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IZA DP No. 12154

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ABSTRACT

Are Sufficient Statistics Necessary? Nonparametric Measurement of Deadweight Loss from Unemployment Insurance*

Central to the welfare analysis of income transfer programs is the deadweight loss associated with possible reforms. To aid analytical tractability, its measurement typically requires specifying a simplified model of behavior. We employ a complementary "decomposition" approach that compares the behavioral and mechanical components of a policy's total impact on the government budget to study the deadweight loss of two unemployment insurance policies. Experimental and quasi-experimental estimates using state administrative data show that increasing the weekly benefit is more efficient (with a fiscal externality of 53 cents per dollar of mechanical transferred income) than reducing the program's implicit earnings tax.

JEL Classification: C14, C20, C31, H2, H23, J64, J65, J68

Keywords: unemployment insurance, partial unemployment insurance, optimal unemployment insurance, sufficient statistics, decomposition, behavioral and mechanical effects, regression kink design, deadweight loss, fiscal externality

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1 Introduction

In designing social insurance and income transfer programs, policymakers are primarily interested in how to best provide adequate material support at minimal taxpayer cost. It has long been recognized that increasing the generosity of income or in-kind transfers may in theory generate unintended work disincentives that lead to additional program costs. In the context of unemployment insurance (UI) programs, the focus of this paper, more generous benefits may reduce job search effort, leading to longer periods of unemployment. These negative labor supply effects have been estimated in an extensive literature (for comprehensive reviews, see Krueger and Meyer, 2002 and Schmieder and von Wachter, 2016).

More recent studies have sought to better understand the normative implications of these estimated behavioral effects for UI (e.g., Gruber 1997; Chetty 2008; Schmieder, von Wachter and Bender 2012; Lawson 2015, 2017; Nekoei and Weber 2017; Kolsrud et al. 2018).[1] The welfare analysis of UI by Chetty (2008), which builds on work by Baily (1978), specifies a stylized job search model in which a representative worker optimizes her search effort in response to UI benefits and a fixed-wage job offer distribution. Using this model, one can express the full benefits and costs of a reform using a small number of reduced-form parameters, one of them corresponding to the labor supply effects that have been central to the empirical labor literature. Using this "sufficient statistics" method, Schmieder and von Wachter (2016) systematically review the most recent wave of empirical studies on UI and provide a mapping of existing estimates of labor supply behavioral elasticities to the fiscal externality—the excess fiscal cost that results from behavioral responses—associated with marginal changes in benefit levels and benefit lengths, which are meant to summarize the full costs of the policy.

While this approach makes welfare analyses analytically tractable, it is unclear to what extent the choice of behavioral model (and hence the consequent sufficient statistics formulae) might itself affect estimates of fiscal costs. As one example, optimal UI models typically assume full benefit take-up among eligible claimants, which is not true in practice (Blank and Card, 1991; Anderson and Meyer, 1997; Currie, 2004 and Vroman, 2009); take-up effects could be an important margin of adjustment in response to a policy reform.

This paper proposes a complementary, nonparameteric strategy for measuring the fiscal externality as-

[1] The framework has been adapted to study policy design in other social insurance and transfer program settings, such as Pell grant (Denning, Marx and Turner (2017)), workers' compensation (Bronchetti (2012)), and sick pay (Bockerman, Kanninen and Suoniemi (2015)).

sociated with changes to income-transfer programs and applyies the approach to UI policies. Instead of specifying a particular behavioral model that leads to an expression for the fiscal externality as a function of reduced-form elasticities or other model primitives, we utilize an accounting identity that holds both for every individual data point and in the aggregate: the impact of a UI reform on total government net expenditures can be decomposed into a "mechanical component," the budgetary impact in the absence of behavioral responses, and a component driven by responses to the reform, the fiscal externality. This externality is identified by the difference between the total effect on benefit expenditures and tax receipts and the "mechanical effect," both of which, given some policy variation of interest, can be easily and directly measured in the data.[2] This "decomposition" approach naturally follows from the widely understood notion in public economics that the impact of a marginal reform on government revenues can be sufficient to characterize marginal efficiencies or deadweight loss. A key benefit of our methodology is that it will produce the same quantity across a very broad class of behavioral models and requires minimal assumptions regarding individual-specific heterogeneity. Although this strategy is the logical consequence of well-known principles in public economics, it has not been employed to estimate of the fiscal externalities of UI policies, and to the best of our knowledge, the approach has yet to be employed in other income-transfer and taxation settings more broadly.[3]

We apply our decomposition approach to a study of the fiscal externalities associated with two distinct policies that affect the UI benefit generosity. First, we estimate the fiscal externality induced by an incremental change in the UI weekly benefit amount, a magnitude that plays a key role in the recent optimal UI literature and the margin of interest in a large number of empirical studies of UI in the labor market (Moffitt, 1985; Meyer, 1990; Card et al., 2015*a*,*b*; Lalive, van Ours and Zweimüller 2006; Chetty 2008; Kroft and Notowidigdo 2016; and DellaVigna et al., 2017). Second, we also estimate the fiscal externality induced by a seldom-discussed feature of UI systems — the fact that all U.S. states allow workers to be "partially unemployed" (i.e., with some limited earnings) and still be able to receive some UI benefits, as dictated by an earnings deduction formula. While some studies (McCall, 1996; Le Barbanchon, 2016) have investigated the behavioral response to variation in the disregard amount (i.e., earnings threshold above which UI benefits

[2] We use the term "nonparametric" to describe this strategy because it allows the behavioral model to have parameters of arbitrary dimension. Instead of deriving an expression for the fiscal externality as a function of a small number of elasticities, we are estimating the fiscal externality directly.

[3] For example, there is a large literature on optimal top marginal income tax rate that tries to estimate the effect of a higher tax rate on government revenue by extrapolating from the elasticity of taxable income (Saez, Slemrod and Giertz, 2012). Hendren (2016) also calculates the welfare consequences of various policy reforms by relying on existing elasticities estimated in the literature.

are reduced), our paper examines the response to a reduction in the implicit tax rate.

We study these policies in the context of Washington State in the mid-nineties. In 1994, the Washington State legislature authorized a randomized experiment to examine the impact of changing the benefit formula for partially unemployed workers. One quarter of all new UI claimants starting from the fourth quarter of 1994 and lasting for one year were randomized into a treatment group that saw their weekly benefit amount reduced by 67 cents for every dollar earned as opposed to the control group's rate of 75 cents per dollar—see the report by O'Leary (1997) for details. We use the experimental variation to estimate the total and behavioral effects of a change in this implicit tax rate on the government's budget. Furthermore, we provide quasi-experimental evidence on the fiscal externality of a marginal change in the weekly benefit amount by applying a regression kink design along the lines of Card, Lee and Pei (2009); Card et al. (2015*a*,*b*). Our analysis uses detailed UI claim and quarterly earnings data for the entire universe of claimants eligible for the experiment in the quarters leading up to the filing and for four quarters or more after the initial date of the claim.

Our empirical analysis finds significant estimates of the fiscal externalities for both the experimental reduction in the implicit tax rate on earnings and the quasi-experimental variation in the weekly benefit amount. For the marginal dollar that is transferred to inframarginal UI claims via an increase in the weekly benefit amount, we estimate that between 27 cents and 79 cents are spent on the behavioral response to the change. In comparison, the reduction in the implicit tax rate — for which there was hope of a *negative* behavioral cost — incurred a fiscal externality of $1.38, and we can statistically rule out magnitudes less than $0.49 per dollar of mechanical transfer. Most of this fiscal externality appears to be driven by the impact of the behavioral response on benefit expenditures, as opposed to lost tax revenue. Comparing our RKD and experimental results suggests that the relatively more fiscally efficient way to transfer income is via an increase in the weekly benefit amount, not a reduction in the implicit tax rate. The total behavioral costs found here are on the high end of the range of effects reported for the U.S. in the survey by Schmieder and von Wachter (2016). Finally, when we compute the same fiscal externality "indirectly" by first estimating claim duration elasticities and then applying an extrapolation from a common job search model used for a sufficient statistics approach, we obtain a significantly higher magnitude, suggesting that the implicit parametric behavioral model used for the calculation is too restrictive.

The remainder of the paper is organized as follows. Section 2 uses a standard public economics framework to define the fiscal externality and to contrast existing identification strategies to the decomposition approach taken in this paper. In Section 3, we describe the Washington State's UI system, the earnings deduction experiment, and aspects of the administrative data relevant to our analysis. We present our results in Section 4 and conclude in Section 5 by summarizing our findings and their implications, as well as identifying some important directions for further work.

2 Conceptual and Econometric Framework

Using a standard public economics framework, this section defines the fiscal externality parameter of interest, and reviews its role in the literature on the welfare analysis of UI. First, we contrast the typical sufficient statistics approach used to empirically analyze the problem with the complementary decomposition strategy that we adopt; then, we describe its implementation in the specific case of using UI benefit variation from a randomized experiment and a regression kink design—both of which we employ in our empirical analysis.

2.1 The Fiscal Externality Parameter and the Social Planner's Problem

We first introduce notation to define the fiscal externality parameter of interest. Let the vector-valued function $\mathbf{Y}(\theta,\tau)$ represent the reduced-form optimal choices of individuals in response to the UI policy rules, parameterized by θ (e.g. benefit levels), and the tax schedule τ (a tax parameter). For simplicity of exposition, we consider θ and τ to be scalars.[4] $\mathbf{Y}$ could include, for example, a "reservation wage" for accepting a job offer, job search effort, the number of hours worked in a given week, an individual's "benefit threshold" beyond which the individual will take up the benefit, or any other related labor supply, consumption, or program participation choices; the function represents the individual's optimal response for any given value of the parameters θ,τ. Let the net transfer (UI benefits less taxes paid) to the individual be represented by the function $B(\mathbf{Y}(\theta,\tau),\theta)-T(\mathbf{Y}(\theta,\tau),\tau)$, where $B(\cdot,\cdot)$ are UI benefits and $T(\cdot,\cdot)$ are taxes, both of which depend on the choices $\mathbf{Y}(\theta,\tau)$ and the policy parameters θ and τ. This function combines the UI benefit formula (e.g. benefits are paid out according to past and future earnings), and the tax schedule, with choices to determine the actual benefits or taxes paid.

We now consider making benefits more generous through increasing θ. Letting the expectation operator $E[\cdot]$ represent averaging over a heterogeneous population, we can decompose the marginal cost of the

[4]The discussion below can be extended to the case of vectors of parameters.

increase in θ in terms of net dollars transferred into two components:

$$\underbrace{\frac{dE\left[B\left(\mathbf{Y}\left(\theta,\tau\right),\theta\right)-T\left(\mathbf{Y}\left(\theta,\tau\right),\tau\right)\right]}{d\theta}}_{\text{"Total"}}=\underbrace{E\left[\left(\frac{\partial B}{\partial \mathbf{Y}}-\frac{\partial T}{\partial \mathbf{Y}}\right)\cdot\frac{\partial \mathbf{Y}}{\partial \theta}\right]}_{\text{"Behavioral"}}+\underbrace{E\left[\frac{\partial B}{\partial \theta}\right]}_{\text{"Mechanical"}} \tag{1}$$

assuming the sufficient regularity conditions that allow for the interchange of expectation and differentiation. The "behavioral" component is composed of the impact of the policy change on behavior $\frac{\partial Y}{\partial \theta}$ and the impact of the behavior on benefits and taxes $\frac{\partial B}{\partial \mathbf{Y}}-\frac{\partial T}{\partial \mathbf{Y}}$. The "mechanical" component, $E\left[\frac{\partial B}{\partial \theta}\right]$, is the change in benefit expense in the absence of any behavioral response; note that by definition, a change in θ has no *direct* effect on taxes paid. The ratio

$$\frac{\text{"Behavioral Effect"}}{\text{"Mechanical Effect"}}=\frac{E\left[\frac{\partial B}{\partial \mathbf{Y}}\cdot\frac{\partial \mathbf{Y}}{\partial \theta}\right]-E\left[\frac{\partial T}{\partial \mathbf{Y}}\cdot\frac{\partial \mathbf{Y}}{\partial \theta}\right]}{E\left[\frac{\partial B}{\partial \theta}\right]}\equiv\beta+\gamma \tag{2}$$

is the fiscal externality parameter of interest. Measured as a proportion of the intended transfer to inframarginal UI claims, it represents the additional net transfer that must occur because of the behavioral response to the policy change. It is an intuitive and easily interpretable magnitude of the unintended fiscal consequence of increasing UI benefits, and can be further decomposed into the part attributable to changes in benefit expenditures $\beta\equiv\frac{E\left[\frac{\partial B}{\partial \mathbf{Y}}\cdot\frac{\partial \mathbf{Y}}{\partial \theta}\right]}{E\left[\frac{\partial B}{\partial \theta}\right]}$ and tax revenues $\gamma\equiv\frac{-E\left[\frac{\partial T}{\partial \mathbf{Y}}\cdot\frac{\partial \mathbf{Y}}{\partial \theta}\right]}{E\left[\frac{\partial B}{\partial \theta}\right]}$.

While this "marginal excess burden" parameter stands on its own as an interpretable summary of the fiscal consequences of the change, it also plays a central role in existing welfare analyses of UI. To review these standard results from the literature, let $V(\cdot,\cdot,\cdot)$ be an individual's utility function with B, T, and $\mathbf{Y}(\theta,\tau)$ as arguments. To aggregate individual utilities to social welfare, we use the function $G(\cdot)$ to translate an individual's utility to a metric that can be averaged across individuals. The social planner's objective is to maximize

$$W=E\left[G\left(V\left(B,T,\mathbf{Y}\right)\right)\right] \tag{3}$$

subject to a balanced budget constraint $E[B-T]=0$ (where we have suppressed arguments for readability).[5]

[5] More generally, loosening the budget neutrality constraint (e.g., Hendren, 2016) will not change the fiscal externality expression (the focus of this paper), but will affect the social valuation of policy. See Pei (2017) for an example of a planner objective function that does not impose a balanced budget.

The balanced budget constraint defines the implicit function $\tau(\theta)$, where

$$\frac{d\tau}{d\theta} = -\frac{dE\left[B-T\right]/d\theta}{dE\left[B-T\right]/d\tau}$$

describes the necessary change in tax parameter τ that is required to offset the budgetary impact of a marginal change in UI parameter θ.[6]

Taking the derivative of (3) with respect to θ subject to budget balance, a marginal increase in the UI parameter will impact social welfare as follows:

$$\begin{aligned}\frac{dW}{d\theta} &= E\left[G'\cdot V_1\cdot\frac{\partial B}{\partial\theta}\right]+E\left[G'\cdot V_2\cdot\frac{\partial T}{\partial\tau}\frac{d\tau}{d\theta}\right]\\ &= E\left[G'\cdot V_1\cdot\frac{\partial B}{\partial\theta}\right]-E\left[G'\cdot V_2\cdot\phi\right]\frac{dE[B-T]}{d\theta}\end{aligned}$$

where $\phi = \frac{\partial T/\partial\tau}{dE[B-T]/d\tau}$ represents the amount of increased taxation each individual experiences to offset a one dollar increase in the deficit. The first line uses the fact that all terms in the total derivatives with respect to θ that reflect changes in welfare through changes in $\mathbf{Y}$ will sum to zero, because we have assumed the individual is already optimizing (i.e. we apply the envelope theorem).[7]

Normalizing the above expression by $E\left[G'\cdot V_2\cdot\phi\right]E\left[\frac{\partial B}{\partial\theta}\right]$, the above expression shows that the social planner assesses optimality by comparing two quantities (after some re-arrangement, and using the accounting identity (1)),

$$\alpha \equiv \frac{E\left[G'\cdot V_1\cdot\frac{\partial B}{\partial\theta}\right]}{E\left[G'\cdot V_2\cdot\phi\right]E\left[\frac{\partial B}{\partial\theta}\right]}-1 \lesseqgtr \frac{E\left[\frac{\partial B}{\partial\mathbf{Y}}\cdot\frac{\partial\mathbf{Y}}{\partial\theta}\right]-E\left[\frac{\partial T}{\partial\mathbf{Y}}\cdot\frac{\partial\mathbf{Y}}{\partial\theta}\right]}{E\left[\frac{\partial B}{\partial\theta}\right]} \equiv \beta+\gamma \tag{4}$$

To interpret the left side of the relation, it is useful to first consider the case of a representative agent, which is standard in the optimal UI literature (see, e.g., Schmieder and von Wachter, 2016). In this standard case without heterogeneity, α simplifies to $\frac{V_1}{V_2\cdot\phi}-1$, where $\frac{V_1}{V_2\cdot\phi}$ represents the individual's valuation of an extra dollar of UI benefits (via an increase in θ) relative to the value of the increase in taxes (via τ) levied to pay for the increase in θ. Since individuals pay taxes when employed and receive UI benefits when unemployed, α reflects the insurance function of the transferring income across employed and unemployed states. In the

[6]The numerator is as defined in equation (1) and the denominator is $\frac{dE[B-T]}{d\tau} = E\left[\left(\frac{\partial B}{\partial\mathbf{Y}}-\frac{\partial T}{\partial\mathbf{Y}}\right)\cdot\frac{\partial\mathbf{Y}}{\partial\tau}\right]-E\left[\frac{\partial T}{\partial\tau}\right]$.

[7]Note that individuals do not anticipate the changes in the tax parameter associated with an increase in θ (i.e., individual response functions remain $\mathbf{Y}(\theta,\tau)$ even as the social planner adjusts τ to balance the budget).

more general case with heterogeneous agents, α will contain the the ratio of *aggregate* welfare gain from an increased UI transfer to the *aggregate* welfare gain from decreased taxation.[8]

Equally important to the social planner's cost–benefit analysis is the right side of the relation (4)—the fiscal externality from the transfer, $\beta+\gamma$. β represents the amount of additional benefits that need to be paid out per one dollar of "mechanical transfer" as a consequence of the behavioral response to the policy change while γ represents the analogous loss in tax receipts. The higher the quantity $\beta+\gamma$ is, the less likely one is to conclude there is a net welfare gain to increasing θ; or put differently, the larger the α would need to be in order to justify the marginal increase in θ. $\beta+\gamma$ can also be compared across different margins of adjustment to determine which margin transfers income more efficiently (e.g., increase in the maximum weekly benefit level or increasing the potential duration of benefits), as suggested in Schmieder and von Wachter (2016).

The comparison of α and $\beta+\gamma$ in (4) is central to a number of studies in the optimal UI literature. These studies typically employ job search models of some form to derive relationships between α, $\beta+\gamma$, and a small number of parameters. For example, Chetty (2009) reviews a number of "sufficient statistics" analyses, and illustrates how the studies of Baily (1978), Gruber (1997), Shimer and Werning (2007), and Chetty (2008) all use welfare optimality conditions that capture α, the value of insurance, or normalized "gap in marginal utilities" between the unemployed and employed state.[9] The assumptions of those models yield explicit expressions for $\beta+\gamma$ as a function of a reduced-form behavioral elasticity, such as the elasticity of the probability of unemployment with respect to the benefit level.[10] A driving motivation for this approach is its empirical tractability; it conveniently allows one to use existing reduced-form estimates of behavioral responses from the literature to inform welfare analyses.

However, the degree of sensitivity of the conclusions of these welfare analyses to behavioral models chosen to derive these expressions is unknown. Each of the analyses referenced above necessarily must make simplifying assumptions and approximations and ignore what is believed to be second-order behavioral

[8]Note that this model can easily be generalized to allow for additional non-UI expenditures (e.g., a public good) that are valued by individuals and included in the government's budget constraint (Lawson, 2017). However, in the case where the policy has an impact on non-UI spending (e.g., program interactions analyzed by Lawson, 2015; and Leung and O'Leary, 2015), the excess spending or savings in non-UI programs will also need to be accounted for in computing the fiscal externality.

[9]In their review of more recent studies on UI, Schmieder and von Wachter (2016) also derive similar expressions for the gap in marginal utilities that corresponds to a marginal change in potential benefit duration and a change in benefit levels.

[10]In the review of Chetty (2009), *MC* in Baily (1978), Gruber (1997), Chetty (2008), and Shimer and Werning (2007) contains or is equal to the elasticity of unemployment with respect to the benefit level divided by the probability of employment. In the review of Schmieder and von Wachter (2016), it is a weighted sum of UI receipt and non-employment duration elasticities with respect to benefits.

aspects in order to express α and $\beta+\gamma$ as a function of a small number of parameters. For example, in Chetty (2008) there is essentially a "two-stage" structure in the job search problem, where the individual is initially unemployed, and once a job offer arrives, it is accepted and she is employed until the final period, even though movements back and forth between UI receipt and full-time employment is observed in the data.[11] As is typically the case in optimal UI analyses, there is little or no discussion of imperfect take-up and the possibility that more generous benefit may impact take-up even though the take-up rate of the benefits is far from complete.[12] Furthermore, a fixed pretax wage is assumed, and more generally the model is of a "representative agent" kind, ruling out various forms of heterogeneity. These models can be extended to rely on fewer simplifications, but doing so without offsetting assumptions about the heterogeneity leads to more parameters to estimate (which may or may not be available in the literature), without meaningfully resolving the question of whether the welfare conclusions are still sensitive to the choice of an inevitable set of modeling restrictions one must make.

Setting aside the question of identifying α as beyond the scope of our study, we focus on $\beta+\gamma$ and propose a complementary decomposition approach to its identification that naturally follows from the insight from the public economics literature that within a broad class of models, the behavioral impact on the government budget is sufficient for measuring economic efficiency for welfare analysis. Specifically, instead of assuming a specific structure and functional form for $B(\cdot,\cdot)$, $T(\cdot,\cdot)$, and $\mathbf{Y}(\theta,\tau)$ that would lead to expressing $\beta+\gamma$ in (2) as functions of reduced-form behavioral elasticities, we rely on the decomposition in (1). The fiscal externality parameter $\beta+\gamma$ is identified as the behavioral component $E\left[\left(\frac{\partial B}{\partial \mathbf{Y}}-\frac{\partial T}{\partial \mathbf{Y}}\right)\cdot\frac{\partial \mathbf{Y}}{\partial \theta}\right]$, the simple difference between the directly measured total impact of the policy change on benefits (net of taxes) $\frac{dE[B-T]}{d\theta}=E\left[\frac{dB}{d\theta}\right]-E\left[\frac{dT}{d\theta}\right]$, and the direct measurement of the mechanical effect $E\left[\frac{\partial B}{\partial \theta}\right]$, normalized by $E\left[\frac{\partial B}{\partial \theta}\right]$. This approach requires policy variation in θ (which is needed in any case), and enough information about individuals to simulate the mechanical effect $E\left[\frac{\partial B}{\partial \theta}\right]$.

By treating the behavioral component as the "residual" in the decomposition, we need not model $B(\cdot,\cdot)$, $T(\cdot,\cdot)$, or $\mathbf{Y}(\theta,\tau)$, nor do we need measurements on all choices $\mathbf{Y}$.[13] This approach of focusing on measurements of impacts on government revenue has been implicitly and explicitly suggested in numerous papers

[11] Thirty percent of the claimants in our data have at least two unemployment spells over the course of their benefit year, where we define the end of a spell to be three or more weeks without a claim.

[12] Blank and Card (1991), Anderson and Meyer (1997), and Vroman (2009) all focus on the unemployed workers who do not receive UI benefits. "Imperfect take-up" could also result in the case of workers who have started receiving UI benefits, but decide, on specific weeks, to not "use up" a week of UI benefits via weekly claiming decisions.

[13] One does need enough information on choices to compute the mechanical effect from the microdata.

throughout the public economics literature, but the estimation of the fiscal externality directly from the microdata has yet to be employed in studies of UI, or to the best of our knowledge, in studies of other income transfer programs.[14]

This decomposition approach to estimating the fiscal externality has two potential advantages. First, when working with administrative program data, which is increasingly prevalent in analyses of UI (Schmieder and von Wachter, 2016), the data on government expenses and revenues are often more complete relative to the measures of labor supply or job search behavior that would be well-suited for the behavioral models that we might construct. For example, it is common in the US context to construct a single spell of UI receipt from claim data, without acknowledging that individuals may move in or out of benefit receipt, or that these measured spells may or may not correspond to actual spells of non-employment. In addition, weekly wages and hours are typically not observed for non-claimants (which can give rise to sample selection issues). Richer behavioral models that allow heterogeneity in wage offer distributions or in reservation wages are likely to require data that are even more difficult or infeasible to obtain.

Second, and more importantly, given that $\mathbf{Y}(\theta,\tau)$ can take on a very general form, with arbitrary dimension, the decomposition approach is consistent with a very wide range of behavioral models; the key necessary assumption for welfare analysis is that individuals are making optimizing choices. Because of this degree of model-insensitivity, nonparametric measures of $\beta+\gamma$ can serve as a useful complementary benchmark for other strategies, such as a "sufficient statistics" analysis, or a more fully parametrized structural approach.

[14] Hendren (2016) recently emphasizes the sufficiency of a policy's causal impact on government revenue for welfare analysis. This notion is illustrated for the policies of increasing the top marginal tax rate and an EITC expansion. For the top tax rate, the study uses the approach of Saez, Slemrod and Giertz (2012), and Giertz (2009) which provide formulas that express the fiscal externality as a function of the elasticity of taxable income, using a Pareto approximation for the upper tail of the income distribution and assuming away income effects. For the EITC expansion, Hendren (2016) notes that "there is no study that estimates the impact of the behavioral response to EITC expansions on government expenditures directly." Thus, in the application to EITC, Hendren (2016) infers the fiscal externality from estimates of the causal impacts of EITC expansions on earnings and labor supply. In a review and extension of the sufficient statistics approach, Kleven (2018) notes the generality of the principle that marginal efficiency loss is given by the behavioral impact on government revenue, tracing the clarification of these principles to discussions in Feldstein (1999), Saez (2004), and Kleven and Kreiner (2005). The focus in the studies cited above is on deriving formulae that connect specific behavioral elasticities (e.g. extensive or intensive labor supply elasticities, or the elasticity of taxable income) to the fiscal externality, as opposed to the direct measurement of the fiscal externality. Ashenfelter and Plant (1990) apply a similar decomposition to the SIME/DIME experimental data, where they compute the difference between actual NIT payments of an experimental group and the predicted payments using the control group to make inferences on labor supply responses, but does not consider its relation to measures of efficiency as recognized in the public economics literature.

2.2 Experimental and Quasi-Experimental Identification of the Fiscal Externality

Fiscal Externality from a Randomized Experiment: Changing the Implicit Tax Rate on Earnings

The decomposition equation (1) shows that the fiscal externality of a change in a transfer policy can be computed from two values: the total effect of the policy on average benefits less taxes, $E\left[\frac{dB}{d\theta}\right] - E\left[\frac{dT}{d\theta}\right]$, and the mechanical effect on benefit payments, $E\left[\frac{\partial B}{\partial \theta}\right]$. In the case of the Washington State earnings deduction experiment, credible identification of these parameters in response to a change in the implicit marginal tax rate of benefits is straightforward. In the experiment, one quarter of the sample was randomly assigned to an alternative UI benefit schedule. The most important difference between the benefit schedules of the treatment and control groups was the variation in the implicit marginal tax rate. In the control group, workers' weekly UI benefits were reduced by 75 cents for every dollar earned above \$5 (the "disregard"), whereas the treatment group members faced a 67 cents per dollar reduction above a \$15 disregard.

Due to the randomization, $E\left[\frac{dB}{d\theta}\right] - E\left[\frac{dT}{d\theta}\right]$ is in principle identified by the simple difference between the treatment and control means of benefits (less taxes) received by claimants. Identification of the mechanical effect $E\left[\frac{\partial B}{\partial \theta}\right]$ is achieved by using the observed data from the control group, and calculating the benefits that they would receive if their benefits were instead determined by the treatment group's benefit schedule, assuming no behavioral response (e.g. no change in earnings or take-up). The difference between the simulated average and the control average yields the mechanical effect.

As discussed more fully in Section 3.2, in practice we do not observe actual tax receipts, but the administrative data do contain individual earnings as reported by the employer to the UI agency, which are the amounts used to determine the actual tax amount. Thus, we very closely approximate actual tax receipts at the individual level by approximating the statutory tax schedule.

Fiscal Externality from a Regression Kink Design: Changing the Weekly Benefit Amount

Fiscal externalities can also be identified using quasi-experimental variation from a regression kink design (Card et al., 2015*b*). Specifically, most UI systems determine the weekly benefit amount as a constant fraction of some measure of past earnings, but the benefit amount is capped at a fixed nominal amount. This feature of the UI formula creates a kink in the relationship between past earnings and the benefit amount. Under a number of smoothness conditions, this allows for identification of an average causal marginal effect through the interpretation of a kink in the conditional expectation function of the outcome with respect to past earnings. Therefore, for the impact of a marginal change in the weekly benefit, there is an RKD analogue to the identification statements made above for the earnings deduction experiment.

Under various smoothness assumptions as specified in Card et al. (2015*b*), we obtain the identification results that

$$RKD_B \equiv \frac{\lim_{v_0\to 0^+} \left.\frac{dE[B|V=v]}{dv}\right|_{v=v_0} - \lim_{v_0\to 0^-} \left.\frac{dE[B|V=v]}{dv}\right|_{v=v_0}}{\lim_{v_0\to 0^+} \left.\frac{dE\left[\tilde{\theta}|V=v\right]}{dv}\right|_{v=v_0} - \lim_{v_0\to 0^-} \left.\frac{dE\left[\tilde{\theta}|V=v\right]}{dv}\right|_{v=v_0}} = E\left[\omega\frac{dB}{d\theta}\right] \tag{5}$$

$$RKD_T \equiv \frac{\lim_{v_0\to 0^+} \left.\frac{dE[T|V=v]}{dv}\right|_{v=v_0} - \lim_{v_0\to 0^-} \left.\frac{dE[T|V=v]}{dv}\right|_{v=v_0}}{\lim_{v_0\to 0^+} \left.\frac{dE\left[\tilde{\theta}|V=v\right]}{dv}\right|_{v=v_0} - \lim_{v_0\to 0^-} \left.\frac{dE\left[\tilde{\theta}|V=v\right]}{dv}\right|_{v=v_0}} = E\left[\omega\frac{dT}{d\theta}\right]$$

where we normalize the location of the kink to zero, and ω is a population weight such that $E[\omega] = 1$. The left-hand expressions are fuzzy RKD estimands, where $\tilde{\theta}$ is the observed benefit parameter (e.g., weekly benefit level) for the individual. The fuzzy RKD allows us to identify causal estimands even in the presence of some forms of measurement error in V (past earnings) and θ, as well as non-compliance with the formula.[15] The right-hand expressions reflect the causal interpretation of these estimands. The difference between these expressions and $E\left[\frac{dB}{d\theta}\right]$ and $E\left[\frac{dT}{d\theta}\right]$ is captured in the population weight ω. In a model of unrestricted heterogeneity in marginal effects across individuals, we must acknowledge that the RKD estimands identify a weighted average of marginal effects, where the weights ω capture 1) the relative likelihood of an individual having V near the threshold, 2) individual heterogeneity in the magnitude of the kink, and 3) the extent of a mass point at zero in the distribution of any possible measurement error in V.[16]

We identify a "weighted average" version of $E\left[\frac{\partial B}{\partial \theta}\right]$, the mechanical effect, by considering the variable[17]

$$\frac{\partial B}{\partial \theta} = B(\mathbf{Y}(\theta,\tau), \theta+1) - B(\mathbf{Y}(\theta,\tau), \theta)$$

This represents the total additional benefits that the individual receives if the weekly benefit amount is

[15]In practice, as we discuss below, there is relatively little deviation between θ and $\tilde{\theta}$ in the analysis of the Washington data.

[16]For a detailed explanation of this interpretation see Card et al. (2015*b*).

[17]As will be evident in our discussion in Section 4, this is a simplification of the simulation of the mechanical effect that we must perform. In particular, the above expression does not incorporate limits on the maximum benefits payable. In our empirical analysis, we ensure that we adjust the data to account for the fact that a higher weekly benefit will cause some individuals to exhaust benefits earlier than observed in the data.

increased by one dollar. The expectation of this variable, conditional on $V = 0$, is

$$E\left[\frac{\partial B}{\partial \theta}|V=0\right] \approx E\left[\omega^{*} \cdot \frac{\partial B(\mathbf{Y}(\theta,\tau),\theta)}{\partial \theta}\right] \tag{6}$$

This represents the weighted mechanical effect of the marginal increase in θ, where, for each individual, the weights ω^* will capture the first and third components of ω as described above. Thus, for $\omega = \omega^*$ to be true, we must assume that there is no heterogeneity in the magnitudes of the kink in the benefit schedule across individuals, which would be the case if the observed weekly benefit amount (*wba*) was perfectly predicted using V and the benefit formula. Given the very close correspondence with the observed *wba* and the *wba* predicted by the formula, we believe that this is a reasonable assumption to make.

With the quantities in (5) and (6), and assuming $\omega \approx \omega^*$, we can thus compute, analogous to the randomized experiment, the fiscal externality as

$$\frac{RKD_B - RKD_T - E\left[\frac{\partial B}{\partial \theta}|V=0\right]}{E\left[\frac{\partial B}{\partial \theta}|V=0\right]} \approx \frac{E\left[\omega \cdot \frac{\partial B}{\partial \mathbf{Y}} \cdot \frac{\partial \mathbf{Y}}{\partial \theta}\right] - E\left[\omega \cdot \frac{\partial T}{\partial \mathbf{Y}} \cdot \frac{\partial \mathbf{Y}}{\partial \theta}\right]}{E\left[\omega \cdot \frac{\partial B(\mathbf{Y}(\theta,\tau),\theta)}{\partial \theta}\right]} \tag{7}$$

While this "weighted average" version of the fiscal externality of an increase in the weekly benefit amount resembles the expression in (2), aside from the presence of the weights ω, there is an additional important difference between (7) and (2): the total marginal effect applies only to individual's responses when θ is equal to the maximum benefit level, and when $V = 0$ (the kink point). This qualification is inevitable for regression kink designs.

For the purposes of using the fiscal externality quantity in (7) to correspond to a policy such as increasing the weekly benefit amount, or alternatively increasing the maximum benefit amount, some additional restrictions on unobserved heterogeneity is required. One approach that would respect the fact that the mechanical effect of these policies may be heterogeneous, and also avoid making assumptions about the weights ω, is to adopt the approximation that at the level of the individual, the behavioral effects are constant proportions of the mechanical effects, so that $\frac{\partial B}{\partial \mathbf{Y}} \cdot \frac{\partial \mathbf{Y}}{\partial \theta} = k_0 \frac{\partial B}{\partial \theta}$ and $\frac{\partial T}{\partial \mathbf{Y}} \cdot \frac{\partial \mathbf{Y}}{\partial \theta} = k_1 \frac{\partial B}{\partial \theta}$, where k_0 and k_1 are constant across the population. This would imply that the RKD fiscal externality quantity in (7) would equal $k_0 - k_1$, and also equal the fiscal externality (2) for the full population. A less stringent extrapolating assumption would be to focus on the policy of raising the *maximum* weekly benefit amount, and assume the constancy of k_0 and k_1 applies only for $V > 0$ and θ at the maximum. This would allow one to interpret (7) as the fiscal externality

(2) associated with a marginal increase in the maximum benefit level, while still allowing for hetereogeneity in both mechanical and behavioral effects, as well as weights ω. We discuss in more detail some sufficient assumptions that would permit this extraploation in Appendix 5.

3 The Unemployment Insurance System in Washington State

This section describes the Unemployment Insurance benefits formula, and then describes the State of Washington's randomized experiment for studying the effects of partial UI, as well as the data used for our empirical analysis.

In Washington, UI claimants are eligible to receive a weekly benefit amount based on the earnings and hours in their "base year" (or "base period"), which is defined as the first four of the last five completed calendar quarters before the week of the initial claim.[18] A claimant must have worked at least 680 hours in the base year to be eligible for UI benefits, a feature that is unique to Washington and thereby leads to employer's reporting of claimants' history of hours worked. If the applicant is eligible, two quantities are computed for her: the weekly benefit amount and the maximum benefits payable. The weekly benefit amount—the amount they can receive without being employed, subject to a job search requirement—is calculated as $1/50$ of the sum of the two highest quarterly earnings within the base year, subject to a maximum and minimum.[19] The maximum benefits payable is the total amount of UI payments that can be made within one year of the initial filing date. This total amount is equal to the lesser of 30 times their weekly benefit amount and $1/3$ of their total base year earnings.

Formally, the weekly benefit amount (*wba*) and the maximum benefits payable (*mbp*) can be expressed as functions of the quarterly earnings in the base year in the following way. Let Q_1 (highest), Q_2, Q_3, and Q_4 (lowest) be the quarterly earnings within the base year. Suppose, for this example, that the maximum weekly benefit amount is \$350.[20] Then

$$wba = \min\left(\frac{Q_1+Q_2}{50}, 350\right)$$

[18]For example, if an individual first files a claim in February 1995, then their base year is defined as starting on October 1, 1993 and ending on September 30, 1994. Claimants who are ineligible for UI benefits under the primary definition of the base year can use the most recent four completed quarters (in this example, from January 1, 1994 to December 31, 1994) to determine their eligibility and benefits.

[19]We will omit the minimum weekly benefit amount from our analysis and in the following discussion because that threshold is binding for only a small proportion of our sample.

[20]The maximum weekly benefit amount was \$343 from July 1994–95, and \$350 from July 1995–96.

$$mbp = \min\left(30 \cdot wba, \frac{Q_1 + Q_2 + Q_3 + Q_4}{3}\right)$$

To identify the impact of a change in the weekly benefit level, we exploit the kink in the *wba* formula as a function of the sum of the two highest quarter earnings in the base year. However, as is clear from the *mbp* formula, there is an additional kink in the the maximum benefits as a function of the total base year earnings. To visualize where these kinks occur, and how they relate to each other, let $V = Q_1 + Q_2$ and $R = \frac{Q_3+Q_4}{Q_1+Q_2}$, and consider regions of V-R space, as in Figure 1.

The figure shows that there are four regions where the *wba* and *mbp* will be computed differently and, as a result, the boundaries between these regions will coincide with a kink in those quantities.[21] Focusing on the evolution of *wba* as a function of V, note that *wba* is kinked at $V = \$17,500$. Furthermore, when $R > 0.8$, *mbp* is also kinked as a function of V at $V = \$17,500$ (denoted boundary I). When $R < 0.8$, there is *only* a kink in the relation between V and *wba* (denoted boundary II). This means that a regression kink design using V as the running variable can identify the marginal effect of simultaneously increasing the *wba* (e.g. by a dollar) and the *mbp* (e.g. by 30 dollars) for those who have $R > 0.8$, and just the effect of increasing the *wba* while keeping *mbp* constant for those with $R < 0.8$. In our analysis, which pools the individuals above and below $R = 0.8$ to estimate the marginal effect of *wba*, we acknowledge that for a subsample of individuals (those with $R > 0.8$), the marginal effect is overstating the effect of *wba* to the extent that there is an independent marginal effect of *mbp*. In our analysis, we normalize V to be zero at the threshold as mentioned in Section 2.2.[22]

The benefits that claimants are paid in any given week also depends on the amount of labor income they earn while claiming UI. Similar to other states in the U.S., Washington has a disregard amount d for which claimants can earn each week without penalty, and a rate τ at which benefits are deducted for each dollar earned above d. During the period of our sample, Washington reduces the amount that they pay a claimant by \$0.75 for each dollar that the individual earns above \$5 in the week of the claim. Formally, if Y_t is the amount of labor income that a claimant earns in week t, d is the disregard, and τ is the implicit tax on

[21]In principle, we can also exploit the kinks labeled III and IV on the graph in separate RKD analyses. For individuals for whom $V < \$17,500$, with a running variable to R, there is a kink in the relation between R and *mbp* at boundary III, but not between R and *wba*. This allows identification of the impact of a marginal increase in *mbp*, keeping *wba* constant. The same kind of marginal effect can be identified using $Q_1 + Q_2 + Q_3 + Q_4$ as the running variable, where there is a discontinuous relation between the full base year earnings and *mbp* at boundary IV.

[22] The threshold was at \$17,150 before July 1995, and \$17,500 thereafter.

earnings above d, then the amount of benefits that the claimant would receive in week t is given by

$$B_t = \max(0, wba - \tau(Y_t - d)\mathbf{1}[Y_t > d])$$

To summarize, as in many states in the U.S., the Washington UI system can be characterized by four parameters: wba, mbp, τ and d. Our goal is to examine the fiscal externality of marginal changes in wba and τ. While there is an extensive literature that studies the effects of changes to wba, less is known about the effect of changes to the partial unemployment insurance parameters. Two papers that have focused on partial unemployment insurance have used different kinds of variation in the disregard level d. McCall (1996) studied whether the level of the partial UI earnings disregard influences a UI recipient's job search behavior. He used variation in the level of earnings disregard across states and within states over time, and found that a higher earnings disregard increases both the part-time and overall re-employment hazards. More recently, Le Barbanchon (2016) uses bunching at the disregard level to show how kinks in the benefit schedule with respect to labor income can lead to intensive margin responses. That study's estimated earnings elasticity with respect to the tax rate and the corresponding counterfactual simulation suggests an optimal benefit-reduction rate of 80 percent. In practice, the disregard level d is quite low in Washington ($5 dollars a week; $15 in the treatment group), so our interpretation of the effects will center around the impacts resulting from variation in τ.[23]

3.1 The Washington State Unemployment Insurance Earnings Deduction Experiment

For one year starting in October 1994, Washington conducted a large randomized experiment to investigate the effects of reducing the amount of benefits deducted from claimants who work while on UI. In particular, Washington randomly assigned 25 percent of all eligible UI claimants to an alternate unemployment insurance system whereby claimants faced a disregard rate of $15 (rather than the usual $5) per week and a marginal tax rate of 2/3 (compared to the usual 3/4). These individuals each received a letter in the mail that explained their more generous benefit schedules. Figure 2 illustrates the net income as a function of their weekly earnings (under a scenario where the calculated wba is $300), for both the treatment and control groups. Although the baseline disregard was more than 0, and the treatment group faced a higher disregard

[23] Furthermore, we note that we find no evidence of "bunching" at the disregard levels, which would be evidence of behavioral responses to the disregard.

($15), the treatment can largely be thought of as a "pivot" of the benefit schedule outward, reflecting the lower τ.

To give a sense of the magnitude of transfers from being in the treatment group, consider how much a claimant with a weekly benefit amount of $300 would receive. According to Figure 2, an individual who does not work would see no difference between the treatment and control benefit schedules. On the other hand, if the individual earned $405 , their benefits would have been just reduced to zero in the control regime, but would be eligible to receive an extra $42 a week—about a 10 percent increase in total income—under the treatment regime.

The goal of the experiment was to measure the extent to which the lower tax rate could induce claimants to work more than they otherwise would and, as a consequence, reduce the amount of UI benefits paid. In the evaluation report of the experiment, O'Leary (1997) found that claimants in the treatment group received $67 more in benefits in the benefit year than the control group, and furthermore, this amount was greater than what the control group would have received if the more generous benefit formula were mechanically applied to the control group's reported earnings. Thus, the behavioral response to the reduction in the marginal tax rate was cost-increasing, rather than cost-reducing. Our analysis interprets these experimental findings (including the impacts on earnings) within the fiscal externality framework, and compares those effects to the quasi-experimental evidence on the marginal effect of increasing *wba* from using the regression kink design.

3.2 Administrative Claims and Employer Earnings and Hours Data

We use administrative data for all individuals who filed for unemployment insurance in Washington between October 1, 1994, and September 30, 1995. The dataset contains detailed records of claimants' demographic characteristics, earnings and hours as reported by the employer on a quarterly basis, before and during the benefit year, the weekly benefit amount, maximum benefits payable, and actual UI payment each week. The administrative measure of quarterly hours is a distinct feature of the Washington data that most other state UI systems do not track, and it provides an alternative measure of work activity throughout the period. The great advantage of using the employer-reported information is that the coverage is probably as complete as any single data source could be: we can capture work activity post-UI-filing irrespective of claiming activity. The alternative source, which is more commonly available in existing UI studies of administrative data, is the employee-reported earnings on the continuing claim forms, which are missing in any week for which the individual does not claim benefits. This source complicates any interpretation of results regarding earnings due to non-random sample selection.

While we have highly detailed information on individuals' earnings, we do not know the exact amount of UI tax paid by each individual, which is needed to compute the tax component of the fiscal externality. Washington, like other states, uses an experience rating system whereby the UI payroll tax rate depends on the employer's history of contributions to and withdrawals from the state's UI trust fund. Since we do not observe the employer's tax rate, we apply the average tax rate of each year, defined as total employer contributions divided by total taxable wages within the year, to individual earnings, up to the statutory cap on taxable earnings.[24]

There were 300,957 UI claimants subject to randomization during the year of the experiment. After removing individuals from the sample who were either 1) ineligible for benefits,[25] 2) ineligible for the experiment because they were part of a timber industry retraining program, or 3) had unexplained errors in their data,[26] the evaluation report on the experiment used a final sample of 278,055 for their analysis (O'Leary, 1997). The analysis in this paper similarly begins with these 278,055 observations. We further drop a relatively small fraction of observations in order to make our analysis less sensitive to outliers in the RKD analysis, so that our main analysis sample contains 272,261 observations.[27]

Table 1 reports the means of baseline characteristics of our main analysis sample, by treatment status. The means of these variables are of similar magnitudes to what one might expect to see in the unemployed population, as measured by the Current Population Survey.[28] For example, 64 percent of the claimants are male and 80 percent are white, whereas the percentages for the unemployed population (including non-claimants) from Washington State in 1994–95 from the CPS are 56 percent and 91 percent, respectively. The average age is 37, and average years of schooling is 10.7 years in our sample. The corresponding estimates

[24]Only the first $19,900 of each employee's wages are taxable in 1994 and 1995 ($20,300 in 1996). Total contribution and taxable wage information is available on the US Department of Labor website: https://workforcesecurity.doleta.gov/unemploy/hb394.asp

[25]Most of these individuals either did not work or worked less than the required 680 hours in their base year to qualify for unemployment benefits.

[26]There are 927 individuals for whom it could not be explained why their weekly UI benefits did not follow the actual benefit formula, minus any deductions recorded in the data.

[27]Specifically, we identified an unusually large number of claimants with their highest two quarterly earnings in the base year totaling to a handful of specific values, which originated from a few employers. This concentration of observations generated "spikes" in the distribution of the running variable. In order to prevent our RKD estimates from being too heavily influenced by a very small number of employers, we dropped all claimants who were previously employed there, leaving us with a final sample size of 272,261.

[28]We restrict the CPS sample to the 1,126 individuals surveyed in Washington between 1994 and 1995 who reported being unemployed.

using CPS data are 35 and 12.8 years. The small variation in means between the treatment and control groups across all the characteristics is consistent with effective randomization of treatment status.

The RKD analysis will produce "weighted average" marginal impacts of an increase in the weekly benefit amount, as described in Section 2.2. While those weights are unobserved, we can nevertheless estimate the weighted means of the baseline characteristics using those same weights. This is given by estimates of the means of the characteristics, conditional on $V = 0$. To give a rough sense of these means, we estimate regressions of each of the characteristics on a polynomial of order 6 in V, adding an interaction between V and a treatment indicator, as well as the treatment indicator variable itself. The intercepts in these regressions give a rough estimate of the conditional means for the control group, and are reported in Column (4) of Table 1. They show that the implicit weights discussed in Section 2.2 are such that the resulting weighted means are similar in magnitude to the overall means from the treatment and control groups. As one might expect, given that the weights will disproportionately upweight those expected to have earnings close to the threshold (which is about $6,000 more than the overall average for two highest quarter earnings), the weighted average age, fraction male, white, and years of schooling—all attributes positively correlated with earnings—are larger than the overall means in the sample.

Column (5) of Table 1 shows the estimated jump in the derivatives from the regressions above. If the conditions for valid causal inference from an RKD hold (Card et al., 2015*b*), we would expect there to be no kinks in any of these baseline characteristics, and overall the estimates are consistent with that prediction. Finally, in Column (6), we report the coefficients on the treatment indicator, which measures the gap between the conditional expectation functions for the treatment and control groups. Since treatment was assigned randomly, the detection of significant differences would be driven by an overly-restrictive functional form (e.g. the 6th-order polynomial). Overall, there are no significant differences found from this specification, indicating that the polynomial of order 6 is a reasonable approximation for gauging these rough magnitudes. That said, in our RKD analysis, we will explore local polynomial estimators of different orders and bandwidths, as has become standard in the RD and RKD literature.

4 Experimental and Quasi-Experimental Fiscal Externality Estimates

This section presents the findings from the Washington UI Earnings Deduction Experiment and the regression kink design analysis. The experiment yields variation in the implicit tax rate τ, whereas the RKD yields variation in the weekly benefit amount *wba*. For both sources of variation, our focus is on estimating the fiscal externality described in the conceptual framework, $\beta + \gamma$. To better understand the factors driving the fiscal externality, we also estimate the behavioral costs due to changes in UI payments (β) and tax receipts (γ) separately, as well as the impact of the policy on various measures of labor supply.

We estimate quantities within varying periods of time since the quarter of the first filing, ranging from the same quarter of the initial filing (indicated in the tables as "Q1") to four quarters ("Q1 to Q4") after the filing. Specifically, since some of our parameters combine behavioral effects on UI payments and tax receipts (which are measured using quarterly earnings), we aggregate the weekly UI payment data so that we measure total payments within calendar quarters. Therefore, "Q1" covers the entire quarter in which the individual initiated the claim, and "Q1 to Q4," for example, covers Q1 and the subsequent three calendar quarters.[29]

4.1 Estimates from the UI Earnings Deduction Experiment

To estimate the fiscal externality of decreasing the earnings deduction rate, we must first decompose the effect of the experiment on UI payments into its behavioral and mechanical components. The top four rows of Table 2 show the average UI payments within each time frame for the treatment and control group. On average, the control group receives a total of $\$2,689$ over four quarters, with most of that amount ($\$2,447$) accumulating within the first three quarters. As in O'Leary (1997), we find with statistical significance that the treatment group received more UI benefit payments than the control group. The causal effect of the lower implicit tax rate grows over time, from \$16 in Q1 to \$67 for the full Q1–Q4 time frame.

A positive effect on total UI payments is to be expected even in the absence of any behavioral response to the more generous benefit schedule. In Table 3, we break the effect up into mechanical and behavioral components. The former measures the additional payments that would result from a formulaic application of the treatment benefit schedule to the control population. To compute these amounts, we simply infer each

[29] Since the initial filing can occur any time within the quarter, the length of the post-filing window varies from 1 to 13 weeks for Q1. Note that in all of our time frames, all outcomes will include pre-filing data; with successful randomization and a valid RKD design, this inclusion should "difference out" and should not impact the consistency of the estimates, but could increase sampling variability.

control group member's implied weekly earnings from their received weekly benefits, and then apply the treatment group's benefit formula to the control group.[30] The difference in total UI payments between the simulated treatment group and the control group is our estimate of the mechanical effect. The difference between the full treatment effect and the mechanical effect is the effect that can be attributed to a behavioral response to the different benefit formula. Our estimates imply that by the fourth quarter, the mechanical effect accounts for \$28 of the total difference, while the remaining \$39 is due to behavioral responses.

With the mechanical effect in hand, we are able to estimate the fiscal externality. We compute the difference in UI payments net of taxes between the treatment and control groups, where tax receipts are estimated by applying the average payroll tax rate schedule on earnings.[31] By subtracting the mechanical effect from this treatment effect, and then taking the ratio of this difference with the mechanical effect, we obtain an estimate of the fiscal externality $\beta + \gamma$, which we report in column (5) of Table 3. The fiscal externality over four quarters is \$1.38 (standard error \$0.45) per dollar of mechanical transfer, meaning that for every dollar transferred inframarginally via a decrease in the earnings deduction rate τ, there is an additional cost of \$1.38 to the government budget due to behavioral responses.

To understand how much of the fiscal externality is driven by increased UI payments or reduced tax receipts, we estimate β and γ separately. The component of the externality due to UI payments β is computed by taking the ratio of the behavioral UI payment effect (column 3) to the mechanical effect (column 2), while the component due to tax receipts is the ratio of the experimental impact on taxes (column 4) to the mechanical payment effect.[32] The results suggest that the entire \$1.38 of the fiscal externality over four quarters is due to the behavioral impacts on UI payments.[33]

[30]For those receiving the zero benefits, we apply the treatment group formula assuming that earnings exceed the break-even point above which a claimant in the treatment group would no longer receive benefits. In effect, we impose that individuals who receive zero benefits in the control group would continue to receive zero benefits in the treatment group. In principle though, earnings can be above the control schedule's break-even point but below the treatment schedule's. An alternative approach to calculating the mechanical effect is to use the reported earnings in the claimant data. In practice, there are some unexplained deviations between the UI payments and that which would be predicted by the earnings reported on the weekly continuing claim form. Our approach will be favorable if the payment data are considered to be measured with less error than the earnings variable in the claims data.

[31]We use average UI payroll tax rates, which were 1.96 percent, 1.92 percent, and 1.88 percent for 1994, 1995, and 1996, respectively. We conduct our welfare analysis from the perspective of the state considering only the fiscal impacts of each policy on the UI benefits and taxes. If one takes a more expansive view of government (i.e. from the perspective of the federal government), one could also consider the impact of the policy on federal income tax receipts, which would be larger. See Schmieder and von Wachter (2016) and Lawson (2017) for more a more detailed discussion.

[32]The standard errors account for the covariance between the estimators for the behavioral and mechanical effects via a stacked regression, in which we estimate the two equations jointly and cluster the standard error by individual (similar stacked regressions can be seen in more detail in Section 4.4.2. of Lee and Lemieux, 2010 and Section 5.3 of Pei, Pischke and Schwandt, forthcoming).

[33]When we use the "full tax wedge" tax rate (31.54 percent) of Schmieder and von Wachter (2016) that accounts for other government expenditures, γ is estimated to be 65 cents (with a standard error 70 cents), resulting in fiscal externality estimate \$2.03 (standard error \$0.85) over four quarters.

We also document the experimental impacts on various measures of labor supply directly. Indeed, one of the motivating questions behind the Washington earnings deduction experiment was whether a lower implicit tax rate in the benefit schedule will increase employment and earnings. The second set of rows in Table 2 reports the average effect of the experiment on earnings. Unlike with UI payments, we do not find any statistically significant differences in earnings between the treatment and control group in any time frame. Furthermore, the magnitudes that we can statistically rule out are small relative to the means. For example, for Q1 to Q4, the 95 percent confidence interval is between \$184 to \$68 on a base of \$13,388—or 1.4 percent to 0.5 percent. Consistent with the null earnings effects, we find no statistically significant impacts of the experiment on cumulative hours over any time frame and the range of the 95 percent confidence intervals are small relative to the mean (third set of rows in Table 2). As seen in the fourth and fifth set of rows in Table 2, we also detect no extensive margin effects: the proportion of non-zero earnings or hours in the treatment and control groups are nearly identical in all time frames, with standard errors between 0.0010 and 0.0018 on means ranging from 0.80 to 0.95. The intensive margin effects, shown in the sixth and seventh set of rows in Table 2 in terms of log(earnings) and log(hours) mirror the results for levels. The treatment and control means are identical to the second decimal place, with differences never greater than 0.004 and statistically indistinguishable from zero.[34] Overall, we consistently find no impact of the experiment on labor supply.

To gain further insight on the drivers of the behavioral effect captured in β, we report in the last six rows of Table 2 the experimental impacts on the frequency of claiming UI and the duration of the first UI spell. From Q1 to Q4, the average number of weeks claimed is 13.23 in the control group and 13.76 weeks (4 percent higher) in the treatment group. This statistically significant increase in claims can potentially be explained by the length of the initial UI spell, defined as the number of weeks of UI payments with no more than a three-week gap, which is 0.60 greater in the treatment group than in the control group. Since there was no detectable effect of the experiment on earnings, this evidence suggests that an increased propensity to claim is the driving force behind the behavioral response to the more generous benefit formula.

Finally, we point out that while the earnings effects are statistically insignificant, they are roughly on par (and opposite sign) to the point estimates of the UI payment effects. An interesting question from a policy perspective, which goes beyond the welfare framework described in Section 2, is whether the policy had an

[34] By construction, the logarithm drops all observations with zero earnings or hours, so the treatment and control means are conditioned on a potentially selected sample. However, as discussed in Lee (2009), if we assume that the treatment effect on sample selection is characterized by a monotonicity condition, then given that we found no differences in the proportions of missing values in the treatment and control populations, we can interpret the observed log-earnings and log-hours differences as reflecting causal impacts of the treatment.

impact on workers' incomes, where income includes both earnings and UI payments. Adding up the effects from the first two sets of rows in Table 2, we find that the the point estimate suggests a small positive impact of about \$9 on income over the four quarters. If we scale this number by treatment-control difference in UI payments, \$67, this implies that a one dollar increase in UI transferred increases the income of workers by 13 cents. A 95 percent confidence interval, however, suggests that income may be reduced by as much as \$1.71 and or increased as much as \$1.98 for every dollar of UI transferred.[35] We will return to this number in the next section, when we estimate the same quantity for the weekly benefit level policy.

4.2 Estimates from the Regression Kink Design

This section reports the regression kink estimates of the fiscal externality associated with a marginal increase in the weekly benefit level (*wba*). As with the analysis of the experiment, we need to estimate the impact of *wba* on UI payments and tax receipts, but in this section, we will use the quasi-experimental variation in benefits generated by the UI benefits cap. We first present graphical evidence of the design's validity and impacts on the behavioral and mechanical components of UI payments and earnings before proceeding to our main estimates. After verifying the robustness of our estimates to various estimating specifications, we turn to estimates of the fiscal externality parameters of interest.

Figure 3 shows the RK first stage, plotting the weekly benefit amount *wba* against the normalized two highest quarter earnings variable, i.e. the running variable V. Each solid circle represents the average of *wba* (left y-axis) within a V-bin of width \$400. The empirical relationship between V and *wba* tracks closely the statutory benefit formula described in Section 3, in the range where V is above –\$14,000. We do see some modest departure from the benefit formula, which is common in RK analyses, possibly due to measurement error or non-compliance with the rule.[36] Therefore, we apply a fuzzy regression discontinuity design as described in Card et al. (2015*b*).

To confirm our understanding of the UI benefit formula, we also plot the maximum benefit payable *mbp* (right y-axis) against V in Figure 3 separately for workers with $R > 0.8$ (hollow circles) and $R \leqslant 0.8$ (hollow triangles), where $R = \frac{Q_3+Q_4}{Q_1+Q_2}$ and Q_i is the earnings from the i^{th} highest paid quarter in the base period. As discussed in Section 3, the relation between V and *mbp* differs depending on whether the ratio R is above or below 0.8. When $R > 0.8$, the rules imply a kink in *mbp*, because $\frac{mbp}{wba}$, the maximum number of weeks

[35]This confidence interval accounts for the estimated impact on UI payments.

[36]The recorded *wba* is within 1 dollar of the nominal *wba* using V for 93.8 percent of the sample.

of UI at the full benefit amount, is exactly 30 throughout. When $R \leqslant 0.8$, we would not expect a kink in the relationship between *mbp* and V. We see both of these features in the empirical relationship when viewing them separately for individuals who have an R above or below 0.8. In our main analysis, we pool observations from both sub-groups to maximize statistical power. We note that any estimated kink from the pooled sample will include the marginal effect of *both* the marginal effects of *wba and mbp* for those with $R > 0.8$. That said, the estimates when only using 72 percent of the sample that has $R \leqslant 0.8$ are similar.

Following Card et al. (2015*b*), a condition for a valid regression kink design is that, conditional on unobservables, the density of V is continuously differentiable at the kink threshold. The first way to test this identifying assumption is to examine the empirical density of V, to determine if there is evidence of "sorting" around the threshold—this is the RKD analogue to the test of McCrary (2008). We plot the frequency distribution of the running variable V in Figure 4. Finding a kink in the density at the threshold would indicate a rejection of the identifying smooth density assumption at the individual level. The frequencies do not reveal any large kinks in the histogram. It is also clear that the observations in the left tail of the V distribution causing the most deviations in Figure 3 are relatively few in number. In subsequent graphical presentations, we will restrict to the range of $V \in [-14000, 14000]$.

A second method of assessing the validity of the identifying smoothness assumptions is to examine whether or not there are kinks in the relationship between the baseline characteristics and V. Column (5) of Table 1 presents evidence showing that a global parametric regression does not reveal significant kinks in those characteristics. In principle, one can perform an in-depth examination of those estimates and their sensitivity to different functional forms and bandwidths, for each variable. As an alternative, we adopt the approach taken in Card et al. (2015*a*) and Card et al. (2017), and examine a "covariate index" for each outcome that we consider. The index is simply a linear combination of the baseline characteristics, where the coefficients are those from a regression of the outcome on the covariates.[37] This constructed index is measured in the same units as the outcome variable, and so we can directly compare the covariate index to the actual outcome of interest in our plots.

We now present graphical evidence on the key outcomes in our analysis before turning to estimation and inference. When indicated, we "de-trend" both the outcome and the covariate index from a linear regression of the outcome (or covariate index) on the running variable V in order to facilitate the visual detection of

[37]The covariates are age, age-squared, sex, ethnicity, years of schooling, veteran status, job center location, occupation, industry, self-employment, week in quarter of initial UI claim, and the earnings and hours in each quarter of the base year.

a kink in the relationship. We hypothesize that it can be difficult to recognize a kink when the slope of the function is steep. Residualizing the outcome (and covariate index) from a linear prediction using V effectively rotates the relationship around the threshold, which may improve the ability to see a feature such as a kink near the threshold. Using this method, Figure 5 plots total UI payments against V in the four time periods. Each dot represents an average payment amount within a bin that is $400 wide. In all four graphs, there is a visible break in the slope of the relationship, with the magnitude of the kink growing as the time frame expands, and as more benefits are received. By contrast, the comparable plots for the covariate index are quite flat with respect to V.

Figure 6 plots the empirical relation between V and the mechanical increase in UI payments from a dollar increase in *wba* within the four time periods. The mechanical effect is the additional benefits that claimants would receive if their weekly benefit amounts were $1 higher, and they did not change their UI take-up or labor supply behavior. To compute this amount, we add a dollar to the benefits that claimants receive each week that they claim, and then restrict their cumulative payments to be no more than their *mbp*, accounting also for the fact that *mbp* increases by $30 for individuals with $R > 0.8$. The difference between the simulated UI payments and the actual UI payments is the mechanical increase in UI payments from a marginal increase in *wba*. For many claimants, this mechanical effect is equal to the number of weeks that they claim UI benefits, because absent any behavioral response, a dollar increase in *wba* simply translates to an additional dollar per week claimed. However, since individuals cannot receive more benefits than their *mbp*, some claimants' mechanical effects are less than the number of weeks that they claim UI payments. We do not de-trend the graphs in Figure 6 in order to show our estimate of the graphs' intercepts, which is the mechanical effect of a marginal increase in *wba* for individuals at the kink. Thus, for example, in the Q1 to Q4 period, the pure mechanical effect of a dollar increase in the *wba* is about 11.

Figure 7 displays the relationship between cumulative earnings and V for each follow-up period. In this case, there are no visible kinks in either the actual outcome or the covariate index for this outcome, although for the Q1 to Q4 period, the variability in the binned averages could potentially be masking kinks in either direction. The covariate index appears to have much less variability and does not appear to show any kinks in the relationship with V. Based on the visual evidence for earnings, unlike the previous figures, we might expect to see statistically insignificant effects of *wba* on earnings.

To estimate the magnitudes of these kinks, we follow the approach in Card et al. (2015*b*) and estimate the fuzzy RKD estimand by taking the ratio of estimated kinks in the outcome and observed *wba* with respect to V, where each kink is estimated by a local polynomial estimator of the change in the derivative at $V = 0$. Numerically, this is equivalent to restricting the data within a bandwidth of the threshold, and regressing the outcome on *wba*, instrumenting with $A \cdot V$, using as controls polynomial terms V^k and $A \cdot V^k$ (excluding the linear $A \cdot V$ term) for k up to order p, where $A = \mathbf{1}[V \geq 0]$ indicates that a worker has base period earnings above the threshold.

Two tuning parameters are required for RK estimation: the bandwidth h and polynomial order p. For h, we use the mean squared error (MSE) optimal bandwidth from the `rdrobust` Stata package by Calonico et al., 2017 (henceforth CCT bandwidth).[38] For polynomial order p, we choose the order from the set $\{1,2,3\}$ that delivers the lowest estimated MSE as per Pei et al. (2018) by using the accompanying `rdmse` Stata package. We also present evidence that the RK estimates of our key outcomes are generally robust with respect to these tuning parameters.

Table 4 presents the fuzzy RK estimation results for the same outcomes as in Table 2. The first two columns report the tuning parameters used in estimation: column (1) reports the polynomial order p that minimizes estimated MSE, and column (2) the CCT bandwidth h for the selected p. A quadratic or linear specification is chosen for the vast majority of outcomes, though in a handful of cases, a cubic polynomial is estimated. In terms of our main outcomes, the effects on UI payments are estimated with a linear specification, while the effects on earnings are generally estimated with a quadratic specification. In general, the optimal bandwidth for linear specifications (ranging from 1,453 to 7,817) is smaller than that for higher order polynomials (ranging from 2,991 to 12,208 for quadratic). As reported in column (3), the corresponding effective sample size, i.e., the number of observations within bandwidth h, ranges from 20,164 to 127,864 for local linear and from 41,164 to 225,983 for local quadratic.

The first stage estimates in column (4) confirm the formulaic relationship graphically presented in Figure 3 between the treatment variable *wba* and the running variable V. In rows where the level of *wba* is the treatment variable — every row except for the last row, for which $\log(wba)$ is the treatment variable — the first stage estimate almost always falls between –0.019 and –0.020; recall that –0.02 is the exact slope

[38] When using the default CCT bandwidth, which includes a regularization term, we find that our estimates in many cases suffer from low precision. This is the case, for example, with the estimate of cumulative UI payments from Q1 to Q4, despite the visually striking and institutionally grounded kink in the lower right panel of Figure 5. Therefore, our results reflect estimates without the regularization term. Consistent with the findings in Card et al. (2017), the regularization term, while asymptotically negligible, greatly impacts the computed value of the RK optimal bandwidth. Following Card et al. (2015*b*), we use the fuzzy CCT bandwidth without regularization as our specification to estimate the various RK effects, but explore the sensitivity of our estimates to bandwidth choices in Figures 8, 9, A.1, and A.2.

change implied by the formula.[39] The first stage is also precisely estimated, and the associated F-statistic is above 1,000 for every outcome.

We present the fuzzy RK estimate in column (5). In all rows but the last, the estimate represents the average marginal change in the outcome in response to a one dollar increase in the *wba*, and in the last row, we report the elasticity of initial claim duration with respect to *wba* in order to connect our estimate to the UI literature. Looking down the first four rows, we see that a dollar increase in *wba* is estimated to increase total UI payments by 4.7 dollars in Q1 and 15.9 dollars in Q1–Q4. These cumulative UI payment estimates are statistically significant, and encompass both the mechanical effect and the behavioral response. The next four rows report the fuzzy RK estimates for cumulative earnings within the four time periods. The effects are statistically insignificant except for that of Q1, and a closer inspection, which we describe below, indicates that the Q1 estimate is likely an outlier. Consistent with the insignificant cumulative earnings result, the RK estimates for the next sets of outcomes—cumulative hours, whether a worker has positive cumulative earnings/hours, and log(cumulative earnings/hours)—are generally statistically insignificant as well.

In the final rows of Table 4, we present our estimates of *wba* on the initial benefit duration, defined as the number of consecutive weeks of UI payments with a gap of no longer than three weeks. To compare with the literature, we redefine the treatment variable (in the last row) to $\log(wba)$ so that the estimate can be interpreted as a duration elasticity. We find an estimated elasticity of 1.06, with a 95 percent confidence interval that allows us to rule out elasticities below 0.25. This point estimate is larger than the elasticity of 0.88 recently found in Card et al. (2015*a*), which uses the same methodology and similar administrative data. As a visual aid for comparison, we plot in Figure 10 the empirical relation between the logarithm of the initial UI spell duration and base earnings, using the same running variable scale and range as Card et al. (2015*a*).[40] While the figure does show a visible break at the discontinuity threshold, variability in the binned averages appears to be larger in Washington compared to Missouri.[41]

Figures 8 and 9 show graphically the sensitivity of our estimates to alternative regression specifications for our two main sets of outcomes. Figure 8 plots the estimated RK effects of *wba* on total UI payments

[39] The only exception is log(cumulative hours), for which a cubic polynomial with bandwidth 14,647 is selected with a resulting first stage estimate of –0.014. This is one of the few cases where the bandwidth seems too large, resulting from the lack of regularization.

[40] We adjust for inflation and the fact that the running variable in Card et al. (2015*a*) was measured in terms of one quarter of earnings.

[41] A key difference between the two contexts is that the threshold in Washington State is much higher than in Missouri in an absolute (inflation-adjusted) and relative (position in the base earnings distribution) sense. When we look at approximately the same range around the threshold, we have much fewer observations in our analysis compared to the pre-recession Missouri sample in Card et al. (2015*a*).

Q1–Q4 (row 4 of Table 4) using different bandwidths and a different polynomial order. The solid black curve plots quadratic estimates for bandwidths ranging from 2,000 to 14,000 in 500 increments, and the short dashed black lines plot the upper and lower end of the pointwise 95 percent confidence intervals. In gray, we generate the analogous series of estimates using a linear specification. Our point estimate of about $16 for total UI payments over four quarters is consistent with those estimated using a number of alternative bandwidths in the stable range for both linear and quadratic specifications, though the standard errors are smaller for the linear estimates. Figure 9 shows that the negligible effect for earnings are robust to both specifications and for a wide range of bandwidth choices. Appendix Figures A.1 and A.2 show the analogous graphs for UI payments and earnings in all quarters (the lower right corners of the two figures replicate Figures 8 and 9, respectively). Although we find statistically significant and positive quadratic RK estimate for earnings effects in Q1 in Table 4, it is only attained for a set of very large bandwidths, and the unregularized CCT bandwidth happens to fall into this range. Therefore, while we report the positive effect using the unregularized quadratic CCT bandwidth in Table 4, we will focus on the more robust Q1–Q4 estimate on cumulative earnings in our discussion.

Table 5 shows our estimates of the fiscal externality $\beta+\gamma$. We first reproduce the estimates on UI payments from Table 4 in the first column. The mechanical effects (column 2) are the estimated intercepts in Figure 6, which we obtain from a regression of the simulated mechanical increase in UI payments on quintic polynomials in V and $A \cdot V$, and the behavioral effects on UI payments are the differences between total and mechanical effects. Column (4) shows the fuzzy RKD estimates of the increase in *wba* on tax receipts, which are calculated by applying the average payroll tax schedule on earnings. We find economically small estimates of *wba* on tax receipts, which is consistent with the negligible estimates earnings effects of Table 4. The Q1–Q4 estimate of the fiscal externality suggests that to transfer one mechanical dollar via an increase in the weekly benefit amount, it costs an extra 53 cents (with a confidence interval of 27 cents to 78 cents) to pay for the behavioral claiming response and the lost UI tax revenue due to changes in labor earnings. In comparison, recall that the fiscal externality of decreasing the implicit tax rate that we estimated earlier is in the range of 49 cents to $2.26 with a point estimate of $1.38. Unsurprisingly, due to the low tax rates, we find that the fiscal externality is driven mostly by the increased benefit payments when we break the estimates down into components due to UI payments and tax receipts (columns 6 and 7).[42,43]

[42]When we use the "full tax wedge" tax rate of 31.54 percent (Schmieder and von Wachter 2016), we obtain a similar estimated γ of –0.02 (standard error 0.15) and a total fiscal externality of 0.53 (standard error 0.24) over four quarters.

[43]We use the same bandwidths and specifications, which minimize estimated MSE when UI payments are the outcomes, to

While the fiscal externality estimates are useful for quantifying the behavioral impacts of a policy, one may also look beyond the standard welfare framework to another economically meaningful metric of how the policy affects overall income of its intended recipients. As before, we estimate the dollar increase in total income, where total income includes UI payments and wages, associated with a dollar transferred to recipients in the form of UI payments. To compute this, we estimate a fuzzy RK specification using income as the outcome and UI payments (instead of *wba*) as a first stage.[44] The resulting estimate suggests that a one dollar transfer of UI payments increases income by 96 cents, with a 95 percent confidence interval suggesting an income increase by at least 66 cents. In contrast with the implicit tax rate policy, which increases total incomes by (a statistically insignificant) 13 cents per dollar transferred, an increase in the weekly benefit level is therefore more effective at increasing recipient income.

5 Implications and Conclusions

This paper proposes a "decomposition" method of estimating fiscal externalities associated with government income transfer policies as a complement to the prevailing sufficient statistics approach. By employing a simple accounting identity, the behavioral component of the effect of a policy on the government budget can be estimated from the microdata as the difference between the estimated total effect and the mechanical effect that can be calculated using knowledge of the income transfer formula. This allows one to compute a fiscal externality quantity that is consistent with a very large class of behavioral models, and therefore can serve as a complementary benchmark to approaches that require more modeling assumptions. While it follows quite naturally from well-known public economics principles, this method of estimation does not appear to have been employed in the empirical analysis of UI, or other income transfer programs.

We use this decomposition framework to evaluate the efficiency of two policy experiments in the Washington State UI system. First, we examine a large randomized experiment that reduced the implicit tax rate on the earnings of UI claimants. While the hope was that the enhanced work incentive could have generated a positive labor supply response and hence reduced the cost to the government, instead we find that for each dollar of inframarginal transfer, the government must pay at least an extra 49 cents to cover the increased benefit expenditure, with a point estimate of $1.38.

By contrast, our confidence interval for the fiscal externality from increasing the maximum weekly

estimate the different components of the fiscal externality.

[44] Bandwidth and polynomial orders are chosen to minimize estimated MSE when the outcome is total income.

benefit amount ranges from \$0.27 to \$0.79 with a point estimate of \$0.53 for each dollar of mechanical transfer. As a point of comparison, Schmieder and von Wachter (2016) survey the US literature, using elasticities (typically the UI spell duration with respect to the benefit level) estimated in the literature to compute the fiscal externality, and find a range of 0.08 to 0.95, with a median of 0.26. Our estimate is thus on the high side of this range.

Comparing the fiscal externality quantities for the above two policies suggests that, despite the intent of increasing work incentives through a lower implicit tax rate, increasing the weekly benefit is a relatively more efficient way of transferring income to UI recipients. The fiscal externality of a lower implicit tax rate in UI has a point estimate of \$1.38, which is more than double the corresponding measure of increasing the weekly benefit amount, \$0.53. The two fiscal externalities are statistically significantly different at the 10 percent level.[45]

We can also directly compare our estimated fiscal externality for an increase in the maximum weekly benefit to one obtained using the formulas of Schmieder and von Wachter (2016) within our empirical setting.[46] With our estimate of the claim duration elasticity of 1.06, the Schmieder and von Wachter (2016) formula yields an estimate of 1.24. This suggests that for our context, the simplifying assumptions invoked to extrapolate from the claim duration elasticity overstates the fiscal externality.

We believe there are a number of avenues of research that may be promising, based on the decomposition approach that we employ. First, our analysis focuses on the margin of increasing the weekly benefit and reducing the implicit tax rate on earnings. But there is little reason why the same approach for estimating the fiscal externality could not be similarly employed to estimate the impact of a policy that lengthened the maximum duration of UI benefits. As suggested by Schmieder and von Wachter (2016), the side-by-side comparison between the fiscal externalities associated with the benefit margin and the maximum duration

[45] Another way to compare the two fiscal externalities while accounting for the sampling errors in both estimates is to consider the following thought experiment. Suppose a policymaker intends to transfer a portion ξ of benefits by increasing *wba* and the remainder $1-\xi$ by decreasing τ. With our estimates of the fiscal externality of increasing *wba* and decreasing τ in hand — denoted by $(\hat{\beta}+\hat{\gamma})_{wba}$ and $(\hat{\beta}+\hat{\gamma})_{\tau}$, respectively — the policy maker can forecast the fiscal externality by using the estimates from this paper:

$$(\hat{\beta}+\hat{\gamma})_{\xi} = \xi(\hat{\beta}+\hat{\gamma})_{wba} + (1-\xi)(\hat{\beta}+\hat{\gamma})_{\tau}.$$

She knows that she should set $\xi = 1$ to minimize the $(\hat{\beta}+\hat{\gamma})_{\xi}$. But given the uncertainty of the estimates, she also wants to minimize the "5 percent worst case" level of fiscal externality. That is, she would like to choose ξ to minimize the upper bound of the one-sided 95 percent confidence interval:

$$(\hat{\beta}+\hat{\gamma})_{\xi} + 1.64 * SE[(\hat{\beta}+\hat{\gamma})_{\xi}].$$

Because $SE[(\hat{\beta}+\hat{\gamma})_{wba}] = \$0.13$ is also substantially smaller than $SE[(\hat{\beta}+\hat{\gamma})_{\tau}] = \$0.45$ and that the estimated correlation between the two fiscal externalities is very close to zero, the policy maker's choice is again $\xi = 1$.

[46] We thank Johannes Schmieder for providing the code for Schmieder and von Wachter (2016) calculations for this exercise.

margin can suggest the margin along which UI transfers can be made with minimal distortionary burden.

Second, although we find that our direct estimate of the fiscal externality of an increase in the benefit amount is somewhat smaller than that computed using the modeling framework and formula of Schmieder and von Wachter (2016), it is still an open question whether the size of this discrepancy is an outlier or a magnitude that could be expected. In principle, our proposed decomposition approach to estimating the fiscal externality can be adapted to the sources of UI policy variation that has been used in previous empirical studies of UI. The systematic comparison between the nonparametric decomposition-based benchmark and estimates using parametric formulae can be informative in assessing parametric modeling approaches, including "sufficient statistic" approaches, to accurately forecast the fiscal externalities of new policies.

Finally, as is evident from the framework in Section 2, there is virtually nothing in the empirical strategy that is specific or unique to UI. In principle, other income transfer programs — as long as one has relatively complete microdata on benefits and taxes, and the inputs needed to simulate a mechanical effect — can be re-examined to compute the fiscal externality parameter. It would be useful to compare the relative efficiency of transfers across different programs as in Hendren (2016), but using a similar decomposition strategy as proposed in this paper.

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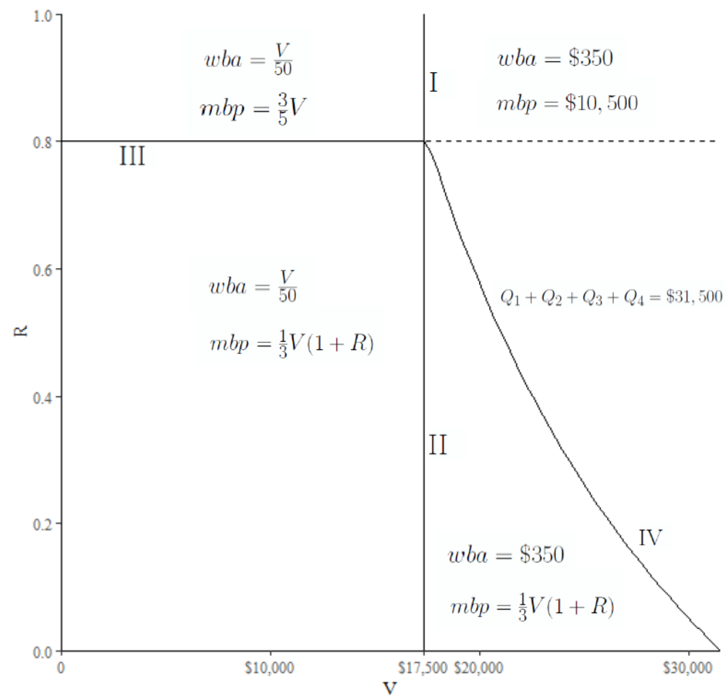
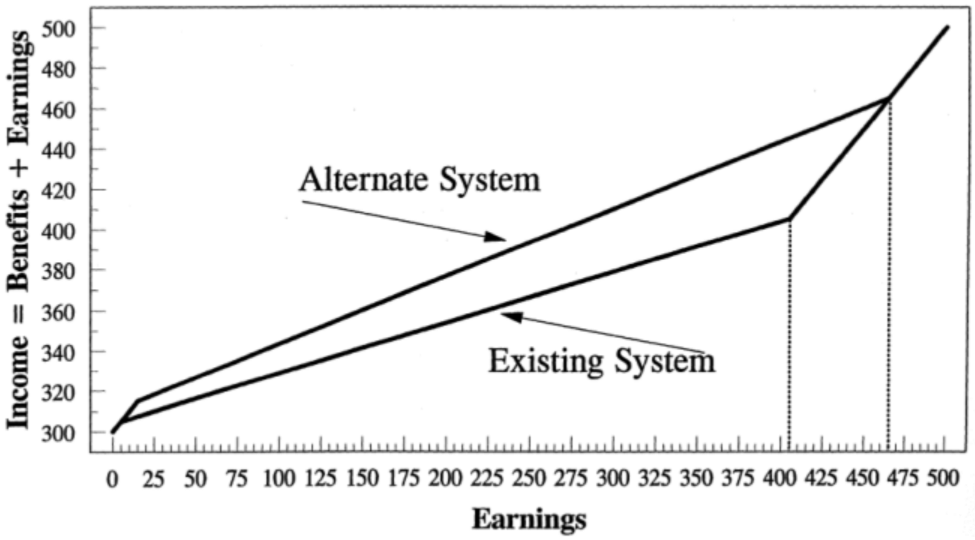
Figure 1: The Relationship Between UI Benefit Parameters and Earnings in the Base Year



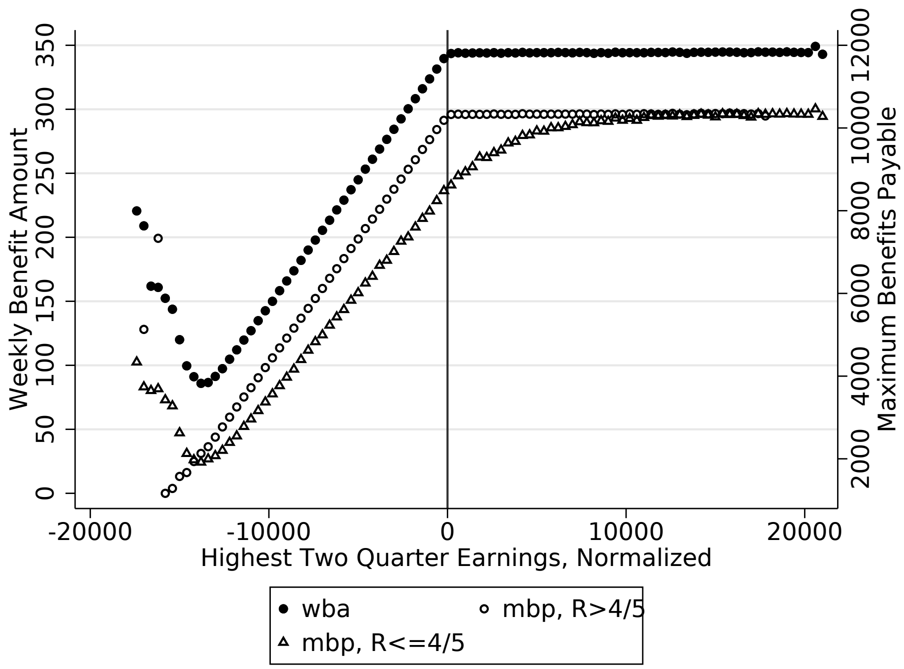
Note: $V = Q_1 + Q_2$ is the sum of the two highest earning quarters in the base year. $R = \frac{Q_3+Q_4}{Q_1+Q_2}$ is the ratio of the two lowest earning quarters in the base year to the two highest earnings quarters in the base year. The slope of *wba* with respect to V changes sharply at $V = \$17,500$ (boundaries I and II). If $R > 0.8$, then the slope of *mbp* also changes at $V = \$17,500$ (I). The slope of *mbp* with respect to R changes at $R = 0.8$ (III). The slope of *mbp* with respect to $V(1+R)$ changes at $V(1+R) = \$31,500$ (IV).

Figure 2: The Relationship Between Weekly Net Income and Earnings, by Treatment and Control Group of the Unemployment Insurance Earnings Deduction Experiment



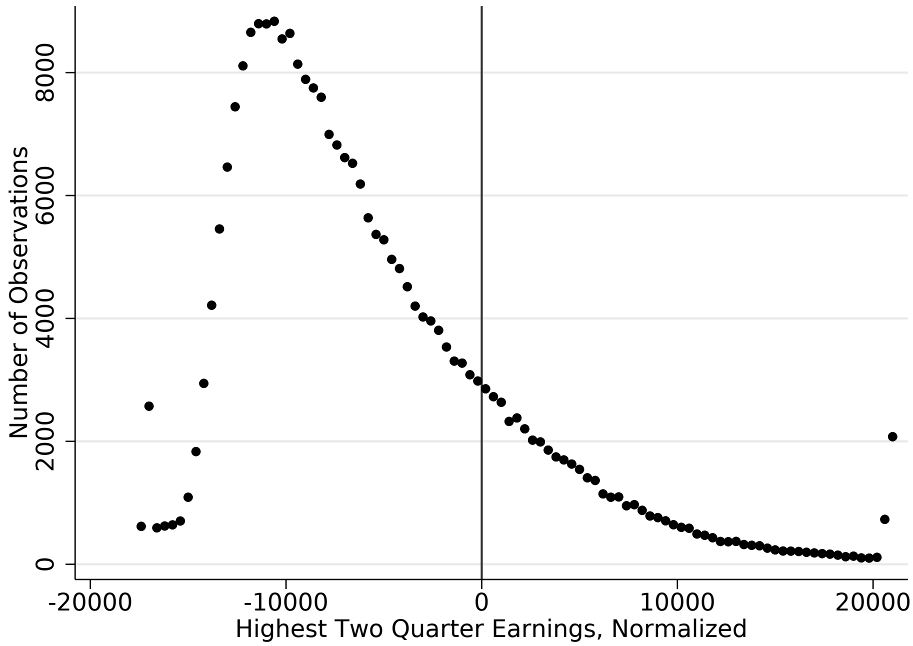
Note: Weekly benefit amount is $300. From O'Leary (1997).

Figure 3: Weekly Benefit Amount and Maximum Benefits Payable versus Base Year Earnings



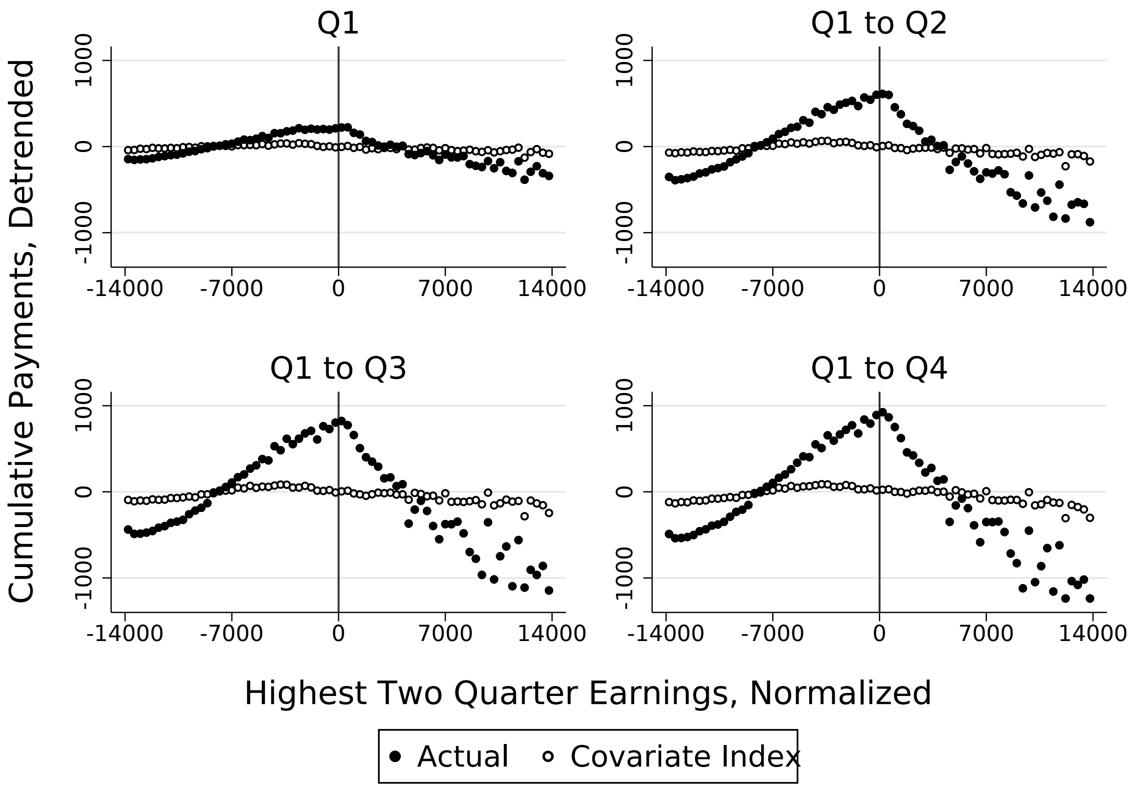
Note: $R = \frac{Q_3+Q_4}{Q_1+Q_2}$ is the ratio of the two lowest earning quarters in the base year to the two highest earnings quarters in the base year. There are 74,817 claimants in our sample with $R > 4/5$ and 195,444 with $R \leq 4/5$ (2,475 claimants had no earnings in the two lowest earning quarters). There are 97 bins (87 bins for $R > 4/5$), each \$400 wide.

Figure 4: Frequency Distribution of Base Year Earnings



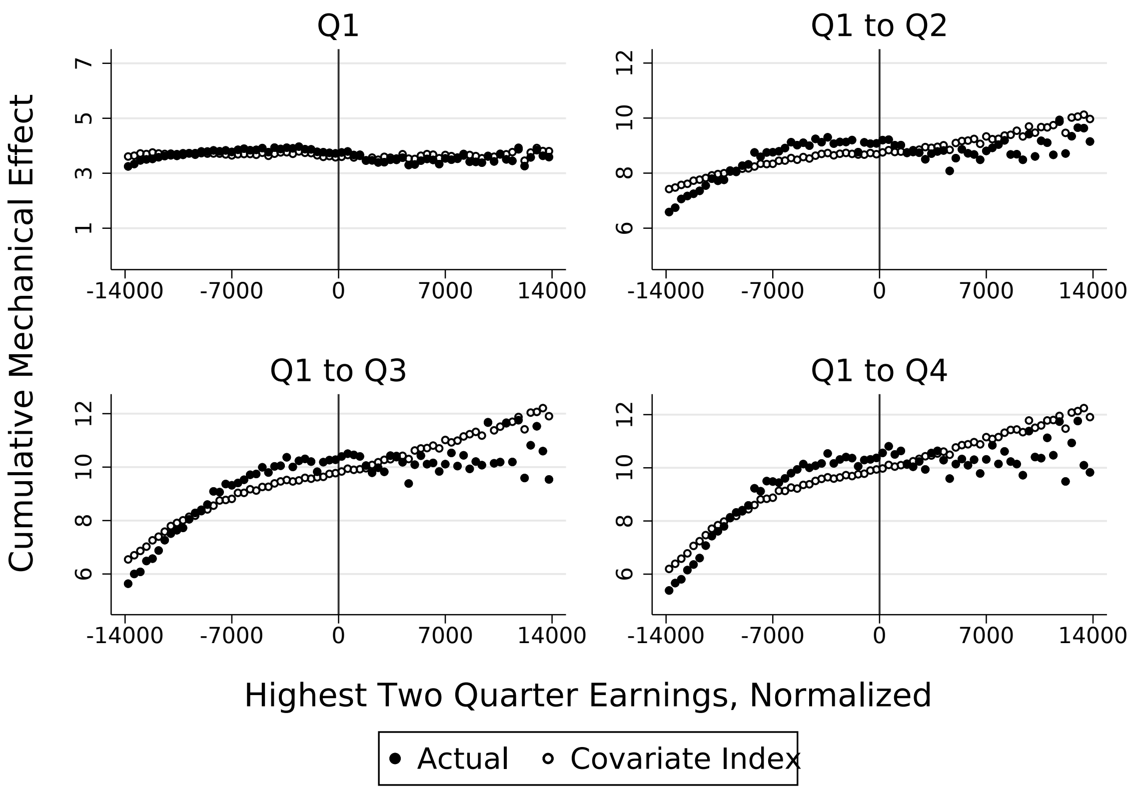
Note: This graph shows the number of observations in each $400 bins of the running variable, the highest two quarters of earnings in the base year. The running variable is normalized relative to the kink threshold ($17,150 before July 1995 and $17,500 after July 1995). There are 97 bins in total.

Figure 5: Cumulative UI Payments versus Base Year Earnings, by Quarter



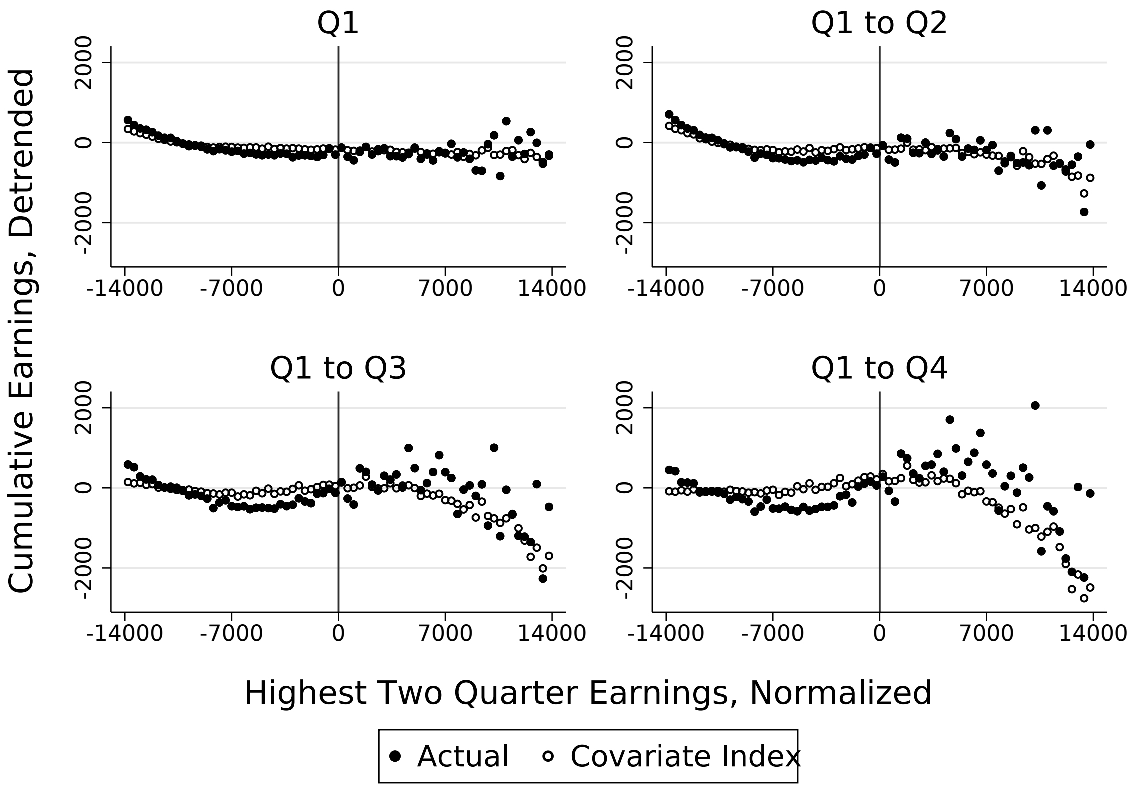
Note: Each graph shows the averages, within bins of highest two quarter earnings, of total UI payments accumulated since the quarter in which the first UI claim was filed. Covariate index is a linear combination of baseline characteristics (details in text). Both dependent variables are deviated from a linear regression fit of the running variable. Only data within $14,000 of the threshold are shown. There are 70 bins, each $400 wide.

Figure 6: Mechanical Increase in Cumulative UI Payments from a Dollar Increase in the Weekly Benefit Amount versus Base Year Earnings, by Quarter



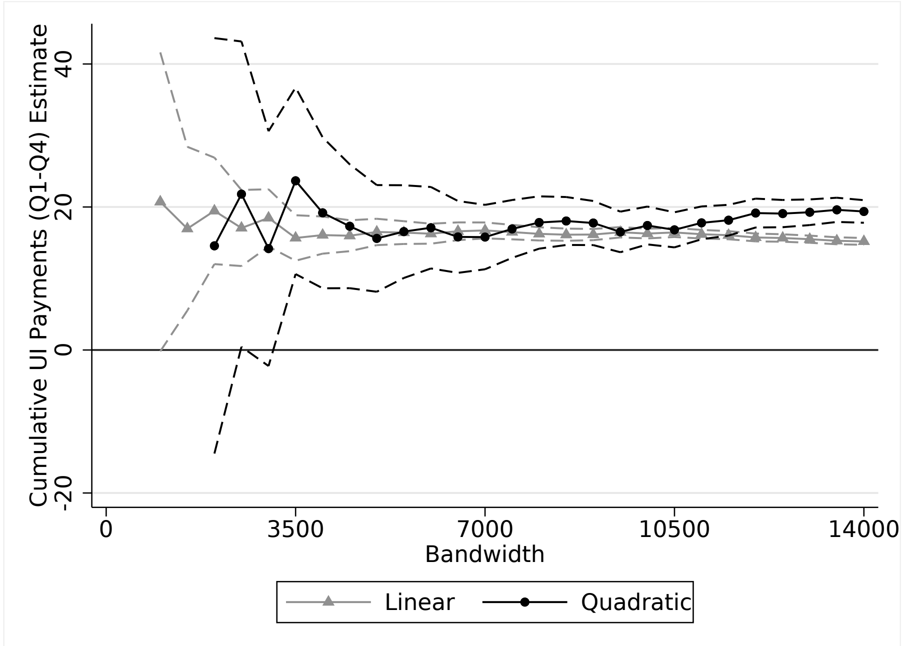
Note: Each graph shows the averages, within bins of highest two quarter earnings, of the mechanical increase in UI benefits that claimants receive since the quarter in which the first UI claim was filed. Covariate index is a linear combination of baseline characteristics (details in text). Only data within $14,000 of the threshold are shown. There are 70 bins, each $400 wide.

Figure 7: Cumulative Earnings versus Base Year Earnings, by Quarter



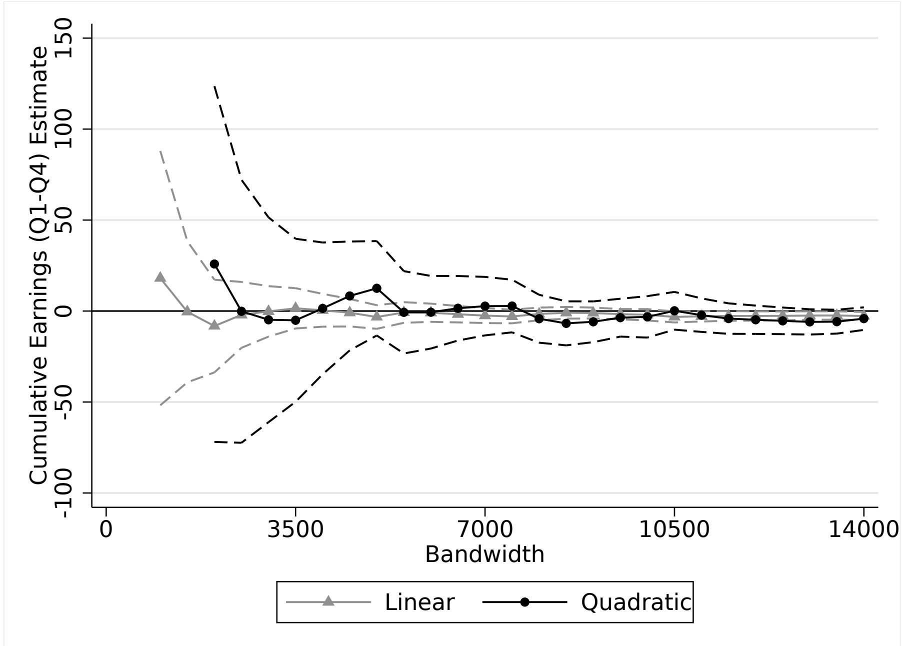
Note: Each graph shows the averages, within bins of highest two quarter earnings, of the expected total administrative earnings accumulated since the quarter in which the first UI claim was filed. Covariate index is a linear combination of baseline characteristics (details in text). Both dependent variables are deviated from a linear regression fit of the running variable. Only data within $14,000 of the threshold are shown. There are 70 bins, each $400 wide.

Figure 8: RK Estimates versus Bandwidths: Cumulative UI Payments Q1-Q4



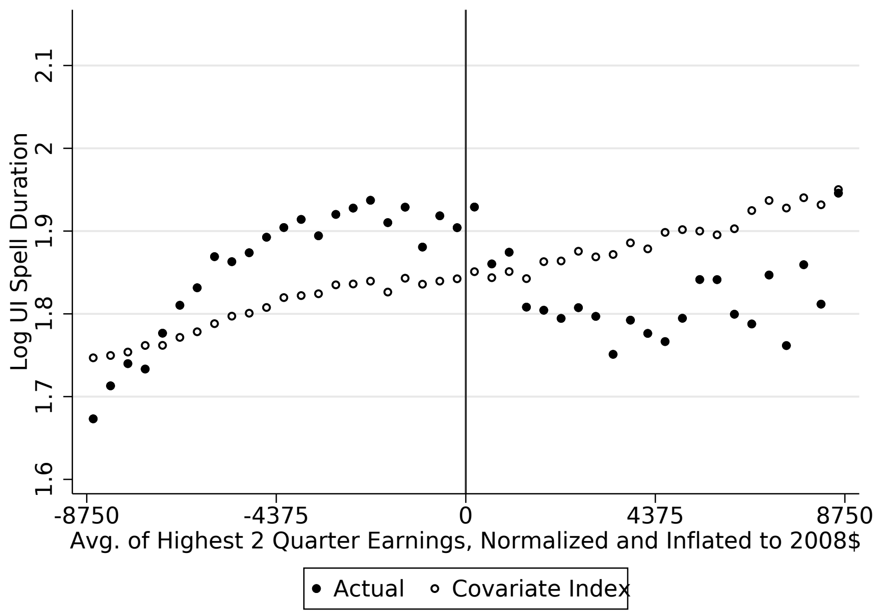
Note: This graph shows the estimated effects of *wba* on cumulative UI payments in Q1-Q4 (corresponding to row 4 of Table 4), when using different bandwidths and an alternative polynomial order. The black and gray series represent quadratic and linear estimates, respectively. The dashed short dashed lines represent the upper and lower end of the pointwise 95 percent confidence intervals.

Figure 9: RK Estimates versus Bandwidths: Cumulative Earnings Q1-Q4



Note: This graph shows the estimated effects of *wba* on cumulative earnings in Q1-Q4 (corresponding to row 8 of Table 4), when using different bandwidths and an alternative polynomial order. The black and gray series represent quadratic and linear estimates, respectively. The short dashed lines represent the upper and lower end of the pointwise 95 percent confidence intervals.

Figure 10: Log Paid Duration versus Base Year Earnings



Note: The scale has been adjusted to be comparable to Card et al. (2015*a*). Paid duration is defined as one plus the number of weeks for which a claimant receives a UI payment before a gap of three or more weeks with no payments. Covariate index is a linear combination of baseline characteristics (details in text). There are 44 bins, each $400 wide.

Table 1: Baseline Characteristics for Experiment and RKD

	Experiment			Regression Kink Design		
	(1) Treatment	(2) Control	(3) Diff	(4) Intercept	(5) dv	(6) Treatment
Age	37.11	37.07	0.034 (0.050)	39.83 (0.105)	0.22 (0.222)	0.036 (0.050)
Male	0.63	0.64	-0.004 (0.002)	0.80 (0.004)	-0.03 (0.009)	-0.003 (0.002)
White	0.80	0.80	0.002 (0.002)	0.89 (0.004)	0.01 (0.008)	0.002 (0.002)
Black	0.04	0.04	0.001 (0.001)	0.03 (0.002)	0.00 (0.004)	0.001 (0.001)
Hispanic	0.10	0.10	-0.002 (0.001)	0.03 (0.003)	-0.00 (0.006)	-0.002 (0.001)
Years of education	10.74	10.69	0.057 (0.019)	11.68 (0.041)	-0.02 (0.086)	0.047 (0.019)
Dropout high school	0.26	0.27	-0.003 (0.002)	0.16 (0.004)	-0.00 (0.009)	-0.002 (0.002)
Veteran	0.13	0.13	-0.001 (0.002)	0.20 (0.003)	-0.01 (0.007)	-0.001 (0.002)
On standby for callback	0.22	0.22	-0.0012 (0.0018)	0.27 (0.004)	-0.02 (0.01)	-0.0012 (0.0019)
Union hiring hall member	0.10	0.10	0.0003 (0.0013)	0.20 (0.003)	0.01 (0.01)	0.0002 (0.0013)
Agriculture	0.08	0.08	-0.001 (0.001)	0.02 (0.003)	0.003 (0.005)	-0.001 (0.001)
Construction	0.16	0.16	-0.001 (0.002)	0.29 (0.003)	0.002 (0.007)	-0.001 (0.002)
Manufacturing	0.18	0.18	0.001 (0.002)	0.23 (0.004)	0.032 (0.008)	0.001 (0.002)
Service Industry	0.22	0.22	-0.002 (0.002)	0.16 (0.004)	-0.006 (0.008)	-0.002 (0.002)
Highest 2 Qtr Base Period Wages	11,489	11,462	27.2 (31.5)	17,215 (1)	0.54 (2.84)	0.658 (0.641)
Total Base Period Wages	18,260	18,202	58.2 (55.4)	28,164 (34)	-12.08 (71.44)	22.364 (16.123)
Highest 2 Qtr Base Period Hours	884	884	-0.2 (1.5)	980 (3)	-3.57 (5.76)	-0.695 (1.299)
Total Base Period Hours	1,377	1,376	0.7 (2.9)	1,607 (5)	-26.34 (11.48)	0.561 (2.590)
Weekly Benefit Amount	213.46	213.15	0.32 (0.41)	345.03 (0.19)	-34.43 (0.39)	0.07 (0.09)
Receiving min wba	0.03	0.03	-0.001 (0.001)	0.06 (0.001)	-0.21 (0.00)	-0.000 (0.001)
Receiving max wba	0.19	0.19	0.000 (0.002)	0.46 (0.001)	0.03 (0.00)	-0.001 (0.001)
Maximum Benefits Payable on Claim	5,689	5,676	14 (13)	9,274 (10)	-556.14 (20.52)	5.291 (4.632)
Date of 1st claim is post July 95	0.20	0.20	0.002 (0.002)	0.186 (0.004)	0.002 (0.008)	0.002 (0.002)
Observations	67659	204602		255927		

Note: Robust standard errors in parentheses. Columns (1)–(3) compare the mean values between the treatment and control group of the experiment. Columns (4)–(6) are estimates from regressing the outcome variable on an indicator for the treatment group, a polynomial of order 6 in the running variable V, and the variable $A \cdot V$ where $A = 1[V \geqslant 0]$. Individuals in the bottom and top 3 percent of the running variable are dropped for this regression. Column (4) is the estimated intercept. The coefficient on $A \cdot V$ is scaled by 1500. Actual age only has 272,247 observations because some claimants were missing their birth date. We impute the 14 missing values with the mean age.

Table 2: Estimated Impacts from Earnings Deduction Experiment

	(1) Treatment	(2) Control	(3) Difference
—Cumulative UI Payments			
Q1	757.4	741.9	15.5 (4.04)
Q1 to Q2	1,899.9	1,855.2	44.7 (8.79)
Q1 to Q3	2,508.8	2,446.5	62.3 (11.92)
Q1 to Q4	2,756.3	2,689.3	66.9 (12.60)
—Cumulative Earnings			
Q1	3,649.0	3,677.9	-28.9 (24.6)
Q1 to Q2	6,377.2	6,447.2	-70.0 (39.1)
Q1 to Q3	9,762.8	9,837.5	-74.6 (49.5)
Q1 to Q4	13,329.6	13,387.7	-58.1 (62.8)
—Cumulative Hours			
Q1	237.2	237.9	-0.74 (0.86)
Q1 to Q2	435.9	436.8	-0.85 (1.49)
Q1 to Q3	685.6	686.5	-0.94 (2.25)
Q1 to Q4	944.9	945.6	-0.71 (3.01)
—Have Positive Cumulative Earnings			
Q1	0.86	0.86	0.00097 (0.0015)
Q1 to Q2	0.91	0.91	-0.00043 (0.0013)
Q1 to Q3	0.94	0.94	-0.00049 (0.0011)
Q1 to Q4	0.95	0.95	-0.00077 (0.0010)
—Have Positive Cumulative Hours			
Q1	0.80	0.80	-0.00055 (0.0018)
Q1 to Q2	0.87	0.87	-0.00053 (0.0015)
Q1 to Q3	0.90	0.90	-0.00031 (0.0013)
Q1 to Q4	0.92	0.92	-0.00077 (0.0012)
—Log Cumulative Earnings			
Q1 (N=233959)	7.91	7.91	-.0031 (0.0050)
Q1 to Q2 (N=248455)	8.41	8.41	.00042 (0.0050)
Q1 to Q3 (N=255067)	8.81	8.81	-.000013 (0.0050)
Q1 to Q4 (N=258283)	9.10	9.10	-.00043 (0.0051)
—Log Cumulative Hours			
Q1 (N=218600)	5.43	5.43	-.0036 (0.0043)
Q1 to Q2 (N=236845)	5.93	5.93	-.00075 (0.0044)
Q1 to Q3 (N=245783)	6.32	6.32	-.0026 (0.0045)
Q1 to Q4 (N=250705)	6.61	6.61	-.0015 (0.0046)
—Number of Weeks with UI Payments			
Q1	3.77	3.66	0.11 (0.017)
Q1 to Q2	9.44	9.12	0.32 (0.035)
Q1 to Q3	12.44	11.98	0.46 (0.046)
Q1 to Q4	13.76	13.23	0.53 (0.050)
Claim Duration (Weeks)	11.50	10.91	0.60 (0.051)
Log Claim Duration	1.85	1.79	0.06 (0.006)

Note: The variable *claim duration* is the number of weeks of received UI payments prior to the first 3 weeks of no payments. *log claim duration* is the natural logarithm of one plus *claim duration*. All remaining variables are averaged over cumulative *calendar* quarters, where Q1 refers to the quarter of the initial UI claim. Unless stated in brackets next to the quarters of accumulation, the number of observations is 272,261.

Table 3: Earnings Deduction Experiment: Separating the Mechanical and Behavioral Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	UI Payments				Fiscal Externality		
	Total	Mechanical	Behavioral	Tax Receipts	$\beta+\gamma$	β	γ
Q1	15.52	7.73	7.79	-0.05	1.02	1.01	0.01
	(4.04)	(0.05)	(4.04)	(0.31)	(0.53)	(0.52)	(0.04)
Q1 to Q2	44.66	19.83	24.84	-0.26	1.27	1.25	0.01
	(8.79)	(0.12)	(8.79)	(0.47)	(0.45)	(0.44)	(0.02)
Q1 to Q3	62.29	26.37	35.92	-0.25	1.37	1.36	0.01
	(11.92)	(0.17)	(11.92)	(0.62)	(0.46)	(0.45)	(0.02)
Q1 to Q4	66.92	28.15	38.77	-0.11	1.38	1.38	0.00
	(12.60)	(0.20)	(12.61)	(0.75)	(0.45)	(0.45)	(0.03)

Note: Column (1) are the estimates from the first four rows of Table 2. Column (2) is the cumulative UI payments that the control group would gain if it was mechanically subject to the treatment group's earnings deduction schedule. Column (3) is (1) minus (2). Column (4) is the experimental impact on tax receipts. Column (5) is equal to the estimate from a regression of tax receipts on the treatment indicator, then divided by column (2). Column (6) is (3) divided by (2). Column (7) is (5) minus (6).

Table 4: RKD Estimates: Effects of an Increase in the Weekly Benefit Amount

	(1)	(2)	(3)	(4)	(5)
	p	*h*	n	First Stage	Fuzzy RKD Estimate
—Cumulative UI Payments					
Q1	1	2,171	31,747	-0.019 (0.0001)	4.7 (1.12)
Q1 to Q2	1	2,130	31,019	-0.019 (0.0001)	13.3 (2.45)
Q1 to Q3	1	7,817	127,864	-0.020 (0.0000)	15.1 (0.47)
Q1 to Q4	1	3,932	58,333	-0.020 (0.0001)	15.9 (1.36)
—Cumulative Earnings					
Q1	2	12,208	225,983	-0.020 (0.0001)	4.1 (1.42)
Q1 to Q2	1	3,392	50,010	-0.020 (0.0001)	1.1 (3.11)
Q1 to Q3	2	9,041	153,672	-0.019 (0.0001)	-0.8 (4.29)
Q1 to Q4	2	11,016	198,899	-0.020 (0.0001)	-2.4 (4.68)
—Cumulative Hours					
Q1	3	10,598	189,040	-0.019 (0.0002)	0.16 (0.18)
Q1 to Q2	1	5,172	78,889	-0.020 (0.0000)	0.04 (0.09)
Q1 to Q3	1	3,412	50,299	-0.020 (0.0001)	0.04 (0.25)
Q1 to Q4	3	8,878	150,225	-0.019 (0.0002)	0.57 (0.82)
—Have Positive Cumulative Earnings					
Q1	2	7,633	124,202	-0.019 (0.0001)	0.00002 (0.0002)
Q1 to Q2	1	3,133	46,056	-0.020 (0.0001)	0.00005 (0.0001)
Q1 to Q3	2	7,944	130,441	-0.019 (0.0001)	0.00012 (0.0001)
Q1 to Q4	2	5,389	82,519	-0.019 (0.0001)	-0.00001 (0.0002)
—Have Positive Cumulative Hours					
Q1	2	6,193	96,660	-0.019 (0.0001)	-0.00024 (0.0003)
Q1 to Q2	2	8,851	149,679	-0.019 (0.0001)	-0.00023 (0.0001)
Q1 to Q3	2	6,597	104,213	-0.019 (0.0001)	-0.00007 (0.0002)
Q1 to Q4	2	4,877	73,740	-0.019 (0.0002)	-0.00005 (0.0003)
—Log Cumulative Earnings					
Q1 (N=233959)	3	5,040	68,272	-0.019 (0.0004)	0.0002 (0.0023)
Q1 to Q2 (N=248455)	2	2,991	41,164	-0.019 (0.0003)	-0.0021 (0.0021)
Q1 to Q3 (N=255067)	1	1,453	20,164	-0.019 (0.0003)	-0.0011 (0.0016)
Q1 to Q4 (N=258283)	2	4,361	62,802	-0.019 (0.0002)	0.0005 (0.0013)
—Log Cumulative Hours					
Q1 (N=218600)	3	14,647	211,255	-0.014 (0.0002)	0.0014 (0.0007)
Q1 to Q2 (N=236845)	1	2,628	34,028	-0.019 (0.0001)	0.0005 (0.0006)
Q1 to Q3 (N=245783)	1	5,595	78,581	-0.020 (0.0000)	-0.0002 (0.0002)
Q1 to Q4 (N=250705)	1	1,767	23,793	-0.019 (0.0002)	0.0008 (0.0012)
—Number of Weeks with UI Payments					
Q1	2	6,660	105,447	-0.019 (0.0001)	0.0039 (0.0026)
Q1 to Q2	2	6,942	110,758	-0.019 (0.0001)	0.0137 (0.0051)
Q1 to Q3	1	3,871	57,402	-0.020 (0.0001)	0.0112 (0.0042)
Q1 to Q4	2	8,900	150,699	-0.019 (0.0001)	0.0140 (0.0053)
Claim Duration (Weeks)	2	9,607	166,186	-0.019 (0.0001)	0.0026 (0.0049)
Claim Duration (Elasticity)	3	10,072	176,894	-0.00006 (0.000001)	1.06 (0.41)

Note: Robust standard errors in parentheses. The variable *claim duration* is the number of weeks of received UI payments prior to the first 3 weeks of no payments. *Claim duration (elasticity)* is a fuzzy RK estimate with *log(wba)* as the regressor and *log(1+claim duration)* as the outcome. All remaining variables are averaged over cumulative *calendar* quarters, where Q1 refers to the quarter of the initial UI claim. Columns (1) and (2) show the polynomial order (chosen between 1 and 3) and CCT bandwidth that minimize estimated MSE. Column (3) reports the effective number of observations within the CCT bandwidth that is computed without regularization. Column (4) reports the first stage estimates and Column (5) the fuzzy RK estimates. Unless stated in brackets next to the quarters of accumulation, the total number of observations in the analysis sample is 272,261.

Table 5: RK Estimates: Separating the Mechanical and Behavioral Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	UI Payments				Fiscal Externality		
	Total	Mechanical	Behavioral	Tax Receipts	$\beta+\gamma$	β	γ
Q1	4.67	3.75	0.92	0.06	0.23	0.24	-0.01
	(1.12)	(0.04)	(1.09)	(0.04)	(0.30)	(0.29)	(0.02)
Q1 to Q2	13.31	9.06	4.25	0.08	0.46	0.47	-0.01
	(2.45)	(0.09)	(2.40)	(0.03)	(0.27)	(0.26)	(0.01)
Q1 to Q3	15.11	10.13	4.98	0.20	0.47	0.49	-0.02
	(0.47)	(0.12)	(0.44)	(0.05)	(0.05)	(0.04)	(0.00)
Q1 to Q4	15.88	10.23	5.66	0.23	0.53	0.55	-0.02
	(1.36)	(0.13)	(1.30)	(0.16)	(0.13)	(0.12)	(0.01)

Note: Robust standard errors in parentheses. Column (1) shows the estimates from the first four rows of Table 4. Column (2) is the estimated mean mechanical transfer at the threshold from increasing *wba*. It is the intercept from a regression of the mechanical effect on a quintic polynomial in V and $A \cdot V$, where $A = 1[V \geqslant 0]$. Column (3) is (1) minus (2). Column (4) is the fuzzy RKD estimate of *wba* on tax receipts, calculated as in Column (1). Column (5) is the fuzzy RKD estimate of the effect of *wba* on UI payments net of tax receipts, divided by column (2), and minus 1. Column (6) is (3) divided by (2). Column (7) is (5) minus (6). The bandwidths and orders of the RKDs for all columns except (4) are chosen to minimize MSE (polynomial order is selected between 1 and 3) where the outcome is UI payments. For column (4), the bandwidth and polynomial order are chosen using tax receipts as the outcome. Standard errors for Columns (5) to (7) are computed by jointly estimating their numerator and denominator, and allowing unrestricted covariances across equations, and then using the delta method.

Appendix

RK and Policy Efficiency Costs

In this section, we consider in greater detail a set of sufficient conditions under which the fiscal externality from an increase in the full weekly benefit amount can be inferred from the estimates obtained from the regression kink design. In the presence of unobserved heterogeneity, the RKD produces esimates that are "local" in nature. It is thus constructive to establish what restrictions on heterogeneity would permit one to interpret the RKD estimates as the fiscal externality for the policy change more generally.

Recall that if the outcome variable of interest is a smooth function $f(B,V,U)$ of the continuous treatment variable B, running variable V, and type variable U, the sharp RK estimand identifies the treatment effect

$$E[f_1(b_0,0,U)|V=0] \tag{A1}$$

where f_1 denotes the partial derivative of the causal response function f with respect to its first argument, and b_0 is the value of the treatment when $V=0$ (Card et al., 2015*b*).[47]

As in Card et al. (2015*b*), we allow unrestricted heterogeneity to be represented by the variable U, and write the key components of UI benefit expenditures as functions of U and the running variable V:

$$\text{Statutory UI amount for fully unemployed: } \tilde{B}_t = \tilde{b}^t\left(V,\theta,\rho\right) = \theta + \min\left[\rho V, 0\right]$$

$$\text{Earnings: } Y_t = y^t(\tilde{B}_t, V, U, \theta, \rho, \tau)$$

$$\text{UI benefit accounting for disregarding a fraction of earnings: } B_t = b^t\left(\tilde{B}_t, Y_t, \tau\right) = \max\left[\tilde{B}_t - \tau Y_t, 0\right]$$

$$\text{Probability of take-up: } P_t = p^t\left(\tilde{B}_t, V, U, \theta, \rho, \tau\right).$$

For concreteness, t is the week indicator, so the total UI benefit received, for example, is $\sum_{t=1} B_t \cdot P_t$. The parameter θ is the maximum weekly benefit amount, ρ the scaled UI replacement rate, and τ the implicit tax rate in partial UI.[48] The individual jointly chooses Y_t and P_t, conditional on the parameters and other pre-determined factors U,V. Note that we omit the tax component of the fiscal externality from this discussion.

[47] In this section, we use g_k to denote the partial derivative of a given function g with respect to its k-th argument.

[48] For simplicity, we abstract away from the partial UI earnings disregard since it was very low ($5) in Washington state during the sample period as mentioned in Section 3.

We focus solely on the benefits component of the fiscal externality to illustrate the types of behavioral assumptions that would be sufficient to extrapolate from the RKD estimates to the full population.

With this notation, we now derive the fiscal externality of marginally changing the *wba* for all UI claimants, which amounts to a small change in the parameter θ. We will impose two exclusion restrictions regarding how Y_t and P_t respond to a change in θ (recall that θ is the fourth argument in both functions y^t and p^t):

$$y_4^t = p_4^t = 0. \tag{A2}$$

The first restriction on Y_t means that θ can only impact earnings through its impact on benefit levels, while the second restriction on P_t similarly requires that θ can only impact take-up decisions through its impact on benefit levels and earnings.

Since the fiscal externality is the ratio of the behavioral and mechanical increase in total UI benefit expenditures, let us first consider the effect of changing θ on the total expected benefit payment in a particular week t — we will drop the t subscript for ease of exposition:

$$\begin{aligned}\frac{dE[P\cdot B]}{d\theta} &= E\left[\frac{d(P\cdot B)}{d\theta}\right]\\ &= E\left[\frac{dp}{d\theta}B + P\frac{db}{d\theta}\right]\\ &= E\left[1\,[B>0]\cdot\left(p_1 B + P\left(b_1 + b_2\frac{dy}{d\theta}\right)\right)\right]\end{aligned}$$

where we have employed $p_4 = 0$ from assumption (A2), and have included the indicator function $1\,[B>0]$ to account for the fact that the partial derivative of b with respect to its arguments will be zero when $B \leq 0$.

Using the other restriction $y_4 = 0$, we have that when $B > 0$

$$\frac{dy}{d\theta} = \tilde{b}_2 y_1 + y_4 = y_1$$

and with the assumptions stated in equation (A2), the above expression reduces to

$$
\begin{aligned}
& E\left[1\left[B>0\right]\cdot\left(p_1 B+P\left(b_1+b_2 y_1\right)\right)\right] \\
= & E\left[1\left[B>0\right]\cdot\left(p_1 B+P\left(1+b_2 y_1\right)\right)\right] \\
= & E\left[1\left[B>0\right]\cdot\left(p_1\frac{B}{P}-\tau y_1+1\right)P\right]
\end{aligned}
$$

Meanwhile, the mechanical effect is

$$E[P\cdot b_1]=E\left[1\left[B>0\right]\cdot P\right]$$

If we assume homogeneity across individuals in the behavioral component $p_1\frac{B}{P}-\tau y_1=\kappa$ (across individuals U,V and benefit levels B and probabilities P) then the fiscal externality reduces to

$$\frac{E\left[1\left[B>0\right]\cdot\left(p_1\frac{B}{P}-\tau y_1\right)P\right]}{E\left[1\left[B>0\right]\cdot P\right]}=\kappa \tag{A3}$$

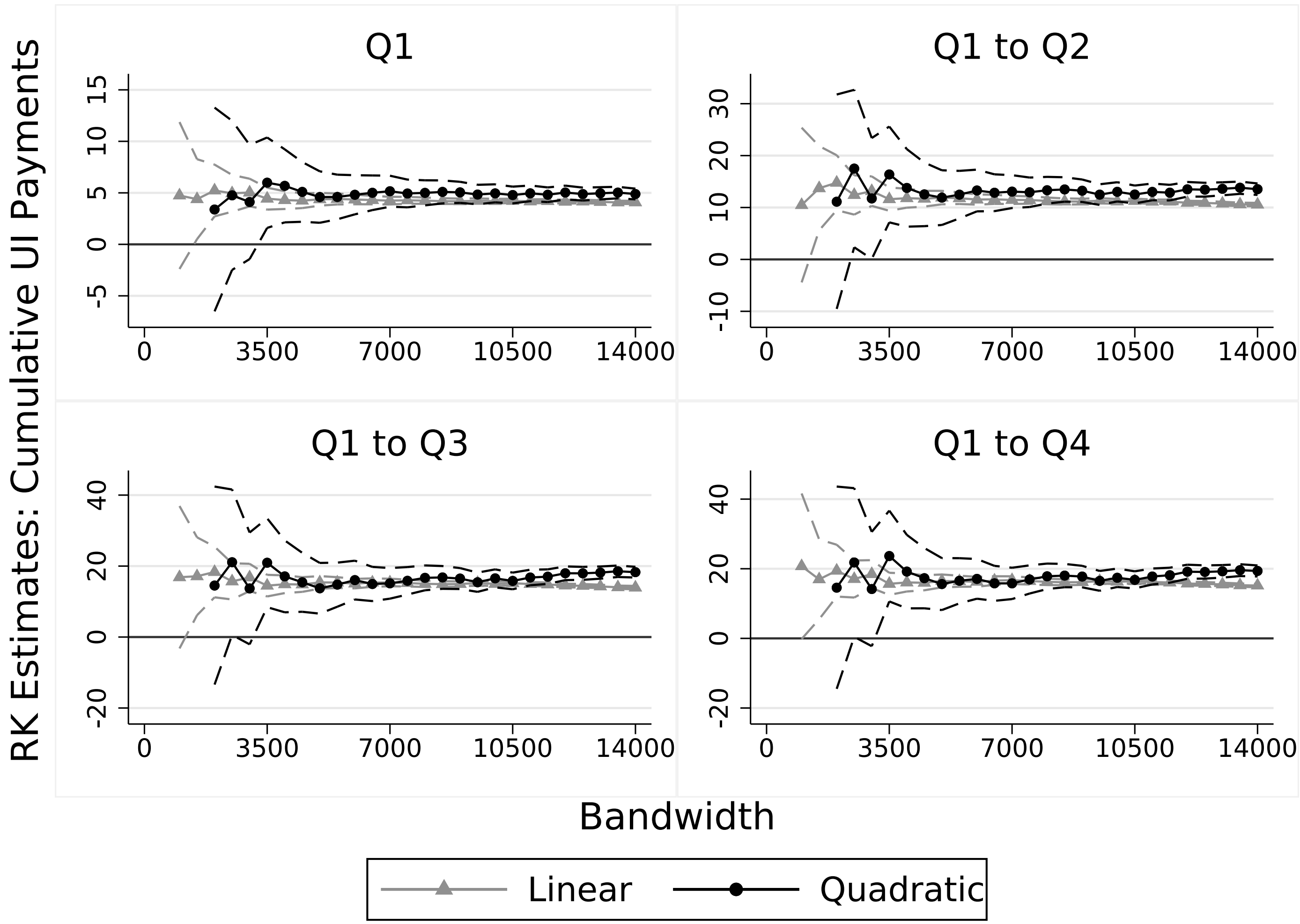
Furthermore, if the homogeneity in this behavioral component extends across all t, then it can be shown that the fiscal externality for the accumulated benefit payment up to time T is also κ.

A similar expression for the RKD fiscal externality estimand can be shown to be, for each week:

$$\frac{E\left[1\left[B>0\right]\cdot\left(p_1\frac{B}{P}-\tau y_1\right)P|V=0\right]}{E\left[1\left[B>0\right]\cdot P|V=0\right]}=\kappa \tag{A4}$$

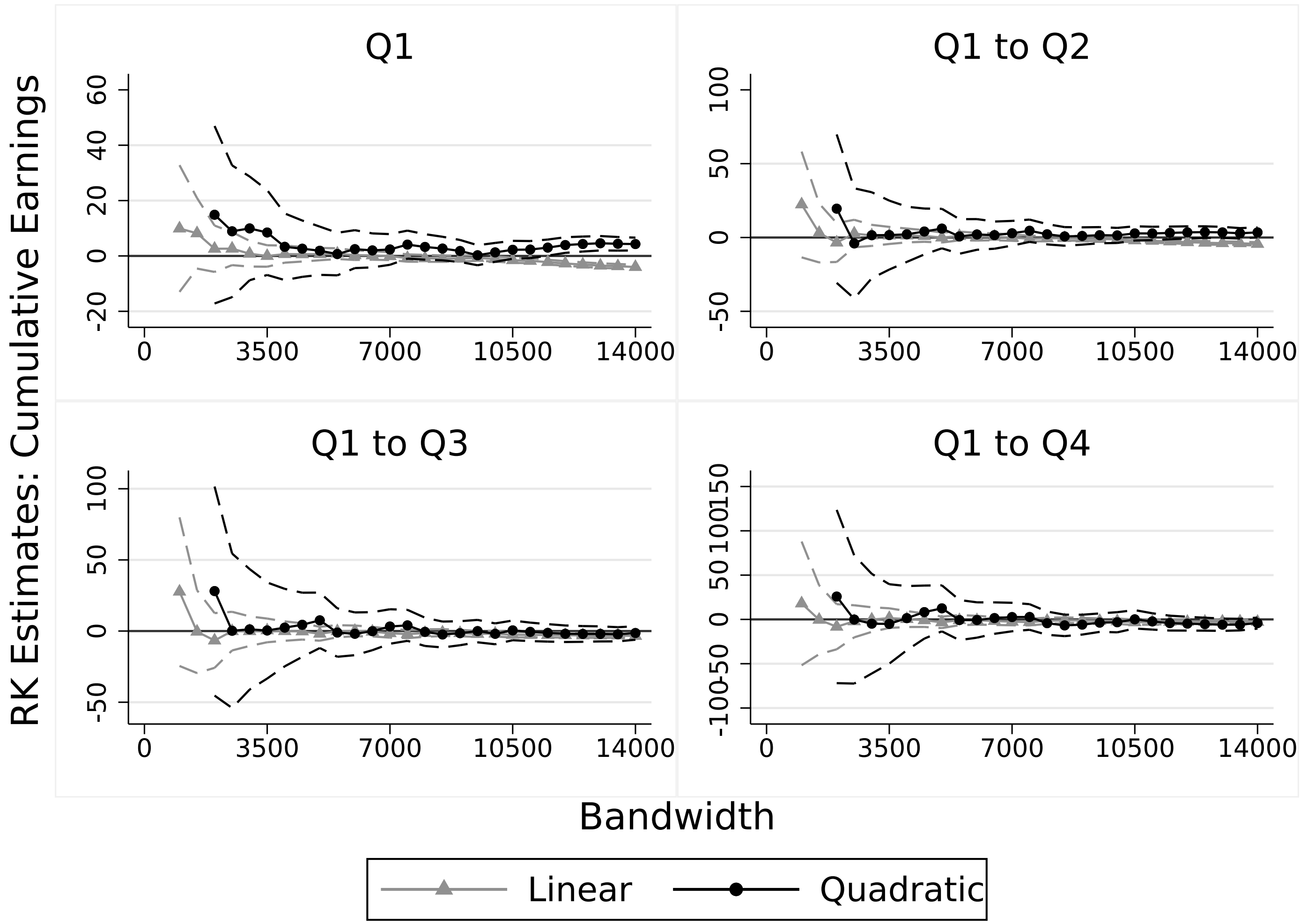
In summary, although we may allow for various forms of heterogeneity (note that the p and y functions contain U as an argument), we must restrict the behavioral responses in some way to extrapolate from the RKD estimand to the fiscal externality quantity for the full population. We have shown that the constancy of $p_1\frac{B}{P}-\tau y_1$, along with the exclusion restrictions (A2), is sufficient for this extrapolation.

Figure A.1: RK Estimates versus Bandwidths: Cumulative UI Payments by Quarter



Note: Each graph shows the estimated effects of *wba* on cumulative UI payments (corresponding to rows 1–4 of Table 4), when using different bandwidths and an alternative polynomial order. The black and gray series represent quadratic and linear estimates, respectively. The short dashed lines represent the upper and lower end of the pointwise 95 percent confidence intervals.

Figure A.2: RK Estimates versus Bandwidths: Cumulative Earnings Q1-Q4



Note: Each graph shows the estimated effects of *wba* on cumulative earnings (corresponding to rows 5–8 of Table 4), when using different bandwidths and an alternative polynomial order. The black and gray series represent quadratic and linear estimates, respectively. The short dashed lines represent the upper and lower end of the pointwise 95 percent confidence intervals.