition to affecting the treated claimants, the supplements could affect employment patterns for other workers as well. The supplements provided substantial fiscal stimulus, with $7 billion of LWA supplements distributed to CA UI claimants. Ganong et al. (2024) estimates large spending effects of UI supplements during the pandemic, which could potentially lead to the creation/preservation of more jobs. Furthermore, the supplements could spill over to others by affecting labor market congestion: by reducing the job-finding rate of LWA recipients, the supplements may have increased the job-finding rate of other workers who may have been competing for the same jobs (Landais, Michaillat and Saez, 2018). While there is little research on the spillover effects of temporary UI supplements, Karahan, Mitman and Moore (2024) show the macro effects of cuts to UI benefit *durations* make up about one third of the total effect on job-finding rates. Appendix Table A2 provides additional detail on the calculation.

6.2 Policy Implications

Thus far, I have shown that LWA supplements had an unprecedentedly small effect on unemployment durations. This finding is consistent with other research showing that labor supply responses to UI benefits dropped during the pandemic (Bell et al., 2024), but is even lower than what has been found for the FPUC supplement programs Ganong et al. (2024).

There are three main reasons why the effects of the LWA supplements are lower than the estimates for the FPUC supplements in Ganong et al. (2024). Firstly, the identification strategies differ. This paper uses a regression discontinuity design, which identifies the average treatment effect for claimants near the LWA eligibility threshold, whereas the dose-response difference-in-difference design in Ganong et al. (2024) uses the entire sample to identify the effect of a marginal change in the replacement rate across workers, while assuming a homogeneous treatment effect for lower and higher-wage workers.[15] If the workers near the eligibility threshold have a lower responsiveness to UI supplements than workers higher in the earnings distribution, this could partially explain the small duration response to LWA supplements estimated in this paper.

Secondly, the LWA supplements occurred immediately after the FPUC supplements expired. Ganong et al. (2024) document that the FPUC supplements (along with federal stimulus checks) raised the checking account balances of unemployed workers. If the FPUC supplements relaxed the liquidity constraints of UI claimants in the spring of 2020, this could lower the scope for a liquidity effect in response to the LWA supplements, which occurred later.

The differences in claimant responses could also be a result of differences in design of the FPUC and LWA supplements. In particular, LWA supplements were only available for 6 weeks, while FPUC supplements were available for 16 weeks (though they were renewed again in 2021). Furthermore, the LWA supplements were technically retroactive for 2 of those 6 weeks, which mechanically reduces the scope for moral hazard effects – claimants cannot adjust their behavior if they are not aware of the change in incentives. This feature of the LWA program could prove to be useful for policymakers. Typically, when designing economic relief packages, there is a trade-off between efficiency and targeting - UI benefit supplements are well targeted, but they also distort incentives (since claimants must remain unemployed to receive the supplements). This may lead policymakers to favor universal stimulus checks instead. However, the LWA program shows that UI supplements can be made retroactive. When job finding rates are low, this will still provide accurate targeting, (since they are provided to claimants who are likely still unemployed), and, because the supplements are short-lasting, will have only a minimal effect on incentives to find work. Looking through the lens of the Chetty (2008) model, large but short-lasting UI supplements can potentially increase the ratio of liquidity to moral hazard effects, making them more efficient.

[15] Ganong et al. (2024) also implement an interrupted time-series design, which identifies the average effect of the entire supplement for all workers.

7 Concluding Remarks

During recessions, policymakers aim to provide economic relief that is targeted to affected workers but minimizes economic distortions. The UI program is naturally targeted to such workers, but benefit extensions and permanent changes to weekly benefit amounts have been shown to prolong unemployment durations (Schmieder and Von Wachter, 2016; Bell et al., 2024). This paper shows that the Lost Wages Assistance program – which provided large, temporary, and partially retroactive UI supplements during the summer of 2020 – generated much smaller distortionary effects than what has been seen in other contexts.

I use administrative data from California's UI system, where a strict eligibility threshold for a 6 week, \$300/week supplement program allows for the implementation of a regression discontinuity design to identify causal effects. I find that the supplements had little effect on unemployment duration, increasing average UI durations by just over a week, relative to a base of 25 weeks. Consistent with this limited duration response, I also find precisely estimated null effects on various measure of job quality.

Despite over \$7 billion of LWA supplements being distributed to California UI claimants by mid-October, I find that the program was not the driving factor behind the elevated unemployment rate, with an upper bound estimate (ignoring spillover effects) that the supplements raised the unemployment rate from 8.2% to 8.8%.

Because the program was short-lived and supplements were not distributed until after the program expired, I am able to separately estimate the moral hazard effects and the liquidity effects of the program. By relaxing borrowing constraints, liquidity effects enable displaced workers to behave in a way that is closer what they would do in the presence of full insurance, and are therefore considered to be welfare-improving. By distorting incentives, moral hazard effects are inefficiently subsidizing leisure, and are welfare decreasing. Chetty (2008) shows that if one can estimate these two effects, their ratio is a sufficient statistic for the welfare effects of marginal changes in benefit amounts. I find that 56% of the total effect of LWA supplements is due to liquidity effects, and therefore the supplements were welfare-enhancing.

Taken together, these findings suggest that large and temporary UI supplements can be a powerful tool for policymakers tasked with designing economic relief/stimulus packages during a recession. Further work should study how such supplement programs vary in contexts outside of the pandemic.

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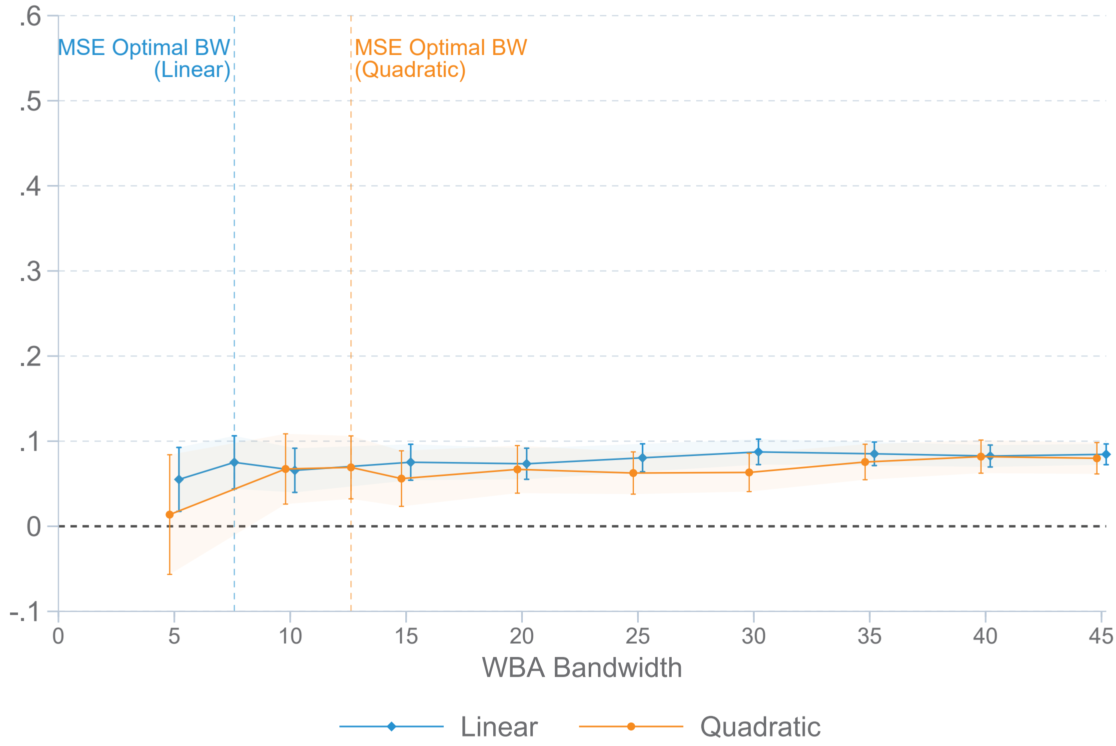
Appendix

A Appendix Figures and Tables

Figure A1: Robustness of Main Estimates (Effect on UI Duration) to Different Bandwidths and Polynomials



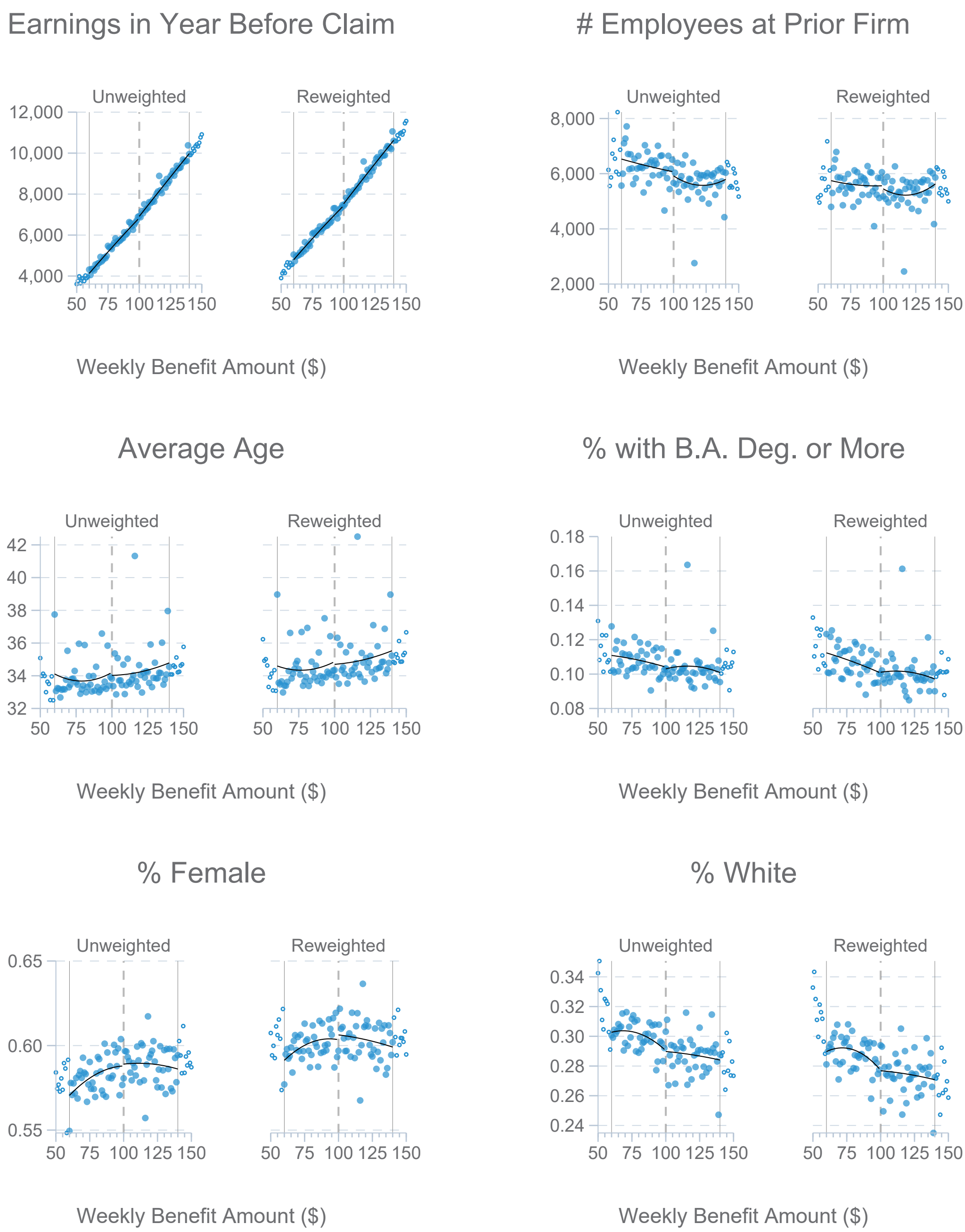
Notes: Sample includes claimants with a BYB date between March 15th and July 25th, who were still receiving UI benefits when LWA began. Each estimate (besides the MSE-optimal estimates) is in a $5 increment, the points on the graph have been slightly shifted horizontally for visualization purposes.

Table A1: Summary Statistics: Prior Industry of Claimants

	Early 2020 Claimants		2019 Claimants		GR Claimants	
	WBAs within $10 BW	All WBAs	WBAs within $10 BW	All WBAs	WBAs within $10 BW	All WBAs
Accommodation and Food Services	0.19	0.19	0.09	0.05	0.12	0.05
Retail Trade	0.15	0.12	0.11	0.07	0.15	0.09
Health Care and Social Assistance	0.08	0.10	0.10	0.08	0.04	0.05
Admin. Support, Waste Mgmt. & Remed. Svcs	0.07	0.08	0.14	0.11	0.11	0.09
Educational Services	0.09	0.07	0.06	0.04	0.07	0.05
Arts, Entertainment, and Recreation	0.06	0.05	0.03	0.02	0.03	0.02
Other Services Except Public Admin.	0.07	0.05	0.02	0.02	0.09	0.04
Manufacturing	0.02	0.05	0.03	0.08	0.04	0.11
Transp. Warehousing & Utilities	0.02	0.04	0.03	0.04	0.02	0.03
Construction	0.02	0.04	0.04	0.11	0.05	0.13
Professional, Scientific, and Tech. Services	0.03	0.04	0.04	0.07	0.04	0.08
Wholesale Trade	0.01	0.03	0.02	0.04	0.02	0.04
Information	0.02	0.03	0.02	0.05	0.02	0.04
Real Estate	0.01	0.02	0.01	0.01	0.01	0.02
Agriculture, Forestry, Fishing, and Hunting	0.01	0.01	0.13	0.11	0.08	0.05
Finance and Insurance	0.00	0.01	0.01	0.02	0.01	0.03
Public Admin	0.01	0.01	0.01	0.01	0.01	0.01
Management of Companies and Enterprises	0.00	0.00	0.00	0.01	0.00	0.01

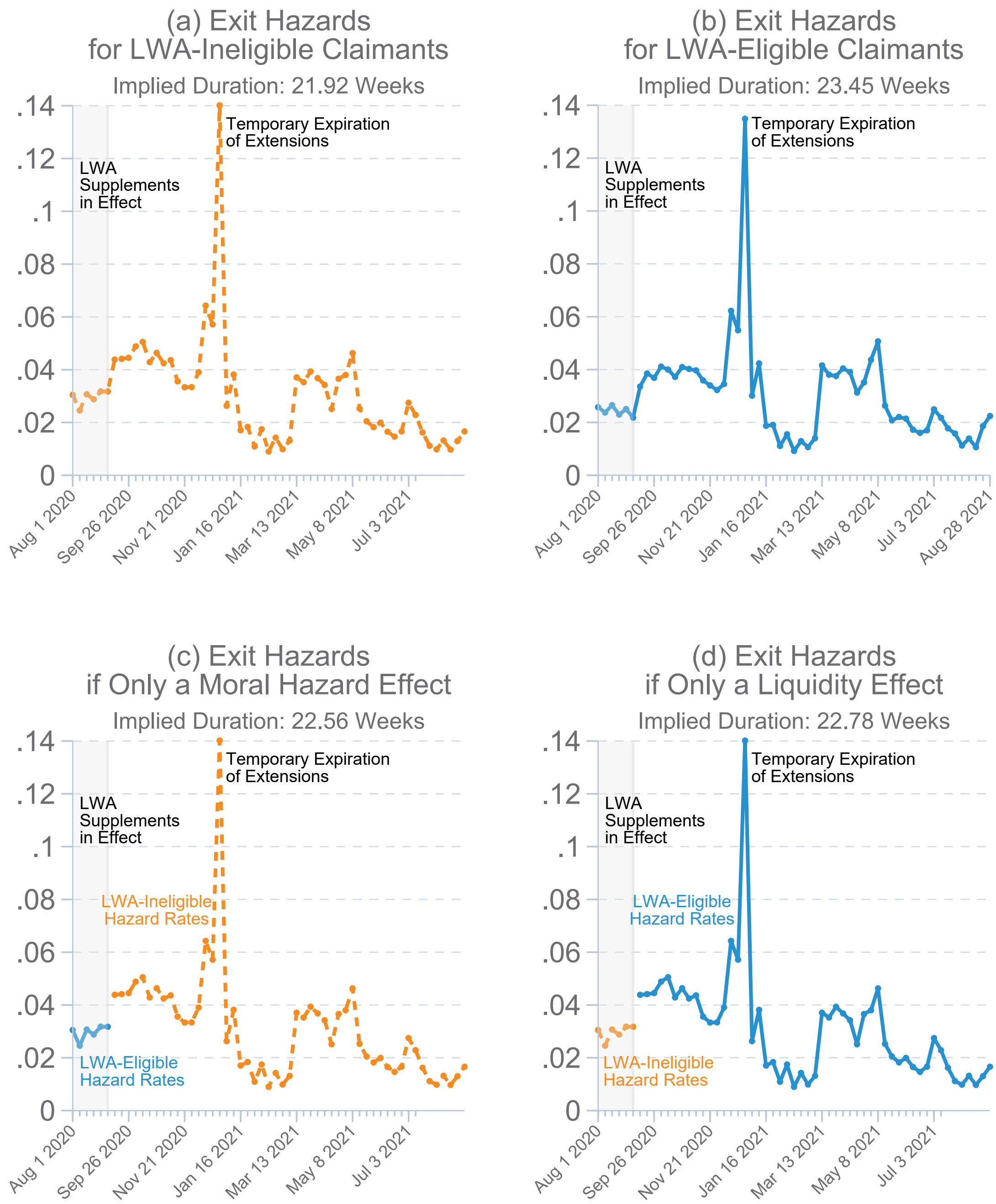
Notes: Column 1 includes claimants with a WBA within 10 dollars of the LWA eligibility threshold, and a benefit year beginning between 3/15/2020-8/8/2020. (The start of the pandemic and prior to the LWA announcement) Column 2 includes all claimants with a benefit year beginning between 3/15/2020-8/8/2020. Column 3 includes claimants with a WBA within 10 dollars of the LWA eligibility threshold, and a benefit year beginning in 2019. Column 4 includes all claimants with a benefit year beginning in 2019. Columns 5 and 6 include all claimants with a benefit year beginning in 2009-2012.

Figure A2: Covariate Smoothness For Claimants Still Unemployed in Late October: With and Without Re-weighting



Notes: Each panel includes claimants who were still unemployed as of October 31st, 2020. The left sub-panels show raw means by WBA, while the right sub-panels show the weighted means, where the weights are constructed so that the entire sample matches the untreated group (LWA $\in [90, 99]$) in that week based on observables (age, education, gender, race/ethnicity, employment history, and characteristics of the prior employer - number of establishments, firm size, and average pay) Regression lines correspond to a quadratic fit with a $40 bandwidth on each side of the eligibility threshold.

Figure A3: Reweighted Hazard Rates



Notes:This figure shows the weighted exit hazard rates for claimants just to the left (WBA $\in [90, 99]$) and just to the right (WBA $\in [100, 109]$) of the LWA eligibility threshold, where the weights are constructed via inverse-propensity scores so that in each week, the entire sample (both sides of the threshold) should match the claimants on the left-hand side of the eligibility threshold based on observables (age, education, gender, race/ethnicity, and characteristics of the prior employer).

Table A2: The Evolution of California's Unemployment Rate With and Without LWA Supplements

	With LWA			Without LWA		
Month	Labor Force	Number Unem-ployed	UE Rate	Labor Force	Number Unem-ployed	UE Rate
August	18,958,900	2,237,500	11.8	18,960,932	2,222,948	11.7
September	18,734,700	1,853,300	9.9	18,736,732	1,787,848	9.5
October	18,752,600	1,651,300	8.8	18,754,632	1,543,852	8.2
November	18,708,600	1,626,500	8.7	18,710,632	1,528,092	8.2
December	18,650,500	1,657,900	8.9	18,652,532	1,572,584	8.4

Columns 2-4 obtained from California Employment Development Department website (Not Seasonally Adjusted). Column 5 adjusts the size of the labor force to remove the (negative) effect of LWA supplements on the probability of UI claimants exiting the Labor force, where I proxy for exiting the labor force by defining it as having no reported earnings at any point from 2020q4-2021q4. Column 6 estimates the effect of LWA on the probability of remaining on UI (unemployed) in each month, and extrapolates this to the entire treated sample to estimate (and remove) the number of additional UI claimants who remained unemployed in that month as a result of LWA.

B How Many Claimants Eligible for LWA Supplements Actually Received Them?

My data does not allow me to directly observe LWA supplement payments. However, I know the eligibility rules for LWA, and I can observe in my data the number of UI payments which *should* be eligible for LWA supplements. For a payment to be eligible for an LWA supplement, it must correspond to a week of unemployment during the LWA period (the 6 weeks ending between August 1st-September 5th), and the claimant must self-attest that they are "unemployed or partially unemployed due to disruptions caused by the COVID-19 pandemic."[16] I cannot observe whether a claimant completed their self-attestation form, but I can calculate the number of payments that were potentially eligible for LWA supplements, as I observe the corresponding week of unemployment for each certification and I observe each claimant's WBA.

On October 22nd, the California EDD posted a News Release (Figure A4), showing that \$7.1 billion in LWA supplements had been paid out through the week ending October 17th.[17] Since the LWA supplements were \$300 per week, regardless of the regular payment amount, this implies 23.67 million LWA-eligible payments were processed.

Because I can observe the processing date for regular UI claims/payments, I can count the number of *potentially* eligible certifications that were processed before October 17th (certifications that should have received supplements if the claimant self-certified) I see that there were 23.7 million potentially eligible payments, almost the exact number we would expect if there were perfect take-up of the supplements. As such, I interpret my RD results as local average treatment effects around the eligibility threshold, as opposed to intent-to-treat estimates.

[16] California EDD News Release No. 0-42. August 27, 2020.

[17] California EDD News Releas No. 20-57. October 22, 2020.

Figure A4: Screenshot of EDD News Release

EDD News Release No.: 20-57 - 4 - October 22, 2020

Unemployment Insurance (UI) ▪ Data Dashboard

		Current Week October 17, 2020	Prior Weeks WE 3/7 -10/10/20	Total WE 3/7 -10/17/20
Total Applications Submitted** (Approx. UI Online applications received from unemployed workers) [1]	Reg. UI	25,011	5,702,518	5,727,529
	PUA	14,976	4,355,981	4,370,957
	PEUC[2]	139,169	1,506,055	1,645,224
	FED-ED[3]	8,475	156,776	165,251
	Total	187,631	11,721,330	11,908,961
Total Claims Processed** (Approx. applications processed by the EDD including reopened claims) [4]	Reg. UI	158,877	9,466,613	9,625,490
	PUA	25,168	3,682,696	3,707,864
	PEUC[2]	153,544	1,612,092	1,765,636
	FED-ED[3]	9,057	122,642	131,699
	Total	346,646	14,884,043	15,230,689
Total Benefits Paid (Approx. combination of first benefit payments and continued claim bi-weekly payments) [5]	Reg. UI	$1.1 billion	$54.2 billion	$55.3 billion
	PUA	$923 million	$36.5 billion	$37.4 billion
	PEUC[2]	$376 million	$4.1 billion	$4.4 billion
	FED-ED[3]	$37.6 million	$454 million	$492 million
	LWA[10]	$1.1 billion	$6.0 billion	$7.1 billion
	Total	$3.5 billion	$101 billion	$105 billion
Individuals Paid Benefits [6,7] (Expressed as four-week rolling totals to account for different bi-weekly payment cycles)	Reg. UI	3,144,000	3,202,000	
	PUA	1,359,000	1,414,000	
	Total	4,503,000[8]	4,616,000[9]	

C Details on LWA Certification Process and Payment Timing

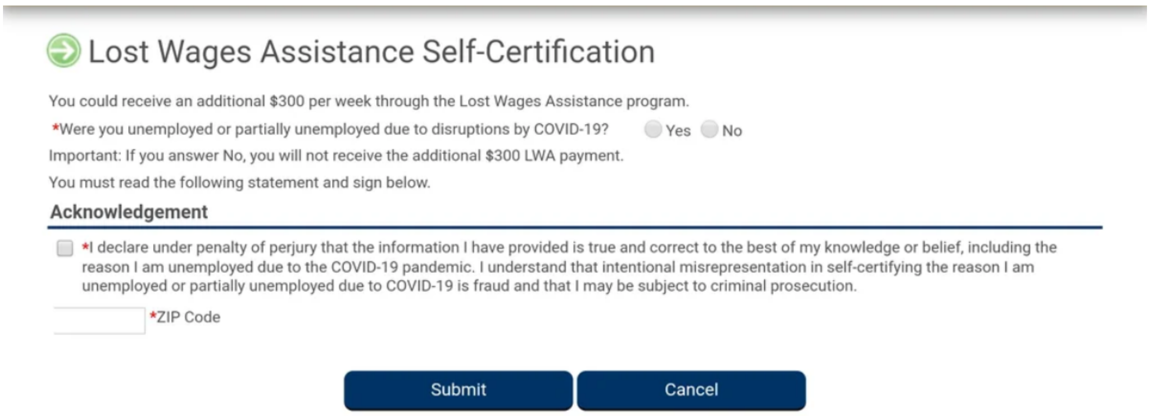
In order to be eligible for LWA supplements, a claimant must have a WBA of $100 or more, and also be unemployed or partially unemployed due to disruptions by COVID-19. Generally, claimants who applied for benefits mid-March would have been presented with questions about being unemployed due to a COVID-19 reason.[18]

EDD began processing LWA payments beginning in the week of September 7th. The payments were rolled out in phases. According to EDD, the first phase covered claimants who previously provided information that they were unemployed due to a COVID-19 related reason on their initial application and have already received their regular state or federal unemployment payments for benefit weeks between July 26 and August 15.[19]

The second phase covered claimants who did not have the opportunity to indicate they were unemployed due to a COVID-19 reason on their initial application and still met the minimum $100 weekly benefit amount eligibility requirement. The EDD sent notifications to claimants to complete a one-time self-certification (Fig A5) to inform the department if they were unemployed due to disruptions caused by COVID-19. The EDD notified claimants by email, SMS text message, or by mail to gather this information in mid-September.[20]

EDD first began paying LWA benefits for the three weeks of unemployment between July 26 and August 15th over Labor day weekend (September 5th-7th, 2020).[21] After those phase one and phase two payments were completed, EDD began to distribute payments for weeks 4 and 5 (for unemployment experienced between August 16th-29th) during the week of September 28th, followed by the final week of supplements, paid in the week of October 12th.[22] [23]

Figure A5: Screenshot of LWA Self-Certification in California



[18] EDD News Release No. 0-42. August 27, 2020

[19] Ibid.

[20] Ibid.

[21] EDD News Release No.: 0-46, Sept. 10, 2020

[22] EDD News Release No.: 0-50, Sept. 24, 2020

[23] EDD News Release No.: 20-54, October 8, 2020