Willingness to Pay for Workplace Amenities *

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Abstract

We present a new method to identify the value of workplace amenities using excess mass in the earnings distribution around budget discontinuities. The approach formalizes the intuition that workers are less responsive to financial incentives when the returns to work depend more strongly on the value of amenities. Applying the approach to the value of workplace safety, we find that workers are willing to forgo 9% of their earnings to reduce fatality risks by 1 in 100000. We also illustrate how the approach can identify aggregate bundles of amenities linked to a job and measure the value of "enjoyable jobs".

JEL-Codes: J17, J22, J28
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1 Introduction

The utility value of a job depends both on monetary and non-monetary compensation. Labor economists have paid increasing attention to non-wage aspects of work: How much do workers value a job they enjoy? How costly is a toxic work environment? How valuable is a safer job? Estimating the monetary value of such non-pecuniary goods is notoriously difficult. The seminal literature on "compensating differentials" shows that wage differences for otherwise similar jobs will reflect the value of amenities.[1] In practice, several empirical challenges hinder the estimation of such values.[2]

This paper develops a new approach to measuring the value of non-wage amenities and dis-amenities based on revealed preferences. Our strategy builds on the quasi-experimental approach of the bunching literature[3] and uses behavior around budget constraint discontinuities to identify workers' preferences for amenities. The existing bunching literature has shown that budget discontinuities (e.g., earnings limits for benefit eligibility) create financial incentives that generate excess and missing mass in the earnings distribution. We show that the amount of this excess mass depends on the value of work which reflects both the value of wages and amenities. Workers who derive large non-wage returns from working — such as the enjoyment of the job tasks or of the work environment — value work time more and are more likely to turn down a financial incentive created by a budget discontinuity. For such workers, budget discontinuities generate a small excess mass (i.e. "bunching"). Conversely, when work produces substantial dis-amenities — like health risk — returns to work-hours are lower, and the incentives created by budget discontinuities will produce

[1] Rosen (1986, 1974); Lucas (1977); Masters (1969)

[2] The canonical approach to estimating the value of amenities are hedonic regressions, which compare wages in high and low amenity jobs (Kahn and Lang, 1988; Black and Kniesner, 2003; Viscusi, 2018; Friedman and Kuznets, 1945). Two of the main challenges to this approach are sorting and frictional wage setting. Both imply that wage differences do not exclusively reflect the value of amenities.

[3] Card, Lee, Pei, and Weber (2017); Card, Johnston, Leung, Mas, and Pei (2015a); Card, Lee, Pei, and Weber (2015b); Kleven (2016); Chetty, Friedman, and Saez (2013)

larger responses and more excess mass in the earnings distribution. Workplace disamenities therefore operate similar to an additional "tax" on working, while amenities act as a "subsidy." The value of this tax/subsidy depends on the willingness to pay (WTP) for the amenity and can be inferred from observing how the excess mass varies when amenities change.

A key advantage of this framework is that it uses variation from natural experiments to estimate the WTP. The approach requires a setting with two features:

a) a discontinuity in the budget constraint (e.g., unemployment insurance eligibility rules, tax thresholds, retirement rules)

b) variation in workplace amenities (e.g., work from home, the launch of a union, changes in leave policies or other workplace policies)

It also makes two identification assumptions. The first assumption is the standard "smoothness" assumption of the bunching literature, which assumes that confounding shocks are smooth at the threshold. This assumption guarantees that behavior around budget discontinuity identifies labor supply choices. The second assumption requires preferences to be uncorrelated with changes in amenities. This holds by construction in settings where workers have homogenous preferences (as in Sorkin, 2018) or in a setting that use *within* worker variation in amenities (i.e. panel data).[4]

Importantly, the approach does *not* require that workers are fully in control of work hours and accommodates settings where hours are partially determined by employers.

We illustrate this framework with two empirical applications. The first estimates the value of workplace safety. This application, exploits the financial incentive generated by earnings eligibility thresholds for partial unemployment insurance (UI) and amplified by the Federal Pandemic Unemployment Compensation (FPUC) scheme in

[4]Empirical setting that use cross-sectional variation and have workers with heterogeneous preferences require that preference heterogeneity is uncorrelated with the workplace amenity of interest. We provide several tests to probe the validity of such an assumption.

March 2020. The FPUC scheme provided workers with an additional $600 weekly UI payments, which, unlike regular UI payments, was available in full below the threshold and not at all above the threshold, creating a "jump" (or notch) in the worker's budget constraint at the partial UI threshold.[5] We show that workers with tasks associated to the greatest deterioration in health risk during the Covid-19 pandemic are more likely to bunch than workers whose tasks did not become significantly more risky. Hairdressers, for example, experienced larger shocks to workplace risk than workers with less interpersonal contact, such as landscape gardeners. Our analysis shows that the excess mass at the UI eligibility thresholds is indeed significantly larger for hairdressers and similar occupations than for occupations with minor increases in risk. Our estimates imply that a one standard deviation increase in workplace risk is equivalent to a reduction in wage by around 30%. To put the standard deviation into context, an increase in risk by one standard deviation is similar to switching from the safest to the riskiest occupation in normal times. Converting standard deviations into easier interpretable units, the estimates imply a willingness to pay of 9% of earnings to lower work fatality risk by one in 100,000. While the results align with estimates from the statistical value of life literature (Viscusi, 2018), they differ greatly from a canonical hedonic wage approach. Using the same data and setting, the hedonic regression yields a willingness to pay of only 0.5% for a standard deviation of risk, which is nearly two orders of magnitudes smaller than our baseline 30% WTP estimate. This discrepancy is likely the result of frictions in wage setting, that make wages less responsive to short-term variation in workplace risks. Wages may not fully reflect risk premia with such frictions, and this, in turn, will bias the hedonic regression downward. This issue highlights the advantage of an approach like ours that can relax the assumption of frictionless wage setting.

We probe the two identification assumptions with a battery of tests. First, we

[5] Thresholds differ by state. Institutional details of the FPUC scheme are presented in Appendix C.1.

consider the "smoothness" assumption and check whether observed changes in the excess mass around the threshold are orthogonal to labor market disruptions during Covid-19. Our setting with multiple UI eligibility thresholds at different earnings levels enables us to probe this assumption directly and allows us to control for changes in the aggregate earnings distribution. The identification then comes from bunching around state-specific threshold levels, net of changes in the aggregate earnings distribution. The missing mass indeed occurs at different parts of the earnings distribution in different states, corresponding to the local UI threshold. We also use a placebo test with workers in the same local labor market not eligible for the FPUC supplement at the state threshold and find no spurious responses for this group. Similarly, a border design focusing on adjacent counties placed in separate states with different eligibility rules allows for explicitly controlling for local demand conditions with border fixed effects. Results are robust also to this demanding identification strategy. We also find similar results when controlling for proxies of local demand or local school closure.

To probe the second preference-orthogonality assumption, we implement a within-worker design that studies the same worker before and after changes in workplace risk. Results from this alternative specification holding worker preferences fixed are very similar to our baseline estimates, suggesting that differential responses are driven by the changing amenity and not, for instance, by cross-worker differences in labor supply elasticity.

To illustrate that the framework applies broadly, we present an additional application that estimates the value of enjoyable jobs and provides a money metric for widely used job satisfaction scores. This application studies retirement behavior among U.S. workers and leverages discontinuities in the lifetime budget constraint at age 62, generated by social security benefit rules. We find that both satisfied and less satisfied workers are more likely to retire at the threshold age, but this excess mass is bigger for individuals with lower job satisfaction. On average, less satisfied worker are willing to give up an extra two quarters of earnings to retire earlier and this difference cannot be explained by observable demographic differences (education, health, industry, occupation, location). Through the lens of our model, this implies that satisfying jobs are worth 12.5% more to workers than non-satisfying jobs.

Related Literature – Non-wage amenities in the labor market have been a central topic in economics since Adam Smith[6] and feature prominently in subsequent seminal work in labor economics (this includes Rosen 1986, 1974; Lucas 1977; Masters 1969) and in recent work on wage dispersion (e.g., Lavetti and Schmutte, 2022; Lamadon, Mogstad, and Setzler, 2022; Roussille and Scuderi, 2022; Lehmann, 2022; Sockin, 2022; Taber and Vejlin, 2020; Morchio and Moser, 2019; Goldin and Katz, 2011, 2016; Card, Heining, and Kline, 2013; Pierce, 2001; Hamermesh, 1999). However, providing credible estimates for the value of such amenities has been difficult in practice.

The empirical work on the value of workplace safety typically uses hedonic wage regressions to estimate such values (Lucas, 1977; Brown, 1980; Hwang, Reed, and Hubbard, 1992; Guardado and Ziebarth, 2019).[7] Hedonic regressions relate occupational wage differences to workplace risk. Several studies use quasi-experiments to improve identification of hedonic wage regressions (Lavetti, 2020; Gruber, 1997; Fishback and Kantor, 1995; Gruber and Krueger, 1991; Summers, 1989). An added challenge is frictions in the labor markets that alter the canonical link between wage differences and compensating differentials (Altonji and Paxson, 1992; Bonhomme and Jolivet, 2009; Ruppert, Stancanelli, and Wasmer, 2009; Lang and Majumdar, 2004). Our approach differs from canonical hedonic wage regressions in that it doesn't aim to identify compensating differentials in wages and estimates the WTP without having to account for or model frictions in wage setting or job switching.

[6]This idea goes back to Adam Smith, who highlights: "Honour makes a great part of the reward of all honourable professions" (Smith, 1776, p. 112).

[7]Hedonic regressions have also been used to estimate the value of other, non-health related, amenities. E.g., Summers (1989); Gruber and Krueger (1991); Gruber (1994, 1997); Fishback and Kantor (1995); Stern (2004).

A recent strand of literature uses stated preference experiments to elicit people's preferences through surveys that make workers choose between cash incentives and hypothetical jobs with specific amenities (Wiswall and Zafar, 2018; Mas and Pallais, 2017; Chen et al., 2021; Le Barbanchon, Rathelot, and Roulet, 2021; Dube, Naidu, and Reich, 2022; Einarsen et al., 2011; Maestas et al., 2018; Folke and Rickne, 2022). If few people choose the financial incentive over the job-amenity on offer, the approach infers that amenities are valued highly. Our approach can be thought of as taking this approach from the survey to the field. We similarly study how workers respond to financial incentives (here created by budget discontinuities) and, in line with the above intuition, show that fewer people respond to such incentives when amenities are valued highly. Unlike the stated preference approach above, our method uses a *revealed* preferences approach and analyzes observed labor market choices rather than hypothetical ones. Both revealed and stated preference approaches have advantages and drawbacks, and the debate about their relative merits is ongoing. We do not take a stance on this debate but rather offer an additional tool that allows researchers to estimate the value of amenities using secondary data and a quasi-experimental approach to identification.

Most closely related to our work are two studies that analyze how workplace amenities affect labor supply decisions (Sorkin, 2018; Powell, 2012). Powell (2012) shows that the presence of amenities reduces the tax elasticity of labor supply and Sorkin (2018) shows that one can rank the quality of employers by studying employer switching behavior. Unlike such an ordinal measure, our paper develops an estimator that provides cardinal money metrics for the value of amenities. Moreover, our approach can be used to quantify the value of both *specific* amenities and the overall amenities at firms.

Finally, our estimate of a monetary value for avoiding workplace risk also relates to the large literature on the "value of a statistical life" (prominent examples include

Ashenfelter and Greenstone, 2004; Viscusi and Aldy, 2003).

2 Estimating the Value of Amenities from Budget Discontinuities

The framework to identify the willingness to pay (WTP) for workplace amenities builds on the influential work on budget discontinuities. The existing literature shows that "kinks" and "notches" in the budget set trigger excess and missing mass in the earnings distribution and provide a quasi-experiment to estimate preferences over leisure and earnings (Card et al., 2015a,b, 2017; Kleven, 2016). We extend this canonical two-good framework to a three-good framework with leisure, earnings, and amenities, and exploit *changes* in the excess/missing mass in response to variation in workplace amenities to estimate the willingness to pay for them.

Consider the standard notch case with an individual who obtains utility from after-tax earnings (or consumption) and pre-tax earnings (cost of effort). We augment this framework with a third good, so that the utility function becomes $U\left(T(m), \frac{m}{z}, a\right)$, with m pre-tax earnings, $T(m)$ after-tax earnings, z worker ability, and a workplace amenities. While the framework applies to a wide range of possible workplace amenities, we use the example of worker health to illustrate the approach. A worker is either healthy (a_0) or sick (a_1).[8] Heterogeneity in ability is captured by a distribution function $f(z)$. Assume that this ability distribution, the tax system and preferences are smooth so that the resulting earnings distribution is also smooth. Denote after-tax earnings by $T(m)$, the tax rate by t, benefits by $\mathcal{B}$ and the earnings eligibility threshold for accessing benefits by m^*. Individuals become ineligible for $\mathcal{B}$ when their

[8]The framework applies to all cases where the total (dis)amenity consumed grows with hours worked, and cases where amenities are a fixed part of work but the probability of experiencing/using the (dis)amenity depends on hours worked. The framework thus accommodates most amenities studied in the literature.

pre-tax earnings exceeds m^*. The worker's budget constraint is therefore:

$$T(m) = \begin{cases} (1-t) * (m + \mathcal{B}) & m \leq m^* \\ (1-t) * m & m > m^* \end{cases} \tag{1}$$

This budget constraint has a notch at m^* and is illustrated in Panel A of Figure 1. This notch creates an incentive to reduce earnings below m^* and generates excess mass (missing mass) in the earnings distribution below (above) m^*, as shown in Panel B of Figure 1. We will show that the WTP for amenities can be identified from *changes* in the excess mass in response to *variation* in amenities.

$$E\left(U(T(m), \frac{m}{z}, a)\right) = [1 - r(m)]\, U\left(T(m), \frac{m}{z}, a_0\right) + r(m) U\left(T(m), \frac{m}{z}, a_1\right).$$

Denote the probability of a work-related accident or illness by $r(m) = m\theta$, where θ is the incidence risk per period and we let risk increase with m instead of hours to simplify notation. We thus assume that the probability of having a positive/negative experience at work increases the more time people spend at work.[9] Expected utility is: A worker is indifferent between paying an amount W or suffering poor health (a_1): $U(T(m), \frac{m}{z}, a_1) = U(T(m) - W, \frac{m}{z}, a_0)$, and W thus captures the compensating variation. Analogous to the iso-elastic quasi-linear assumption of the two-good bunching literature, we assume that utility is separable and quasi-linear in earnings. This utility takes the form:

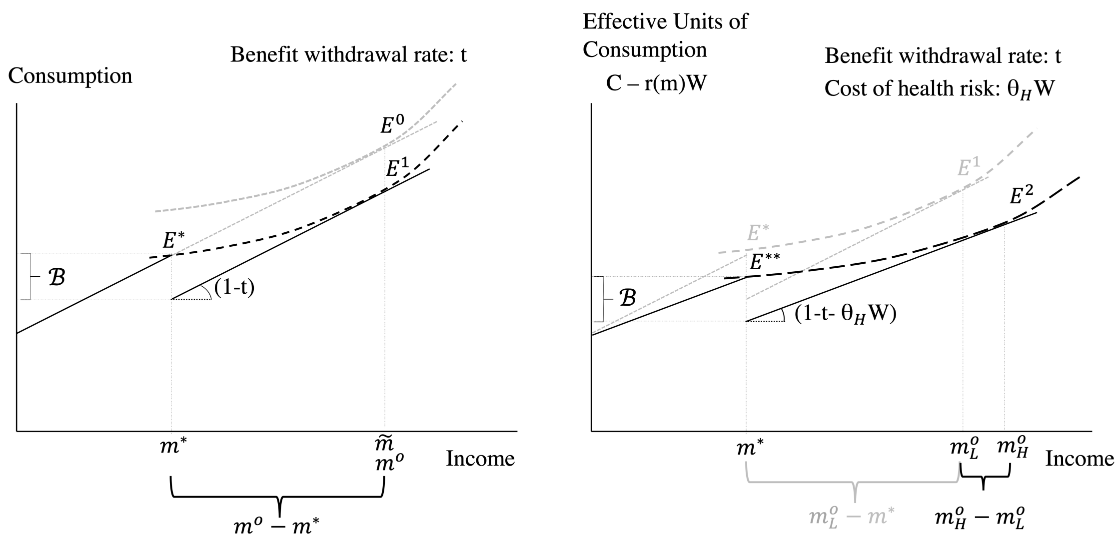
$$U\left(T(m), \frac{m}{z}, a\right) = T(m) - \frac{z}{1 + 1/e} \left(\frac{m}{z}\right)^{(1+1/e)} + a$$

where e is the labor supply elasticity.[10] Using the definition of W, expected utility

[9] We do not extend the model to amenities that are independent of work time. Such an extension would require studying extensive margin decisions, not the intensive margin we focus on here.

[10] The linearity of utility in a is without loss of generality since a has no units and we can redefine

Panel A: Labor Supply with Budget Notch



Panel B: Excess and Missing Mass in the Earnings Distribution

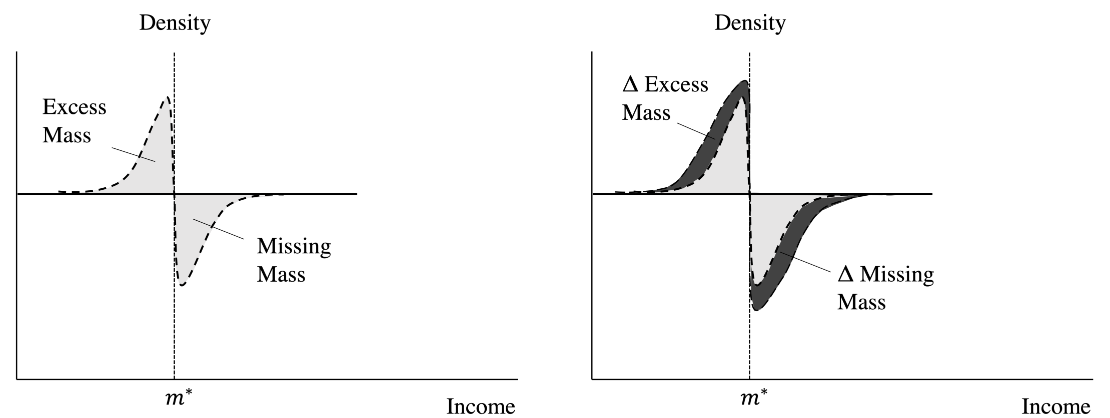


Figure 1: Worker Response to Budget Notch

Note: The left side of the figure shows the budget constraint from equation (1) and the indifference curve from equation (2) in Panel A and the resulting excess mass in Panel B for $\theta = \theta_L$. The right side shows the same for θ_L and θ_H. Panel A has total expected post-tax earnings (wage plus amenity) on the y-axis and labor supply (pre-tax earnings) on the x-axis. Panel B shows excess mass relative to the notch point m^*

becomes: $E\left(U\left(T(m), \frac{m}{z}, a\right)\right) = U\left(T(m), \frac{m}{z}, a_0\right) - r(m)W$. Normalizing $a_0 = 0$ we can therefore express expected utility as:

$$E\left(U\left(T(m), \frac{m}{z}, a\right)\right) = T(m) - m\theta W - \frac{z}{1+1/e}\left(\frac{m}{z}\right)^{(1+1/e)}. \tag{2}$$

The health risk acts like an additional tax, with tax rate θW, and reduces the expected return to work.[11] This setup nests the canonical bunching case when $W = 0$. In the more general setting, an additional moment is required to identify the parameter W.

First, consider the standard indifference condition of the "marginal buncher," who is indifferent between choosing the notch point m^* and an interior point $\tilde{m}$, $EU^* = E\tilde{U}$. The indifference curve for this worker is shown in Figure 1. At the interior point $\tilde{m}$ the first order condition from maximising (2) implies:

$$\tilde{m} = z(1 - t - \theta W)^e \tag{3}$$

and hence the indirect utility $E\tilde{U}$ is:

$$E\tilde{U} = \frac{z}{1+e}(1 - t - \theta W)^{(1+e)}. \tag{4}$$

At the notch point m^*, utility EU^* is:

$$EU^* = (1 - t - \theta W)m^* + (1-t)\mathcal{B} - \frac{z}{1+1/e}\left[\frac{m^*}{z}\right]^{(1+1/e)} \tag{5}$$

Using $EU^* = E\tilde{U}$ together with equations (4), (5) and the fact that $z = m^o/(1-t-$

any $f(\tilde{a}) \equiv a$. The assumption of an additive value of amenities is common in the literature (e.g., Morchio and Moser (2019)). For a more general utility function, see Appendix D.3.

[11] The implicit tax imposed by health costs is linear in this case, but the framework holds more broadly. The linear tax is an artifact of the functional form assumption on the utility, in the more general case WTP may vary with earnings and thus lead to a non-linear cost. As long as the optimization problem remains quasi-concave, the above linear framework still works and provides a local approximation that captures the WTP at earnings level m^*.

$\theta W)^e$ yields:

$$\frac{(1-t)\mathcal{B}}{(1-t-\theta W)m^*} = \frac{m^o}{m^*}\gamma - 1 \tag{6}$$

where $\gamma = \frac{1}{1+e} + \left(\frac{m^o}{m^*}\right)^{-\frac{1+e}{e}} \frac{e}{1+e}$. The left side of equation (6) is akin to the replacement rate, and captures the relative value of $\mathcal{B}$ (payoff when not working) and m^* (payoff from working), and the right-hand side captures the behavioral response to the budget notch ($\frac{m^o}{m^*}$) and parameters. The LHS (replacement rate) increases with θ since health risks at work raise the cost of working, make work less attractive and increases the value of benefit income relative to earned income. Equation (6) shows that an increase in the replacement rate leads to an equivalent increase in the RHS, implying more excess mass at the notch ($\frac{m^o}{m^*}\uparrow$). Health risks, therefore, result in more excess mass at the notch.

The next step uses equation (6) to pin down the values of W and e. These utility parameters depend on observed policy parameters $t, \mathcal{B}, \theta$, m^* and on $\frac{m^o}{m^*}$, a quantity we can estimate from behavioral responses. The literature identifies $\frac{m^o}{m^*}$ from the amount of excess mass (denoted by η) at the budget discontinuity. The link between observed η and $(m^o - m^*)$ is: $\eta = \int_{m^*}^{m^o} d_0 = (m^o - m^*)d_0$, where d_0 is the baseline earnings distribution.[12] The canonical bunching approach assumes $W = 0$ and uses equation (6) to solve for e. When $W \neq 0$, we can identify W by observing excess mass at the notch in a high and low-risk setting (θ_H, θ_L, respectively).

We will express the willingness to pay (denoted by WTP) as the share of after-tax earnings, as this makes WTP independent of units and ensures the value is bounded between 0 and 1. A worker is willing to give up $WTP(r)$ to avoid exposure to workplace risk of r: $WTP(r) \equiv \frac{r(m)W}{m^*(1-t)}$. Note that $WTP(r)$ is different from W in two ways. W is an absolute dollar amount and it is the compensating variation for falling sick ($r = 1$), while $WTP(r)$ is the cost of an increase in sickness risk by r and

[12]The last equality assumes d_0 is constant and simplifies the expression. The same approach, however, also works for cases with more flexible functions of d_0.

is expressed as a share of earnings. Denote the high and low risk levels by θ_H and θ_L and the associated behavioral responses by $m^0_L, m^0_H, \gamma_L, \gamma_H$. We solve for $WTP(r)$ by evaluating (6) in L and H states and taking the ratio of the two. Normalising $\theta_L = 0$ and re-arranging the ratio yields:

$$WTP(r) = 1 - \frac{\frac{m^o_L}{m^*}\gamma_L - 1}{\frac{m^o_H}{m^*}\gamma_H - 1} \tag{7}$$

$$\simeq 1 - \frac{\frac{m^o_L}{m^*} - 1}{\frac{m^o_H}{m^*} - 1} \tag{8}$$

$$= \frac{m^0_H - m^0_L}{m^0_H - m^*}. \tag{9}$$

Equation (7) shows that the WTP can be expressed in terms of the labor supply response to the notch in high and low-risk settings $(m^0_H/m^*\gamma_H, m^0_L/m^*\gamma_L)$.[13] We can further show that for regression kink designs $\gamma_L = \gamma_H = 1$ and similarly for notches, $\gamma_L, \gamma_H \to 1$ when labor supply elasticity e is small. Most empirical estimates find small values of e, making this a particularly relevant approximation. In such cases, the WTP further simplifies to expression (9), making it independent of e. The WTP becomes a simple ratio with the response in the high-risk state H $(m^0_H - m^*)$ as the denominator and the additional response when risk increases from θ_L to θ_H $(m^0_H - m^0_L)$ as the numerator. Put simply, we compare the magnitude of excess mass when workplace risks are high and low. If the excess mass is the same in both cases $(m^0_H = m^0_L)$, then $WTP(r) = 0$. In contrast, a large $WTP(r)$ implies that the excess mass increases sharply with risk $(m^0_H > m^0_L)$.

Using the approximation in (9) instead of the structural equation in (7) has several advantages. First, it yields an extremely simple expression that depends only on two behavioral responses. These can be transparently estimated from behavior around budget notches using familiar quasi-experimental tools. Second, the expression is

[13] When implementing this approach empirically, one also has to account for potential frictions in work-hour choices. We will address this issue in the online Appendix section D.2.

independent of the precise values of e and accommodates a wider range of functional form assumptions and/or adjustment frictions. We show that, in practice, the cost of using the approximation is small. Equation (9) is a lower bound for the structural WTP, and Appendix D.1 shows that the approximation performs extremely well under a wide range of plausible functional form assumptions, yielding a tight bound for the true parameter value. Using the approximation thus comes at little cost and has the advantage of being less dependent on functional form assumptions.

The WTP approach shares several of the advantages of canonical budget discontinuity designs. The behavioral responses can be estimated non-parametrically using transparent quasi-experimental tools. The theoretical framework translates these estimates into structural parameters that hold validity beyond the specific estimation context, enabling the study of policy counterfactuals.

In the online Appendix, we extend this framework in several dimensions: we consider the role of adjustment frictions (D.2), different functional form assumptions (D.3), and the role of income effects (D.4).

3 Willingness To Pay for Workplace Safety

We now use the WTP method to estimate workers' willingness to pay for workplace safety using work-shift data and studying workers' labor supply response to the increased workplace risk triggered by Covid-19 breakouts.

3.1 Data and Sample

We obtained data on worker hours and earnings from Homebase, a private company used by small businesses to track the hours and earnings of their workers.[14] The data mainly covers sectors with hourly and frontline workers (such as those in the

[14]The data is provided and licensed by Homebase (joinhomebase.com).

restaurant, food and beverage, retail, health and beauty, and healthcare industries), the type of worker who faced the decision whether to reduce their work hours to diminish the risk of contracting Covid-19.[15]

This data has several advantages and drawbacks. The first crucial advantage is that we have earnings and hours records at a daily frequency, which we can aggregate at a weekly level. This is important because partial unemployment eligibility is evaluated based on weekly earnings, and most existing datasets only report monthly or quarterly hours and earnings. A second advantage is that the data are automatically reported through an app. This circumvents the well-known issue of noise in self-reported work hours (c.f., classic work by Bollinger, 1998; Bound and Krueger, 1991). Providing accurate hours data is the core product feature of Homebase: workers use a mobile phone app to clock in and out of work, and the phone's geo-location tracking ensures accurate clocking.[16] A third important advantage is data coverage. Typical administrative UI records cover only a single state and/or are available with a substantial time lag. Our sample includes data from 21 states and is available at near real time. Studying multiple states simultaneously offers a source of institutional variation. The key advantage in our application is that each state has its own UI eligibility threshold, making for a stronger identification strategy. In addition, it enables us to use border designs and compare neighboring counties with similar characteristics but different UI eligibility thresholds.[17]

A drawback of this type of private-sector data is that it lacks information on individuals who exit the sample. When individuals are not observed, they could have either changed employers or left the labor force entirely. This is a lesser concern in

[15] In Appendix B, we compare the Homebase data with nationally representative data. Our sample's weekly earnings, hourly wages and hours worked are similar to the average hourly worker in small firms in the 21 states under analysis.

[16] When the app recognizes that workers get to or leave the workplace, it sends a check-in/out notification as shown from the app screenshot in Appendix figure A1.

[17] Strategic misreporting of work hours is also less of a concern with Homebase data since these records are not used to administer UI benefits.

our setting since the theoretical framework focuses on intensive margin changes and we exclude weeks with zero earnings from the main analysis. In Appendix E.4, we show that the results are robust to including workers that leave the homebase sample.

We impose four restrictions on our sample along the following dimensions: time period, eligibility for partial UI benefits, geography and work-spell length.

First, we restrict data to the time window between October 1, 2019, and July 31, 2020 (the end of the FPUC program) – five months before and five months after the onset of Covid-19 pandemic in the U.S. in March 2020.

Second, we restrict the sample to individuals eligible for the partial UI policy we study. The eligibility criteria are similar to regular UI payments. In addition to those criteria, workers are only eligible to partial UI when their earnings fall below a threshold level. In principle, the relevant threshold varies across workers based on their past earnings. However, for all workers that qualify for *maximum* weekly benefits (MWB) the same threshold applies within each U.S. state. For simplicity and transparency, we focus on workers eligible for MWB and thus study one eligibility threshold per state.[18] Homebase does not directly report whether workers are eligible, but we can infer eligibility based on retrospective work histories and state-specific eligibility rules.[19] While for most workers we observe the full earnings history required to determine benefit eligibility, for workers who have only a partial history, or might have a second job that is not in the Homebase system, we calculate theoretical quarterly earnings based on their hourly wage multiplied by 40 hours and 13 weeks. We then estimate whether they would be eligible for maximum weekly benefits based on these theoretical quarterly earnings. Given the higher uncertainty implied by this estimate, we down-weight these observations based on the ratio of observed quarterly

[18] Workers who qualify for benefits below the MWB also face a partial UI earnings threshold, which is difficult to compute for them, whereas the threshold for MWB-eligible workers is clearly defined as a function of MWB. Our analysis concentrates on these latter workers.

[19] We rely on information collected by the Department of Labor to reconstruct eligibility rules. In Appendix C.2 we report all sources and details for our calculations.

earnings over theoretical quarterly earnings. With this weighting scheme, workers with shorter earnings histories have a smaller weight in the analysis.

Third, data availability limits the analysis to a subsample of U.S. states. We exclude states where Homebase is not active and states where only few workers in our sample meet the earnings requirements to qualify for MWB.[20] The resulting sample covers 21 U.S. states.

Fourth, the baseline analysis also excludes the least attached workers who only work in the period before the onset of the pandemic or only after it. Relaxing this restriction has minimal impact on the result (see E.4). Finally, the baseline sample is "balanced" in the sense that each worker is in the sample for the same number of weeks before and after the onset of the pandemic, so that each worker contributes equally to the pre- and post-Covid-19 earnings distributions. While this is not strictly necessary, it alleviates concerns about selection effects and makes it easier to interpret excess and missing mass as changes among the same pool of workers.[21] We again show that relaxing this restriction has minimal effects on the estimates (see Appendix E.4).

Summary statistics for the sample are reported in Table 1. Panel A reports worker information. The sample includes 9,063 workers and 169,450 worker-week observations. On average, they work 36 hours per week and earn \$660. The median hourly wage is \$16 and does not vary much (the 25^{th} percentile is \$14, and the 75^{th} is \$20). Panel B of Table 1 shows summary statistics for the 3,500 small businesses in our sample. On average, they have 1.1 branches and 13.26 employees, of which 97% are hourly waged workers in the median firm. 32% of all firms operate in the Food and Drink sector, with Retail, Health Care, and Professional Services being the next most represented sectors in the data.

[20] For these states, we would therefore have insufficient workers to study behavior at the threshold.

[21] Take the specific example of a worker that worked continuously but had a two-week temporary absence (e.g., sickness or holidays) before the start of Covid-19 pandemic. We include all the active weeks before Covid-19 and trim the last two active weeks in the post-Covid-19 period for this worker to maintain a balanced number of work-week observations before and after Covid-19.

Table 1: Descriptive Statistics

	Mean	S.D.	p50	p25	p75
Panel A: Workers					
Weekly earnings	660.04	345.06	617.63	449.59	813.02
Weekly hours	36.49	12.78	38.58	29.17	44.47
Hourly wage	18.35	8.15	16.00	14.00	20.00
Number of weeks in data per worker	27.33	10.07	30.00	18.00	36.00
Worker-week observations	169,450				
Number of workers	9,063				
Panel B: Firms					
Size	13.26	20.05	8.03	4.25	15.52
Share of salaried workers	0.10	0.17	0.03	0.01	0.13
Number of Branches	1.14	0.64	1.00	1.00	1.00
Food and Drink	0.32	0.47	0.00	0.00	1.00
Health Care	0.18	0.39	0.00	0.00	0.00
Professional Services	0.04	0.21	0.00	0.00	0.00
Retail	0.03	0.18	0.00	0.00	0.00
Number of firms	3,500				

Note: Homebase data between November 1, 2019, and July 31, 2020. Sample of hourly workers with sufficient past earnings to qualify for MWB payments in their home state.

3.2 Baseline Response to the Budget Notch

To implement the WTP approach, we estimate the magnitude of excess mass at the budget notch threshold and study how this excess mass varies with workplace risks. We start by pooling all levels of risk and estimate baseline excess mass on average, and then explore heterogeneity in excess mass by risk levels.

The identification strategy leverages a budget notch created by the Federal Pandemic Unemployment Compensation (FPUC program), which introduced a lump-sum \$600 expansion of UI weekly benefits and was approved as part of the Coronavirus Aid, Relief, and Economic Security (CARES) Act, enacted on March 27, 2020, and ended on July 31, 2020.[22] Importantly, workers can qualify for FPUC while working,

[22] No FPUC benefits were payable between July 31, 2020, and December 26, 2020. The FPUC was re-established by the Continued Assistance Act as a \$300 per-week supplement to unemployment

as long as they are eligible for partial unemployment insurance, which requires their earnings to be below a threshold level (the "earnings test").[23] Above the threshold, workers become ineligible for FPUC. This creates incentives not to exceed the threshold earnings, potentially resulting in excess and missing mass in the earnings distribution around the threshold (for a theoretical illustration, see Fig 1). Standard additional rules aimed at mitigating moral hazard are also in place, e.g. UI recipients are not allowed to refuse job offers, and job loss or hours reduction should not in principle be due to the fault of the worker. These rules are notoriously difficult to enforce and a large literature on UI benefits studies the moral hazard problems that may prevail despite these rules.[24] Consistens with this, we do see workers bunch at the eligibility thresholds.

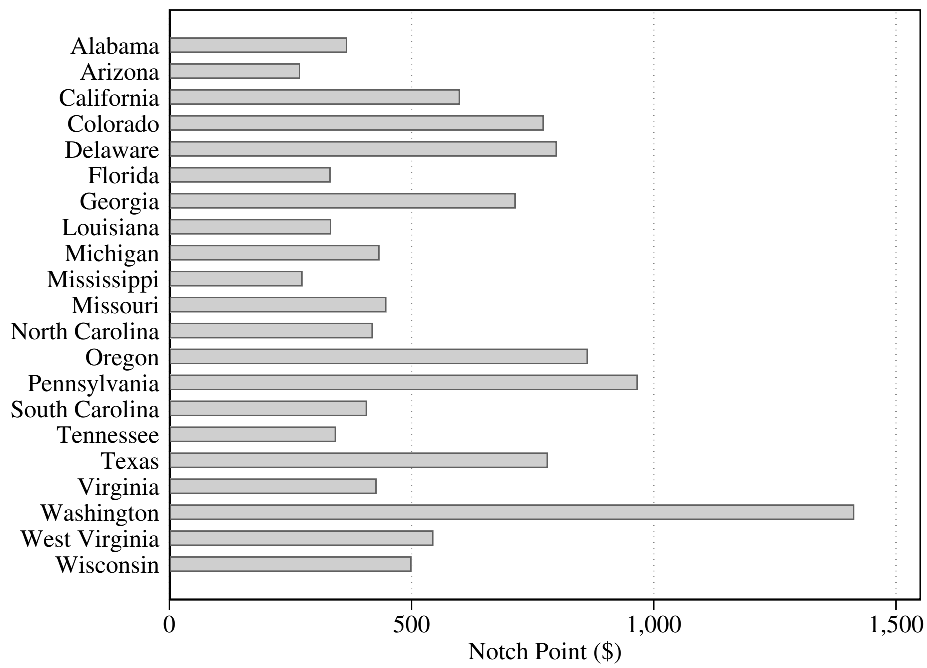
While FPUC was introduced uniformly in all US states, the administration of the benefit was left to the states and therefore depended on pre-Covid state-specific eligibility thresholds. We have calculated these thresholds for workers eligible for maximum UI benefits based on information collected by the Department of Labor. In Appendix C.2 we report all sources and details for our calculations. Figure 2 shows the variation of the Partial UI eligibility threshold across states. A worker earning $500 a week would be eligible for benefits in California and Pennsylvania, but not in Arizona or Florida. To identify the baseline labor supply response to the budget notch, we stack these different thresholds to combine 21 difference-in-differences (DiD) analyses across the sample states. Each DiD compares workers in a window below and above the state-specific threshold before and after the onset of the Covid-19 pandemic to estimate the excess mass below the threshold.

benefits from December 26, 2020, to March 14, 2021. Please consult Online Appendix C.1 for more details on FPUC and subsequent programs.

[23]Formally, FPUC is paid to all individuals on UI and on partial UI benefits. The qualifying criteria for these benefits vary by state and for our sample states these criteria always include an earnings test.

[24]Monitoring was especially week during the first weeks of the pandemic when unemployment offices prioritised processing the major inflow of claims and executing a variety of new programs. Authorities also had an incentive to allow people to stay home to reduce the spread of infections.

Figure 2: Notch Point by State



Note: The figure shows maximum allowable earnings while receiving FPUC payments for maximum weekly benefit (MWB) recipients across US states.

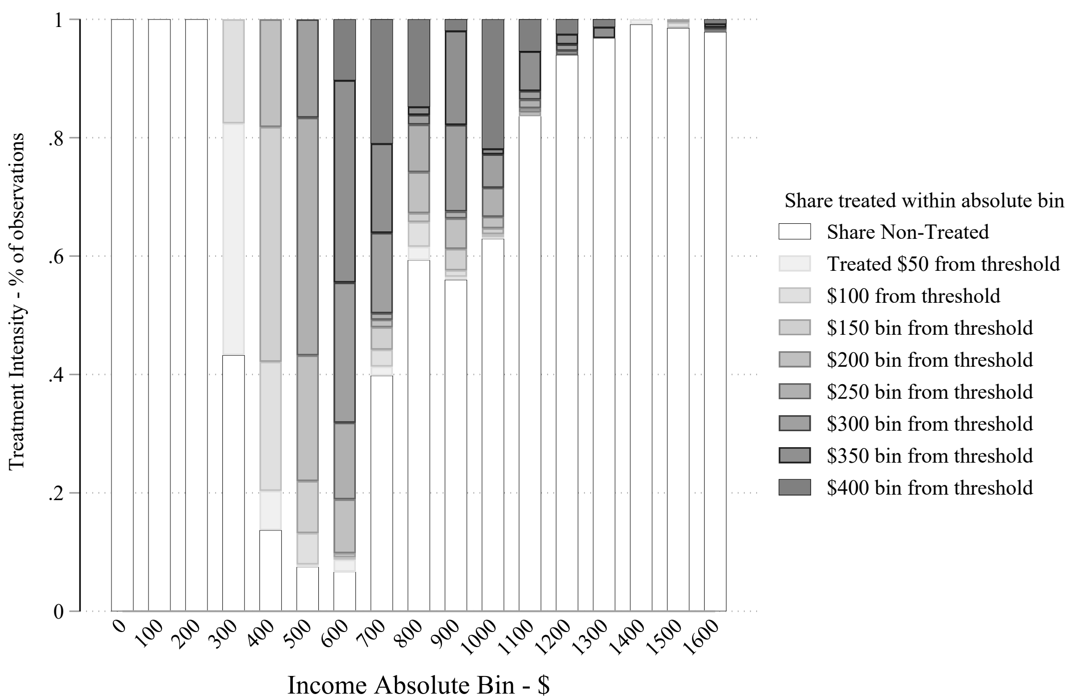
We estimate excess and missing mass with the following DiD specification:

$$E_{wtmk} = \pi^{tm} + \sum_{k=-650}^{1300} \beta^k \cdot I_k + \sum_{k=-650}^{1300} \eta^k \cdot I_k \cdot C_t + \varepsilon_{wtmk} \tag{10}$$

where E_{wtmk} is a dummy with value 1 if a workers' w earnings are in range m, in week t, \$$k$ away from the UI eligibility threshold, and C_t is an indicator with value 1 after the onset of Covid-19 pandemic. The coefficients are η^k, β^k, π^{tm}. β^k captures the excess or missing mass around the eligibility threshold *before* Covid, and η^k captures the same *after* the onset of the pandemic and is the main parameter of interest. π^{tm} are fixed effects that capture changes in the aggregate earnings distribution and vary by \$100 bins of earnings (m) and before/after the onset of the Covid-19 pandemic (t). The remaining identifying variation comes from whether earnings fall to the left or the right of the local FPUC eligibility threshold. Instead of a single eligibility indicator, we use finer dummies that capture the distance to the eligibility threshold (k). Theory would predict that responses are starkest close to the eligibility threshold and weaker further away from the threshold. I_k is an indicator that takes value 1 if a workers' earnings are in a \$50-wide bin \$$k$ away from the UI eligibility threshold. Given that π^{tm} controls for absolute earnings levels, η_k captures differences in the behavior of individuals with identical earnings, say \$300, but on different sides of the eligibility thresholds.

This strategy goes beyond a standard DiD regression that controls for spurious aggregate fluctuations with simple time fixed effects. Such fixed effects, however, would only capture aggregate fluctuations that affect all groups equally. One may worry that the recession had different impacts to the left and right of the threshold and generated spurious changes in the distribution. To avoid such effects, we interact time fixed effects with \$100 earnings bins and flexibly control for shocks that affect particular parts of the earnings distribution. Introducing such granular controls is feasible because the same earnings bin is treated in some states but not in others. The analysis thus compares individuals with identical earnings who happen to fall on different sides of their respective state's eligibility threshold. Figure 3 provides graphical intuition for the variation leveraged in our analysis: it plots the share of treated workers in each \$100 bin of earnings and dis-aggregates "treatment" into intensity bins I_{wtk} , that capture the distance (\$50, \$100 ... or \$400) from the state-specific earnings threshold. The figure illustrates that there is much variation in treatment status for individuals with identical absolute earnings, which allows for an identification strategy that compares the behavior of individuals with identical earnings facing different financial incentives. This empirical setting can therefore relax the identifying assumption of canonical bunching designs, which rely on spurious shocks being smooth through the cutoff. In this setting, a shock that violates the identifying assumption would have to affect specific earnings ranges and different ones in different states in ways that correlate with the state-specific thresholds.

Figure 3: Identifying variation

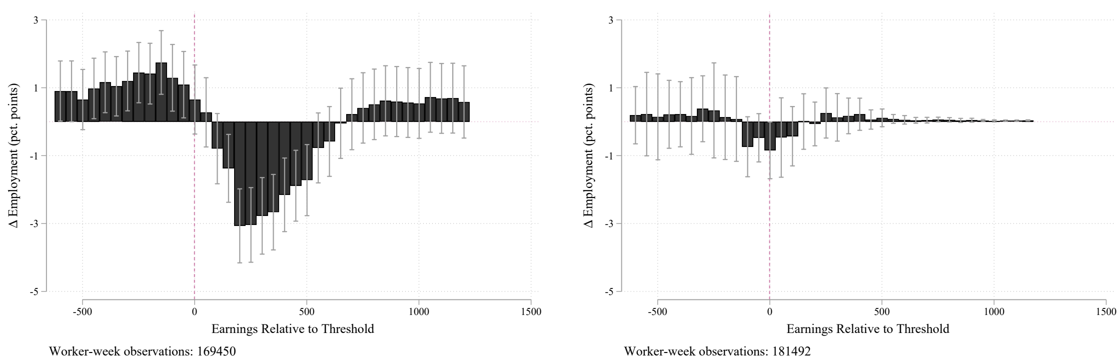


Note: The figure shows the variation in the treatment exposure for individuals with the same earnings. The share of workers subject to the financial incentive (treated) is shown in dark colors and the share of those not subject to it (not-treated) is shown in white. The x-axis shows earnings in $100 bins. The treatment is disaggregated by treatment intensity, i.e. earnings levels close to the relevant state-specific eligibility earnings threshold are shown in lighter grey.

The first set of results show the excess and missing mass around the eligibility threshold. Figure 4 plots η_k for each $50 bins around state-specific eligibility thresholds after the onset of the pandemic. We define these earnings bins relative to the threshold and normalize the threshold bin to zero. Positive values indicate earnings above the state-specific threshold and negative ones below the threshold. Panel A shows that the launch of FPUC after the onset of the pandemic created strong incentives to move earnings below the eligibility threshold. We see a large missing mass in earnings ranges that make workers ineligible for FPUC and excess mass below the eligibility threshold. The share of workers in bins above the threshold declines 3 percentage points for the bin with the largest drop (i.e., the bin "threshold+$250"), which corresponds to a 33% decrease in frequency relative to a baseline frequency of 9% in that bin.

Figure 4: Excess and Missing Mass around the Partial UI Notch

(a) FPUC Eligible Workers **(b)** Placebo Sample: Ineligible Workers



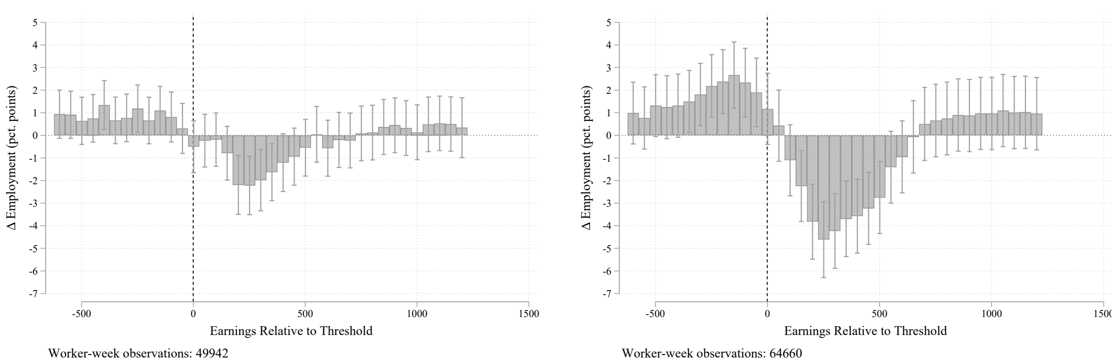
Note: The figure shows η_k coefficients from equation (10). Standard errors are clustered at the state, earnings bin, and week level, and 95 percent confidence intervals are reported. The sample in panel (a) is hourly workers with sufficient past earnings to qualify for MWB payments in their home state (169,450 worker-week observations). The baseline mass in the most affected bin is around 9%. The sample in panel (b) is hourly workers with insufficient past earnings to qualify for MWB payments in their home state (181,492 worker-week observations). For these workers, the threshold should not be relevant. Source: Homebase.

The pattern around the threshold broadly aligns with labor supply intuition. Workers are most likely to respond when they are close to the threshold, and there is a smaller response for workers who would need to reduce their earnings a lot. The corresponding excess mass below the threshold spreads out over a broader earnings range and peaks for earnings close to, but a bit further below the threshold. At first, it may seem surprising that excess mass appears over a broader range and not just at a single point right at the threshold. One plausible reason are adjustment frictions in work hours. Earnings responses can be spread out over a broader range if workers' shifts are indivisible and workers have to drop entire shifts to reduce their earnings levels. We test whether such frictions explain a weaker bunching pattern by splitting the sample into industries with more or less flexible scheduling policies.

Data on scheduling flexibility comes from the American Time Use Survey (ATUS) Leave Module questions on workers' ability to choose their shifts' start and end times. We indeed find that excess mass is sharper and much larger for industries that have more flexible schedule policies (Figure 5). This confirms that the non-sharp nature of the excess mass is partly due to the effective ability of workers to adjust hours. Additional potential factors that may explain any remaining non-sharp excess mass in industries with high hour flexibility are measurement error in how we calculate the partial UI eligibility earnings threshold[25] and workers dropping entire working shifts (e.g. giving up entire 8-hour work-shifts) to move below the FPUC-eligibility threshold, so that they cannot target the threshold earnings amount precisely.[26]

Figure 5: Excess and Missing Mass by Hour Flexibility

(a) Least Flexible Quartile **(b)** Most Flexible Quartile



Note: The figure estimates excess mass patterns in industries where workers have more or less flexibility in choosing work hours. Information on flexibility comes from the 2017-2018 ATUS data. We calculate the average ability to frequently adjust work hours at 2-digit NAICS industry-level. Panel A shows the bottom 25% and Panel B the top 25% of the distribution of schedule flexibility. The sample covers hourly workers with sufficient past earnings to qualify for MWB payments in their home state. Source: Homebase.

We next explore whether spurious shocks in the labor market could explain the

[25]When determining the partial UI threshold, differences in observed earnings and UI relevant earnings arise in some jurisdictions from allowances for families, special circumstances, or multiple jobholders.

[26]Notable also is the excess mass (albeit non-significant) starting at around $600 above the threshold, which might be due to within-firm labor market adjustments: to allow workers reducing their supply to drop entire shifts, those who are far enough from the threshold and hence are not incentivized by it, might have to pick up some hours.

observed pattern. We test this possibility using a placebo design based on individuals for whom the FPUC eligibility threshold does not bind. The placebo sample is composed of either workers ineligible for FPUC or workers facing a different threshold, and for simplicity we refer to them as "ineligible workers". These workers share the same labor market shocks but have no incentives to respond to the benefit eligibility thresholds we study. We can thus check if there are spurious shocks that generate the observed patterns around the thresholds. The results are shown in Figure 4 (b), which plots the behavioral response around the eligibility threshold for ineligible workers. The effects are insignificant and small in magnitude, confirming that there are no spurious shocks. Indeed, this test rules out many alternative explanations for the observed bunching patterns – the pattern only exists for workers who face the eligibility threshold.

3.3 Workplace Risk and Change in Excess Mass

We next study how the excess mass at the FPUC eligibility threshold varies in response to fluctuations in workplace risk driven by Covid-19 waves. We use several complementary identification strategies, exploiting variation based on (pre-determined) task characteristics and panel variation within an individual's work spell.

Note that these specifications use only variation in risk *after* the onset of the pandemic, and therefore the results are orthogonal to the broader pandemic-related changes in workplaces that occurred at the start of the pandemic and remained in place during our sample period. In other words, the analysis does not capture the impact of some of the major changes in workplace amenities at the start of the pandemic (e.g. mask wearing and social distancing). The estimates will, however, capture the impact of other amenities that vary due to changing Covid risk levels (e.g. changes in customer behavior). For most policy purposes, this combined parameter is indeed the parameter of interest since policies cannot vary these factors in isolation.[27]

Our first identification approach takes advantage of the fact that tasks in some workplaces are particularly vulnerable to Covid-19 exposure. Workers with close interpersonal contact, like hairdressers, experience larger shocks to workplace risk than workers with less interpersonal contact, such as landscape gardeners. We estimate excess mass separately for such groups and show that industries with tasks particularly vulnerable to Covid-19 exhibit more excess mass at the FPUC threshold. The risk scores are computed by combining information on tasks' risks developed by Basso et al. (2021)[28] with American Community Survey data on the distribution of occupations and tasks across 3-digit industries. Our risk index is the product of each task's riskiness and the frequency of the task in the industry.[29] An advantage of this task-based measure is that it predicts potential Covid-19 risks based only on pre-Covid-19 task content and is therefore pre-determined with respect to workers' decisions and local Covid-19 induced demand shocks.

To estimate industry heterogeneity, we summarise the excess mass shown in Figure 4 in a single coefficient for each industry that captures the average excess/missing mass within a $400 treatment window around the threshold.[30] Figure 6 shows that a higher workplace risk is associated with larger responses to the FPUC threshold and more excess mass, consistent with a negative amenity shock.[31] These results are highly significant, with excess mass increasing by 0.51 percentage points for a standard deviation increase in risk. The differential excess mass helps quantify the

[27]For instance, policies aimed at reducing workplace risk on construction sites are always bundled with minor changes in other disamenities, such as wearing a helmet, harness, etc.

[28]Basso et al. (2021) use O*NET data to compute task-specific risk measures based on proximity to others at work and the possibility of working remotely. The risk scores are reported at the occupation level and we compute industry averages for the lowest-digit industries available in the ACS (mostly 3 and 4 digit) by taking an employment-weighted average of occupational risks in each industry.

[29]We compute the riskiness at the industry level rather than the occupation level because our worker data only includes industry information.

[30]Results with alternative treatment windows are reported in Appendix A2.

[31]The omitted industry is real estate services.

amount of money workers leave on the table to avoid risk and thus identifies worker WTP for workplace safety.[32]

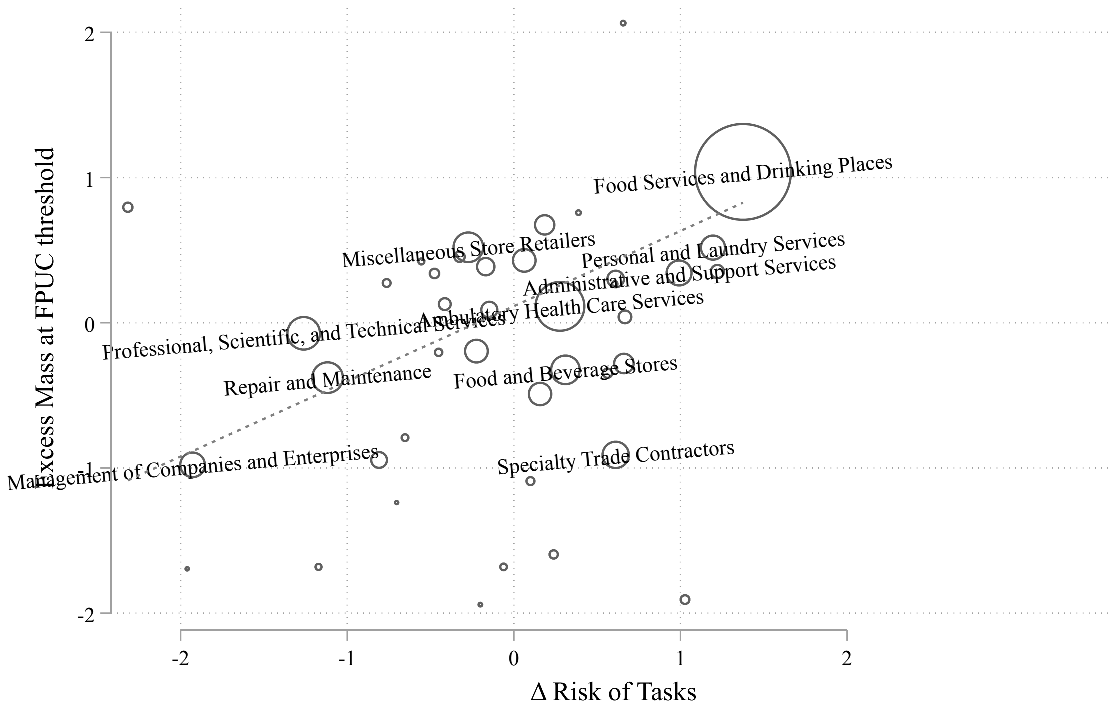
The identification assumption in this first simple analyis is that workers would respond similarly to notches if they face similar health shocks, similar to the assumption in Sorkin (2018) that requires homogenous preferences across workers. Imposing such a homogenous preference assumption also ensures identification here. However, the approach in this paper can identify the WTP under the weaker assumption of orthogonality of preferences and workplace safety shocks. Several features of the shock help make this plausible. Covid risks arose unexpectedly and workers with high/low taste for workplace safety have not sorted into jobs with high/low risk shocks, making the shock plausibly orthogonal to such preferences. Moreover, we check and confirm that the variation is uncorrelated with greater labor supply elasticities measured by industry before Covid-19. In what follows, we also present an alternative within-worker design that holds preferences constant and delivers very similar estimates.

A second empirical strategy uses a richer measure of workplace risk, combining the previous time-invariant, pre-determined industry risk score with time-varying data on local outbreaks. A key advantage of this strategy is that it provides us with panel data, allowing us to track changes in behavior for the *same individual* under changing risks. By construction the identification assumption that preferences are unrelated to risk holds in within-individual designs. The panel approach thus further strengthens the identification strategy. Denote the time-invariant risk score for industry i by P_i and denote the local fatality rate in the c' neighbor counties of c by $R_{c',t}$.[33] The product of these two components yields a time-varying, county- and industry-specific

[32]It is also noteworthy that the variation in workplace safety explains half of the variation in excess mass at the FPUC threshold across industries (the R^2 of the regression is 0.56), suggesting that concerns over workplace safety were an important determinate of labor supply behavior during Covid-19.

[33]Note that we focus on fatality rates – rather than infection rates – to measure risks because of a lack of reliable infection data during the first months of the pandemic.

Figure 6: Effect of Workplace Safety on Labor Supply – Task Risk Proxy



Note: The figure shows the amount of excess mass at the FPUC threshold for 3-digit NAICS industries, relative to the omitted industry (real estate services, NAICS 531). The y-axis shows the excess mass generated by the FPUC eligibility threshold in industry i relative to the omitted industry. The x-axis is based on the riskiness of tasks used in industry i, using the data on Covid-19 risk in specific tasks from Basso et al. (2021) standardized to have a standard deviation of 1. Industry titles are shown for the ten largest industries and for display purposes we only show industries with at least 1,000 observations. The size of the markers corresponds to the number of observations in the industry and regressions are weighted by this number. The fitted line has a slope coefficient of 0.51 and an $R^2 = 0.56$ Source: Homebase.

workplace risk measure:

$$\theta_{ict} = R_{c't} \cdot P_i \tag{11}$$

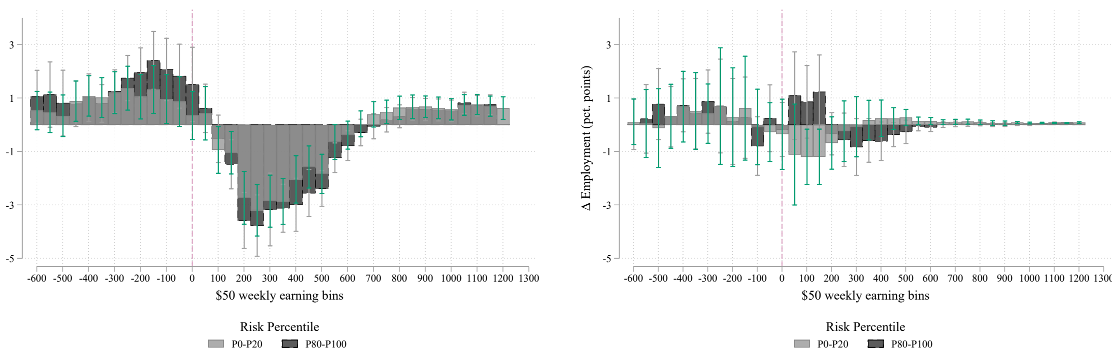
θ_{ict} is zero for all industries in county c if there are no local outbreaks, increases with local outbreaks, and does so proportionally more for more vulnerable industries. $R_{c',t}$ proxies local outbreaks with neighboring counties' fatality rates (c') to avoid potential reverse causality issues that could arise from mass outbreaks at local employers.[34] Since $\theta_{i,c,t}$ has no natural units, we normalize this variable to start at 0 and have a

[34]The results are similar if we use the local fatality rates c instead, or if we use R_{ct} without the interaction with industry risks (Appendix E.3).

standard deviation of 1, so that treatment can be read in terms of standard deviations.

Figure 7: Excess and Missing Mass around the Partial UI Notch for Low and High-Risk Settings

(a) FPUC Eligible Workers **(b)** Placebo Sample: Ineligible Workers



Note: The figure shows η_k coefficients from equation (10) for the highest and lowest quintiles of Covid-19 risk (θ_{ict}). The gray bars represent the response in the lowest quintile and the black bars in the highest quintile. The sample in panel (a) covers hourly workers with sufficient past earnings to qualify for MWB payments in their home state and is based on 169,450 work-week spells. The sample in panel (b) has 181,492 work-week spells and covers hourly workers with insufficient past earnings to qualify for MWB payments in their home state. For these workers, the threshold should not be relevant. Source: Homebase.

We replicate the graphical evidence in Figure 4 but separately for the highest and lowest health risk quintile. Figure 7 (a) shows the response in the lowest risk quintile in grey and the highest risk quintile in black. Both the excess mass and the missing mass are magnified in the expected bins around the threshold in high-risk settings: the excess mass rises at earnings levels just below the threshold and the extra missing mass occurs in the bins above the threshold, which is consistent with the magnified labor supply response illustrated in Figure 1b. We repeat the previous placebo test with ineligible workers and again find no evidence of spurious shocks near the thresholds (Figure 7b).

Finally, we estimate the WTP for workplace safety using equation 9. This requires an estimate of excess mass under baseline risks (shown in Figure 4) and the change in excess mask when risk varies (shown in Figure 7). To obtain a point estimate that summarizes the visual patterns of the figures, we interact a dummy that captures an aggregate \$400 treatment window T_k[35] with the continuous risk variable θ_{ict} from equation (11) and estimate the following triple interaction specification:

$$E_{wtmkic} = \pi_{tm} + \delta \cdot T_k \cdot C_t \cdot \theta_{ict} + \boldsymbol{X\beta} + \varepsilon_{wtmkic} \tag{12}$$

with δ the impact of higher risk levels on the amount of excess mass at the threshold, C_t and T_k are the Covid-19 period dummy and the \$400 treatment window. $\boldsymbol{X}$ is a vector of pairwise interactions and single variable entries of T_k, C_t, and θ respectively, while $\boldsymbol{\beta}$ is the associated vector of coefficients.

The results from this regression are shown in Table 2. Panel A shows results for the denominator of equation (9) (excess mass at baseline), while Panel B shows the numerator (changes in excess mass when workplace risks increase). Panel A estimates excess mass at average risk levels and shows that FPUC creates an excess mass of around 0.86 percentage points in earnings bins around the threshold. Panel B shows that this excess mass increases by 0.26 percentage points for a standard deviation increase in risk.[36] Combining these results ($= \frac{0.26}{0.86} \times 100$) implies the willingness to pay for a standard deviation of risk is 30.3% of weekly earnings, or around \$187 in weekly earnings for the median earner in our sample (Panel C).

The sizable magnitude of the WTP partly reflects the large changes in risks we study. A standard deviation of $\theta_{i,c,t}$ corresponds to an increase in fatality rates by 33.65 fatalities per million workers.[37] In terms of fatality rates, the estimates imply

[35] In practice, we replace the granular bin dummies I_k in equation (10) with a categorical variable (T_k) that takes value 0 outside the ±\$400 treatment window, and inside the window takes value 1 below the threshold (excess mass), and value -1 above the threshold (missing mass). We use the term *excess mass* for the sake of simplicity, however the coefficient of T_k captures both excess and missing mass effects.

[36] In Figure A2 we test the sensitivity of our DiD estimate to changing treatment windows around the threshold. Our estimate is statistically significant if we consider a window of \$150 around the threshold. We identify only a subset of the response if we focus on a narrow window: once the window is \$250 or bigger, the effect is very stable.

[37] Data on weekly local industry-specific death rates are not available. Therefore, we rely on county/week death counts ($D_{c,t}$) and compute the death counts in each industry by apportioning the deaths to industries based on time-invariant fatality rates in industries and based on the employment

Table 2: Willingness To Pay for Workplace Safety

	(1)	(2)	(3)	(4)	(5)
		Panel A: Baseline Excess Mass			
FPUC	0.858	0.858	0.858	0.858	0.858
	(0.096)	(0.010)	(0.010)	(0.096)	(0.010)
		Panel B: Additional Excess Mass			
FPUC $\times$ Workplace Risk (std. dev.)	0.260	0.234	0.232	0.254	0.230
	(0.053)	(0.052)	(0.052)	(0.052)	(0.052)
		Panel C: WTP (% of weekly income)			
Workplace Risk (std. dev.)	30.3	27.3	27.0	29.6	26.8
Workplace Risk - structural est.	34.4	31.0	30.7	33.6	30.5
Workplace Risk (deaths per mio.)	0.90	0.81	0.80	0.88	0.80
	Panel D: Value of Statistical Life (million \$)				
VSL (perfect information)	\$ 5.56	\$ 5.01	\$ 4.95	\$ 5.43	\$4.91
VSL (worker beliefs)	\$ 7.97	\$ 7.18	\$ 7.10	\$ 7.78	\$7.05
income x time FE	yes	yes	yes	yes	yes
interaction of income x time FE x FE for		state	county	industry	individual

Note: The Table shows how Covid-19 risk affects excess mass at the FPUC eligibility threshold. Panel A shows excess mass around the FPUC threshold for average risk. Panel B shows δ estimates from equation (12) and captures how excess mass changes with fatality rates. Willingness to pay in Panel C is based on equation (9), and is the ratio of panel B and panel A estimates. The structural estimation row additionally uses an estimate of labor supply elasticity of $e = 0.25$ and the average FPUC eligibility threshold $m^* = 409$. Panel D computes $VSL = \frac{WTP*m}{\Delta fatality}$, where m is median earnings ($m = \$617$), and $\Delta fatality$ is one standard deviation of workplace risk increases fatality rates by 33.65 cases (perfect information), or 23.46 cases (worker beliefs) per million workers. Controls are state, county, and two-digit NAICS fixed effects, interacted with a dummy for the Covid-19 period and a continuous earnings variable. The results are based on 169,450 worker-week spells. Source: Homebase, Chen et al. (2021).

that workers are willing to pay around 0.9% ($= \frac{30.3}{33.65}$) of their earnings to cut weekly fatality risks by one in a million (Panel C). Note that these changes in workplace

share of the industry ($\frac{l_{i,c}}{\sum_i l_{i,c}}$). For example, a worker in an industry with twice the fatality rate gets a weight of 2, and we thus assign twice as many deaths to the industry relative to the average industry. This exercise requires data on industry-specific fatality rates (ρ_i). Such data are not available at the national level and we instead use data from California, for which such rates are published by Chen et al. (2021). Employment counts come from the ACS 2014-2018. Combining all these steps, our proxy for local industry-specific fatality rates is $R_{i,c,t} = D_{c,t} \frac{l_{i,c} \cdot \rho_i}{\sum_i l_{i,c} \cdot \rho_i}$

risks are large compared to the magnitude of risks workers face in normal times. The most deadly occupation in normal times is fishing and hunting, with a fatality rate of 28 cases per million workers per week.[38] A one standard deviation increase in our Covid risk measure is thus similar to moving from a zero-risk occupation to one of the riskiest occupations in non-Covid times.

The previous placebo test indicates that our results are not driven by spurious shocks and we confirm this again by introducing controls. Specifically, we interact the control for demand shocks $\pi_{m,t}$ with either state (column 2) or county fixed effects (column 3). Such controls absorb the potential impact of location-specific policies, such as local lockdowns or school closures. The remaining identifying variation in $\theta_{i,c,t}$ comes from cross-industry heterogeneity in risk within the local area. The results are similar to our baseline results.

Finally, we return to the assumption that worker preferences are uncorrelated with our risk shock and use the panel feature of the data to probe this assumption. We introduce *time varying* worker (or industry) fixed effects by interacting the fixed effects with a dummy for the start of the Covid-19 pandemic (columns 4 and 5 of Table 2). These specifications are slightly more sophisticated than standard individual fixed effect regressions and control not only for workers time-invariant characteristics but also for characteristics with time-variant effects and absorb, for example, differences in labor supply elasticities — and thus different responses to FPUC — across workers (or industries). These specifications absorb differences in ability or willingness to start bunching at start the Covid-19 pandemic and identify the impact of $\theta_{i,c,t}$ from the remaining *within* worker (or industry) variation in risk over time. The specifications thus hold workers' willingness or ability to *adjust* work hours fixed.[39] These

[38]Source: BLS Census of Fatal Occupational Injuries (CFOI). The standard deviation of fatality rates across occupations is 4 in a million per week.

[39]The theoretical section showed that adjustment frictions do not affect the theoretical predictions. Such frictions, nevertheless, could affect empirical results if frictions differed between workers exposed to high and low risks.

within-worker results are again close to the baseline and suggest that heterogeneity in worker characteristics (or constraints) are not driving the results. For peace of mind, we additionally check whether constraints on hours flexibility are correlated with our industry-specific risk measure and find a very low and insignificant correlation (correlation coefficient of 0.057), suggesting that such issues are unlikely to substantially bias our results.[40]

So far, the WTP calculation used the approximation in equation (9). This is a lower bound to the true WTP, and we can obtain the corresponding structural parameter to assess whether the lower bound is close to the true parameter. This exercise requires estimates of the labor supply elasticity e and the value of m^*. We parameterize these values based on the meta-study by (Chetty, 2012), which finds $e = 0.25$, and the average threshold in our setting $m^* = \$409$. Using indirect inference, we find that our WTP approximation of 30.3 corresponds to a structural WTP of 34.4 (Panel C). The lower bound is thus around four percentage points below the structural parameter and illustrates that the approximation provides a relatively tight lower bound for the true structural parameter.[41]

3.4 Further Robustness Checks

We address any lingering concerns about the impact of spurious effects of deteriorating economic conditions. Because states use different eligibility thresholds, we can implement a border design. This design narrows in on the counties at state borders, where different partial UI thresholds apply but arguably demand conditions are similar (see Appendix E.1). We also examine whether rising excess mass could be explained by employers becoming more willing to let workers adjust their hours

[40] Data on hours flexibility are computed using American Time Use Survey (ATUS) - 2017-2018 special Module on job flexibility. And we define hours flexibility as the share of workers that report frequently choosing their work start and end hours.

[41] See Appendix D.1 for more details about bounds on structural parameters.

when demand softens. We add controls for demand variation at the local level and allow these to have different effects around our thresholds.[42] The results remain virtually unchanged (see Appendix E.2). Finally, we consider the possibility that the labor supply reaction is driven by the increased childcare responsibility rather than by health risk. Controling for local school closures (Parolin and Lee, 2021a,b) again has little effect on the results (see Appendix E.2). All these checks confirm that other shocks are orthogonal to our threshold design.

In Appendix E.4 we also discuss the robustness of our estimates to different sample selection strategies and to the inclusion of an extensive labor supply margin. We relax the work-week restriction and extend the analysis to less-attached workers. The resulting estimates for the willingness to pay remain very close to the baseline estimate (29% of weekly earnings instead of 30%). Finally, estimates obtained using alternative approaches to consider extensive margin responses in the analysis range between 23% and 26% of weekly earnings, slightly smaller, but in the ballpark of our baseline estimates.

3.5 Comparison with Hedonic Wage Regressions

To compare this novel WTP method to a canonical hedonic wage regression, we run a hedonic regression on the same data and regress hourly wages on our measure of workplace risk. Individual fixed effects control for time-invariant worker ability and ensure that selection effects do not bias these results. We find that wages are broadly unchanged by workplace risk, and the point estimate is insignificant. The estimate is also quantitatively small and suggests that wages increased by 11 cents with one standard deviation of $\theta_{i,c,t}$, which corresponds to a 0.5% wage increase (results are available upon request). Interpreted through the lens of a hedonic regression, these estimates would lead us to conclude that workers attach next to no value to workplace

[42] Controls include employment, business revenues, and the number of open businesses at the week and county level from https://tracktherecovery.org/ by Chetty et al. (2020a,b).

safety. However, another explanation for the small coefficient is that wages are slow to adjust (despite several high-profile examples of hazard pay). Wages are thus unlikely to fully price in changes in workplace risk, highlighting a key challenge of the canonical hedonic regression approach. Our approach, by contrast, does not assume that wage setting is fully flexible. We find that workers respond substantially to workplace risks and find a WTP that is two orders of magnitude greater than the hedonic result.

3.6 Value of a Statistical Life

A popular approach for quantifying responses to health risks is to compute a "value of a statistical life" (VSL), which infer the implicit value of life from observed responses to risks. Such estimates typically assume that individuals know and understand their exposure to risk and that the fear of dying is the sole driver of the observed behavior. Since higher fatality rates are typically accompanied by unpopular safety measures and by risks of non-fatal injuries, this assumption effectively imposes that workers attach zero value to such non-fatal aspects. Under these assumptions common to the VSL literature, we can compute VSL as the ratio of WTP (in absolute dollars) to the change in fatality risk: $VSL = \frac{WTP * m}{\Delta fatality}$ with WTP being our main estimate from Table 2 column 1 and m the median earnings in our sample. Using our estimates, we find $VSL = \frac{0.303 * \$617}{33.65/1{,}000{,}000} = \$5.56$ million (Panel D of Table 2). A value of \$5.56 million broadly aligns with the literature, a recent meta-study by Viscusi (2018) concludes that VSL is somewhere between \$3 and \$13 million (in 2020 USD). Our results align with these findings and lean towards the lower side of this range.

The main purpose of this exercise is to benchmark our WTP method and illustrate that it produces reasonable results. However, we also have a unique opportunity to assess the importance of the perfect information assumption. Ideally, researchers would relax the perfect information assumption and compute $VSL = \frac{WTP}{E[\Delta fatality]}$, where $E[\Delta fatality]$ is the workers' perception of fatality risk. Since these perceptions are not usually observed, studies instead use the statistical fatality rates as a proxy for perception, thereby imposing perfect information and rational expectations assumptions.[43] During the Covid-19 outbreak, beliefs about fatality risks were collected as part of the Understanding America Study (UAS), which allows us to relax the perfect information assumption.[44] Our approach can be thought of as an instrumental variable approach that instruments fatality beliefs with our risk measure. Adjusting the VSL estimate for the perception data ($E[\Delta fatality]$), the VSL value increases to \$7.97 million (Panel D of Table 2). Accounting for imperfect information thus increases the VSL estimate by nearly 50%, highlighting the importance of the popular assumptions underpinning VSL calculation.[45]

3.7 Workplace Safety Policy

Our results relate to the broader policy debate about workplace safety regulations (Leigh, 2011; Lee and Taylor, 2019; Johnson, 2020; Johnson, Levine, and Toffel, 2022). In the US, worker safety is overseen by the Occupational Safety and Health Administration (OSHA) and the degree of enforcement of health rules has been controversial. Assessing this issue has been challenging, in part because it is difficult to quantify the gains from such non-wage regulations. The rationale for policy interventions is however similar to minimum wage regulation: in imperfect competition, firms may not fully internalize the cost of high-risk jobs and thus may expose workers to excessive risks. In practice, all governments implement some level of worker safety regulation,

[43] Frequent violations of these assumptions are famously documented in Kahneman and Tversky (1979).

[44] The data covers a representative sample of the US population and uses weekly rounds of interviews. Individuals were asked about their probability of contracting Covid-19 and conditional on this, their probability of dying. We use this data to compute expectations at the week-state-industry level and then use these to impute expectations for our sample. The expectation measure thus undoubtedly includes measurement error.

[45] It is unclear whether individuals are particularly poorly informed in our context. On the one hand, we study an event with enormous press coverage that was almost certainly salient to everyone. On the other hand, there was substantial uncertainty around the risks of Covid-19.

albeit with large differences in stringency and enforcement. In the design of such policies, the monetary value of improved workplace safety plays a central role and determines the welfare gains from such policies.

Our results suggest that workers value workplace safety highly and that more stringent safety regulations provide substantial gains to workers. To illustrate this point, we perform three back-of-the-envelope calculations. The first quantifies the hazard pay required during Covid-19 to make workers indifferent between working when exposed to Covid risk vs when not. Our results imply that the utility cost of working under an increase of Covid risk by one standard deviation would require an offsetting hourly wage increase of \$4.8. This is larger than the wage change we observe in practice (about 11 cents) and also larger than reported hazard rates at large retailers which top out between \$2 and \$4. A substantial part of the increased cost induced by Covid-19 workplace risk was thus not priced into wages. Second, we turn to the construction industry, one of the largest industries with substantial workplace risk. Weekly fatality rates in this industry in the US are 3 workers per million full-time employees per week, while comparable estimates for Germany and the UK are respectively 0.4 and 0.7 weekly deaths per million workers.[46] Our estimates imply that reducing US fatality rates to the level seen in the UK or Germany would provide substantial gains to workers, valued equivalently to a wage increase of 2.2%. Such gains happen to be similar in magnitude to the average wage gains from the introduction of a \$15 minimum wage in the industry, a popular labor market intervention proposal.[47] Finally, we consider the gains implied by switching between industries with different risk levels. Such an exercise helps to evaluate the potential of compensating differentials to explain the dispersion of wages. The gains from greater safety by changing from the construction sector to the safer accommodation and food

[46]ILO data is converted to weekly deaths per million workers for comparison. Annual fatality rates are 160 per million workers in 2018. Source: ILOSTAT, series "INJ FATL ECO RT A" 2018.

[47]The minimum wage calculation computes the wage floor that is equivalent to a 2.2% mean wage increase (assuming no employment loss). The data source is the 2019 and 2020 CPS ASEC data.

services sector are worth around 2.5% of earnings, while moving to the riskier agricultural sector is equivalent to a wage loss of 8%. The magnitude of these gains is comparable to the value of other work amenities analyzed by Maestas et al. (2018), who find values ranging from 2% to 16%.[48]

When generalizing our estimates to non-Covid-19 workplace risk, we need to consider that the WTP for a non-transmissible illness or injury might be lower. Our WTP estimate could partly reflect workers internalizing the risk of Covid-19 transmission to others. The higher the weight workers place on others' health in their utility function, the more likely our estimate represents an upper bound for non-transmissible workplace risk. Conversely, in the canonical case of self-interested individuals, who only care about their own utility, the WTP for transmittable and non-transmittable health risks coincide.[49] To get a sense of the importance of the pro-social feature in our WTP estimate, we perform a back-of-the-envelope calculation for a worker who cares about the well-being of other household members in Appendix F. The exercise suggests that pro-social concerns make up less than 1% of the estimated WTP and the concern for one's own health is the main component of the WTP estimate.

4 The Value of Enjoyable Jobs

A strength of the WTP approach is that it can be used widely for different types of amenities. To illustrate this, we present a second case study that estimates the value of enjoyable work. Enjoyment of work scores are widely collected in labor market surveys and provide information on the perceived quality of work. Yet, it is hard to interpret categorical enjoyment scores without a money metric for these scores. Work

[48] Maestas et al. (2018) study the value of schedule autonomy, telecommuting, physical activity, sitting, relaxed work environment, work autonomy, PTO, teamwork, training, and opportunity to serve.

[49] The prior literature almost exclusively considers self-interested agents when interpreting risky behavior of individuals.

enjoyment can be affected by various factors and captures an aggregate (net) value of amenities in a given job. This value of "good" and "bad" jobs has been central in labor economics (for an overview, see Lavetti (2023)).

The empirical strategy analyzes bunching around the U.S. early retirement age threshold. Workers accumulate social security entitlements for each quarter worked and once they reach age 62 the marginal value of additional quarters changes, creating a kink in the lifetime budget constraint.[50] At age 62 individuals also become eligible to claim retirement benefits, potentially alleviating liquidity constraints. We restrict the sample to individuals with sufficient savings to delay retirement and exclude people with less than a year's income in savings to mitigate the impact of the liquidity channel. We then study how bunching at the 62 age threshold differs for workers in high and low-enjoyment jobs. An important limitation relative to the workplace safety application is that we lack panel data and now use cross-sectional data comparing individuals with different job enjoyment. To interpret this heterogeneity, we must ensure that both groups of workers would behave similarly if they held similarly enjoyable jobs (the homogenous preferences assumption of Sorkin, 2018, is again a sufficient assumption).

The analysis uses data from the US Health and Retirement Survey (HRS) between 1992 to 2018. Figure 8 plots retirement rates per quarter and shows that there is substantial bunching at the age 62 threshold.[51] The figure plots retirement rates separately for workers in enjoyable and less enjoyable jobs.[52] During ordinary quar-

[50]Each additional month worked beyond 62 increases the retirement benefit by 0.4%-0.6% until individuals reach the full-benefit retirement age (65-66 depending on birth year). The rewards for working extra months beyond this age are 0. 5% - 0. 8%. It is important to note that only convex kinks generate bunching. While in principle, it is ambiguous whether the kink in the lifetime budget constraint at age 62 is convex (the answer depends on the replacement rate, life expectancy, and the discount rate), in our context, an overwhelming majority of individuals do face a convex kink, and we, therefore, treat the kink as convex.

[51]We restrict the sample people who were in the workforce before turning 60. The figure shows the share of this restricted sample retiring each quarter.

[52]Enjoyment is measured in the previous year. If data is missing (12% of cases) we use the next closest year we have data. High enjoyment are people who strongly agree with the statement, "I

ters, around 2% of satisfied workers retire, while this rate spikes to around 10% in the quarter they turn 62 (Panel A of Table 3 shows excess mass estimates). Using these estimates in the traditional bunching framework implies that workers who enjoy their job retire (0.1-0.02)/0.02=4 quarters early because of the kink. The opportunity cost of reducing work time is smaller for individuals who enjoy their work less, and as predicted in our model, they indeed respond more to the kink. Their excess mass jumps to 15%, implying that workers in less enjoyable jobs retire (0.14-0.02)/0.02= 6 quarters earlier and reduce their retirement age by 2 quarters more than workers who *are* enjoying their jobs (see Panel B). Using these results in the WTP formula from above, we find that an enjoyable job is worth an extra (6-4)/4 /4 = 12.5% of annual income (see Panel C). On average, a worker would thus accept a 12.5% wage cut to move from a less enjoyable job to a more enjoyable one.[53]

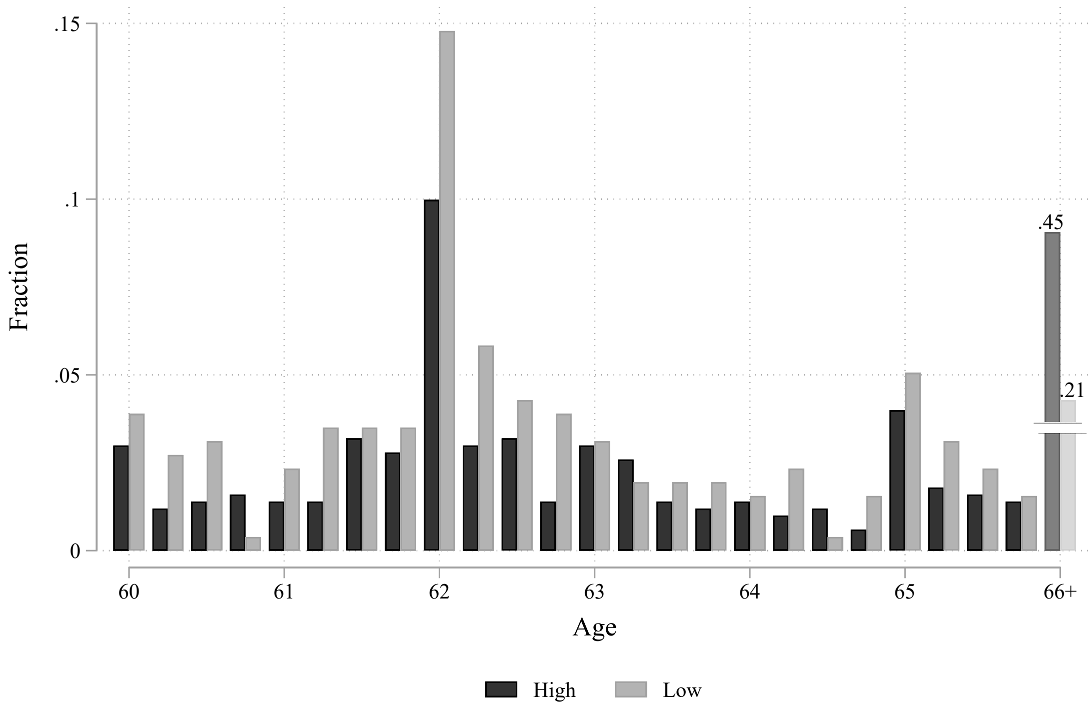
We probe the importance of confounders that could give rise to spurious differences in retirement patterns. We interact all controls with age to allow for different retirement patterns across the demographic groups. The first strategy introduces industry fixed effects to absorb industry-wide retirement practices and exploits variation in enjoyment *within* a given industry. The results remain similar to the baseline (see Table 3, Column 2). Similarly, adding proxies for health has little impact on the results, suggesting that our threshold design is orthogonal to variation in health (column 3). Next, we add several proxies for human capital. Adding occupation-fixed effects again has little impact on the results (column 4). Proxies for education and location also have little impact on the results (columns 5 and 6).[54]

really enjoy going to work." Workers who disagree or strongly disagree are coded as not enjoying their work.

[53]We are not aware of a directly comparable estimate. Work that estimates the importance of non-wage amenities for inequality found that amenities explain between 15% and 26% of inequality (Lavetti (2023); Taber and Vejlin (2020); Sorkin (2018)).

[54]A further potential concern is inflated bunching at the eligibility threshold from delayed retirement reporting. Individuals have little incentive to report a retirement age before the age of 62 to the Social Security Administration since it would not lead to additional benefits, potentially resulting in a spike in reports at age 62. Instead of admin data reports, we use survey reports on the age people stop working, which is less likely to suffer from target-date reporting problems.

Figure 8: Retirement Age by Work Enjoyment



Note: The figure shows the share of people retiring at any given age among people who had not retired by age 60, separately for those with high and low work enjoyment before retiring. The last bar shows the share of people who had not retired by age 66.

5 Conclusions

This paper presents a new revealed-preference method to estimate the value of non-wage amenities based on bunching in the earnings distribution around budget discontinuities in response to varying amenities. This approach formalizes the idea that workers will be less responsive to financial incentives when non-wage amenities are more valuable. We apply this method to measure the value workers attach to safe workplaces and the comprehensive value of enjoyable jobs.

To estimate the value of safe workplaces, our identification leverages a budget constraint notch created by the launch of FPUC extra UI benefits in March 2020. We find substantial baseline workers' reaction to this notch and show that these

Table 3: Effect of Work Enjoyment on Retirement

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: High Work Enjoyment			
Excess Mass at Age 62	0.079	0.078	0.079	0.078	0.079	0.079
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
			Panel B: Low Work Enjoyment			
Excess Mass at Age 62	0.119	0.118	0.119	0.119	0.119	0.120
	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
			Panel C: WTP for Enjoyable Work			
% of income	0.125	0.127	0.125	0.130	0.126	0.127
FE, interacted w/ age		industry	health	occupation	location	education
Observations	18192	18096	18192	18120	18179	18168

Note: The table shows how work enjoyment affects the likelihood of retiring at age 62 (first three months), which is when workers become eligible for social security benefits. The baseline specification (1) shows results from a pooled OLS regression on a balanced quarterly panel of 758 individuals aged 60-65. Only people who had not retired by 60 are included. We exclude people with less wealth than annual income before age 60. The dependent variable is an indicator for the age of retirement. Panel (A) shows the additional mass retiring at the threshold age among people with high enjoyment of work, and panel (B) among those with low enjoyment. Columns (2) to (6) show the results when controlling for industry, general health level, occupation, region-division of residence, and years of education. All specifications include a linear age control and a dummy for retiring at 66 or later.

labor supply responses increase during periods of heightened Covid-19 risks, creating magnified excess mass. The estimates imply that workers are willing to sacrifice 30% of their weekly earnings to decrease their risk by one standard deviation. This is equivalent to giving up 9% of earnings to avoid a 1 in 100,000 risk of dying. These estimates are two orders of magnitude larger than canonical hedonic wage regressions, a difference that is likely driven by the slow adjustment of wages in response to shocks. Our novel framework is designed to provide unbiased estimates of the value of workplace amenities even if the perfectly competitive wage-setting assumption of the compensating differentials theory fails.

This flexible approach can also be used to estimate the bundled value of amenities and quantify the value of "enjoyable" jobs. Our second empirical application exploits the budget discontinuity created by the US early retirement age threshold and the differential excess mass at the threshold for workers who enjoy their jobs and those who do not. The estimates imply that workers are willing to pay 12.5% of their earnings to work in jobs they enjoy.

The revealed-preference framework for estimating the value of non-wage amenities developed in this paper has several advantages and disadvantages relative to other existing methods (e.g. those based on stated preferences). It does require empirical features (a budget discontinuity and changing amenities) that might not be readily available for every application, but has the advantage of being applicable to existing surveys or administrative data and does not require running ad-hoc survey experiments. It can also be flexibly applied to the estimation of WTP for specific or bundled amenities.

We hope this novel method will expand the set of empirical tools available to researchers interested in estimating non-wage amenities, which constitute a large and increasing part of workers' compensation, as signalled by the prominent role these amenities are playing in the discussions around the changing nature of work, from the gig economy to work-from-home and the "Great Resignation".

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Online Appendix for the manuscript

"Willingness to Pay for Workplace Amenities"

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A Online Appendix Figures

Figure A1: Scheduling App Screenshot

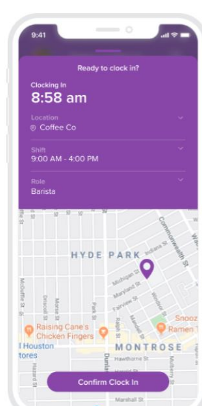
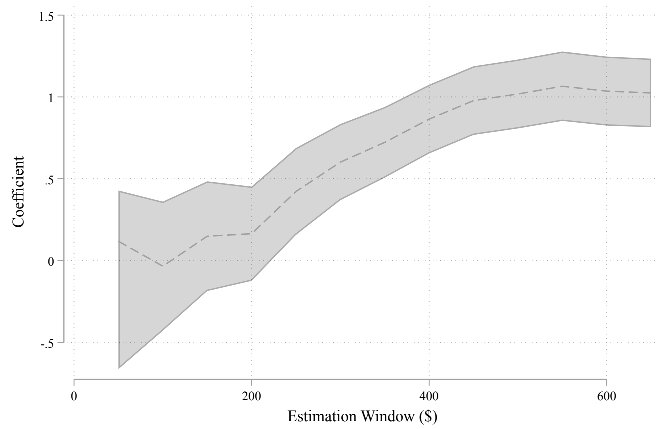


Figure A2: Effect of FPUC with Alternative Treatment Windows



Note: This figure shows results for equation (12) with alternative treatment windows T_k. The horizontal axis refers to the width of T_k to the left and right of the threshold.

B Homebase Data Benchmarking

We compare the Homebase data and our analysis sample to the characteristics of the labor force from the Annual Social and Economic (ASEC) supplement to the Current Population Survey (CPS) and the Quarterly Workforce Indicators (QWI). Table A1 presents summary statistics for wages, weekly earnings, and hours worked and Table A2 lists the distribution of observations by 2-digit NAICS sectors in these data.

Homebase provides 6 digit NAICS codes but ASEC does not provide an industry classification that uses NAICS. Therefore, to allow for comparability of ASEC to the Homebase sample, the industry classification in ASEC is first crosswalked to NAICS using the crosswalk provided by IPUMS.[1] Next, the ASEC sample is restricted to Homebase NAICS codes in a step-by-step manner: if an ASEC industry is linked to a

[1]See "IND AND INDNAICS: CODES FOR INDUSTRY (IND) AND NAICS INDUSTRY (INDNAICS) IN THE 2000 CENSUS AND THE ACS/PRCS SAMPLES FROM 2000 ONWARD" `https://usa.ipums.org/usa/volii/indtoindnaics18.shtml` and "ATTACHMENT 9: INDUSTRY CLASSIFICATION: Industry Classification Codes for Detailed Industry (4 digit) (Starting January 2020)" `https://www2.census.gov/programs-surveys/cps/methodology/Industry%20Codes.pdf`.

6-digit NAICS code, it is classified as being in the Homebase sample only if it matches a 6-digit Homebase code, and it is classified as not in the sample if it does not match any 6-digit Homebase code. Next, if an ASEC industry is linked to a 5-digit NAICS code, it is classified as in the Homebase sample if it matches the first 5 digits of a 6-digit Homebase NAICS code. This process is repeated until all ASEC NAICS codes are classified, and the resulting crosswalk is used to restrict ASEC in column (4).

Table A1: Summary Statistics: Hourly Wages, Weekly Earnings, and Hours Worked

	(1) ASEC Full	(2) HB Full	(3) ASEC HB States	(4) ASEC Sample	(5) HB Sample	(6) QWI Sample
Hourly wage	18.69	12.37	18.46	16.68	18.35	
	(10.84)	(4.858)	(10.66)	(9.101)	(8.146)	
Weekly earnings	1016.7	381.8	999.6	631.8	660.0	805.5
	(724.4)	(245.5)	(716.1)	(432.3)	(345.1)	(328.4)
Hours usually worked per week at all jobs	39.25		39.32	35.78		
	(11.30)		(11.11)	(11.00)		
Hours usually worked per week at main job	38.55	30.03	38.66	35.13	36.49	
	(10.84)	(13.26)	(10.69)	(10.60)	(12.78)	
Hours worked last week	38.45		38.49	34.84		
	(12.80)		(12.63)	(12.06)		

Note: Mean coefficients and standard errors are in parentheses. ASEC and HB Full data include 2019 and 2020. QWI data is from 2019 only. Column (3) ASEC is restricted to the 21 HB states. Column (4) ASEC sample is restricted to hourly workers, who are not self-employed, working in small businesses (< 25 employees) in a HB state and industry. Column (5) HB sample is restricted to individuals eligible for full UI benefits (defined as meeting state-specific earnings requirements in previous quarters) with a balanced number of week spells before and after the onset of the Covid-19 pandemic. Column (6) QWI sample is restricted to privately owned small firms (< 20 employees) in HB states. Weekly earnings are calculated from the average monthly earnings (divided by 4.345) of the beginning-of-quarter employment. Source: Homebase, ASEC, QWI.

C Institutional Details

C.1 FPUC

Federal Pandemic Unemployment Compensation (FPUC), the weekly $600 supplement to unemployment benefits, was introduced by the CARES act enacted on March

Table A2: Distribution of Observations by NAICS 2

	(1) ASEC Full %	(2) HB Full %	(3) ASEC HB States %	(4) ASEC Sample %	(5) HB Sample %	(6) QWI Sample %
11 Agriculture	1.52	0.33	1.60	2.29	0.30	2.20
21 Mining	0.50	0.00	0.55	0.11		0.32
22 Utilities	0.84	0.00	0.86	0.41	0.00	0.11
23 Construction	7.06	1.45	7.40	15.41	4.32	11.05
31–33 Manufacturing	9.82	0.72	9.67	2.86	1.40	4.88
42 Wholesale Trade	2.16	0.00	2.21	0.60		4.58
44–45 Retail Trade	10.43	13.53	10.66	10.74	16.06	10.20
48–49 Transportation	4.82	1.04	4.92	4.90	1.87	2.60
51 Information	1.82	0.43	1.78	0.80	0.22	1.16
52 Finance & Insurance	4.73	0.18	4.34	1.38	0.34	2.95
53 Real Estate	2.04	0.31	2.15	1.47	1.15	3.17
54 Professional Services	8.00	2.20	8.04	5.98	4.68	10.59
55 Management	0.09	1.34	0.10		3.14	0.26
56 Admin. & Support	4.31	1.02	4.61	6.93	2.79	5.50
61 Education. Services	9.25	1.50	8.78	4.16	1.80	1.54
62 Health Care	13.60	4.75	13.09	14.45	13.10	16.18
71 Arts, Entertainment	2.30	3.78	2.27	3.15	3.58	2.08
72 Accommodation & Food	7.29	62.20	7.50	16.13	37.90	11.74
81 Other Services	4.78	5.14	4.88	7.10	7.13	8.90
92 Public Administration	4.64	0.04	4.56	1.13	0.21	0.00

Note: ASEC and HB Full data include 2019 and 2020. QWI data is from 2019 only. Column (3) ASEC is restricted to the 21 HB states. Column (4) ASEC sample is restricted to hourly workers, who are not self-employed, working in small businesses ($<$ 25 employees) in a HB state and industry. Column (5) HB sample is restricted to individuals eligible for full UI benefits (defined as meeting state-specific earnings requirements in previous quarters) with a balanced number of week spells before and after the onset of Covid-19 pandemic. Column (6) QWI sample is restricted to privately owned small firms ($<$ 20 employees) in HB states, and only beginning-of-quarter employment.

27, 2020, and ended on July 31, 2020.[2] No FPUC benefits were payable between July 31, 2020, and December 26, 2020. FPUC was re-established by the Continued Assistance Act as a $300 per week supplement to unemployment benefits from December 26, 2020, to March 14, 2021.[3] American Rescue Plan Act extended FPUC through

[2]U.S. Department of Labor news release dated April 4, 2020.

[3]U.S. Department of Labor news releases dated December 30, 2020, and January 5, 2021.

September 6, 2021.[4] Any individual eligible to receive at least $1 of state unemployment benefits was also eligible to receive federally-funded FPUC for that week. Individuals who were working part-time and fulfilled state eligibility requirements for partial UI benefits were also eligible to receive FPUC payments.[5]

During the gap in FPUC payments, from August 1, 2020, Lost Wages Assistance (LWA) program was funded through Federal Emergency Management Agency (FEMA). States had the option of choosing between two weekly benefit amounts, $300 or $400, with different cost-sharing requirements.[6]

FPUC and LWA together supplemented weekly unemployment benefits in the following periods depending on eligibility: $600 (FPUC) from March 28, 2020, through July 31, 2020; $300 (LWA) or $400 from August 1, 2020, through the week ending September 5, 2020 (week ending August 22, 2020, in Florida); gap between September 5, 2020 and December 26, 2020; and $300 (FPUC) from December 26, 2020 through September 6, 2021, with some states ending the program early.[7]

C.2 Eligibility for Partial Unemployment Insurance

The $600 FPUC benefit was received by all workers on Unemployment Insurance or Partial Unemployment Insurance (i.e. who reduced their hours worked or are working a limited amount of hours while on unemployment), hence by every worker with earnings below the threshold determining the access to Partial Unemployment Insurance. Between March and July of 2020, individuals crossing this earnings threshold exhausted all remaining UI benefits and forwent the $600 FPUC benefit. Crucially

[4] U.S. Department of Labor news release dated March 16, 2021.

[5] Attachment to Unemployment Insurance Program Letter No.15–20, Change 1, U.S. Department of Labor, dated May 9, 2020.

[6] U.S. Department of Labor news release dated August 12, 2020, Lost Wages Supplemental Payment Assistance Guidelines.

[7] Unemployment Insurance Program Letter No. 14–21, U.S. Department of Labor, dated March 15, 2021.

for identification, this threshold differs across the 21 US states. Table A3 shows the threshold for each state in column 5, as we calculated it based on State-specific UI eligibility rules reported by the Department of Labor (DOL) for the year 2020 in their document titled "The Comparison of State Unemployment Insurance Laws".[8]. In most states, an individual is considered partially unemployed in some week if working less than full-time with earnings less than the weekly benefit amount or less than a percentage of, or less than a multiplier of the weekly benefit amount. Since we do not observe the actual UI benefits in our sample, we focus our analysis on workers who have an earnings history that makes them eligible for maximum UI benefits. In columns 1 and 2 of Table A3 we thus report, from Table 3-5 of the DOL document, the maximum UI weekly benefit amount (WBA) allowed in each state. In three states, the maximum WBA is slightly higher for individuals with dependence. For these three states, we consider the higher maximum WBA as a reference for our calculations. In columns 3 and 4 we report, from Table 3-8 of the DOL document, the maximum amount of labor market earnings allowed to retain eligibility for partial UI benefits and the earnings to be disregarded when this maximum amount is calculated.[9]. In column 6 of Table A3 we report how we have processed the information provided by the DOL to calculate the Partial UI thresholds of column 5. In Michigan, Washington and Wisconsin, eligibility for partial UI benefits is also conditional on workers reducing hours below a certain amount of hours per week. Considering the sample of workers under analysis, in most cases reducing earnings to an amount below the earnings threshold also corresponds to meeting the hour condition. For instance, consider the case of the 32 maximum weekly hour threshold for Wisconsin: given an average hourly wage of $18 and the $500 earnings threshold, a worker would work 27

[8] Available at https://oui.doleta.gov/unemploy/comparison/2020-2029/comparison2020.asp

[9] All states disregard some earnings as an incentive to take part-time or short-term work.

hours a week, well below the 32-hour condition. Therefore, at the cost of a potential small amount of measurement error, we focus only on the earnings threshold to determine FPUC eligibility also for these three states. During the first month of Covid-19 emergency, Georgia has temporarily increased the earnings amount disregarded for the calculation of the Partial UI threshold. We have considered this temporary change relative to the DOL document in our calculation.

Table A3: State-specific eligibility rules for access to partial UI benefits

	(1)	(2)	(3)	(4)	(5)	(6)
State	**Max WBA ($)**	**Max WBA with dependence ($)**	**Definition of Partial UI. Earnings less than:**	**Earnings Disregarded**	**Threshold ($)**	**Calculation**
Alabama	275		WBA	⅓ WBA	367	Max WBA + ⅓*Max WBA
Arizona	240		WBA	$30	270	Max WBA + Earnings Disregarded
California	450		WBA	Greater of $25 or ¼ of wages	600	Max WBA/0.75
Colorado	561	618	WBA	¼ WBA	773	1.25*Max WBA with dependence
Delaware	400		WBA + greater of $10 or ½ WBA	Greater of $10 or ½ WBA	800	Max WBA + 2*0.5*Max WBA
Florida	275		WBA	8 x Federal hourly minimum wage	333	Max WBA + 8*7.25
Georgia	365		WBA	$50	715	Max WBA + Earnings Disregarded + $300
Louisiana	221	284	WBA	Lesser of ½ WBA or $50	334	1.5* Max WBA with dependence
Michigan	362		1.6 x WBA	For each $1 earned, WBA is reduced by 50 cents (benefits and earnings cannot exceed 1 3/5 WBA)	434	0.6*Max WBA/0.5

Missis.pi	235		WBA	$40	275	Max WBA + Earnings Disregarded
Missouri	320		WBA + $20 or 20%WBA, whichever is greater	$20 or 20% WBA, whichever is greater	448	Max WBA + 0.2*Max WBA
North Carolina	350			20% WBA		
Oregon	648		WBA	Greater of $120 or $\frac{1}{3}$ WBA	864	Max WBA + Max WBA/3
Penns.nia	561	569	WBA + 40% WBA	Greater of $21 or 30% WBA	967	1.4*Max WBA with dependence + 0.3*Max WBA with dependence
South Carolina	326		WBA	$\frac{1}{4}$ WBA	408	1.25*Max WBA
Tennessee	275		WBA	Greater of $50 or $\frac{1}{4}$ WBA	344	Max WBA + Max WBA/4
Texas	521		WBA + greater of $5 or $\frac{1}{4}$ WBA	Greater of $5 or $\frac{1}{4}$ WBA	782	Max WBA + 2*Max WBA/4
Virginia	378		WBA	$50	428	Max WBA + $50
Wash.ton	790		1 $\frac{1}{3}$ WBA + $5	$\frac{1}{4}$ wages over $5	1.414	(1.33*Max WBA + $10)/0.75
West Virginia	424		WBA + $61	$60	545	Max WBA + $60 + $61
Wisconsin	370		500	$30 plus 33% of wages in excess of $30	500	No benefits are payable if weekly earnings exceed $500.

D Model Extensions

D.1 WTP Approximation and Bounds

Here we show that the approximation in (9) holds exactly in the case of kinks and provides a tight lower bound for notches.

First, consider the case of a regression kink design, where the marginal tax rate increases by Δt at m^* and will show that equation (9) holds. Recall that the definition of labor supply elasticity is $e = \frac{m^o - m^*}{m^*} / \frac{\Delta \tilde{t}}{1-\tilde{t}}$, where $\tilde{t}$ is the implicit tax rate $t + \theta W$. We derive an expression for W by evaluating the elasticity in two risk scenarios with $\theta_L = 0, \theta_H$. Assuming that risks are smooth at the threshold, we can use the ratio of the two elasticity expressions to obtain:

$$1 = \frac{m_L^o - m^*}{m_H^o - m^*} \frac{1-t}{1-t-\theta_H W} \tag{13}$$

Next, we can prove the claim by re-arranging this expression and using the definition of $WTP(r) = \frac{rW}{m^*(1-t)}$:

$$WTP(r) = \frac{m_H^o - m_L^o}{m_H^o - m^*} \tag{14}$$

Next consider the case of notches. Here the approximation in equation (9) provides a lower bound estimate of the true WTP. To see this, recall that $WTP(r) = 1 - \frac{\frac{m_L^o}{m^*}\gamma_L - 1}{\frac{m_H^o}{m^*}\gamma_H - 1}$. The approximation result sets $\gamma_H = \gamma_L = 1$. The difference between such an approximation and the true WTP can be approximated by:

$$\begin{aligned}
\Delta_{approx} WTP(r) &= \frac{\partial WTP(r)}{\partial \gamma_L} d\gamma_L + \frac{\partial WTP(r)}{\partial \gamma_H} d\gamma_H && (15)\\
&= -\frac{\frac{m_L^o}{m^*}}{\frac{m_H^o}{m^*}\gamma_H - 1} d\gamma_L + \frac{m_H^o}{m^*} \frac{\frac{m_L^o}{m^*}\gamma_L - 1}{(\frac{m_H^o}{m^*}\gamma_H - 1)^2 d} d\gamma_H && (16)\\
&= \frac{-\frac{m_L^o}{m^*}(\frac{m_H^o}{m^*}\gamma_H - 1)d\gamma_L + \frac{m_H^o}{m^*}(\frac{m_L^o}{m^*}\gamma_L - 1)d\gamma_H}{(\frac{m_H^o}{m^*}\gamma_H - 1)^2} && (17)
\end{aligned}$$

In order to show that the approximation is a lower bound, we want to sign this expression and show that it is negative. First note that the denominator is positive and we can therefore focus on the sign of the numerator to sign the overall expression. We will take the check-and-verify approach:

$$-\frac{m_L^o}{m^*}(\frac{m_H^o}{m^*}\gamma_H - 1)d\gamma_L + \frac{m_H^o}{m^*}(\frac{m_L^o}{m^*}\gamma_L - 1)d\gamma_H < 0 \tag{18}$$

and re-arranging:

$$\frac{m_L^o - m_H^o}{m^*}d\gamma_L + \frac{m_H^o}{m^*}(\frac{m_L^o}{m^*}\gamma_L - 1)(d\gamma_H - d\gamma_L) < 0 \tag{19}$$

Consider the two terms separately. The first term has two components. $m_L^o < m_H^o$ implies that the $\frac{m_L^o - m_H^o}{m^*}$ is negative. Moreover, we can show that $d\gamma_L$ is positive. The approximation sets $\gamma_L = 1$, and hence $d\gamma_L = 1 - \gamma_L$. Using the fact that $\gamma_L < 1$ proves that $d\gamma_L > 0$. The first term is therefore negative.

The second term has three components. The first two components are both positive: $\frac{m_H^o}{m^*} > 0$ because $m > 0$ and $(\frac{m_L^o}{m^*}\gamma_L - 1) > 0$ because $(\frac{m_L^o}{m^*}\gamma_L - 1) = \frac{B(1-t)}{m^*(1-t-\theta W)} \geq 0$. The sign of the final term therefore depends on the final component: $(d\gamma_H - d\gamma_L)$. Using $d\gamma_L = 1 - \gamma_L$ and $d\gamma_H = 1 - \gamma_H$ we can write this term as:

$$d\gamma_H - d\gamma_L = \gamma_L - \gamma_H = \frac{1}{1+e}\left[(\frac{m^*}{m_H^o})^{\frac{1+e}{e}} - (\frac{m^*}{m_L^o})^{\frac{1+e}{e}}\right] < 0 \tag{20}$$

where the last equality uses the definition of γ. We can sign this expression because $m_L^o < m_H^o$ and $e > 0$. Combining this result with the first term means that both terms in (19) are negative and hence:

$$\Delta_{approx} WTP(r) < 0 \tag{21}$$

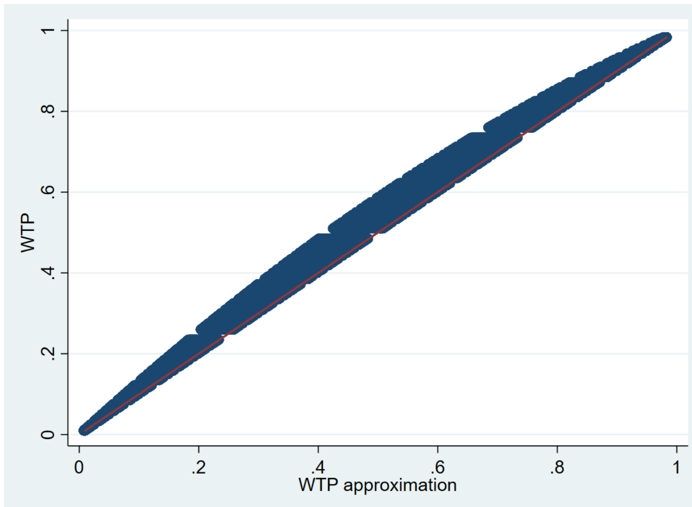
This shows that the approximation is always smaller than the true WTP and hence that the approximation is a lower bound for the WTP.

We now assess how tight this bound is and perform a simulation to compare the approximation to the true WTP for plausible parameter values.[10] In our FPUC case B is \$600 and we run simulations varying the tax rate t between 0 and 0.9, θW between 0 and 0.91, spanning the full range of plausible values. We let m^* vary from \$200 to \$1,000, covering the eligibility thresholds in our sample states. For the labor supply elasticity, there exists a range of estimates from a very large literature on this parameter. The meta-analysis by Chetty (2012) concludes that a plausible estimate is around 0.25. Since there is substantial disagreement about this parameter, we use a wide range between 0.02 and 0.92, which includes most estimates.

Figure A3 shows the results and compares the WTP approximation to the true WTP value. The dots are close to the 45-degree line, meaning that the approximation performs extremely well. As we proved above, the approximation provides a conservative, lower bound, estimate of the WTP and the approximation values are smaller or equal to the structural WTP. In addition, the results show that the lower bound estimate is always fairly close to the true WTP and the approximation thus provides a tight bound. The maximum bias occurs at a WTP of 50 percent of earnings (a fairly high WTP). In this case, the worst approximation estimates WTP to be around 41 percent, even this worst-case scenario thus still provides a very reasonable approximation. On average the bias is 3 percentage points and thus substantially smaller. Using the approximation therefore comes at relatively little cost, but has the major advantage that it allows the researcher to be agnostic about the size of the labor supply elasticity.

[10] Note that equation (17) provides a closed form solution for the bias and could be used to assess the magnitude of the approximation bias. But the equation is hard to interpret and we therefore perform a simulation.

Figure A3: Performance of WTP Approximation



Note: The figure shows the results of the WTP approximation from equation (9) in simulated data and plots the approximation and the true structural parameter. The 45-degree line presents the line of perfect fit. The fact that the true values lie above the 45-degree line reflects the fact that (9) provides a lower bound estimate. The simulation uses the following parameter ranges $m^* \in [200, 1000]$,$t \in [0, 0.9]$,$\theta W \in [0, 0.91]$, $e \in [0, 0.92]$.

D.2 Adjustment Frictions

A sizable literature discusses how adjustment frictions affect responses to budget discontinuities and proposes solutions to deal with such frictions (Chetty et al., 2011; Chetty, 2012; Kleven and Waseem, 2013; Einav, Finkelstein, and Schrimpf, 2017). In principle, any of these solutions could be applied to our setting. However, this is not required because our approach can handle frictions in a less parametric way and does not require correction methods that could be sensitive to assumptions (c.f., Einav, Finkelstein, and Schrimpf 2017).

First, consider the canonical friction case, where only a fraction α of workers can adjust their labor supply. This will reduce the excess mass (η) at the threshold relative to the frictionless benchmark, and η becomes: $\eta = \int_{m^*}^{m^o} d_0 = \alpha(m^o - m^*)d_0$. Now η depends on α and $(m^o - m^*)$, and multiple combinations of α and $(m^o - m^*)$ are consistent with the observed η. Note, however, that the impact of α cancels out in WTP estimates. We can re-write WTP in (6) as the ratio of excess mass in high

(η_H) and low (η_L) risk settings:

$$WTP \approx 1 - \frac{\eta_L}{\eta_H} = 1 - \frac{\alpha(\tilde{m}^o - m^*)d_0}{\alpha(m^o - m^*)d_0} = 1 - \frac{(\tilde{m}^o - m^*)}{(m^o - m^*)} \tag{22}$$

Thus, α affects both the numerator and the denominator proportionally and cancels out. The WTP estimate is thus unaffected by the presence of standard adjustment frictions.

More complex adjustment frictions arise from indivisible shifts, in which workers can add or drop entire shifts but cannot adjust their labor supply by the minute, or when workers negotiate hours with their employer and can only choose from a limited number of shift options. Both of these scenarios are isomorphic in the model and create two distortions that affect the excess mass at the eligibility threshold. First, workers are unable to adjust their labor supply exactly to the threshold earnings m^*, and instead have to reduce their earnings more to become eligible for $\mathcal{B}$. Second, some workers may be deterred from responding to the threshold because the indivisibility friction would force them to take a large earnings cut. Workers are thus less responsive to the threshold than in the frictionless benchmark.

Addressing the first challenge is relatively straightforward. The excess mass, η, now spreads over a wider earnings range. While it may be empirically more difficult to identify the spread out excess mass, such a spread-out mass does not pose any conceptual challenges to our approach.[11] In other words, the first challenge affects the estimation strategy but does not affect the link between the estimates and WTP. The second challenge can be addressed in a similar fashion as the canonical adjustment friction above. Denote the fraction of individuals who do not respond because of the indivisibility friction by $(1 - \alpha)$. If $(1 - \alpha)$ is constant, equation (22) applies again and implies that the WTP estimate is unaffected by this friction. Our framework

[11] Canonical bunching methods focus on excess mass right at the threshold and would fail to fully capture more spread-out excess mass.

thus identifies WTP, even if there are indivisibility constraints and hours decisions are not fully flexible.

D.3 Cobb-Douglas

Consider a case where utility is non-separable in health and cost of effort $U\left((T(m), \frac{m}{z}, a\right)$ (= $U\left(T(m), g(\frac{m}{z}, a)\right)$ and take the Cobb-Douglas case with $g(\frac{m}{z}, a) = m^{\alpha} h^{1-\alpha}$. The FOC becomes:

$$1 - t - \Delta t = (1-r)\alpha \left(\frac{a_0}{m}\right)^{(1-\alpha)} + r\alpha \left(\frac{a_1}{m}\right)^{(1-\alpha)} + \theta[m^{\alpha} a_1^{1-\alpha} -^{\alpha} a_0^{1-\alpha}] \qquad (23)$$

From $u(m^o, a_1) = u(m^o - W(m), a_0)$ we can derive an expression for a_1:

$$m^{\alpha} a_0^{1-\alpha} = W(m) + m^{\alpha} a_1^{1-\alpha}$$

Substituting this in equation (23) and simplifying yields:

$$1 - t - \Delta t - (1+\alpha)\theta W(m) = \alpha \left(\frac{a_0}{m}\right)^{(1-\alpha)}$$

Notice that the implicit tax imposed by the health risk increased by factor α relative to the separable case. This additional cost arises from the health effect on the marginal utility of leisure. A second change is that the marginal cost of a health shock increases the more a worker works ($m \uparrow$). And the value of health (W) now depends on the level of earnings m. This non-linearity in the cost of health shocks makes health risks operate like a non-linear progressive tax system, with increasing cost at higher m.

D.4 Income Effects

The canonical bunching approach uses quasi-linear utilities and thus assumes that there are no income effects. In many contexts where notches are small, the absence of income effects is plausible. Recent work, however, stresses that small notches may not be salient (Chetty, Friedman, and Saez, 2013). Moving to larger notches is thus attractive but leads to the added complication that such notches produce income effects. Structural estimates have previously used utility functions with income effects (Blundell, MaCurdy, and Meghir, 2007). Below we aim to cover a middle ground between the functional form flexibility of structural work and the quasi-experimental approach to identification of the bunching literature. We will show that introducing income effects implies that excess mass does not only appear at m^* but also at lower earnings levels.

Consider a general labor supply function that allows for income effects:

$$\tilde{m^o} = \tilde{z} + e\tilde{w} - \gamma\tilde{y} \tag{24}$$

$\tilde{x}$ indicates log values for x and w is the wage $\gamma\tilde{y}$ captures the income effect. When $\gamma = 0$ this equation collapses to the canonical quasi-linear utility case without income effects.

The introduction of a lump sum benefit $\mathcal{B}$ reduces labor supply if $\gamma < 0$. This effect changes the impact of the non-linear benefit schedule studied above. For a worker with earnings ε above the eligibility notch, introducing $\mathcal{B}$ reduces labor supply to $m^* + \varepsilon - \gamma\mathcal{B}$ which is below m^* if ε is small. The labor supply response thus creates excess mass below m^* and the excess mass at the notch point therefore does not fully capture the labor supply response. Hence, with income effects, excess mass (η) does not appear only at m^* but spreads out across a broader range of earnings. This creates additional identification challenges and we will return to the issue below.

The excess mass η is closely linked to the labor supply response of the marginal buncher. Individuals with pre-period earnings between m^* and the earnings of the marginal buncher $m^* + \Delta m$ make up the excess mass and η is thus given by:

$$\eta = \int_{m^*}^{m^*+\Delta m} d_0 dm$$

$$\Delta m = \eta / d_0 \tag{25}$$

where d_0 is the pre-notch earnings distribution between m^* and $m^* + \Delta m$, and to keep notation simple, we assume that d_0 is constant over this segment.[12]

To compute Δm we need to estimate d_0 and η. If data on the pre-notch distribution is available, we can compute d_0 directly from this data.[13] A second step is to estimate η, the extra mass generated by bunching individuals. η is the difference between the observed post-notch earnings distribution (d_1) and the distribution of non-bunchers (d_0'):

$$d_1 = \eta + d_0', \tag{26}$$

While we observe d_1, d_0' is not directly observed and needs to be estimated. Typically $d_0' \neq d_0$ and the pre-distribution does not provide a valid counterfactual. To see why, consider workers at m^* in the pre-period, they are below the eligibility threshold and thus part of the non-bunchers. Without income effects, their behavior would be unaffected by a lump sum benefit payment $\mathcal{B}$ and the pre-benefit distribution is a valid estimate for the frequency of this group. However, with income effects $\mathcal{B}$ reduces the labor supply of this group to $m^* - \gamma\mathcal{B}$ and no non-buncher is working at m^* after the introduction of $\mathcal{B}$. Now the pre-benefit distribution of earnings d_0 is a bad counterfactual for the distribution of non-buncher after the launch of $\mathcal{B}$

[12]This assumption simplifies notation but is not required and richer baseline distributions can be included in the estimation.

[13]Without data on the pre-period, d_0 can still be estimated with "untreated" earnings ranges away from the notch point. This requires estimating d_0 in such untreated earnings ranges and then extrapolating to earnings levels in the treatment range. The researchers will need to make an assumption about which earnings ranges are untreated, and this requirement of an ad-hoc assumption has been controversial (Blomquist et al., 2021). The presence of income effects worsens the problem. Bunching is more spread out with income effects and less sharp at the threshold, making it harder to define untreated earnings bins.

because $d_0'(m^*) = 0 \neq d_0(m^*)$. Using d_0 as counterfactual will bias the results, $d_0'(m^*) = 0$ implies that *all* individuals at $m = m^*$ are bunchers and the spike in density above neighboring cells ($\hat{\eta} = d_1(m^*) - \hat{d}_0(m^*) < \eta$) underestimates the true extend of bunching. Much of the debate about income effects focuses on the difference in compensated and uncompensated labor supply elasticities. It is important to note that the impact is more severe in the context of bunching estimates. Here, income effects not only affect the interpretation of the elasticity as (un)compensated but additionally bias the labor supply response estimate itself.

Valid estimates can be obtained with a difference in difference analysis. A first advantage of the difference-in-differences approach is that it can detect any deviations from the pre-notch distribution, not just spikes in one specific location. As we saw above, this is important with income effects. Additionally, the difference in difference approach can overcome the identification challenge created by $d_0' \neq d_0$. When leisure is a normal good ($\gamma < 0$), the introduction of benefits reduces labor supply among the non-bunchers. Note, that while the local distribution of m is changed, the total mass of non-bunchers below m^* is unaffected by the notch:

$$\int_0^{m^*} d_0' = \int_0^{m^*} d_0 \equiv \pi$$

Using this result in (26), we can show that the notch generates total excess mass:

$$\int_0^{m^*} \eta = \int_0^{m^*} d_1 - \int_0^{m^*} d_0$$

which is the difference in the total density below the notch before and after the notch reform. $\int_0^{m^*} \eta$ can be estimated in a difference in difference regression that compares the density below m^* before and after the introduction of the notch. In difference in differences notation:

$$Pr(I = m)_{t,m} = \phi \cdot 1[t > t^*] + \pi \cdot 1[m < m^*] + \bar{\eta} \cdot 1[t > t^*] \cdot 1[m < m^*] + \varepsilon_{t,m}$$

where t^* is the time of the reform, π is captured by the coefficient on the dummy $1[m < m^*]$. The coefficient $\bar{\eta}$ captures the average rise in density below m^*. Substituting this estimate into (25) yields the labor supply response of interest Δm.

The setting also yields an identification check in the spirit of a parallel trend check. This test is based on the distribution of the excess mass relative to the notch point. If the notch generates the excess mass, the excess mass should peak near the notch and decline as we move away from the notch. To test this, we estimate a specification similar to a dynamic DiD, and let the η coefficient vary across earnings ranges:

$$Pr(I = m)_{t,m} = \phi \cdot 1[t > t^*] + \pi \cdot 1[m < m^*] + \eta_m \cdot 1[t > t^*] \cdot 1[m < m^*] + \varepsilon_{t,m}$$

Plotting η_m provides a visual check on the assumption that the notch generates excess mass. The excess mass should peak at m^*, and its mirror image, missing mass, should peak above m^*. Finally, for m further from m^*, the effects should diminish.

Similar "difference in bunching" approaches have been used in the literature (Brown, 2013; Best et al., 2015), typically as a check on the identification assumption of canonical bunching estimators. In the set-up above we explicitly leverage the additional degrees of freedom to broaden the applicability of bunching methods to preferences with income effects.

D.4.1 Compensated Elasticity

The observed uncompensated labor supply elasticity reflects both an income and a substitution effect. The canonical bunching approach assumes that the latter is zero and that compensated and uncompensated elasticities coincide. In the more general case, we need to know the income effect γ to quantify the compensated elasticity from observed uncompensated elasticities. With this additional unknown parameter we require one additional moment condition. This section will show that the dispersion of excess mass away from m^* can be used as an extra moment condition. Without income effects all excess mass would arise at m^*, while the excess mass is more spread out over larger earnings ranges the bigger the income effect.

To derive a solution for γ, we take advantage of the location of the bunching. Note that all bunchers below m^* are at an interior solution, we call them "interior bunchers". At the earnings level m^*, there are several individuals who are at a corner solution and one individual for whom m^* is an interior solution; call this person the marginal buncher from the left. Before the notch the earnings of this person were $d_0 = m^* + p$. And using those two labor supply decisions in (24), we can show that:

$$m^* + p - \tilde{z} - e\tilde{w} + \gamma\tilde{y} = m^* - \tilde{z} - e\tilde{w} + \gamma(\tilde{y} + \mathcal{B})$$

$$\gamma = p/\mathcal{B}$$

We can thus solve for γ by deriving p. Notice that everyone with $d_0 \leq m^* + p$ is an interior buncher and the total mass of interior bunchers is thus:

$$I = \int_{m^*}^{m^*+p} d_0$$

The excess mass below the notch point (I) thus pins down p, e.g. with d_0 constant $p = I/d_0$. And using p, we can solve for $\gamma = \frac{I}{d_0\mathcal{B}}$. If all excess mass arises at the notch point then $I = 0$ and consequently $\gamma = 0$ and the analysis collapses to the quasi-linear case. This approach can thus be used to check the validity of canonical bunching estimates. But more powerfully, it can be used to identify labor supply responses from large and salient notches in budget constraints.

E Robustness Checks

E.1 Border Design

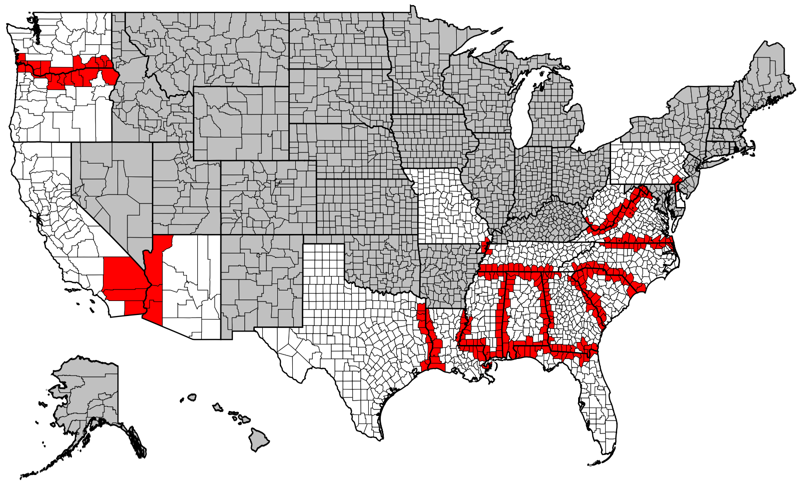
In this section we narrow our sample to counties along state boundaries, and thus with similar characteristics but facing different UI eligibility rules. The border counties are shown in Figure A4. These border communities generally have integrated labor markets and thus share many of the same demand shocks. In such a setting, empirical identification relies on comparing equally-paid workers across state borders with different incentives: such workers are likely to face similar demand shocks, however, one might be eligible for UI while the other might not, simply because of differences in the pre-determined exogenous eligibility thresholds. Our data is comprised of observations from 21 states, between which there are 24 state borders. In the border sample we exclude borders where we don't have data from border counties on both sides, which leaves us with the 17 unique state borders highlighted in Figure A4.

In the first step, we repeat the baseline analysis on the sample of border counties and find very similar effects to the baseline (Column 1 of Table A4). Next, we exploit the idea that neighboring counties experience similar demand shocks and allow all fixed effects to be specific to each border stretch. In practice, this implies that each border stretch is its own DiD experiment and we stack the 17 border DiDs into a single regression. The results are again close to our baseline estimates (Column 2).

E.2 Controls for demand shocks and school closures

In an additional robustness test, we add controls for demand shocks to our baseline specifications. Specifications that control for local employment, revenues of small businesses, business closures, school closures, combinations of these or all of these yield

Figure A4: Border Counties in Sample



Note: The figure shows counties along the state borders that are included in our border sample. There are in total 17 borders for which we have data from counties on both sides of the border.

Table A4: Excess Mass around UI Eligibility Threshold - Border Counties Sample

	(1)	(2)
Excess Mass (ptp)	0.965	0.914
	(0.146)	(0.146)
Interact income x time FE with		border IDs
Observations	20,596	20,596

Note: The Table reports results from equation (12). The border sample is restricted to counties at state borders shown in Figure A4. Source: Homebase.

results close to the baseline estimate (Table A5).[14] This provides further evidence that the estimation strategy is not confounded by changes in the state of the local economy.

[14] Employment and Small Businesses daily data are obtained from Chetty et al. (2020a), while the share of in-class instruction is obtained from Parolin and Lee (2021a)

Table A5: Robustness to Labor Demand Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Additional Excess Mass			
Workplace Risk	0.260	0.254	0.256	0.255	0.254	0.261	0.254
(std. dev.)	(0.0527)	(0.0523)	(0.0524)	(0.0523)	(0.0523)	(0.0527)	(0.0524)
Controls		# Employees	Small Business Revenues	Change in # merchants	Revenues X Merchants	Share of in-class instruction	All

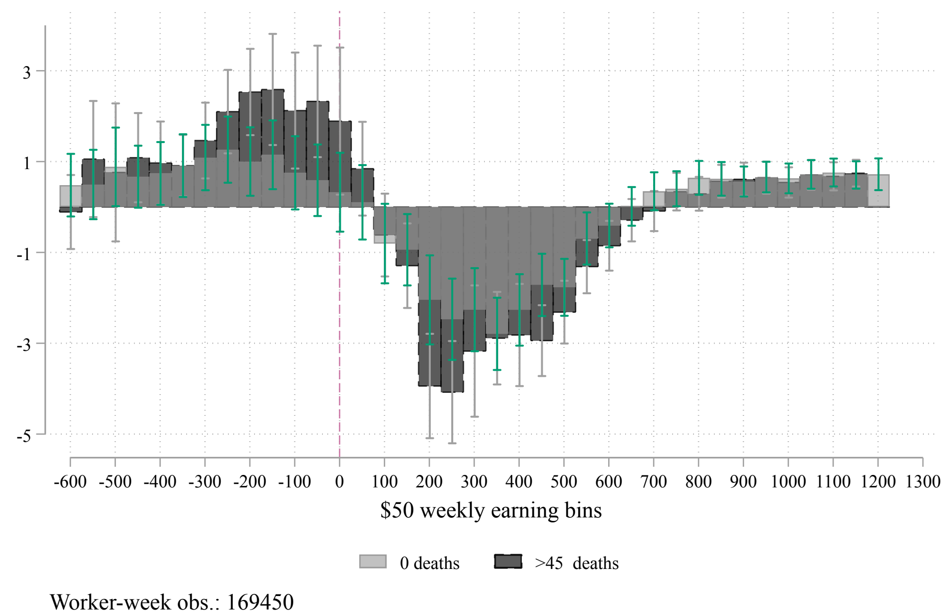
Note: Columns (2) through (7) supplement the main specification of Panel B of Table 2 (also presented in column (1)) by controlling for demand shock proxies, interacted with a dummy for the Covid-19 period and a continuous earnings variable. Column (2) controls for the number of active employees from Paychex, Intuit, Earnin and Kronos, varying at state-week-industry level. Column (3) controls for the percent change in net revenue for small businesses from Womply, varying at state-week-industry level. Column (4) controls for the percent change in the number of small businesses open from Womply, varying at state-week-industry level. Column (5) interacts the percent change in net revenue with the percent change in the number of small businesses from Womply. Employment, revenue and merchants data are downloaded from Opportunity Insights Economic Tracker. Column (6) controls for the share of in-class instruction from Parolin and Lee (2021), varying at county-month level. The share of in-class instruction is defined as the complement of the share of all schools in an area with at least 50% year-over-year decline in visitors, consistent with the Parolin and Lee definition. Column (7) controls for all demand shock proxies together. Sources: Chetty et al. (2020a); Chetty et al. (2020b); Parolin and Lee (2021a); Parolin and Lee (2021b).

E.3 Alternative Measure of Covid-19 Exposure

In this section, we estimate the labor supply response to the increase in workplace risk using an alternative measure of Covid-19 exposure $\theta_{i,c,t}$ and use the local Covid-19 fatality rate in the county, measured as in deaths per 1 million people.

In Figure A5, the excess/missing mass in grey represents the behavioral response to FPUC in counties with zero recorded new deaths and the black area represents the "magnified response" in very high-risk settings (more than 45 weekly new deaths per million people). The excess mass in these high-risk settings is visibly larger, consistent with the results presented in Figure 7.

Figure A5: Excess and Missing Mass around the Partial UI Notch – Fatality Rate in County



Note: The figure shows $\eta_{k,\theta}$ coefficients from equation (12) for the highest and lowest levels of Covid-19 risk ($\theta_{i,c,t}$). Different from Figure 7 $\theta_{i,c,t}$ is measured in deaths per million in the week in the local area. The gray bars represent the response in area-weeks with no new deaths and black bars in areas with more than 45 deaths per million. The other details are the same as in Figure 7.

E.4 Sample Selection and Extensive Margin

In this section, we explore alternative samples and show that the results are robust to alternative choices. The baseline sample studies weeks with positive earnings among more attached workers who are employed by the same establishment before and after the onset of the pandemic. In the baseline sample, we additionally ensure that the earnings distributions before and after the onset of the pandemic are based on the same number of observations by using a window of 15 pre- and 15 post-weeks. For workers with a missing week, we keep an equivalent shorter symmetric window (e.g. a worker with 13 pre- and 15 post-weeks, we keep 13 weeks on both sides of the pandemic onset). This symmetry restriction guarantees that changes in excess and missing mass are not driven by exit effects but by workers moving up or down in the earnings distribution.

In this Appendix, we show WTP estimates for alternative samples, relaxing each of the restrictions above. Column (1) of Table A6 uses the baseline sample and replicates the baseline estimate from the main analysis. Column (2) relaxes the symmetry restriction and allows workers to have more work weeks before or after the pandemic. This leaves the total number of workers unchanged but extends the number of observations (i.e., worker-weeks). In Column 3, we add workers with less workforce attachment and include workers whom we observe exclusively before or after the onset of the pandemic. This doubles the number of workers and worker-week observations relative to our baseline specification. Panel A shows that the results remain very close to the baseline estimates.

We next consider extensive margin responses. For workers who leave the Homebase data, we cannot tell whether they stopped working or started a new job outside the Homebase sample. In the baseline analysis, we thus exclude exits and focus on the intensive margin of hour adjustment. In Panel B row 1 we add a zero-earnings week at the end of the work spell for workers who leave the data. The estimate is similar and in line with potential measurement error, slightly attenuated with a WTP of 23%.[15] In row 2, we take a more conservative approach and only include temporary exits in the analysis. It seems more likely that temporarily absent workers remained with the Homebase employer and the absence from the data are true zero earnings weeks. Using this in the analysis, we again find similar results to the baseline. In line with reduced measurement error, the effects are slightly larger than before at 26%. Finally, row 3 of Panel B combines the two extensive margin approaches and yields a WTP estimate of 24%. The results thus remain in the same ballpark for alternative sample choices.

Table A6: Robustness to sample selection and extensive margin

	(1) Baseline	(2) Asymmetric Sample Window	(3) Less Attached Workers
A - Intensive Margin	0.303	0.295	0.285
Worker weeks	169,450	228,591	315,566
Workers	9,063	9,063	21,418
B - Extensive Margin:			
1) zero earning for last week	0.226	0.216	0.226
Worker weeks	177,108	236,249	331,805
Workers	9,063	9,063	21,418
2) zero earnings for temporary absences	0.258	0.235	0.241
Worker weeks	182,350	241,749	333,648
Workers	9,063	9,063	21,418
1) + 2) zero for inner and last week	0.237	0.215	0.232
Worker weeks	186,183	245,324	345,003
Workers	9,063	9,063	21,418

[15] An additional reason why the results are smaller than the baseline is that at corner labor supply solutions, the WTP approach may yield a lower bound of the true WTP.

F Transmittable vs. Non-Transmittable Health Risks

This section analyzes the difference between the WTP for a transmittable illness (with externalities) and a non-transmittable one. Denote the utility weight of other household members by Ω, the number of other household members by n and the intra-household secondary fatality rate by s. The relation of WTP for a transmittable (WTP_T) and a non-transmittable disease (WTP_{nT}) is: $WTP_T = (1+\Omega \cdot s \cdot n) \cdot WTP_{nT}$. For the back of the envelope calculation, note that the intra-household secondary fatality rate is $s = 0.002$ and assume that the worker cares as much about others' utility as her own ($\Omega = 1$).[16] For household size, consider a four-person household, i.e. a household at the 90th percentile of the US size distribution (three other household members: $n = 3$). In this case, $WTP_T = 1.006 \cdot WTP_{nT}$ and WTP_{nT} is thus only 0.6% smaller than our baseline estimate. In other words, our baseline estimate of 30.3% would be reduced to roughly 30.3%/1.006=30.1% of weekly earnings for a non-transmittable disease. Quantitatively, the concern for one's own health is thus the main component of the WTP estimate, with a quantitatively small additional contribution from pro-social concerns.[17]

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[16] s is obtained multiplying the 30% intra-household transmission rate (Lewis et al., 2020) with the 0.68% infection fatality rate, that is the fatality rate conditional to being infected (Meyerowitz-Katz and Merone, 2020)

[17] Pro-social concerns will play a more important role for diseases with more aggressive transmission rates and play a minor role in this setting because s is small.

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