The Duration and Wage Effects of Long-Term Unemployment Benefits: Evidence from Germany's Hartz IV Reform*

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Abstract

In many countries, displaced workers who exhaust their initial stream of unemployment benefits may apply for long-term unemployment assistance. I analyze Germany's 2005 Hartz IV reform, which reduced the generosity of these long-term benefits. Using date-stamped administrative data, I exploit cross-cohort and within-cohort variation in the timing of Hartz IV's effective onset to estimate how long-term benefit cuts affect claimants' jobless durations and subsequent wages. Workers exposed to the new benefit schedule find jobs faster in anticipation of benefit cuts; exhibit a larger "spike at exhaustion" than was evident pre-reform; and are 11.3 percent less likely to experience a one-year jobless spell. The cuts had little net impact on wages: exposed workers accept lower-paying jobs, but they do so at shorter durations associated with greater wage recovery. Hartz IV's impetus to individual job-finding—if not offset in general equilibrium—may have lowered Germany's steady-state unemployment rate by 0.9 percentage points.

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1 Introduction

Many displaced workers exhaust their initial stream of unemployment benefits before returning to work. Rather than ceasing benefit payments altogether, many countries—including Germany, France, the United Kingdom, Austria, Sweden, and Spain—rely on two-tiered systems of unemployment insurance (UI) that combine generous time-limited benefits with more modest unemployment assistance thereafter (Esser et al., 2013). These long-term benefits take on added importance during recessions, when more UI claimants exhaust their initial entitlements (Schmieder et al., 2012), and they loom especially large for workers at elevated risk of lengthy jobless spells, which erode job prospects, deplete savings, and impose fiscal externalities throughout the tax and transfer system (Kroft et al., 2013; Ganong and Noel, 2019; Nekoei and Weber, 2017). While a voluminous literature has analyzed changes in workers' *initial* benefit streams, much less is known about how *long-term* unemployment benefits affect jobless durations and other labor market outcomes.

This paper analyzes the employment and wage effects of Germany's 2005 Hartz IV reform, a controversial law that reduced long-term unemployment benefit levels for both new and incumbent UI claimants. On January 1, 2005, existing long-term benefit recipients—who numbered 2.2 million and comprised 5.3 percent of the civilian labor force on the eve of reform—were switched overnight to the new, in most cases lower, post-reform benefit level.[1] Subsequent inflows into long-term unemployment assistance were subject to the new rules upon exhausting their initial stream of benefits. The lack of grandfathering for incumbent claimants, together with concurrent changes in labor market conditions and institutions, has impeded prior efforts to isolate convincingly exogenous variation in exposure to this signature UI reform. To overcome these challenges, I exploit cross-cohort and within-cohort variation in the timing of Hartz IV's effective onset—based on individual heterogeneity in the potential duration of the initial ("short-term") benefit stream—to identify the causal effects of policy-induced reductions in long-term benefit generosity.

Using administrative data on about 337,000 UI claims made by prime-age German workers during 2001–2005, I find that exposure to the reform increases the hazard rate of reemployment beginning several months before the cuts take effect. The rising hazard rate—indicative of forward-looking behavior on the part of jobseekers—culminates in a much larger spike in job-finding at benefit exhaustion than was evident before the reform. My preferred estimates imply that being subject to the new benefit regime reduces the likelihood of having a one-year jobless spell by 4.2 percentage points, a proportional decline of 11.3 percent. My research design disentangles Hartz IV from the other "Hartz reforms" Germany enacted in the mid-2000s, and it further nets out any mechanisms unrelated to the (idiosyncratic) timing of long-term benefit cuts. Although political and academic debate has centered on the cuts themselves, Hartz IV may have operated

[1] Source: caseload statistics published by the German Federal Employment Agency.

partly through other channels, such as stricter job-search requirements or increased stigmatization of long-term benefit receipt. To the extent that such channels promote job-finding as workers approach short-term benefit exhaustion, specifically—net of any impacts on job-finding more generally—they will contribute to the treatment effects I estimate. While Hartz IV's combined effect is of interest in its own right, I provide direct evidence that workers responded to the severity as well as the timing of benefit cuts, suggesting that the cuts were indeed central to the law's overall impact.

I next analyze how long-term benefit cuts affect the daily wages workers receive upon being reemployed. As prior research has noted, this effect is theoretically ambiguous (Schmieder et al., 2016; Nekoei and Weber, 2017). On the one hand, benefit cuts may prompt workers to accept lower-paying jobs at a given point in time by worsening their outside options in the event of staying unemployed (the *selectivity effect*). On the other hand, if joblessness reduces earnings capacity through stigma or skill depreciation, then benefit cuts may actually mitigate wage declines by shortening jobless spells (the *time-out-of-work effect*). Consistent with diminished selectivity, I find that—*conditional on their completed jobless durations*—workers who accept jobs after exhausting short-term benefits earn 4 to 8 percent lower wages than they would have absent the reform. But averaging across completed durations, accounting for reductions in time out of work, and correcting for changing patterns of selection into reemployment, I find that Hartz IV had at most a slight *unconditional* impact on mean reemployment wages. While the sign of the overall effect varies across specifications, I can rule out (at the 95 percent level) a net change in wages exceeding 1.6 percent in either direction. In short, net job gains among UI claimants do not appear to be accompanied by any substantial decline in their average wages, relative to what they counterfactually would have earned.

To shed additional light on the labor supply shifts induced by Hartz IV, I next estimate competing-risks specifications to examine how the reform affected transitions into different kinds of jobs. First, I show that net employment gains are mostly driven by full-time jobs. Although I do not observe hours worked, I find no change in the part-time share of employment, suggesting that the relative stability of post-reemployment daily wages is unlikely to mask changes in hourly wages offset by shifts along the intensive margin of labor supply. Second, I track transitions into a legally defined class of low-paid, marginal jobs that figure prominently in the political debates surrounding Hartz IV. My preferred employment concept excludes these so-called "mini-jobs", which often supplement UI receipt and hence are unlikely to represent true returns to gainful employment. Broadening the definition of work, I find that Hartz IV diverted claimants from mini-jobs into those covered by social insurance. This result adds an important nuance to the received wisdom that Germany's Hartz reforms have fueled the rise of alternative work arrangements, which have attracted growing attention in the United States as well as Europe (Katz and Krueger, 2019).

This paper makes two contributions. First, I conduct the first unified analysis of how reductions in long-term unemployment benefit generosity affect jobless durations, wages, and job characteristics. Although an extensive literature has examined the labor market impacts of changes in the level and potential duration of *short-term* UI benefits (e.g., Card and Levine, 2000; Lalive, 2008; Johnston and Mas, 2018), explicit systems of long-term unemployment assistance have received much less attention.[2] Generous, indefinite-duration benefits of the kind that existed in pre-reform Germany may strongly impact search behavior among the unemployed, as these benefits insure against the tail risk of a long or permanent jobless spell. In an influential series of papers, Ljungqvist and Sargent (1998, 2008) attribute European economies' persistently high levels of long-term unemployment to the prevalence of such generous long-term benefits. My job-finding analysis confirms that cutting long-term benefits can lead to substantial reductions in jobless durations. My wage analysis, in turn, complements an active literature that finds conflicting effects of changes in short-term benefit generosity on reemployment wages and match quality (e.g., Card et al., 2007a; Schmieder et al., 2016; Nekoei and Weber, 2017).

Second, I present the first comprehensive, quasi-experimental evaluation of the microeconomic effects of Hartz IV, one of the most prominent social insurance reforms in recent memory.[3] Internationally, Hartz IV—the centerpiece of the broader package of Hartz reforms—has become a byword for efforts to strengthen job-search incentives among the unemployed. But despite sustained academic and policy interest, prior work has struggled to isolate variation in exposure to Hartz IV, which was rolled out simultaneously and uniformly throughout Germany. Nagl and Weber (2016) find that UI claimants return to work faster after the reform, but their main estimates are identified by time-series variation and potentially confounded by other labor market reforms enacted during this period.[4] Engbom et al. (2015) find that wages among previously displaced workers fell relative to those of non-displaced workers during the Hartz era, but their strategy hinges on a strong parallel trends assumption and, as they acknowledge, "cannot reliably identify which element of the reform package was responsible for its effects." I overcome these identification challenges by isolating cross-worker variation in exposure to benefit cuts within UI entry cohorts.

A complementary literature in macroeconomics has debated Hartz IV's contribution to the so-called

[2] A notable exception is Kolsrud et al. (2018), who study changes in short- and long-term Swedish UI benefit levels. Building on a theoretical literature in public finance, they show that the optimal slope of the UI schedule depends on how benefits paid at different durations affect job search and consumption smoothing. Using a regression-kink design, they find that jobless durations do respond to changes in long-term benefit levels, but less so than they do to comparable changes in short-term benefits. The benefit changes I study bind, on average, at much later durations: 12 months for the modal German worker, as opposed to 5 months in the Swedish context. Aside from differences in institutional setting, research design, and the nature of the treatment, I complement Kolsrud et al. by analyzing impacts on wages and job characteristics as well as on jobless durations.

[3] The Economist (December 29, 2004) is representative in calling Hartz IV "Germany's most important labour-market reform since the war". At the international level, OECD (2007) asserts that "Germany has had the most active and controversial series of reforms within the OECD area", singling out Hartz IV as an especially important component of the Hartz package.

[4] Nagl and Weber's main specification compares job-finding rates between pre-reform (2001–2003) and post-reform (2007–2010) UI cohorts, after balancing on claimant characteristics and controlling for macroeconomic indicators. This approach is vulnerable to aggregate time effects not fully proxied by macroeconomic conditions and is unlikely to fully purge any effects of Hartz I–III. They also find, in an extension, that the post-reform rise in job-finding is strongest for workers nearing benefit exhaustion, but their model controls for long-term benefit receipt (an endogenous outcome directly manipulated by Hartz IV) and credits to Hartz IV any overall shifts in job-finding (as well as relative shifts among near-exhaustees).

"German employment miracle", which saw the national unemployment rate fall from 10.3 percent in June 2004 to 7.7 percent in June 2009 and still further to 5.0 percent in June 2014.[5] This literature calibrates search-and-matching models, often to pre-reform data, to simulate the aggregate impact of Hartz IV (Krause and Uhlig, 2012; Krebs and Scheffel, 2013; Launov and Wälde, 2013; Bradley and Kügler, 2019; Hochmuth et al., 2019). These papers reach disparate conclusions about the impact of benefit cuts on steady-state unemployment, with headline numbers ranging from 0.1 to 2.8 percentage points. Although my paper is silent on general equilibrium mechanisms such as congestion externalities or endogenous job creation, a back-of-the-envelope calculation suggests that the partial equilibrium effects I identify—if not augmented or offset by market-level forces—may have lowered Germany's steady-state unemployment rate by 0.9 percentage points. Almost all of this decline accrues to the long-term component of the overall unemployment rate, underscoring the role of long-term benefit levels in shaping the incidence of lengthy jobless durations.

Section 2 describes the institutional setting. Section 3 presents a model of job search to motivate the empirical strategy. Section 4 details the administrative data I use. Section 5 lays out the research design. Section 6 estimates the effects of benefit cuts on job-finding and jobless durations. Section 7 discusses mechanisms and shows that claimants responded to the severity as well as the timing of the benefit cuts. Section 8 analyzes wages. Section 9 uses a competing-risks approach to distinguish among transitions into full-time, part-time, and mini-jobs. Section 10 concludes. An Online Appendix contains proofs of theoretical results, additional empirical analysis, and further details on data preparation.

2 Reform of the German UI System

I begin by describing the essential features of the German UI system before and after Hartz IV. To gauge how Hartz IV impacted the generosity of long-term unemployment assistance, I then use the OECD Tax-Benefit Model to simulate reform-induced changes in household income among those who exhaust short-term UI.

2.1 The safety net pre- and post-Hartz IV

Prior to 2005, Germany had a two-tiered system of unemployment benefits consisting of limited-duration unemployment insurance (*Arbeitslosengeld*) which, when exhausted, could be followed by a second stream of means-tested unemployment assistance (*Arbeitslosenhilfe*). I refer to these sequential benefit streams as "short-term" and "long-term" benefits, respectively.[6]

[5] Source: seasonally adjusted unemployment rates published by Germany's Federal Statistical Office, based on the International Labour Organization's definition of the unemployed as those out of work, seeking work, and available for work.

[6] The term "unemployment *assistance*" reflects the fact that long-term benefits are financed out of general tax revenues rather than dedicated insurance funds. "Short-term" benefits can be quite lengthy, lasting up to 26 months for workers in my sample. Conversely, some workers transition to "long-term" benefits quite early in their spells. This terminology should not be confused

Hartz IV left short-term benefits unaltered. To be entitled to short-term UI, an individual must have worked at least 12 months over the preceding 3 years in jobs covered by social insurance. In the event of job loss, eligible workers are entitled to benefits equal to 60 percent of their previous after-tax earnings (67 percent if they have dependent children). Benefit payments are not taxed, and claimants may work at most 15 hours per week under an earnings disregard equal to the larger of €165 per month or 20 percent of the benefit amount. The potential duration of short-term benefits (P), measured in months, is a step function that depends on age at claim initiation (a) and on months worked over the past seven years (x):

$$P = \min(\bar{P}(a), 2 \cdot \text{floor}(x/4)), \tag{2.1}$$

where $\bar{P}(a)$ is an age-specific maximum duration. Thus 12 months of work yield 6 months of benefits, and every 4 additional months of covered work extend the entitlement by 2 months. For workers under 45, benefits are capped at 12 months. For older workers, the cap rises first to 18 months (at age 45), then to 22 months (at age 47), then to 26 months (at age 52), and finally to 32 months (at age 57). Although new entitlements last at least 6 months, shorter durations are possible for seasonal workers and for those resuming earlier, unexhausted UI claims.[7] The left panel of Figure 1 summarizes the benefit accrual rules applicable to the prime-age workers I study. The right panel, discussed later, plots the empirical distribution of potential benefit duration for claimants in my estimation sample.

Upon exhausting short-term benefits, a UI recipient could apply for long-term unemployment assistance. Unlike short-term benefits, long-term benefits were means-tested on the basis of household assets and income. For a worker passing the asset test, benefits equaled 53 percent of prior net earnings (57 percent with children), with reductions for spousal earnings and other sources of income. Long-term benefits lasted indefinitely, subject to annual means testing. Poor households could also apply for means-tested social assistance to top off their UI benefits. The combination of a generous long-term benefit level and indefinite duration set Germany apart from its OECD counterparts (Wunsch, 2006). The UI caseload grew steadily in the early 2000s: by June 2004, 2.2 million workers claimed long-term benefits, on top of another 1.7 million claiming short-term benefits (Online Appendix Figure 1). The growing fiscal burden, together with a widespread view that the safety net was too generous, created political pressure for labor market reform.

In March 2002, the German government convened a commission led by former Volkswagen director Peter Hartz to recommend a reform package. The commission's report, released in August, proposed a wide range of carrots and sticks to put the unemployed back to work. The first reform measures, which took effect

with notions of "long-term unemployment" that depend only on time since job loss, without regard to UI eligibility.

[7] Seasonal workers are entitled to 3 [or 4] months of benefits if they have worked for at least 6 [8] months. Also, a claimant who exits UI without having used up her short-term benefits remains entitled to any remaining benefits in the event of a new job loss. Due to these carryover provisions, potential duration (in days) can assume any integer value from 1 to the maximum.

in January and April 2003 (Hartz I and II) and January 2004 (Hartz III), liberalized the temporary help sector, expanded favorable tax treatment for mini-jobs, provided start-up subsidies for entrepreneurs, and restructured the Federal Employment Agency. While these earlier measures may have had important effects in their own right, the Hartz IV overhaul of UI is widely regarded as the centerpiece of the reform package.[8] Hartz IV was passed by the lower house of parliament in December 2003, confirmed by the upper house in July 2004, and implemented on January 1, 2005.

The crux of Hartz IV was to consolidate long-term unemployment assistance and social assistance into a single, means-tested income-support program. In contrast to the old regime, long-term benefits would no longer be indexed to prior wages. Instead, each long-term claimant would receive a standard monthly payment (equaling €345 in the West and €331 in the East in 2005, with slight increases in subsequent years), plus additional benefit payments for dependent spouses and children as well as assistance with housing and heating expenses. To ease the transition to the new benefit regime, some long-term beneficiaries were also eligible for temporary supplemental payments, which were gradually phased out. Means testing was tightened relative to pre-reform criteria, and changes in benefit levels were accompanied by tighter job search requirements and stricter sanctions for those who violated program rules.

UI claims initiated after February 1, 2006 were subject to additional policy changes, including tighter eligibility rules as well as reductions in potential short-term duration for older workers.[9] Critically for my purposes, existing claims were not subject to these subsequent reforms. I restrict my sample to UI spells begun prior to 2006, so that my analysis is not confounded by these subsequent measures.[10]

2.2 Simulating reform-induced changes in household income

Although the post-reform UI system was broadly less generous than its predecessor, prior research suggests that some claimants saw their net government receipts—inclusive of rental assistance and other changes in taxes and transfers—go up after the reform (Bloß and Rudolph, 2005). To establish that Hartz IV had bite, and to gauge the impact of UI reform on net transfers to the unemployed, I adapt programmatic rules from the OECD Tax-Benefit Model to simulate the reform-induced change in household income experienced by each claimant in my estimation sample (defined below, in Section 4.2). Some important inputs, notably

[8] Writing about the Hartz IV benefit cuts in *The New York Times*, Landler (2004) noted, "[E]conomists say these will be the most important measures in the whole package." See Ebbinghaus and Eichhorst (2009) for a detailed discussion of other components of the reform package and Tompson (2009) for a nuanced account of the political context.

[9] Under a deferred provision of Hartz III, the lookback period for establishing a UI entitlement was reduced from three years to two years, and special provisions for seasonal workers were eliminated. Under the Labour Market Reform Act—separate from Hartz IV but adopted concurrently—the cap on short-term duration fell to 12 months for workers ages 45–54 and to 18 months for workers 55 and over. These changes were partly reversed in 2008 (Lichter, 2016).

[10] Dlugosz et al. (2014) show that, in the weeks preceding February 1, 2006, some workers strategically timed job losses so as to be covered under the old rules. While these retimers were few in number, compositional changes due to strategic retiming could bias estimation of Hartz IV's effects. I show in Section 6.3 that my results are robust to either controlling for unobserved heterogeneity by quarter of UI entry or excluding claims initiated after July 2004, suggesting this is not a serious concern.

spousal earnings and household assets, are not reported in the microdata. Online Appendix A describes my simulation procedure, including the imputations I perform to account for unrecorded inputs.[11]

After feeding each claimant's observed and imputed characteristics through Germany's 2004 and 2005 tax/benefit rules, I construct two measures of Hartz IV's impact on (prospective) household income, measured just after short-term benefit exhaustion. First, to quantify the law's direct impact on pure cash transfers—excluding housing benefits but inclusive of social assistance top-ups and temporary supplemental payments—I compute the hypothetical change in long-term cash benefits that a claimant's household faces as a result of switching from the 2004 to 2005 rules:

$$\%\Delta\,(\text{cash benefits}) \equiv \frac{\Delta(\text{long-term unemployment benefits} + \text{social assistance})}{\text{net household income under 2004 rules}} \tag{2.2}$$

Second, to account for housing assistance and any offsetting changes in other taxes and benefits—as other parts of the safety net may adjust to pick up part of the slack—I measure the overall financial impact of Hartz IV as the net change in post-exhaustion household income induced by the new rules:

$$\%\Delta\,(\text{household income}) \equiv \frac{\Delta(\text{net household income after exhausting short-term UI})}{\text{net household income under 2004 rules}} \tag{2.3}$$

Denominating both measures by post-exhaustion income under the pre-reform rules allows me to assess Hartz IV's impact on households' bottom lines. Given the imputations needed to simulate household income in these data, both measures should be interpreted with caution. Nonetheless, they provide a useful window into how Hartz IV altered the level and makeup of household income in the event of benefit exhaustion.

Figure 2 plots smoothed pdfs of these measures across the claimants in my sample. The left panel shows that Hartz IV cut post-exhaustion cash benefits substantially for most claimants, with a mean decline equal to 24.2 percent of counterfactual income. There is a point mass around zero driven by a subset of married claimants, mainly women, who appear to be ineligible for long-term benefits under both old and new rules by virtue of having high-earning spouses.[12] Even so, large majorities of both men and women (including many married claimants) faced steep cuts to their long-term cash benefits.

The right panel examines overall income. Though housing assistance and other transfers considerably mitigate the direct loss in cash benefits, I find a mean net income decline of 4.4 percent, with roughly 75 percent of claimants incurring apparent drops in post-exhaustion household income as a result of Hartz IV. There is again a point mass near zero, but substantial gains are rare: fewer than 5 percent of claimants

[11] In brief, I back out a proxy for spousal earnings based on claimants' implied income-tax liability (exploiting Germany's use of "income-splitting" to levy income taxes on married couples). I follow the OECD in assuming that UI claimants have negligible assets by the time they exhaust short-term benefits, so that asset tests bind under neither pre- nor post-reform rules. I impute rent and utility expenses as a function of household size based on 2005 regulations for the city of Berlin.

[12] A second mass point, with benefit drops equal to about 40 percent of counterfactual income, reflects the role of temporary supplemental payments that compress the distribution of benefit drops for a subset of eligible claimants.

garner benefit increases exceeding 5 percent of counterfactual income.

Hartz IV likely reduced benefits by more than these numbers suggest. First, my simulations conservatively assume that the (increasingly stringent) asset test does not bind, plausibly understating the share of claimants who became ineligible for long-term benefits under the new rules. Second, the "on-impact" income losses shown in Figure 2 were joined by additional cuts once the temporary supplemental benefits expired. Third, Hartz IV shifted the composition of net transfers towards housing assistance, which may be valued less than nominally equivalent cash transfers. On balance, Hartz IV appears to have worsened most workers' financial outlooks under long-term unemployment. To simplify the exposition, I refer to reform-induced benefit changes as "cuts", recognizing that a minority of claimants may have experienced slight gains instead.

Three other aspects of Hartz IV deserve mention. First, incumbent claimants were *not grandfathered in* under the pre-reform rules. Effective January 1, 2005, anyone already claiming long-term benefits was immediately converted to the new benefit regime. I account for this policy feature throughout my analysis. Second, the reform became publicly salient in July 2004, when the government mailed existing long-term beneficiaries a 16-page questionnaire to gauge their benefit eligibility under the new means test (Tompson, 2009). The questionnaire alerted incumbent claimants to the looming benefit cuts and sparked protests in dozens of German cities.[13] Third, because Hartz IV replaced a wage-indexed benefit with a uniform one akin to cash welfare, high-earning workers tended to receive higher absolute benefit levels before 2005 but steeper cuts thereafter. Even if I could perfectly measure each claimant's reform-induced benefit cut, variation in these cuts would therefore proxy for earnings capacity, which in turn may be correlated with responsiveness to a given benefit cut. My core identification strategy will sidestep this concern by relying on the timing of benefit changes rather than their simulated magnitude. For completeness, however, I also present corroborating results based on cross-worker heterogeneity in the size of the cuts.

3 Theoretical Framework

To clarify how reductions in long-term unemployment benefits might affect individual jobless durations and subsequent wages, I use a continuous-time job search model in the spirit of Mortensen (1977) to develop three predictions tailored to my empirical setting. First, a reduction in the long-term benefit level increases the reemployment hazard and decreases the reservation wage at all durations, as forward-looking agents react to future benefit cuts. Second, these behavioral responses limit to zero as cuts lie increasingly far in the future. This limiting result offers a theoretical justification for my research design, which posits that

[13] Consistent with this timing, Google Trends shows a sharp uptick in searches for the terms "Hartz IV" and "Arbeitslosengeld" in the summer of 2004 (Price, 2017). "Hartz IV" was also Germany's 2004 word of the year. Although July 2004 was clearly an information event, attentive claimants may have anticipated the benefit cuts prior to this date. I discuss the timing of anticipatory behavior in the context of a placebo exercise in Section 6.3 and an analysis of incumbent claimants in Section 7.

claimants facing far-off benefit cuts are a suitable reference group for counterfactuals in which these cuts do not occur at all. Third, although benefit cuts depress mean reemployment wages via lower reservation wages, this effect is at least partly offset by wage gains due to shorter durations.

Consider a displaced worker who searches for a job until reemployed. Search yields job offers at flow rate s and costs $\psi(s)$, where $\psi(\cdot)$ is convex and satisfies Inada conditions that ensure an interior optimum. Wage offers are drawn from a stationary continuous distribution $G(\cdot)$. Once accepted, a new job lasts forever. The worker receives flow utility of consumption $u(\cdot)$, discounted at rate $\delta > 0$. She cannot borrow or save.

In line with the empirical setting, I model UI as a two-tiered benefit schedule. The potential duration of short-term benefits is P. Letting $R \in [0, P]$ denote the *remaining* duration of benefits at a given point in time, UI benefits equal

$$b(R) \equiv \begin{cases} \bar{b} & \text{if } R > 0 \\ \underline{b} & \text{if } R = 0 \end{cases} \qquad \text{with } 0 < \underline{b} < \bar{b} \tag{3.1}$$

The benefit step-down is the sole source of nonstationarity in this model, and R is the sole state variable. Hartz IV operates by lowering $\underline{b}$, with effects varying as workers approach short-term benefit exhaustion.

Let $U(R)$ denote the indirect utility from being unemployed with R months of benefits remaining, and let $J(w) = \frac{u(w)}{\delta}$ denote the indirect utility from being employed forever at wage w. The optimal policy entails a cutoff strategy with reservation wage $\underline{w}$, so that $U(R)$ admits a Bellman representation:

$$\delta U(R) = \max_{s,\underline{w}} \; u(b(R)) - \psi(s) + s(1 - G(\underline{w}))(\mathbb{E}(J(w) \mid w \geq \underline{w}) - U(R)) - \dot{U}(R) \tag{3.2}$$

where $\dot{U}(R) \equiv \frac{dU(R)}{dR}$. The solution consists of policy functions $\underline{w}(R)$ and $s(R)$, denoting the reservation wage and search intensity chosen at each duration. The hazard rate of reemployment is $\lambda(R) \equiv s(R)(1-G(\underline{w}(R)))$, reflecting the need to first obtain a job offer and then accept it.

In Online Appendix B, I prove the following:

Proposition.

(a) A long-term benefit cut increases search intensity and decreases the reservation wage throughout the unemployment spell. That is, $\frac{ds(R)}{d\underline{b}} < 0$ and $\frac{d\underline{w}(R)}{d\underline{b}} > 0$ for all $R \geq 0$. It follows that $\frac{d\lambda(R)}{d\underline{b}} < 0$, so that a benefit cut increases the hazard rate of reemployment at all durations.

(b) These behavioral responses tend to zero for benefit cuts lying arbitrarily far in the future. That is,

$$\lim_{R\to\infty} \frac{ds(R)}{d\underline{b}} = \lim_{R\to\infty} \frac{d\underline{w}(R)}{d\underline{b}} = \lim_{R\to\infty} \frac{d\lambda(R)}{d\underline{b}} = 0$$

(c) There are offsetting effects on mean accepted wages. Observed wages decline conditional on completed duration, but workers accept jobs at earlier durations when reservation wages are higher.

Part (a) is intuitive: benefit cuts make long-term unemployment more onerous, and workers try harder to avoid it. Part (b) reflects discounting: as R grows, search intensity and reservation wages asymptote to the values workers would choose if benefits remained perpetually at the elevated level $\bar{b}$. These limiting values are invariant to $\underline{b}$. Part (c) captures the ambiguous effect of UI generosity on post-UI earnings, noted previously by Schmieder et al. (2016) and Nekoei and Weber (2017). Since a worker's mean accepted wage offer, $\mathbb{E}(w \mid w \geq \underline{w})$, is increasing in her reservation wage, benefit cuts depress the path of accepted wages through a selectivity effect. But they also shift the distribution of jobless durations to the left, so that greater weight is placed on earlier periods when reservation wages (and hence accepted wages) are high. Though absent from the model, duration dependence in wage offers due to skill depreciation or employer stigma would amplify this offsetting effect.[14] Which effect dominates is an empirical question.

4 Data

To study the effects of UI reform on unemployed workers' jobless durations and wages, I use individual work histories drawn from Germany's Integrated Employment Biographies, an administrative dataset that combines records on employment, unemployment, and benefit receipt. I accessed two anonymized extracts under agreement with the data custodian, Germany's Institute for Employment Research (IAB). For some tabulations, I supplement the IAB data using DIW Berlin's German Socioeconomic Panel (SOEP), a longitudinal household survey covering about 12,000 prime-age individuals per year.

4.1 The IAB data

Most of my analysis uses the IAB/IZA Administrative Evaluation Dataset (AED), a 4.7 percent random sample of all individuals who registered with the unemployment office anytime during 2001–2008 (Eberle and Schmucker, 2015). For each worker, I observe rich demographics (sex, year of birth, nationality, education, household structure, and district of residence), employment status (average daily earnings, part-time/full-time, and an establishment ID), and detailed information about periods of unemployment and UI receipt. These work/benefit histories span the years 1993–2010, and all spells are recorded at daily frequency. Since the AED is not representative of pre-2001 flows into unemployment, parts of my analysis instead rely on the SIAB, a 2 percent random sample of everyone who appears in the underlying data universe during 1975–2010 (vom Berge et al., 2013). The SIAB reports all of the data elements listed above.

The IAB data have several limitations relevant to my analysis. First, as already noted, I do not

[14] In a closely related model, Nekoei and Weber (2017) show that marginal cuts to short-term benefits have ambiguous effects on reemployment wages: either force may dominate, even if (as here) time-limited benefits are the only source of nonstationarity.

observe all of the inputs into the means-testing procedures for long-term unemployment and social assistance. Second, although I observe realized short- and long-term benefit claims (including net benefit levels) prior to 2005, data on long-term benefit receipt are frequently missing in 2005–2006 as a result of administrative transitions during the rollout of Hartz IV. These data gaps prevent me from analyzing take-up of long-term benefits, but they pose no other challenges to my analysis. Other limitations include minimal detail about hours worked and a lack of direct information about UI eligibility for workers who never claim UI. Finally, the underlying social security data exclude civil servants and the self-employed. Using SOEP data averaged over 2001–2005, I estimate that 84.1 percent of employed German workers ages 25–54 held jobs that would be detected in the IAB data, with 6.0 percent employed as civil servants and 9.9 percent self-employed.

4.2 Constructing the estimation sample

My main estimation sample consists of prime-age displaced workers who entered UI during 2001–2005. I therefore analyze UI inflows up to four years before Hartz IV and up to one year after it.[15] Using the AED, I first restrict to claimants aged 25–54 at UI entry to abstract from schooling, apprenticeship, and retirement decisions.[16] Second, to ensure that I am capturing new unemployment spells, rather than claims resumed after brief interruptions, I drop claimants who received short-term benefits in the 90 days preceding the new claim. Third, I restrict to claimants who separated from a socially insured job at most 30 days before entering UI. About three-quarters of new UI spells satisfy this criterion, with the remainder preceded either by unrecorded statuses (like self-employment and civil service) or by voluntary quits, which preclude a worker from claiming benefits for 12 weeks after separation. Requiring an observed separation lets me better measure the start of nonemployment, ensures that I observe key features of the worker's previous job, and reduces the risk that workers will exit from UI into employment statuses that are unrecorded in the administrative data.[17] Since quitters are (in principle) excluded, I call these claimants displaced workers.

Given these restrictions, some people appear in multiple (disjoint) UI spells. I retain all such spells, so that my estimates are representative of new flows into UI. I cluster standard errors by individual throughout the paper, and I show that my results are robust to randomly selecting only one UI claim per individual.

Employment and benefit receipt are recorded at daily frequency. Since benefits accrue in 60-day increments, I divide each jobless spell into 30-day "months". I follow each spell until the exact date of reem-

[15] Restricting attention to UI claimants excludes both individuals who are ineligible for UI and those who, though eligible, decline to take up benefits. This restriction is needed for the accurate calculation of potential short-term benefit duration. The choice of entry cohorts is dictated by the AED sampling frame—which is representative of UI inflows only from 2001 onward—and by subsequent reforms that applied to UI claims initiated after February 1, 2006.

[16] Besides often being used to define prime-age workers, these age cutoffs correspond to special benefit-sanction rules for claimants under 25 (van den Berg et al., 2014) and to provisions for partial retirement that kick in at age 55 (Berg et al., 2015).

[17] Tabulations in the SOEP, where I can observe workers employed outside of the social security system, show that workers who enter UI from self-employment or civil service jobs are more likely to return to one of these unobserved statuses.

ployment; I censor unfinished spells at 24 months but also report results using 12- or 36-month horizons.[18] In most of my analysis, I define reemployment as returning to a job covered by social insurance, which I call a "regular job". This employment concept excludes tax-favored "mini-jobs", which can pay at most €325–400 per month and which are often held concurrently with benefit receipt (Tazhitdinova, 2019). Since workers may use mini-jobs to supplement, rather than supplant, their UI benefits, transitions into regular jobs are likely to be a better measure of how long it takes to find gainful employment (and of one's earnings upon doing so). I revisit mini-jobs in Section 9.

Employers are required by law to report each worker's average daily earnings at least once per year. I deflate earnings to 2005 EUR using Germany's consumer price index, then multiply by 30 to obtain monthly earnings, which I call "wages". I record prior wages using the final wage report for the last regular job preceding UI receipt; likewise, I record reemployment wages using the earliest wage report for the first post-UI job. IAB wage records are topcoded at the maximum earnings level subject to social security contributions, but the topcode seldom binds in my sample.[19] For this reason, I do not adjust for topcoding, but my wage results are only trivially affected if I exclude workers whose pre-UI wages were censored. To minimize the influence of outliers, I winsorize all wage measures at the 0.5th and 99.5th percentile of pre-UI wages within the estimation sample.[20] As a control for underlying earnings potential, I assign workers to deciles of prior wage within cells defined by sex $\times$ West/East residence $\times$ year of UI entry.

I assign each worker to one of three education groups. Because employers often fail to report workers' educational attainment, I use Fitzenberger et al.'s (2006) algorithm to impute missing levels of education based on the longitudinal structure of the data. I code each worker as a German native or non-native based on the earliest reported nationality. I partition claimants into seven age bins and three household types based on their recorded status at UI entry.[21] As a proxy for labor force attachment, I assign workers to one-year "experience" bins of days worked in regular employment over the past seven years.

Table 1 presents summary statistics for both the estimation sample and a comparison group of prime-age employed workers. The estimation sample comprises about 337,000 new UI claims filed by 245,000 distinct individuals. Men account for 62.3 percent of claims, somewhat exceeding their share of employment.

[18] Using a two-year horizon ensures that all spells are censored prior to the 2008 financial crisis, which may have altered workers' sensitivity to UI exhaustion. I do not prematurely censor spells in cases of labor force exit because (i) labor force dropouts are still at risk of reappearing in the employment records later; (ii) deregistration from unemployment may be endogenous to future UI benefits; and (iii) Hartz IV created a seam in the unemployment rolls by obligating welfare recipients to register as unemployed. This data seam confounds measurement of labor force exit but does not otherwise jeopardize my analysis.

[19] Just 1.8 percent of claimants in my sample had pre-UI wages exceeding 98 percent of the wage ceiling. Among claimants reemployed within 24 months, just 1.2 percent had post-UI wages exceeding this threshold.

[20] The 99.5th percentile of the observed wage distribution lies above the statutory topcoding threshold, perhaps reflecting occasional errors in recorded wages. Winsorizing trims these potentially errant wages. I obtain virtually identical results if I use unadjusted wage reports or if I cap all wage reports at the topcoding threshold in lieu of winsorization.

[21] The education groups are: no apprenticeship or university-entrance (*Abitur*) exam; apprenticeship or *Abitur* only; and university or polytechnic degree. The age bins are five-year bins below 45, then bins mirroring the age cutoffs in the UI benefit schedule (45–46, 47–51, and 52–54). The household types are unmarried, married without children, and married with children.

Claimants are adversely selected in a number of respects. About one-third of claims originate in economically distressed East Germany, compared with just one-fifth of total employment. Mean pre-tax monthly earnings prior to job loss were €1,861, well below mean earnings in the comparison group (€2,521); claimants are slightly younger, more likely to be non-German natives, and less likely to have worked for at least four of the preceding seven years. Initial UI benefits average €807 per month. The distribution of completed jobless durations is markedly skewed: 46.9 percent of spells end within six months, but fully 26.7 percent last over two years. Among claimants returning to regular work within two years of UI onset, average monthly earnings in the first new job were €1,776, about 5 percent below average pre-UI earnings.

4.3 Computing potential benefit duration

The research design presented below hinges on accurate measurement of potential short-term benefit duration. Although the IAB data do not explicitly record potential duration at the start of a new UI claim, I can infer it from realized UI duration together with a variable recording unused benefits, if any, remaining at the end of a UI spell. Let R denote the duration of these residual benefits (in days), let D denote the completed duration of a short-term benefit spell (in days), and let $\bar{P}(a)$ denote the age-specific maximum benefit duration (in months). I compute start-of-spell potential benefit duration, measured in months, as

$$P \equiv \min\left(\bar{P}(a), \text{round}\left(\frac{D+R}{30}\right)\right), \tag{4.1}$$

that is, I set the potential duration equal to the realized duration plus any time remaining in the worker's claim, rounding this sum to the nearest month, and override the result if it exceeds the legal maximum. Online Appendix C explains this imputation procedure in detail, presents evidence validating my measure of potential duration, and shows that my results are robust to using an alternative procedure that relies only on information observed prior to UI entry.

The right panel of Figure 1 plots the pdf of potential short-term benefit durations across new UI spells, subdivided by age. About half of claimants are massed at their age-specific duration ceilings, especially the 12-month ceiling for workers under age 45. Just over one-sixth of claimants (necessarily over 45) have entitlements exceeding 12 months. A similar share have entitlements no longer than six months. I exploit variation in UI duration between and within age groups to identify the causal effects of Hartz IV.

5 Research Design

Hartz IV's uniform rollout, lack of grandfathering, and institutional complexity pose challenges for researchers evaluating its effects. In this section, I explain how I exploit cross-worker variation in the potential duration of short-term UI benefits to identify the effects of Hartz IV on individual jobless durations. Later sections adapt this methodology to investigate other outcomes of interest.

5.1 Heuristic analogy to difference-in-differences

My research design can be understood as a generalization of difference-in-differences. To build intuition, I begin with a descriptive comparison of job-finding rates between claimants who enter UI in 2001—long before Hartz IV took effect—and those who enter in 2005, after Hartz IV was fully in place. Within each entry cohort, I distinguish claimants entitled to 12 months of short-term UI benefits from claimants entitled to only 6 months of benefits. Although my main specification is much more general, this simple 2×2 contrast clarifies how I exploit within-cohort differences in benefit duration, together with pre-/post-reform variation in the policy environment, to isolate the causal effects of exposure to Hartz IV.

The left panel of Figure 3 plots the empirical hazard rate of reemployment among claimants entitled to 12 months of benefits (the modal entitlement), separately for those entering UI in 2001 vs. 2005. As in other studies of unemployment, job-finding rates initially rise with duration and then decline, with an uptick in the vicinity of short-term benefit exhaustion. All else equal, we would expect the 2005 entrants to find jobs faster than the 2001 entrants, since they incur steeper benefit drops upon exhaustion. At short durations, post-reform claimants in fact have slightly lower job-finding rates—perhaps due to the slacker labor market of 2005—but, consistent with a pro-employment effect of Hartz IV, they exhibit a much larger increase in job-finding as they approach benefit exhaustion than pre-reform claimants do.

Of course, other relevant factors—such as labor demand, credit supply, or claimant characteristics—might have changed in these years. If such contemporaneous changes differentially impact job-finding at different jobless durations, then the uptick in job-finding at around 12 months may not be a true causal effect of Hartz IV. Differencing out such potential confounds requires variation in potential benefit duration, so that I can isolate the impact of the UI reform—whose effects, according to search theory, depend on *time remaining until UI exhaustion*—from other factors whose effects may vary with *time since job loss*.

To distinguish greater responsiveness to UI exhaustion from changes in duration dependence unrelated to UI reform, the right panel plots the hazard rate among claimants entitled to only 6 months of short-term benefits (another common entitlement). As before, we would expect the 2005 cohort to find jobs faster, but this tendency should—and does—manifest more quickly for the 6-month claimants, for whom

Hartz IV binds earlier in their jobless spells.

5.2 Benchmark hazard specification

My formal research design generalizes beyond this 2×2 example by incorporating workers with any possible benefit entitlement—not just 6 or 12 months—who entered UI anytime between 2001 and 2005. To do this, I estimate a dynamic econometric model that separately identifies the pre-reform "main effect" of benefit exhaustion and the incremental effect of the Hartz IV benefit cuts. The model allows me to control flexibly for calendar time effects, compositional changes, and other important determinants of individual job prospects, and to obtain causal effects estimated using the full population exposed to Hartz IV.

Let worker i begin a UI spell on date u_i. Grouping the data into 30-day increments ("months"), indexed by d, I define two key durations corresponding to changes in the UI benefit level. The first of these, d_i^E, is the duration at which the worker exhausts short-term benefits:

$$d_i^E \equiv P_i \tag{5.1}$$

where potential short-term benefit duration P_i, measured in months, is computed as in Equation 4.1.

The second event, d_i^H, denotes the duration at which the worker first receives long-term benefits under the post-reform rules. Let

$$d_i^{2005} \equiv \min\{d \in \mathbb{N} \mid u_i + 30d \geq \text{January 1, 2005}\} \tag{5.2}$$

denote the first duration observed after the reform legislation takes effect. Then

$$d_i^H \equiv \max\{d_i^E, d_i^{2005}\} \tag{5.3}$$

Hence d_i^H, which may or may not coincide with d_i^E, is the duration at which Hartz IV "bites" for a given worker. Figure 4 plots hypothetical examples of these events for successive cohorts of claimants with 12 months of potential benefits.

Following common practice in the UI literature, I estimate a discrete-time proportional hazard model using the complementary log-log link (Prentice and Gloeckler 1978; Meyer 1990). Letting D_i denote the completed jobless duration (rounded up to the nearest month), I specify the conditional probability of being reemployed during the dth month of the spell as

$$\lambda_{id} \equiv \Pr(D_i = d \mid D_i > d - 1) = 1 - \exp(-\exp(\mathbf{x}'_{id}\beta)), \tag{5.4}$$

where the instantaneous log hazard rate is given by

$$\mathbf{x}_{id}'\beta = \alpha_d + \gamma_t + \mathbf{z}_{id}'\phi + \sum_{k=-9}^{4} \delta_k^E \mathbf{1}\{\tau_{id}^E = k\} + \sum_{k=-9}^{4} \delta_k^H \mathbf{1}\{\tau_{id}^H = k\} \tag{5.5}$$

and $t \equiv u_i + 30d$ denotes the end-of-period calendar date. I estimate the model via maximum likelihood, with standard errors clustered by individual to allow for arbitrary correlation for workers with repeated spells.

In Equation 5.5, α_d represents a full set of duration dummies, allowing job-finding rates to vary freely as a function of months since beginning a claim (i.e., I allow for a nonparametric baseline hazard). The shifters γ_t control for a variety of calendar-time effects. First, I include a full set of quarter $\times$ year interactions, allowing the hazard rate to shift proportionally in response to changes in labor market conditions and other aggregate time effects. Second, I include a full set of month dummies (not interacted with year) to absorb higher-frequency seasonal effects, such as retail hiring just before Christmas. Third, with slight abuse of notation, I also take γ_t to include interactions between quarter dummies and a set of 3-month duration bins, constructed by partitioning durations into the segments $\{1–3,\ 4–6,\ \ldots,\ 22–24\}$. These interactions, which live at the $d \times t$ level, allow for seasonal fluctuations that differentially affect workers early vs. late in their spells, such as end-of-winter recalls from temporary layoff. To explore sensitivity, I sometimes interact the 3-month duration bins with quarter $\times$ year (rather than quarter only), thereby allowing for changes in the degree of duration dependence over the course of my sample period. $\mathbf{z}_{id}$ is specified below.

The key explanatory variables are flexible functions of event time relative to each benefit step-down:

$$\begin{aligned} \tau_{id}^E &\equiv \min\{d - d_i^E, 4\} \qquad &\text{(months relative to short-term benefit exhaustion)} \\ \tau_{id}^H &\equiv \min\{d - d_i^H, 4\} \qquad &\text{(months relative to reform-induced benefit cut)} \end{aligned} \tag{5.6}$$

The associated coefficients δ_k^E and δ_k^H allow the hazard rate to vary flexibly in a window around each benefit change. Each omitted group comprises periods 10 or more months before the benefit change, and I impose the same coefficient on observations 4 or more months after the change.[22] I report the normalized hazard ratios $\exp(\hat{\delta}_k^E) - 1$ and $\exp(\hat{\delta}_k^H) - 1$, which represent the predicted proportional change in the instantaneous reemployment hazard associated with event times τ_{id}^E and τ_{id}^H, respectively, relative to the predicted hazard 10 or more months before the corresponding benefit change occurs. In using as the reference group observations for which benefit changes lie far in the future, I am implicitly assuming that the causal effects of benefit cuts vanish at long durations. This modeling choice is justified by the theoretical result in Section 3 that

[22] The appropriate endpoints are not entirely obvious. However, if the "left" endpoint were to exceed 12 months pre-exhaustion, then some of the coefficients would be identified solely by older claimants with over 12 months of benefits. Similarly, if the "right" endpoint were to extend far beyond 4 months, then some of the post-Hartz coefficients would be identified by only a small subset of cohorts and benefit durations. My choices trade off flexibility against these considerations.

behavioral responses to future benefit cuts tend to zero at sufficiently long horizons.[23] To the extent that claimants already start responding to future benefit cuts as early as 10 months beforehand, my estimates will be a conservative lower bound on the true causal effect.

Without additional controls, these event-time variables would be mechanically correlated with age and experience, the determinants of potential benefit duration. In all specifications, therefore, $\mathbf{z}_{id}$ includes controls for seven age bins and for one-year bins of time worked in the seven years preceding UI receipt. I allow the shape of the hazard function to vary flexibly with age and experience by interacting each of these controls with 3-month duration bins. The remaining control variables in $\mathbf{z}_{id}$ account for other demographic characteristics that are correlated with job-finding rates. Since job-finding patterns differ markedly by sex, I interact the duration dummies with a female indicator, allowing the baseline hazard function to differ arbitrarily between men and women. Next, given the sharp West/East disparity in economic conditions, I control for East German residence interacted with 3-month duration bins. Finally, I include dummies for deciles of prior wage, three education groups, German nationality, and three household types. I measure all demographic characteristics at UI entry, so that they are fixed within each spell.

6 Effects on Jobless Durations

How did Hartz IV affect jobless durations among UI claimants? I first present hazard estimates based only on the 2001 and 2005 cohorts of UI entrants, for whom exposure to Hartz IV is easiest to characterize, then broaden the sample to include intermediate cohorts as well. After subjecting my results to a bevy of sensitivity checks, I use my preferred estimates to quantify the impact of Hartz IV on individual reemployment trajectories and, more speculatively, steady-state unemployment.

6.1 Estimates using only 2001 and 2005 UI entrants

Recall that Figure 3 presented raw empirical hazard rates among workers with 6 or 12 months of short-term benefits who entered UI in either 2001 or 2005. As a stepping-stone towards my benchmark hazard specification, I first redefine the sample to include workers with any potential benefit duration, while continuing to restrict to 2001 and 2005 UI entrants. Because all of these claimants are subject to a fixed benefit schedule during the 24-month observation window, I estimate a simplified version of my hazard specification that is suitable for claimants facing only a single benefit step-down. For each cohort $Y \in \{2001, 2005\}$, I run a

[23] In the terminology of Abbring and van den Berg (2003), this supposition embodies the "no anticipation" assumption that underlies identification in timing-based research designs.

discrete-time hazard model that replaces the instantaneous log hazard rate from Equation 5.5 with

$$\mathbf{x}_{id}'\beta = \alpha_d + \gamma_t + \mathbf{z}_{id}'\phi + \sum_{k=-9}^{4} \delta_k^Y \mathbf{1}\{\tau_{id}^E = k\}, \tag{6.1}$$

where the event-time coefficients δ_k^{2001} and δ_k^{2005} capture changes in job-finding as workers approach benefit exhaustion under the old and new UI rules, respectively. Given the nonparametric baseline hazard, these estimates are identified by individual variation in potential short-term benefit duration: holding constant time since UI onset, workers differ in time to benefit exhaustion.

Figure 5 plots the normalized hazard ratios $\exp(\hat{\delta}_k^Y) - 1$. The 2001 series replicates a classic finding: prior to the reform, there is a clear "spike" in the job-finding hazard at the point of benefit exhaustion (Moffitt, 1985; Meyer, 1990; Katz and Meyer, 1990a).[24] What is novel here is the pre/post change. For the pre-reform cohort, I find a 43 percent higher hazard rate in the month of exhaustion, relative to the hazard rate 10 or more months prior to exhaustion. For the post-reform cohort, the corresponding increase is 102 percent (rising further to 129 percent in the following month), suggesting that job-finding rates respond strongly to the generosity of post-exhaustion transfers.[25]

6.2 Benchmark estimates using the full sample

By design, Figure 5 abstracts from a key feature of Hartz IV: because no one was grandfathered under the pre-2005 rules, many claimants who entered UI under the old rules were *partially exposed* to Hartz IV, in the sense that the benefit cuts bind after benefit exhaustion, but early enough to affect behavior in a two-year window after UI entry. The lack of grandfathering poses a difficult identification challenge: drawing a clean comparison between *fully exposed* and *de facto non-exposed* UI entrants obligates the econometrician to compare cohorts spaced several years apart,[26] but doing so amplifies the potential for intervening changes in labor demand, credit supply, or institutions—such the Hartz I–III reforms adopted in 2003 and 2004—to confound identification of the causal effects of Hartz IV. I now incorporate these interim cohorts into the analysis, enabling me to control flexibly for secular changes in job-finding unrelated to the timing of benefit cuts. Including the interim cohorts also enables me to retain a key population affected by Hartz IV: incumbent long-term claimants who were "caught in the storm" when the reform was announced.

[24] Card et al. (2007b) question the conventional wisdom about exhaustion spikes. Using Austrian administrative data, they find a large spike in exits from registered unemployment but only a small increase in job-finding when benefits run out. The estimates in Figure 5 (and throughout the paper) reflect true job-finding, not deregistration from unemployment.

[25] For a claimant whose short-term entitlement is an exact multiple of 30 days (as is most often the case), the time conventions adopted in this paper imply that event time $\tau_E = 0$ corresponds to the 30-day period leading up to, and ending on, the date of benefit exhaustion, with event time $\tau_E = 1$ representing the 30 days immediately following exhaustion.

[26] If incumbent claimants had been shielded from the reform, one could use an RD design to compare claimants who enter UI just before/after January 1, 2005. These groups would face sharp differences in long-term benefit generosity but identical labor market conditions and institutions. Absent grandfathering, however, both groups are equally exposed to the new regime.

The benchmark specification laid out in Section 5.2 allows each worker to respond to two distinct benefit step-downs: the main effect of short-term benefit exhaustion, plus the incremental effect of reform-induced benefit cuts. Figure 6 plots the estimated effect of these benefit drops on the hazard rate of reemployment. The left panel shows that, conditional on time since UI entry, the job-finding hazard is initially stable in the months leading up to short-term exhaustion, then rises sharply. The point estimate for $\tau_{id}^{E} = 0$ indicates that the hazard rate at exhaustion is 43 percent higher than the hazard rate 10 or more months prior to exhaustion. These estimates mirror the pre-reform exhaustion effects seen in Figure 5.

The right panel shows the estimated causal effect of Hartz IV on the job-finding hazard. As workers approach reform-induced benefit cuts, the normalized hazard ratio rises steadily, peaking in the month after the benefit drop ($\tau_{id}^{H} = 1$). This pattern of rising hazard effects in the months preceding a benefit cut is indicative of forward-looking behavior on the part of UI claimants. The behavioral response is large: relative to claimants for whom Hartz IV lies 10 or more months in the future, claimants who have just undergone benefit cuts are 47 percent more likely to return to regular work at that time. Given the proportional hazards structure, the Hartz IV effects greatly magnify the pre-existing spike at benefit exhaustion. For claimants fully exposed to the post-reform benefit schedule, the hazard rate just after exhaustion is estimated to be 106 percent greater than the hazard rate when exhaustion lies 10 or more months away ($= \exp(\hat{\delta}_1^E + \hat{\delta}_1^H) - 1$), conditional on duration since UI entry and other observables. This is in the same ballpark as the exhaustion spike measured for post-reform claimants in the simpler specification used in Figure 5.[27]

A natural question here is why the Hartz IV coefficients, like those pertaining to benefit exhaustion itself, decline soon after the cut rather than remaining constant at a high level. In the simple search model of Section 3, where the environment is stationary after benefits run out, the exit hazard rises in the run-up to a benefit cut and then stays constant. In practice, however, UI studies generally find an exhaustion "spike" rather than an exhaustion "plateau". The literature has advanced several explanations for this pattern. Boone and van Ours (2012) argue that many job offers are "storable", so that jobseekers strategically time their start dates to coincide with benefit exhaustion. DellaVigna et al. (2017) posit that jobseekers have reference-dependent preferences anchored to recent income. In this view, the hazard rate declines post-exhaustion because workers become accustomed to lower income and consequently search less. A third possibility is that the spike reflects unobserved individual heterogeneity: if the workers who are most sensitive to benefit reductions return to work when benefits fall, the claimants who remain will be the ones least responsive to benefit cuts. While this last explanation is potentially concerning, I show in Online Appendix D that I obtain

[27] Online Appendix Figure 2 estimates the model separately by sex. Men and women show similar proportional responses to Hartz IV, but the main effect of benefit exhaustion is much stronger for women. This disparity likely reflects the role of means testing on the basis of spousal income: since married women tend to have higher-earning spouses than do married men, they are more likely to be ineligible for long-term benefits even under the pre-reform rules. Consistent with this explanation, the exhaustion spike is much stronger for married women than for singles, whereas married and unmarried men show similar spikes.

similar results using an alternative research design that is immune to concerns about dynamic selection.

6.3 Robustness

My benchmark hazard results are robust to a variety of control strategies and sample modifications. I present these robustness checks graphically in Figure 7. To avoid cluttering the figure, I omit confidence intervals but present the estimates and their standard errors in Online Appendix Table 1.

Specification 1 redisplays my benchmark Hartz IV effects. Specification 2 allows for compositional changes in the pool of new UI claimants by adding a dummy for each quarter × year of entry into UI. Although I already control for a rich set of observable covariates, the effects of Hartz IV could potentially be confounded by *unobserved* changes in claimant characteristics across cohorts.[28] Reassuringly, this specification yields similar (and in fact somewhat larger) effects.

The next two series stress-test the logic underlying my identification strategy. First, in specification 3, I allow the age × duration and experience × duration interactions in $\mathbf{z}_{id}$ to differ before and after July 1, 2004, when Hartz IV became salient. Suppose that, for reasons unrelated to benefit cuts, younger workers experienced a differential improvement in their job prospects around this time. Because young workers tend to have briefer UI entitlements (and are thus more exposed to Hartz IV), such an improvement might be falsely credited to the benefit cuts. Adding interactions between age bins, duration bins, and a post-reform dummy addresses this concern. The experience interactions serve a similar function. Second, specification 4 allows the *shape* (as well as level) of the baseline hazard to change over time by interacting each quarter × year dummy with the full set of 3-month duration bins. This more flexible specification allows the job-finding hazards at short, medium, and long durations to evolve independently of one another from quarter to quarter. In effect, this demanding specification compares the job-finding patterns of similar workers who are observed at similar durations at a given point in time, but who differ in the timing of benefit cuts. In both of these specifications, the results are strikingly stable, again modestly larger in magnitude.

Some individuals in my estimation sample appear in multiple, disjoint spells. Although it is not obvious why repeat spells would present any problems (recalling that I cluster on individual), specification 5 restricts to a single UI spell per individual by selecting one spell at random among people who experience multiple spells. Again, this specification yields very similar estimates.

The final series addresses a more specific selection concern. Under a standard model of UI take-

[28] For example, workers laid off in 2005 may be positively selected on unobservables relative to those laid off during the tighter labor market of 2001 (Mueller, 2017). Since late-sample workers are more exposed to Hartz IV, failing to control for cohort effects could lead me to overstate the pro-employment effects of the reform. In practice, the scope for composition bias appears to be limited, as there is little observable time-series variation in claimant characteristics. Online Appendix Figure 3 plots mean predicted jobless duration by quarter of entry into UI, based on a Weibull model fitted to fixed claimant characteristics. There is a slight decline in predicted duration over my sample period, but no discontinuity or trend break around Hartz IV.

up (Anderson and Meyer, 1997), benefit cuts should deter some workers from claiming UI. These take-up "compliers" (who would claim benefits only under the pre-Hartz rules) may differ unobservably from those who would claim benefits under both regimes. To limit the scope for such differential take-up, specification 6 restricts the sample to workers who enter UI before July 2004. I again obtain very similar estimates, suggesting that differential take-up does not drive my results.[29]

6.4 A placebo exercise using pseudo-reforms

Was January 2005 unique in generating these effects? Consider two threats to identification. First, suppose that German UI claimants became steadily more responsive to short-term benefit exhaustion over the course of my sample period for some reason unrelated to the Hartz reforms. This could occur if, for example, the supply of consumer credit contracted as economic conditions worsened in the early 2000s. Second, suppose that the earlier Hartz measures—implemented in January 2003 and January 2004—differentially affected job-finding among claimants who were close to exhausting benefits. Either phenomenon could potentially result in spurious "Hartz IV" effects even if the UI reform itself had no causal effect on job finding.

To assess these threats, I estimate placebo specifications that alter the assumed date of the UI reform to January 1 of each year $Y \in \{1998, 1999, \ldots, 2004, 2005\}$.[30] Figure 8 confirms that 2005 was different. In the left panel, the "actual reform" series replicates my benchmark estimates using the smaller SIAB sample, whose sampling frame is better suited to this exercise. For the 1998 through 2003 pseudo-reforms, the estimated placebo effects are close to zero, militating against a secular rise in sensitivity to benefit exhaustion. The stability of the placebo estimates is especially encouraging because Germany's labor market picture was changing rapidly in these years. For 2004, I do find positive placebo effects, but they are reassuringly much smaller than the true Hartz IV effects. In Online Appendix E.1, I discuss three possible explanations for these modest placebo effects: an earlier tightening of the asset test for long-term benefits; a short-lived increase in the frequency of benefit sanctions; and anticipatory responses to Hartz IV itself.

As an additional check, the right panel presents estimates from a richer specification that allows the baseline hazard to evolve flexibly over time. This refinement, which more stringently partials out changes

[29] I note two further robustness checks. First, my research design exploits variation in potential benefit duration stemming from both age and experience. In Online Appendix Figure 4, I isolate each source of variation in turn by reestimating the model among workers under 45 (isolating variation due to work history) and then among workers with maximal durations given their age (isolating variation due to age). These models yield qualitatively similar results. Second, estimated hazard effects may be biased in the presence of unobserved heterogeneity ("frailty") in individual hazard rates (van den Berg, 2001). A model allowing for normally distributed frailty yields nearly identical estimates.

[30] For placebo year Y, I select UI claims initiated between January 1 of $Y-4$ and June 30 of Y, then reestimate Equation 5.5 with event-time recoded based on the placebo reform date. I censor incomplete spells on June 30 of Y to avoid misattributing the causal effect of Hartz IV to the placebo reforms. Given the abbreviated post-"reform" period, I impose a single coefficient for all post-event periods. Different age cutoffs for the maximum benefit duration applied to UI claims made before April 1999. Placebo results use whichever cutoff was in effect at the onset of the claim and are robust to excluding pre-April 1999 claims. To avoid visual clutter, I omit confidence intervals but present the detailed estimates in Appendix Table 3 and 4.

in duration dependence unrelated to benefit cuts, strengthens the true effects while attenuating the 2004 placebo, lending further support to a causal interpretation of my estimates. Further evidence in Section 7, based on an alternative source of identifying variation unrelated to potential benefit duration, confirms the tight temporal link between the onset of UI cuts and changes in job-finding among UI claimants.

6.5 Magnitudes

Having argued that my estimates are causal effects of Hartz IV on job-finding hazards, I now quantify what these estimates imply for both individual jobless durations and, more speculatively, aggregate unemployment.

Individual jobless durations

How do the estimated hazard effects translate into effects on jobless durations? Though proportional hazard effects are informative about behavioral responses among still-unemployed workers, the overall impact depends on the underlying distribution of durations.[31] To quantify shifts in the path of cumulative job-finding, I predict successive UI cohorts' reemployment rates both under the fitted model—which incorporates the Hartz IV benefit cuts—and under a counterfactual scenario in which these cuts do not occur.

For each UI spell, I use my benchmark estimates to fit the discrete-time hazard rate at each duration:

$$\hat{\lambda}_{id} \equiv \widehat{\Pr}(D_i = d \mid D_i > d-1) \equiv 1 - \exp(-\exp(\mathbf{x}'_{id}\hat{\beta})). \tag{6.2}$$

Chaining these hazard rates yields each claimant's probability of being reemployed by a given duration:

$$\hat{F}_{id} \equiv \widehat{\Pr}(D_i \leq d) \equiv 1 - \prod_{s=1}^{d}(1 - \hat{\lambda}_{is}). \tag{6.3}$$

To predict reemployment rates absent Hartz IV, I recompute this expression with the time-to-Hartz variable τ^H_{id} recoded to the omitted category at all durations.[32] This yields counterfactual reemployment rates $\hat{F}^{cf}_{id}$. Finally, I compute the average effect of the reform on claimants entering UI in year Y as

$$\widehat{\Delta F}^{Y}_{d} \equiv \frac{1}{N_Y}\sum_{i|u_i \in Y}(\hat{F}_{id} - \hat{F}^{cf}_{id}). \tag{6.4}$$

Table 2 reports $\widehat{\Delta F}^{Y}_{d}$ by entry cohort for $d \in \{6, 12, 24\}$ months. For claimants who enter UI in

[31] To see this, write the cumulative reemployment rate recursively as $F(d) \equiv \sum_{k=1}^{d} f(k) = \sum_{k=1}^{d} S(k-1)\lambda(k)$, where $S(\cdot) \equiv 1 - F(\cdot)$ is the survival function. Letting $S^{cf}(\cdot)$ and $\lambda^{cf}(\cdot)$ denote counterfactual survival and hazard rates absent reform, a useful approximation to the change in reemployment is $dF(d) \approx \sum_{k=1}^{d} S^{cf}(k-1)\lambda^{cf}(k)\mathrm{dlog}\lambda(k)$. This expression makes it clear that Hartz IV's effects depend not only on the proportional hazard effects, but also on how many workers remain at risk and on the counterfactual hazard that the proportional effect magnifies. Long-term benefits matter more when workers tend to reach long-term unemployment and when, upon doing so, they are on the margin of finding work.

[32] That is, I replace $\mathbf{x}'_{id}\hat{\beta}$ with $\mathbf{x}^{cf\prime}_{id}\hat{\beta} \equiv \hat{\alpha}_d + \hat{\gamma}_t + \mathbf{z}'_{id}\hat{\phi} + \sum_{k=-9}^{4}\hat{\delta}^E_k \mathbf{1}\{\tau^E_{id} = k\}$, which partials out the Hartz IV term.

2001, the predicted effect of Hartz IV is mechanically zero: two years after entry, all workers in this cohort are still at least 10 months away from the benefit cuts, so $\hat{F}_{id}^{cf} = \hat{F}_{id}$ by construction. At the other extreme, claimants who enter UI in 2005 are fully exposed to Hartz IV: they encounter the new regime as soon as they exhaust short-term benefits. Interim cohorts are partially exposed, depending on when they enter UI and when their benefits run out. As expected, the effects of Hartz IV cumulate steadily for successive cohorts.

The 2005 cohort offers my best estimate of the steady-state impact of Hartz IV on jobless durations. For this cohort, Figure 9 plots the full path of estimated reemployment effects for 24 months after entry into UI.[33] The effects accrue rapidly for the first 13 months, then decline slightly thereafter as the counterfactual series partly catches up to the factual series.[34] Employment gains are largely persistent at 24 months, suggesting that benefit cuts have enduring effects on cumulative job-finding even at lengthy durations.[35]

For statistical purposes, Germany defines long-term unemployment—as distinct from long-term *benefits*, whose timing varies across workers—as a jobless spell lasting over one year. I estimate that Hartz IV increased the probability of being reemployed within 12 months of UI entry by 4.2 percentage points, relative to a counterfactual probability of 62.7 percent. In proportional terms, these estimates imply that Hartz IV reduced the likelihood of reaching long-term unemployment by 11.3 percent.[36]

Steady-state unemployment rate

Once derided as the "sick man of Europe" (Dustmann et al., 2014), the German economy embarked on a remarkable turnaround just as Hartz IV took effect: between June 2004 and June 2014, unemployment fell by 5.3 percentage points. To what extent did Hartz IV contribute to this dramatic decline?

My research design identifies partial equilibrium impacts of benefit cuts on individual job-finding. In general equilibrium, the direct effect of increased search effort may be either mitigated by congestion externalities or amplified by job creation. Although a full reckoning of Hartz IV's aggregate impact is beyond the scope of this paper, I use a back-of-the-envelope calculation to gauge what my partial equilibrium

[33] In the spirit of Chernozhukov et al. (2013), I construct confidence intervals by drawing 500 parameter vectors using the estimated, asymptotically normal variance-covariance matrix, replicating the quantification exercise, and taking the standard deviation across estimated effects. I use the same procedure for similar quantification exercises later in the paper.

[34] When I estimate the model separately by sex, these impacts are larger for women, who have longer mean jobless durations and hence are more likely to exhaust UI benefits and come face-to-face with benefit cuts (see Online Appendix Figure 2).

[35] In the Online Appendix, I conduct two additional exercises to further probe the persistence of these effects. First, to verify that my results are not sensitive to the censoring horizon, I reestimate my benchmark specification with incomplete spells censored at either 12 months or 36 months. Online Appendix Figure 5 reports net employment effects under these alternative horizons. The magnitudes are very similar; the net employment effect continues to shrink beyond 24 months but remains sizable even 36 months after UI entry. Second, in Online Appendix D, I report results from an alternative research design that enables me to examine Hartz IV's impact on the share of UI claimants *currently* employed at a given duration after claim initiation (as distinct from the share who have *ever* been reemployed by that horizon). I find similar impacts on this outcome, suggesting that the job gains induced by Hartz IV were not quickly undone by workers slipping back into unemployment.

[36] While the elasticity of jobless durations with respect to long-term benefit generosity is of obvious interest, the substantial uncertainty surrounding the size of changes in benefit levels and net household income (Section 2.2) precludes such a calculation. The difficulty is compounded by the fact that, as discussed in Section 7, Hartz IV may have operated partly through other mechanisms that amplified the impact of the benefit cuts on claimants' responsiveness to benefit exhaustion.

estimates, if taken at face value, imply for Germany's steady-state unemployment rate.

In Online Appendix F, I derive simple steady-state formulas for the short- and long-term unemployment rates (defined as the share of the labor force unemployed for under/over 12 months). Based on the observed time series, I set Germany's pre-Hartz steady-state unemployment rate to 10.0 percent, divided equally between the short- and long-term components. Using my estimated hazard model to predict month-by-month job-finding hazards in the absence of Hartz IV, I calibrate the rate of job separation—together with the (unmodeled) job-finding rate beyond 24 months—to satisfy the two steady-state formulas. I then recompute steady-state unemployment using job-finding rates inclusive of Hartz IV's effects. The short- and long-term unemployment rates fall to 4.79 percent and 4.36 percent, respectively.

Though stylized, this exercise suggests that Hartz IV may have reduced Germany's unemployment rate by 0.85 percentage points—a measurable, if not preponderant, share of its "employment miracle".[37] Strikingly, the steady-state impact of Hartz IV is driven almost entirely by a 0.64 percentage point reduction in the *long-term* unemployment rate. This finding underscores the point that long-term benefit generosity is especially relevant for prolonged jobless spells, and it echoes the Ljungqvist and Sargent (1998, 2008) hypothesis that generous UI benefits may contribute to persistently high levels of long-term unemployment.

7 Benefit Cuts and Alternative Mechanisms

I have shown that job-finding rates rise as UI claimants approach long-term benefit cuts induced by Hartz IV. My research design disentangles this fourth wave of the Hartz package from the three waves that preceded it, and it further zeroes in on behavioral changes tied to the timing of the cuts. Nonetheless, Hartz IV may have influenced claimants' sensitivity to benefit exhaustion through other channels as well, including changes in caseworker behavior, greater stigma associated with prolonged reliance on the welfare state, or confusion sown by media hype about draconian cuts to the safety net. While I cannot rule out a role for alternative mechanisms, this section offers direct evidence that long-term benefit cuts were central to the increase in job-finding. These results establish tight links among the timing, severity, and effects of benefit cuts, and they militate against some candidate mechanisms related to the broader institutional and cultural context.

For this analysis, I focus on the subset of prime-age claimants who *exhausted* UI benefits under the old rules—and who were therefore immediately subject to the new benefit level on January 1, 2005. Using the SIAB, I select new short-term UI claims that began between January 1994 and June 2004 and culminated in benefit exhaustion (with no intervening return to work) by June 30, 2004. By restricting attention to claims

[37] A leading estimate of the local GE effects of UI generosity comes from Lalive et al. (2015), who study an Austrian policy that extended UI benefits for a subset of workers within select counties over 1988–1993. Using both individual and local variation in UI benefit duration, they find that the "macro elasticity" of job-finding with respect to changes in UI is about 20 percent smaller than the "micro elasticity". If the same is true here, my aggregate impacts should be multiplied by four-fifths.

exhausted before Hartz IV became salient, I sidestep concerns that news of the reform might impact selection into this sample. For these "UI exhaustees", I observe realized long-term benefit receipt and—conditional on receipt—long-term benefit levels net of means testing. These features enable me to compare changes in job-finding rates (i) between exhaustees who subsequently received long-term benefits and exhaustees who did not, and (ii) between benefit recipients subject to more severe vs. less severe benefit cuts.

For each exhaustee, I create an indicator LTB_i for receiving long-term unemployment assistance within two days of benefit exhaustion.[38] I then estimate complementary log-log models of job-finding at quarterly frequency over 1999–2006.[39] I model the instantaneous log hazard rate as

$$\mathbf{x}'_{id}\beta = \alpha_d + \gamma_t + \mathbf{z}'_{id}\phi + \theta_t \text{LTB}_i, \tag{7.1}$$

where α_d is a set of dummies for quarters since UI exhaustion, γ_t is a full set of quarter $\times$ year interactions, and $\mathbf{z}_{id}$ controls for demographic characteristics (sex $\times$ duration, region $\times$ duration, age bins, nationality, and household structure). The coefficients of interest, θ_t, capture differences in job-finding rates between long-term benefit recipients and non-recipients. Short-term UI exhaustees who did not receive long-term benefits under the old regime—either because they were ineligible or because they chose not to apply—are unlikely to be impacted by the Hartz IV benefit cuts. But since most of them remain registered as jobseekers at the unemployment office, they may still respond to changes in job-placement services or changes in cultural norms about long-term joblessness. Comparing the pre/post-Hartz IV evolution of job-finding rates between long-term benefit recipients and non-recipients can therefore help purge any channels through which Hartz IV impacted unemployed jobseekers generally, independently of reductions in benefit generosity.

The left panel of Figure 10 plots $\hat{\theta}_t$. Until the summer of 2004, long-term benefit recipients and non-recipients transition back to work at similar rates; if anything, relative job-finding rates among recipients are *falling* during this period. That changes sharply in the last quarter of 2004: recipients exhibit a dramatic increase in relative job-finding, which remains elevated at least through the end of 2006. The timing of this response closely matches that of Hartz IV announcement and implementation, providing further evidence that my estimated effects do not actually stem from earlier waves of the reform package.

This divergence in job-finding rates suggests an increase in the intensity or efficacy of job search among workers relying on long-term benefits, net of any factors common to all UI exhaustees regardless of subsequent benefit status. Lower benefit levels, stronger sanctions, and heightened stigma towards long-term

[38] LTB_i is fixed at baseline, regardless of subsequent changes in benefit status. Among exhaustees, 73.9 percent transition to long-term benefits 1–2 days after exhaustion. Of those who do not, just 6.4 [15.8] percent transition within 30 [180] days of exhaustion, suggesting that most non-receipt reflects ineligibility or non-take-up rather than application or processing delays.

[39] I start this analysis in 1999 (retaining left-censored spells) so that the earliest time effects and time $\times$ LTB_i interactions are not identified by only a small set of new exhaustees. Because the sample becomes thin at long durations and at later calendar dates, I censor incomplete spells 60 months after entry to long-term benefits or at the end of 2006, whichever comes first.

transfer receipt could all contribute to this pattern. By contrast, I have partialed out any changes in job-placement services available to all exhaustees, as well as any broader stigmatization of long-term joblessness per se.[40] The persistence of these effects also weighs against a story wherein media hype led UI claimants to respond proactively to benefit cuts that turned out to be modest in the end: any uncertainty about the depth of the cuts would have been resolved early in 2005, but pre-reform beneficiaries continued to find jobs at excess rates thereafter. The results from this comparison also have an important policy implication: that Hartz IV not only reduced *new inflows* into long-term assistance, but also boosted job-finding among the *existing stock* of long-term benefit recipients—perhaps its principal policy objective.

If benefit cuts are responsible for the effects of Hartz IV, then workers facing harsher cuts should exhibit stronger behavioral responses. Because the law replaced a wage-indexed benefit with a uniform benefit unrelated to pre-UI earnings, claimants with generous benefits under the old rules—owing largely to greater prior earnings—typically received steeper benefit cuts under the new rules. To exploit this variation, I restrict my exhaustee sample to long-term benefit recipients ($\text{LTB}_i = 1$), then divide these recipients into terciles $B_i \in \{1, 2, 3\}$ of initial net long-term benefit level, stratifying by sex, region, household type, and exhaustion year. I then estimate a variant of Equation 7.1, with the log hazard modeled as

$$\mathbf{x}_{id}'\beta = \alpha_d + \gamma_t + \mathbf{z}_{id}'\phi + \sum_{b \in \{2,3\}} \theta_t^b \mathbf{1}\{B_i = b\}. \tag{7.2}$$

Here, θ_t^2 and θ_t^3 capture the evolution of job-finding rates among middle- and top-tercile recipients, relative to bottom-tercile recipients. All other explanatory variables are specified as before.

The right panel of Figure 10 plots θ_t^2 and θ_t^3. Conditional on other observables, job-finding rates were quite similar across terciles from 1999 through early 2004. Beginning at the end of 2004, however, there is a clear divergence: consistent with the fact that cuts were steeper for those with generous pre-reform benefits, the second and (especially) third terciles show sharp job-finding gains relative to the first. As with the contrast between recipients and non-recipients, these stark time patterns establish a tight temporal link between the post-Hartz IV shift in job-finding dynamics and concurrent changes in UI generosity. More to the point, the fact that workers responded monotonically to the severity of the benefit cuts offers strong direct evidence for the benefit-cut mechanism.[41]

[40] Under a little-noted provision of Hartz IV, local employment offices had to remit a lump-sum payment to the central government for each short-term UI claimant who went on to claim long-term benefits. This created an institutional incentive for local agencies to spur job-finding prior to benefit exhaustion through sanctions or other means. But claimants who exhausted short-term benefits before the last quarter of 2004 were exempt from this financing scheme. The evidence in Figure 10 shows that Hartz IV significantly boosted job-finding rates among claimants for whom these institutional incentives were absent.

[41] A complementary exercise is to ask whether, among new UI claimants, groups that faced especially severe benefit cuts were especially responsive to Hartz IV. In Online Appendix Figure 6, I estimate my benchmark hazard model separately for 36 cells defined by sex, region, household type, and short-term benefit level. Across cells, the change in job-finding in the month Hartz IV binds is negatively correlated with simulated changes in long-term cash benefits (Equation 2.2), with $\rho = -.36$ overall, $-.25$ for men, and $-.49$ for women. I find similar correlations using simulated changes in net household income (Equation 2.3).

8 Effects on Reemployment Wages

I have shown that Hartz IV spurred UI claimants to find jobs faster. But how did it affect the wages they receive on those jobs? An oft-cited rationale for UI is that, in addition to smoothing consumption, it enables jobseekers to prolong their job searches and thereby obtain higher-paying positions. But cutting long-term benefits has theoretically ambiguous effects on post-UI wages: benefit cuts can lower wages by depressing reservation wages or weakening workers' bargaining power, but they can also raise wages by shortening jobless spells that erode earnings capacity (Schmieder et al., 2016; Nekoei and Weber, 2017). Identification of wage effects is further complicated by the fact that wages are only observed for those who eventually become reemployed (Heckman, 1979; Ham and Lalonde, 1996). This section develops an empirical framework for quantifying and decomposing the net wage impact, accounting for selection.

8.1 Accepted wages in the lead-up to benefit drops

Search theory predicts that workers who face looming benefit cuts will have lower reservation wages—and thus accept lower-wage jobs on average—than workers for whom benefit cuts lie far in the future. To detect such behavioral responses, I begin by estimating the effects of benefit cuts on the wage paid in a worker's first regular job after UI, *conditional on her completed jobless duration*. To motivate this analysis, Figure 11 plots the mean difference in log monthly earnings before and after UI as a function of claimants' completed jobless durations. Whereas earnings fall only slightly in the wake of brief jobless spells, spells lasting over a year are associated with wage declines on the order of 20 to 25 percent, with especially sharp drops just after modal short-term exhaustion points. I show that Hartz IV has contributed to this descriptive pattern by deepening the post-exhaustion dip in accepted wages. Section 8.2 will combine these conditional effects with changes in the timing of reemployment to evaluate the unconditional wage impact of Hartz IV.

Formally, let w_{id} denote the log ratio of reemployment wages to pre-UI wages for a worker reemployed d months into her UI spell. By analogy with the benchmark hazard specification laid out in Section 5.2, I run OLS regressions of log wage ratios on functions of benefit status at the moment of hiring:

$$w_{id} = \alpha_d + \gamma_t + \mathbf{z}'_{id}\phi + \sum_{k=-9}^{4} \delta_k^E \mathbf{1}\{\tau_{id}^E = k\} + \sum_{k=-9}^{4} \delta_k^H \mathbf{1}\{\tau_{id}^H = k\} + \varepsilon_{id} \tag{8.1}$$

The explanatory variables are identical to those used in the hazard model. The duration dummies α_d allow wages to vary with completed jobless duration per se, as a result of either structural duration dependence or dynamic selection. The time effects γ_t control for secular and seasonal shifts in wage offers. The event-time coefficients δ_k^E and δ_k^H capture how wages evolve in a window around each benefit drop.[42] The logic of this

[42] The vector of demographics $\mathbf{z}_{id}$ again includes (among other controls) indicator variables for each decile of pre-UI wages,

exercise is to compare post-UI wage recovery among similar workers who took equally long to find new jobs, and did so concurrently, but who were differentially exposed to Hartz IV at the time they found work.

Figure 12 plots estimated changes in newly accepted wages as workers approach (and then pass) the two step-downs in the benefit schedule. The left panel shows the main effect of short-term benefit exhaustion. Workers exhibit a gradual deterioration of reemployment wages as they approach benefit exhaustion, with sharper declines as benefits run out. All else equal, those who accept jobs in the month after exhaustion, when the effect is largest, receive wages 7.2 percent lower than those reemployed 10 or more months before exhaustion. These negative effects—identified by cross-sectional variation in potential benefit durations among observationally similar claimants with the same ex post jobless duration—suggest that reservation wages fall as workers approach benefit drops.[43]

The right panel shows the *incremental* impact of benefit cuts induced by Hartz IV. Wages start to decline about six months before the new rules bind, with effects peaking at 7.7 percent for workers taking jobs in the month after the benefit cut. This pattern of falling wages in advance of the benefit cut is indicative of anticipatory behavior on the part of jobseekers.[44] As with the hazard effects in Section 6, the total impact of benefit exhaustion under the new rules is obtained by combining the main and incremental effects (as $\exp(\hat{\delta}_1^E + \hat{\delta}_1^H) - 1$): for fully exposed claimants, jobs accepted just after benefit exhaustion are predicted to pay 14.3 percent lower wages than jobs accepted 10 or more months before benefits run out.[45]

Online Appendix Figure 7 explores the robustness of these estimates to control strategies analogous to those used for the earlier hazard specification. Adding additional controls—such as quarter-of-entry dummies to soak up unobserved heterogeneity across cohorts, interactions between the age/experience effects and an indicator for the post-reform period, and duration $\times$ calendar time interactions—attenuates the effects by up to about one-half, but I find negative impacts on accepted wages in all specifications.

A question at this point is whether these wage impacts represent dynamic selection—i.e., treatment-induced changes in the types of workers who are reemployed—rather than causal impacts of Hartz IV on individual wages. It is important to stress that Equation 8.1 already accounts for three distinct forms of selection. First, by comparing pre- and post-UI wages, I am netting out any time-invariant differences in

allowing post-UI wage recovery to vary flexibly throughout the distribution of prior wages. I again cluster by individual to allow for correlated errors between multiple UI spells experienced by the same individual.

[43] Schmieder et al. (2016) also find that workers reemployed in the month of short-term UI exhaustion accept lower wages (see their Figure 6). My results differ from Schmieder et al.'s in two ways: first, I find that wage responses begin several months prior to exhaustion; second, accepted wages remain depressed after exhaustion. My revealed-preference approach to inferring changes in reservation wages complements Le Barbanchon et al. (2019), who find that French workers' self-reported reservation wages (measured at job loss) do not respond to quasi-experimental changes in potential benefit duration.

[44] An alternative explanation relates to bargaining: even if reservation wages do not change, contracted wages could also decline as workers approach benefit cuts if there are quasi-rents to bargain over and if firms can identify which job applicants are on the verge of benefit drops. Given the complexity of the benefit calculation—which requires precise information on the exact timing of prior work and UI spells—this story would place a heavy information burden on potential employers.

[45] Estimating this specification separately by sex yields similar results for men and women (Price, 2017). The main effect of benefit exhaustion is somewhat stronger among women, but the incremental effect of Hartz IV is very similar for both groups.

earnings levels across workers. Second, by conditioning on realized jobless durations, I am also netting out compositional changes in the pool of the unemployed at each duration. Third, by controlling for sex, age, and other observables, I am allowing different groups of workers to experience differential wage recovery after job loss. Nonetheless, one might still worry that the workers who find jobs in the face of Hartz IV exhibit earnings dynamics that are *unobservably* different from those of other workers. While it is impossible to rule out arbitrary forms of dynamic selection, I can exploit the richness of the IAB data to test—and control—for time-invariant unobserved heterogeneity in post-UI wage recovery. In Online Appendix E.2, I use repeat spells to show that allowing for such heterogeneity does not qualitatively alter my estimates.

Overall, a balanced reading of the evidence is that Hartz IV lowered reemployment wages by about 4 to 8 percent for workers accepting jobs in the immediate aftermath of benefit cuts. These effects—which are consistent with workers becoming less choosy about job offers as their outside options deteriorate—augment the "drop at exhaustion" already evident in wage offers accepted prior to the reform.[46]

Importantly, these 4–8 percent *conditional* wage impacts give only a partial view of the overall wage effect. First, many workers find jobs well before the Hartz IV cuts bind (and thus before their reservation wages have responded to the threat of cuts). Second, Hartz IV shifts the distribution of realized jobless durations to the left. For both of these reasons, we would expect a smaller (or even positive) *unconditional* impact on individual wages. I now combine my hazard and wage models to translate these point-in-time wage impacts into average effects on the wage paid in a Hartz-exposed worker's first post-UI job.

8.2 Quantifying and decomposing the net wage effect

Following Schmieder et al. (2016), we can distinguish *changes in the path* of reemployment wages from *shifts along the path*. Let D_i denote claimant i's realized jobless duration, let $p_{id} \equiv \Pr(D_i = d)$ be the associated pdf, let w_i denote the realized log wage (relative to the pre-UI wage), and let $\mu_{id} \equiv \mathbb{E}(w_i \mid D_i = d)$ denote a claimant's expected wage realization conditional on being reemployed d months after entering UI. Finally, let $F_{id} \equiv \sum_{s=1}^{d} p_{is}$ denote the probability that a worker is reemployed by horizon d. I take each of these variables to be conditioned on $\mathbf{x}_i$, the full vector of explanatory variables used in the hazard and wage equations. Iterating expectations, a worker's expected wage (conditional on reemployment) equals

$$\mathbb{E}(w_i \mid \mathbf{x}_i, D_i \leq 24) = \sum_{d=1}^{24} q_{id}\mu_{id}, \tag{8.2}$$

[46] To provide auxiliary evidence that Hartz-exposed jobseekers are less selective about job offers, Online Appendix E.3 examines recalls of UI claimants to their previous employers. Distinguishing recalls from transitions to new employers, I find robust increases in both competing risks. Insofar as recall is "a process not requiring search" (Katz and Meyer, 1990b)—because workers are contacted by their former employer and simply exercise or decline the option to return—this rise in the recall hazard in response to benefit cuts confirms a reduction in workers' perceived continuation value of remaining unemployed.

where $q_{id} \equiv \Pr(D_i = d \mid D_i \leq 24) = \frac{p_{id}}{F_{i,24}}$ is the conditional pdf of jobless duration. Averaging across workers, the mean wage among those reemployed within 24 months is

$$\mathbb{E}(w_i \mid D_i \leq 24) = \sum_{i=1}^{N} \pi_i \sum_{d=1}^{24} q_{id}\mu_{id}, \tag{8.3}$$

where $\pi_i \equiv \frac{F_{i,24}}{\sum_{j=1}^{N} F_{j,24}}$ is worker i's (probabilistic) share of the reemployed pool. Labeling counterfactual values "cf", the effect of Hartz IV on the mean observed reemployment wage is

$$\Delta\mathbb{E}(w_i \mid D_i \leq 24) \equiv \sum_{i=1}^{N} \pi_i \sum_{d=1}^{24} q_{id}\mu_{id} - \sum_{i=1}^{N} \pi_i^{cf} \sum_{d=1}^{24} q_{id}^{cf}\mu_{id}^{cf} \tag{8.4}$$

There are two problems with interpreting Equation 8.4 as the causal effect of Hartz IV on individual workers' post-UI wages. The first is a compositional bias: Hartz IV changes the set of workers who are reemployed within 24 months, so that workers are given different weights in the two terms above ($\pi_i \neq \pi_i^{cf}$). The second is a more subtle form of selection that would arise even if workers were identical. Heuristically, suppose that workers can experience "short" jobless spells (up to 12 months), "long" spells (13–24 months), and "censored" spells (over 24 months). My estimates imply that Hartz IV shifted the distribution of jobless durations to the left, so that some long spells become short and some censored spells become uncensored (either short or long). Under Hartz IV, therefore, we observe wage realizations for a set of spells—many of them "long"—that would otherwise have gone unobserved. Since long spells are associated with less post-UI wage recovery, this second effect would likely impart a negative bias to estimates of the net wage impact.

In Online Appendix G, I develop a selection correction that addresses both of these issues. The basic idea is to define a *truncated* distribution of jobless durations that traces out each worker's reemployment trajectory under Hartz IV, then discards the "excess" probability mass that would have been allocated past the censoring horizon in the absence of Hartz IV. Employing this correction, I estimate the quantity

$$\Delta\widetilde{\mathbb{E}}(w_i \mid D_i \leq 24) \equiv \sum_{i=1}^{N} \pi_i^{cf} \left(\sum_{d=1}^{24} \widetilde{q}_{id}\mu_{id} - \sum_{d=1}^{24} q_{id}^{cf}\mu_{id}^{cf} \right) \tag{8.5}$$

where $\widetilde{q}_{id}$ is the conditional pdf of jobless durations under Hartz IV using the truncated distribution. This expression can be interpreted as the effect of Hartz IV on post-UI wages in cases where, probabilistically, a worker would have been reemployed within 24 months even if Hartz IV had not been enacted.

I decompose this net effect as

$$\Delta\widetilde{\mathbb{E}}(w_i \mid D_i \leq 24) = \underbrace{\sum_{i=1}^{N} \pi_i^{cf} \sum_{d=1}^{24} q_{id}^{cf}(\mu_{id} - \mu_{id}^{cf})}_{\text{selectivity effect}} + \underbrace{\sum_{i=1}^{N} \pi_i^{cf} \sum_{d=1}^{24} (\widetilde{q}_{id} - q_{id}^{cf})\mu_{id}}_{\text{time-out-of-work effect}} \tag{8.6}$$

Holding jobless durations constant, a benefit cut may affect reemployment wages by changing mean accepted wages μ_{id} at each possible duration. I call this the *selectivity effect*. Holding μ_{id} constant, a benefit cut may affect reemployment wages by shortening jobless durations, so that workers tend to be reemployed at earlier durations when accepted wages are higher. I call this the *time-out-of-work effect*.

To calculate these expressions, I jointly estimate my hazard and wage specifications by maximum likelihood (as in, for example, Caliendo et al., 2013), then use the fitted model to predict $\hat{\lambda}_{id}$ and $\hat{\mu}_{id}$, as well as counterfactual analogues that set the Hartz IV event-time variable τ_{id}^{H} to its omitted value.[47] Using the identity $p_{id} \equiv \lambda_{id} \prod_{s=1}^{d-1}(1-\lambda_{is})$, I can then compute each of the terms in Equations 8.5 and 8.6.

Table 3 reports estimates of the overall, selectivity, and time-out-of-work effects. In column 1, which uses my benchmark hazard and wage models, I estimate that exposure to Hartz IV reduced the mean initial reemployment wage by 1.1 percent. This modest negative impact masks offsetting selectivity and time-out-of-work effects: holding jobless durations fixed at their counterfactual distribution, the downward shift in the path of accepted wages accounts for a 2.2 percent reduction in the mean reemployment wage, but the shift towards shorter jobless spells yields a countervailing 1.0 percent wage gain.[48] At the 95 percent level, I am able to rule out a net negative wage impact exceeding −1.6 percent.[49]

Column 2 presents estimates from a more flexible specification that adds interactions between 3-month duration bins and quarter × year time dummies to both the hazard and wage equations. This specification yields a slight *positive* impact, reflecting the fact that adding duration × time interactions augments the hazard effects while attenuating the conditional wage impacts (see Figure 7 and Online Appendix Figure 7). In this specification, I can rule out a net wage gain greater than 1.6 percent.

While the sign of the overall wage effect varies across specifications, it is consistently small in magnitude. My analysis suggests that the substantial employment gains found in Section 6 are not accompanied by any substantial degradation in job quality, as measured by initial wages.

[47] Concretely, I specify the reemployment hazard λ_{id} as in the benchmark specification estimated in Section 6.2. I specify the log wage as $w_{id} = \mu_{id} + \varepsilon_{id}$, where μ_{id} is the fitted value of wages in Equation 8.1 and where I assume $\varepsilon_{id} \sim N(0, \sigma_{\varepsilon}^2)$. Combining the hazard and wage terms, the likelihood function for claimant i is

$$\mathcal{L}_i(\theta) = \begin{cases} \lambda_{iD_i} \prod_{s=1}^{D_i-1}(1-\lambda_{is}) \frac{1}{\sqrt{2\pi\sigma_{\varepsilon}^2}} \exp\left(-\frac{(w_i-\mu_{iD_i})^2}{2\sigma_{\varepsilon}^2}\right) & \text{if } D_i \leq 24 \\ \prod_{s=1}^{24}(1-\lambda_{is}) & \text{if } D_i > 24 \end{cases} \tag{8.7}$$

The first case represents workers reemployed D_i months after entering reemployment (so that $w_i \equiv w_{iD_i}$ by construction). The second case represents workers for whom I don't observe a post-UI job within 24 months.

[48] As with any Oaxaca-Blinder expression, the effect attributed to each channel depends on the order in which the terms are decomposed. The bottom panel of Table 3 shows that I obtain similar results using the alternative decomposition

$$\Delta\widetilde{\mathbb{E}}(w_i \mid D_i \leq 24) = \sum_{i=1}^{N} \pi_i^{cf} \sum_{d=1}^{24} \widetilde{q}_{id}(\mu_{id} - \mu_{id}^{cf}) + \sum_{i=1}^{N} \pi_i^{cf} \sum_{d=1}^{24} (\widetilde{q}_{id} - q_{id}^{cf})\mu_{id}^{cf}.$$

[49] Note that the selectivity effect, as expressed in Equation 8.6, does not depend on $\widetilde{q}_{id}$, the selection-corrected conditional pdf. Assuming that the time-out-of-work effect is weakly positive (as both theory and my estimates suggest), the selectivity effect itself provides a lower bound on the net wage impact that does not rely on my chosen selection correction. This more conservative lower bound lets me rule out negative wage impacts larger than −2.7 percent at the 95 percent level.

9 What Kind of Jobs?

The Hartz reforms coincided with—and may have contributed to—a marked rise in the prevalence of part-time and non-traditional work within the German labor market.[50] To explore how long-term benefit cuts affect employment along the intensive margin of labor supply, this section presents competing-risks specifications that track how UI claimants transition into different kinds of jobs. Partitioning regular jobs into full-time and (non-mini) part-time jobs, I find that net employment gains are mostly driven by full-time jobs, with little change in the part-time share. Broadening the employment concept to encompass low-paid mini-jobs often held during UI receipt, I find that Hartz IV *diverted* claimants from mini-jobs.

9.1 Full-time vs. part-time

In Section 8, I concluded that long-term benefit cuts have at most a slight effect on claimants' initial monthly earnings upon returning to regular work. While I have interpreted this result as reflecting falling reservation wages offset by shorter jobless durations, the relative stability of post-UI earnings may mask countervailing shifts in hourly wages and *hours worked*. Although IAB data do not report hours, I can track shifts into or out of part-time work to gauge the likelihood of meaningful shifts in hours worked more generally.

Formally, I adapt my hazard model to allow for competing risks of accepting a full-time or part-time job. To mirror the wage analysis, I again restrict attention to regular jobs: although some part-time jobs are legally classified as mini-jobs, many others are covered by social insurance and hence fall within the employment concept used in earlier sections. I treat each job type as an absorbing state, abstracting from subsequent transitions between job types. I estimate a separate discrete-time hazard specification for each job type, censoring spells if and when a worker is reemployed into the other kind of job. I use the same explanatory variables as in my benchmark (single-risk) hazard specification.

The left panel of Figure 13 plots the estimated effect of reform-induced long-term benefit cuts on the competing risks of finding full-time or part-time jobs. Hartz IV has similar, positive proportional effects on both hazards. But cause-specific hazards are hard to interpret without the strong assumption that the risks are mutually independent (Heckman and Honoré, 1989). In addition, to assess how shifts between full-time and part-time jobs impact estimated changes in post-UI wages, what matters are not the hazard rates, but rather how these hazards translate into the share of workers who end up in each state.

[50] Between 2001 and 2007, part-time jobs grew from 25.6 to 30.5 percent of aggregate employment (own calculation, public-use statistics published by the German Employment Agency), reflecting roughly equal growth in mini-jobs and in other part-time jobs (Online Appendix Figure 10). This statistic excludes mini-jobs held as secondary jobs, which first appear in these data in April 2003 and also grew rapidly in subsequent years.

I therefore use the fitted model to estimate cumulative incidence functions, which measure the share of workers absorbed into each job type at each duration since baseline (Fine and Gray, 1999).[51] Let D_i denote completed jobless duration, and let $J_i \in \{\text{full-time}, \text{part-time}, \text{no job}\}$ denote the first type of job obtained (with $J_i = \text{no job}$ if $D_i = \infty$). Then the cumulative incidence for job type j at duration d is

$$I_{id}^{j} \equiv P(D_i \leq d \cap J_i = j). \tag{9.1}$$

The overall reemployment rate F_{id} can be expressed as a sum of cumulative incidences:

$$F_{id} \equiv I_{id}^{\text{full}} + I_{id}^{\text{part}} \tag{9.2}$$

This identity allows me to decompose reemployment rates—or, more to the point, the change in reemployment induced by Hartz IV—into full-time and part-time components. Adapting the procedure used in Section 6.5, I obtain predicted and counterfactual cumulative incidence functions for each UI claim. I compute the gap between these functions at durations $d \in \{1, 2, \dots, 24\}$, and I average this gap across all UI claims begun in 2005 as a measure of how Hartz IV affected workers fully exposed to the new benefit schedule.

The gap is plotted in the right panel of Figure 13. Net employment gains predominantly represent full-time jobs—a sensible result given the cross-sectional fact that most regular jobs in my sample are full-time. My estimates imply that the part-time share of employment among fully exposed UI entrants was unaffected by Hartz IV, remaining essentially constant at 12.5 percent. Though I am unable to rule out shifts in hours *within* the part-time and full-time job classes, the available evidence suggests that the net wage impacts estimated in Section 8.2 do not mask substantial intensive changes in labor supply.

9.2 Regular jobs vs. mini-jobs

Up to now, I have focused exclusively on jobs subject to social insurance contributions. This employment concept excludes "mini-jobs", a special class of low-paid, part-time jobs that are partly exempt from these contributions (Tazhitdinova, 2019; Gudgeon and Trenkle, 2019).[52] In June 2004, mini-jobs held as a worker's primary job accounted for 15.3 percent of aggregate employment (Online Appendix Figure 10). Critics of Hartz IV allege that it has fueled the growth of such marginal positions by driving the unemployed to accept

[51] Cumulative incidence is interpretable without assuming independent risks. To see why, consider an analogy to an RCT of a program that helps unemployed workers apply to different kinds of jobs. Under random assignment, the cumulative incidences—i.e., the effect of the treatment on reemployment into each job type—are identified by simple differences of means. But without further assumptions, the econometrician cannot tell how the treatment affected individual search intensity towards each job type, since observed changes in cause-specific hazards may partly reflect dynamic changes in the composition of the risk set.

[52] Workers in mini-jobs are exempt from paying social insurance and income taxes, but employers are still liable for their portion of social insurance contributions. Until 2003, mini-jobs were capped at €325 per month, with a maximum of 15 hours per week. In April 2003, Hartz II eliminated the hours ceiling, raised the earnings ceiling to €400 per month, and allowed workers in regular employment to hold a mini-job on the side without increasing their total tax liability.

any work they can find, potentially at the expense of job security and other amenities. A priori, however, long-term benefit cuts may either promote or deter transitions of UI recipients into mini-jobs. Thanks to the earnings disregard, UI receipt and mini-jobs are not mutually exclusive. The income loss from a benefit cut may induce some claimants to obtain mini-jobs in lieu of pure unemployment, but it also reduces the attractiveness of dual UI receipt/mini-job employment relative to seeking a socially insured job.[53]

I therefore broaden the definition of reemployment to put regular and mini-jobs on the same footing. To focus on transitions that occur after the onset of UI receipt, I drop the 6.5 percent of claimants who hold a mini-job on the date they enter UI. The left panel of Figure 14 plots the estimated effects of Hartz IV on the competing risks of entering regular jobs or mini-jobs, together with the effect on the single risk of entering a job of any kind whatsoever. The grey series shows that Hartz IV continues to have large, positive effects on job-finding rates when I expand the employment concept to include mini-jobs. The point estimates are noticeably smaller than the analogous estimates in Figure 6, however, providing a first hint that Hartz IV promotes transitions into regular jobs, rather than mini-jobs. The blue and red series support this interpretation: whereas transitions into regular jobs become more likely as workers approach reform-induced benefit cuts, transitions into mini-jobs become *less* likely.

The right panel of Figure 14 plots the implied effects on the cumulative incidence functions. Regular jobs more than account for the net employment gains. Perhaps surprisingly, I find that Hartz IV had a modestly *negative* causal effect on the share of workers drawn into mini-jobs. Though not a foregone conclusion, this result is quite plausible, since Hartz IV made it less attractive for workers to claim long-term benefits while keeping their earnings low to remain eligible for benefits.[54]

In wage specifications that put regular and mini-jobs on equal footing, I find that long-term benefit cuts actually *increase* earnings on the initial post-UI job by diverting workers from low-paid mini-jobs. This positive effect is unsurprising. Many mini-job holders continue to claim UI, and those who do so can supply only limited hours if they wish to remain below the earnings disregard. Insofar as mini-jobs act as an adjunct to UI for the population I study, my preferred employment concept—which excludes mini-jobs—should better capture how Hartz IV affected earnings potential upon reemployment. Taken together, my results suggest

[53] To illustrate, consider a static model in which workers choose among pure unemployment with UI benefit b, benefit receipt coupled with a mini-job at wage w_M, or a regular job at wage $w_R > b + w_M$. The corresponding payoffs are

$$\begin{aligned} u_1 &= u(b) \\ u_2 &= u(b + w_M) - c_M \\ u_3 &= u(w_R) - c_R \end{aligned}$$

where c_M and $c_R > c_M$ are the disutilities of marginal and regular work. A benefit cut reduces both u_1 and u_2 while leaving u_3 unchanged; moreover, provided that $u(\cdot)$ is concave, $\frac{d}{db}(u_1 - u_2) = u'(b) - u'(b + w_M) > 0$, so that lower benefits reduce the attractiveness of pure unemployment relative to the dual claim-work strategy. The result is that workers switch from strategy 1 to strategies 2 and 3 and from strategy 2 to strategy 3, so that the net effect on mini-jobs is unclear.

[54] Although Hartz IV appears to have acted as a brake on transitions from UI into mini-jobs, the broader package of Hartz reforms (particularly the mini-job reform of April 2003) may well have fostered marginal employment. My analysis also abstracts from any general equilibrium mechanisms through which benefit cuts may have altered aggregate job composition.

that Hartz IV induced workers to seek gainful employment in lieu of mini-jobs that are implicitly subsidized by UI, with at most a modest adverse impact on their initial post-UI wages.

10 Conclusion

Many countries offer long-term unemployment assistance for claimants who have exhausted their initial stream of unemployment benefits. Long-term benefits are especially relevant for workers at elevated risk of experiencing lengthy jobless spells—a group of special policy interest, since such spells may erode skills, discourage job search, and lead to permanent exit from the labor force. Despite the prevalence of these two-tiered UI systems, the labor market effects of long-term unemployment benefits are not well understood. Generous long-term benefits—which, in the case of pre-2005 Germany, could potentially replace over half of prior net earnings for an indefinite period of time—may lead to especially long jobless spells by disincentivizing job search, with attendant declines in earnings potential and heavy burdens on public finances. Conversely, they may provide the liquidity needed for displaced workers to engage in efficacious job search, especially when labor demand is slack.

This paper identifies the effects of long-term unemployment benefit cuts on individual employment, wages, and job characteristics by isolating within-cohort variation in the timing of exposure to those cuts. Using a large sample of UI claimants drawn from administrative records, I find that Germany's 2005 Hartz IV reform reduced the probability of experiencing a one-year jobless spell by 11.3 percent, with net employment gains concentrated in full-time jobs. Claimants are less likely to transition into low-paid "mini-jobs", but they receive lower wages in regular jobs, conditional on completed jobless duration. These direct wage losses—which I attribute to declines in reservation wages as workers approach benefit step-downs—are at least partly offset by wage gains due to shorter jobless spells. Though the sign varies across specifications, I am able to rule out, at the 95 percent level, net wage changes exceeding 1.6 percent in either direction. Wage effects in this range appear small relative to the sizable reductions in jobless durations.

Hartz IV was of major policy importance in its own right, and its impact on the German labor market has been heavily debated (e.g., Dustmann et al., 2014; Burda and Seele, 2016). This paper presents the first comprehensive, quasi-experimental evidence on Hartz IV's direct effects on unemployed workers, a key input into evaluating its overall effect. Taken at face value, estimated increases in individual job-finding can explain roughly a 0.8 percentage point decline in Germany's steady-state unemployment rate. Although my analysis differences out general equilibrium effects felt by all jobseekers, my partial equilibrium estimates may serve as a useful point of reference for papers assessing the market-wide impact of Hartz IV. Given the lack of academic consensus about Hartz IV's macroeconomic effects, credible estimates of its microeconomic effects can help inform debates about the reform's possible contributions to rising wage inequality (Dustmann et al., 2009), changes in equilibrium job composition (Gudgeon and Trenkle, 2019), and the "German employment miracle" of the late 2000s (Burda and Hunt, 2011).

Focusing solely on the steady-state impact of Hartz IV would overlook the population it most immediately affected: the 2.2 million workers already claiming long-term unemployment assistance in June 2004, who faced (often steep) benefit cuts overnight if still unemployed on New Year's Day. Germany's generous safety net was historically protected by a "reform bottleneck" that stymied efforts to bring long-term replacement rates in line with international practice (Jacobi and Kluve, 2007; Tompson, 2009). Given political gridlock, displaced workers—especially older workers—who entered UI under the pre-reform regime might reasonably have expected to claim generous, wage-indexed benefits until aging into retirement, effectively becoming labor market participants in name only. Hartz IV presented such claimants with a stark choice: either accept a lower consumption stream or return to the workforce after a long hiatus, under the shadow of stigma or skill depreciation. A closer look at how benefit cuts impacted these long-standing UI beneficiaries would be a fruitful direction for future work. At a time of depressed labor force participation and lackluster wage growth in the United States and abroad, further study of these "long-long-term unemployed" may yield fresh insights about how extended periods out of work affect human capital and earnings potential.

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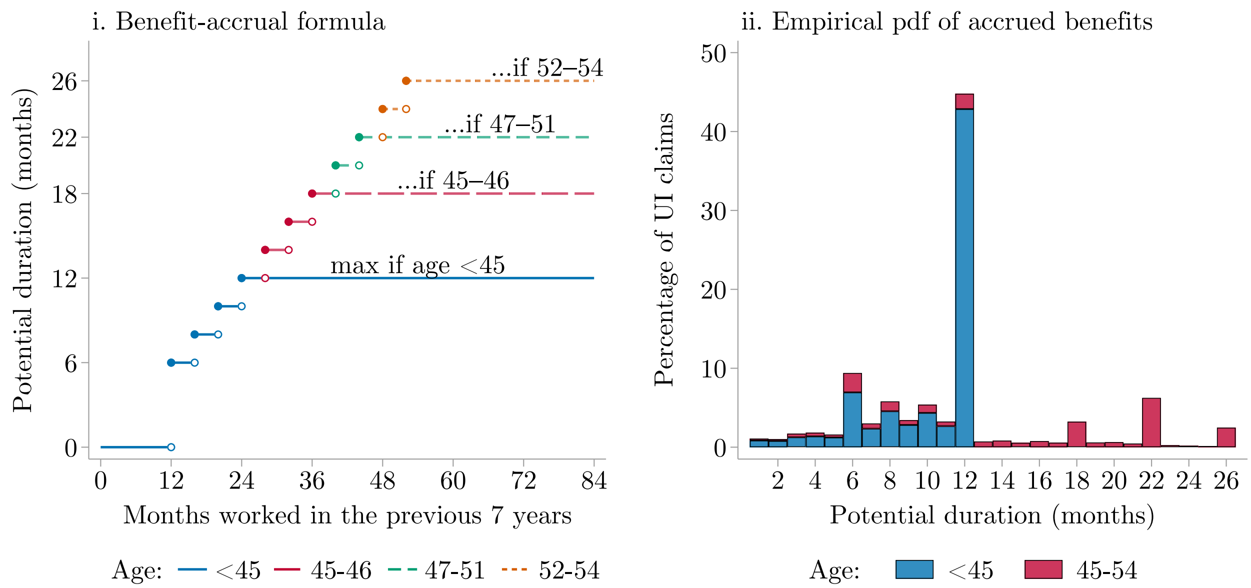
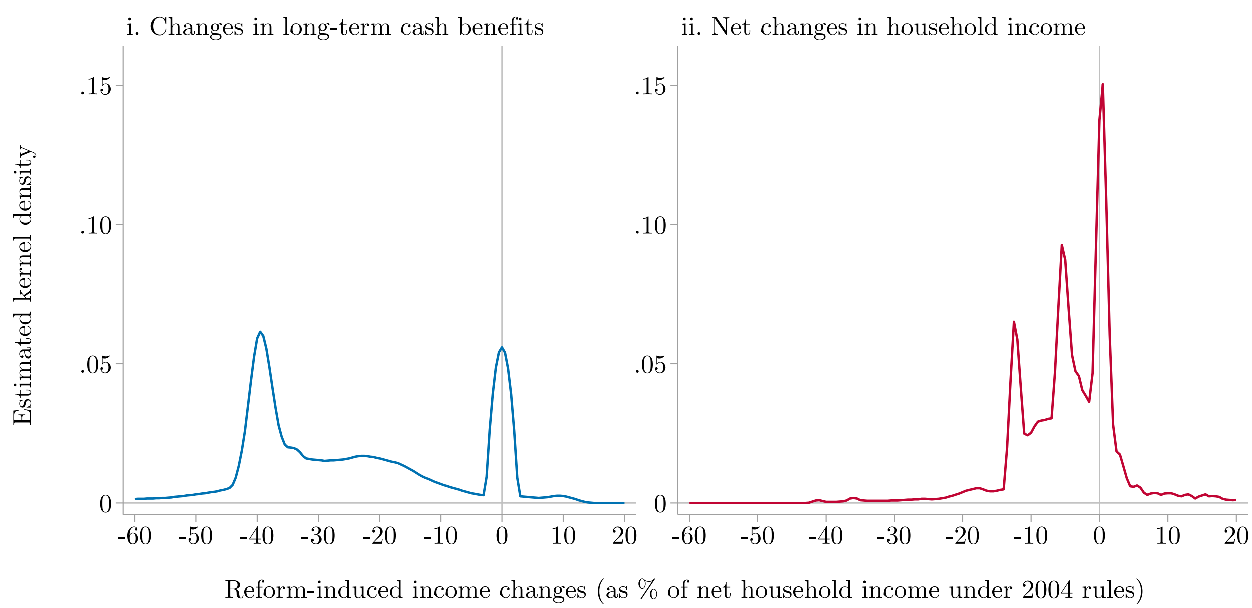
Figure 1: Potential short-term benefit duration for new UI claimants



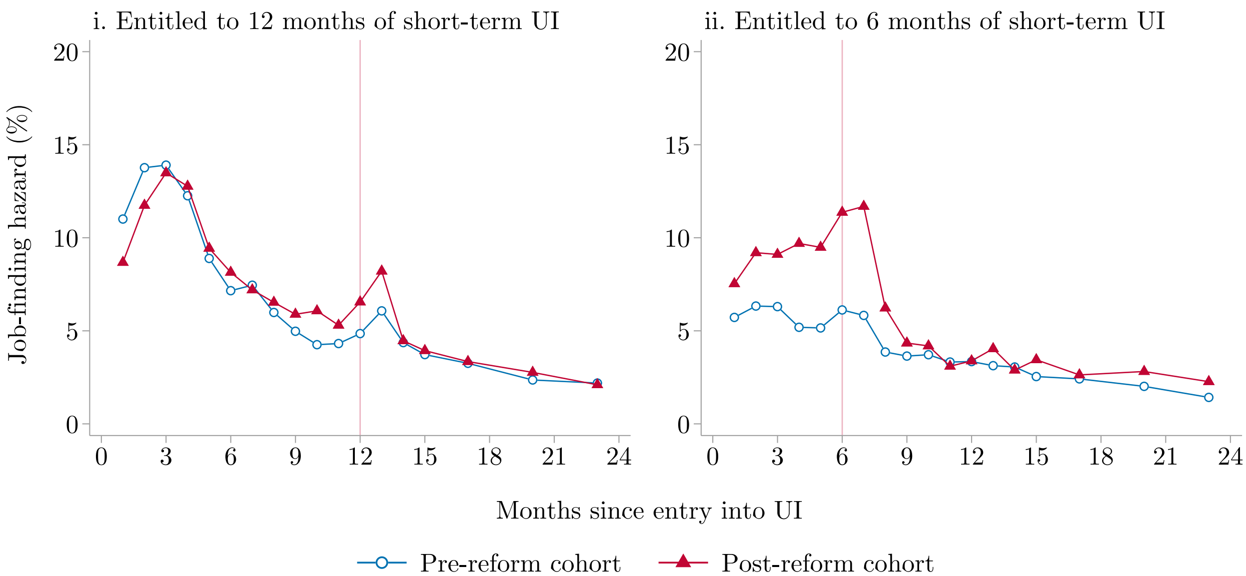
Notes: As shown in the left panel, potential short-term benefit duration is a step function of months worked over the past 7 years, up to a ceiling determined by age at entry into UI. The right panel shows the observed pdf of potential duration for claimants in my estimation sample (rounded to the nearest month). Although a standard UI entitlement lasts at least 6 months, seasonal workers can receive briefer entitlements. Since unused benefits are carried forward in the event of a subsequent job loss, potential duration (in days) can range from 1 day to the age-specific ceiling.

Figure 2: Simulated reform-induced changes in post-exhaustion household income



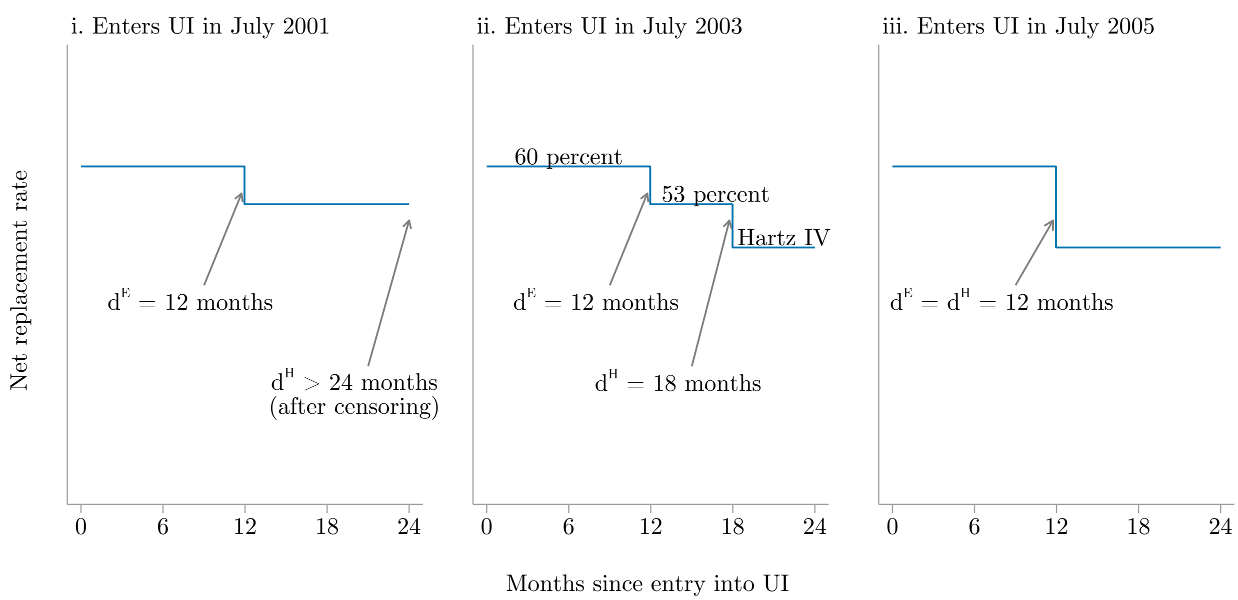
Notes: I adapt the OECD Tax-Benefit Model to simulate Germany's 2004 and 2005 tax-transfer systems. I then simulate household income, taxes, and transfers for each claimant in my estimation sample on the basis of observed and imputed claimant characteristics. The left panel shows smoothed simulated changes in long-term cash benefits (unemployment assistance + social assistance) under the 2005 vs. 2004 rules, denominated by net post-exhaustion household income under the 2004 rules (Equation 2.2). The right panel shows simulated changes in net household income, using the same denominator (Equation 2.3).

Figure 3: Empirical job-finding hazards by timing of UI entry × potential benefit duration



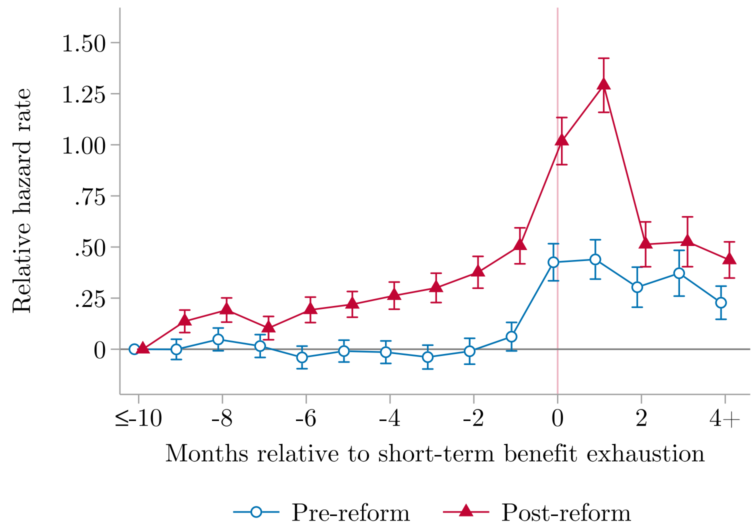
Notes: Raw monthly job-finding hazards among new claimants entering UI in 2001 ("pre-reform") or 2005 ("post-reform"). All pre-reform spells are completed or censored by December 2003, prior to final passage of Hartz IV, and all post-reform spells are immediately subject to the new long-term benefit level upon exhaustion of short-term benefits.

Figure 4: Timing of benefit step-downs for pre-reform, interim, and post-reform cohorts



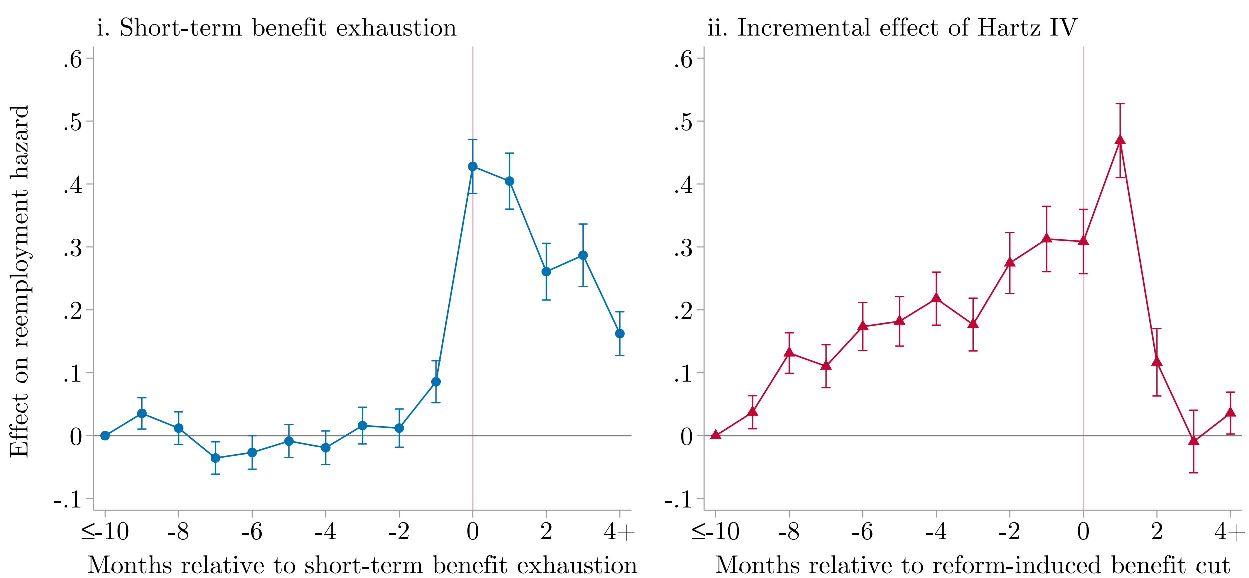
Notes: Hypothetical unemployment benefit schedules for successive cohorts of claimants entitled to 12 months of potential benefits. The fall in replacement rate from 60 percent to 53 percent represents a childless claimant who passes the means test for long-term benefits under the pre-reform rules. The magnitude of the subsequent, reform-induced benefit change depends on a complex set of household characteristics and programmatic rules and varies across individuals.

Figure 5: Relative job-finding hazards among pre- and post-reform cohorts



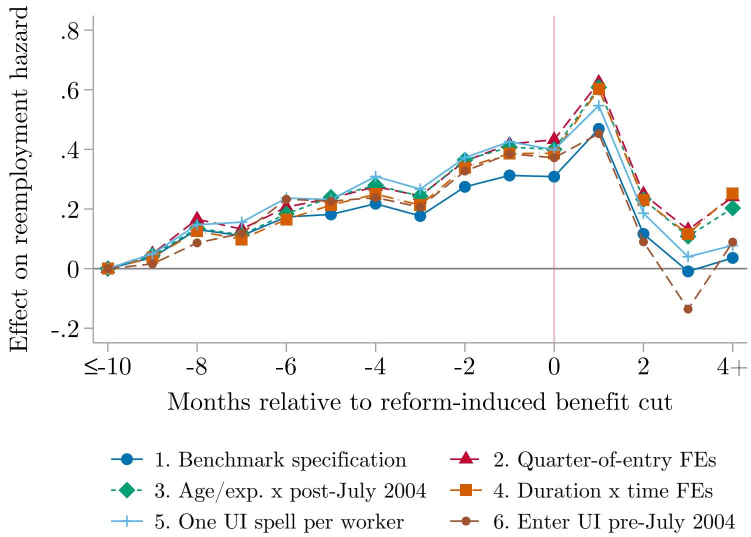
Notes: Estimated proportional effects of short-term benefit exhaustion on transitions to employment using the discrete-time hazard specification of Equation 6.1. Each series plots normalized hazard ratios corresponding to short-term benefit exhaustion for claimants entering UI in either 2001 (pre-reform) or 2005 (post-reform). Points are offset horizontally for visual clarity. Capped spikes denote 95 percent confidence intervals, clustered on individual.

Figure 6: Benchmark effects of benefit step-downs on the hazard rate of reemployment



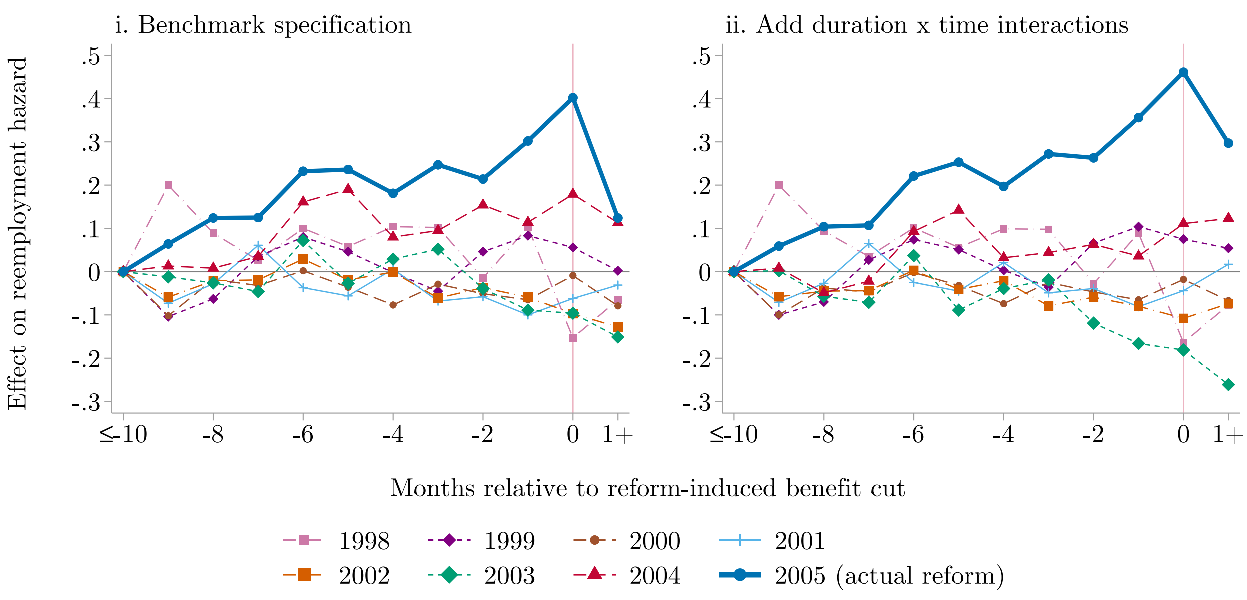
Notes: Estimated proportional effects of UI benefit changes on transitions to employment using the discrete-time hazard specification of Equations 5.4 and 5.5. The left panel reports normalized hazard ratios corresponding to the main effect of short-term benefit exhaustion ($\exp(\hat{\delta}_k^E) - 1$). The right panel reports the incremental hazard effect of Hartz IV ($\exp(\hat{\delta}_k^H) - 1$). To allow for duration dependence, aggregate time effects, and seasonality, the model includes a nonparametric baseline hazard (interacted with sex), quarter × year dummies, month dummies, and interactions between four quarter dummies and 3-month duration bins. I also include controls for East German residence, age, and work history, as well as interactions between these controls and 3-month duration bins. Finally, I include controls for pre-UI wages, education, German nationality, and household type. See text for details. Capped spikes denote 95 percent confidence intervals, clustered on individual.

Figure 7: Robustness of hazard effects to alternative specifications and sample selections



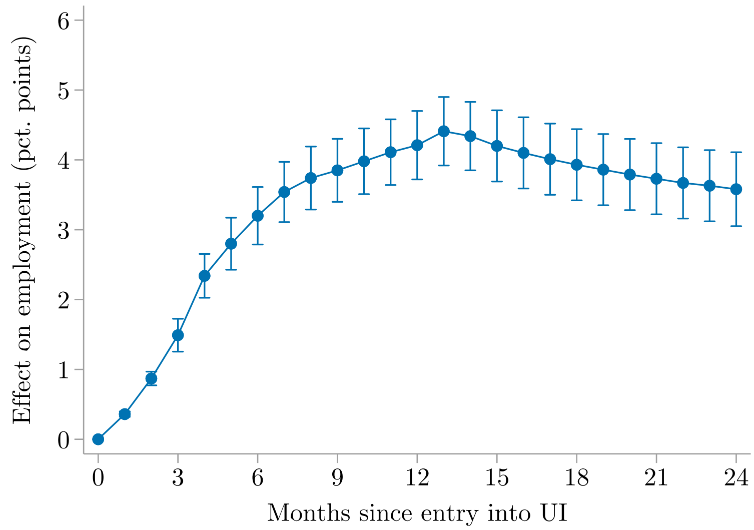
Notes: Incremental hazard effects of reform-induced benefit cuts. Specification 1 replicates the benchmark estimates from Figure 6. Specification 2 absorbs cohort heterogeneity by adding quarter-of-entry fixed effects. Specification 3 allows the effects of age and experience to differ before/after Hartz IV became salient. Specification 4 adds a flexible set of duration × time interactions, allowing the shape of the hazard function to vary freely over calendar time. Specifications 5 and 6 limit the sample as indicated. The same estimates are presented (with standard errors) in Online Appendix Table 1.

Figure 8: Hazard effects of real and placebo UI reforms



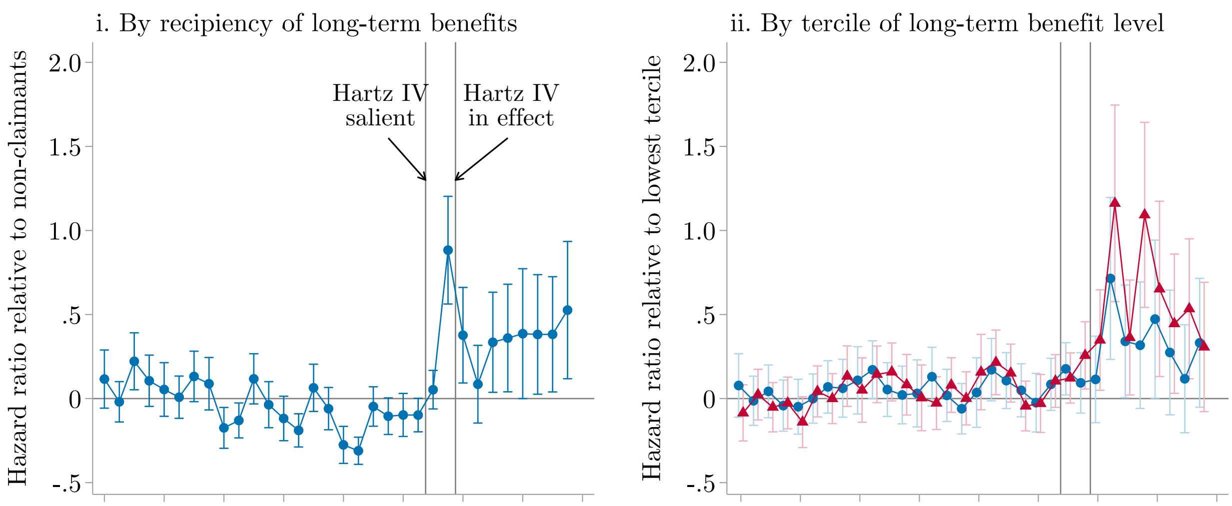
Notes: Estimated proportional hazard effects of real/placebo UI reforms, using either the actual reform date (January 1, 2005) or a placebo date (January 1 of 2001, 2002, 2003, or 2004). For each "reform" year Y, I construct a 2 percent sample of new UI claims begun between January 1 of year $Y-4$ and June 30 of year Y, then recode the time-to-Hartz event-time variable to measure time until the supposed reform binds. The right panel augments each specification with a full set of interactions between 3-month duration bins and quarter × year effects.

Figure 9: Path of implied reemployment effects for the fully exposed 2005 UI cohort



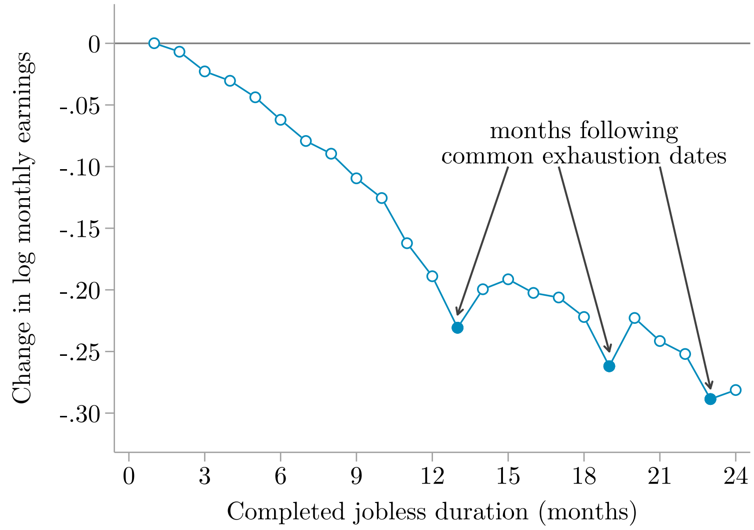
Notes: Implied effects of Hartz IV on individual reemployment rates. I use the benchmark estimates from Figure 6 to predict the probability that a claimant returns to work within 1–24 months of entering UI both under Hartz IV and under a counterfactual scenario where Hartz IV does not occur. I compute the mean gap between these predicted values for the 2005 cohort of UI entrants, who are fully exposed to the post-reform benefit schedule. Capped spikes denote 95 percent confidence intervals, based on 500 draws from the estimated variance-covariance matrix.

Figure 10: Relative job-finding rates among UI exhaustees, by proxies for exposure to benefit cuts



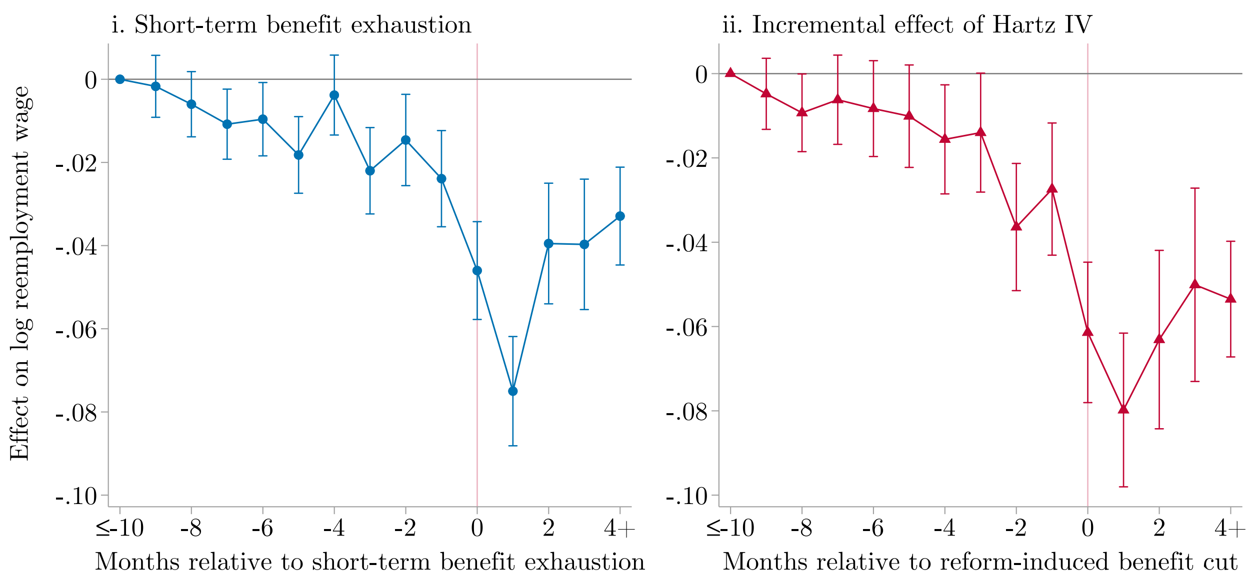
Notes: I construct a 2 percent sample of short-term UI claims that begin between January 1994 and June 2004 and end, via benefit exhaustion, no later than June 30, 2004. I code these UI exhaustees as long-term benefit recipients or non-recipients based on whether they transition to long-term benefits within two days of exhaustion. I further divide long-term recipients into terciles based on their initial long-term benefit level (net of means testing). I then estimate discrete-time hazard models of job-finding at quarterly frequency over 1999–2006 (see text for details). Left panel: estimated coefficients on benefit receipt × quarter × year interactions, showing how job-finding rates among recipients evolve over time relative to those of non-recipients. Right panel: estimated coefficients on tercile × quarter × year interactions, showing how job-finding rates among the top two benefit terciles evolve relative to those of the lowest tercile. Points are offset horizontally for visual clarity. Capped spikes denote 95 percent confidence intervals.

Figure 11: Pre-/post-UI wage changes as a function of completed jobless duration



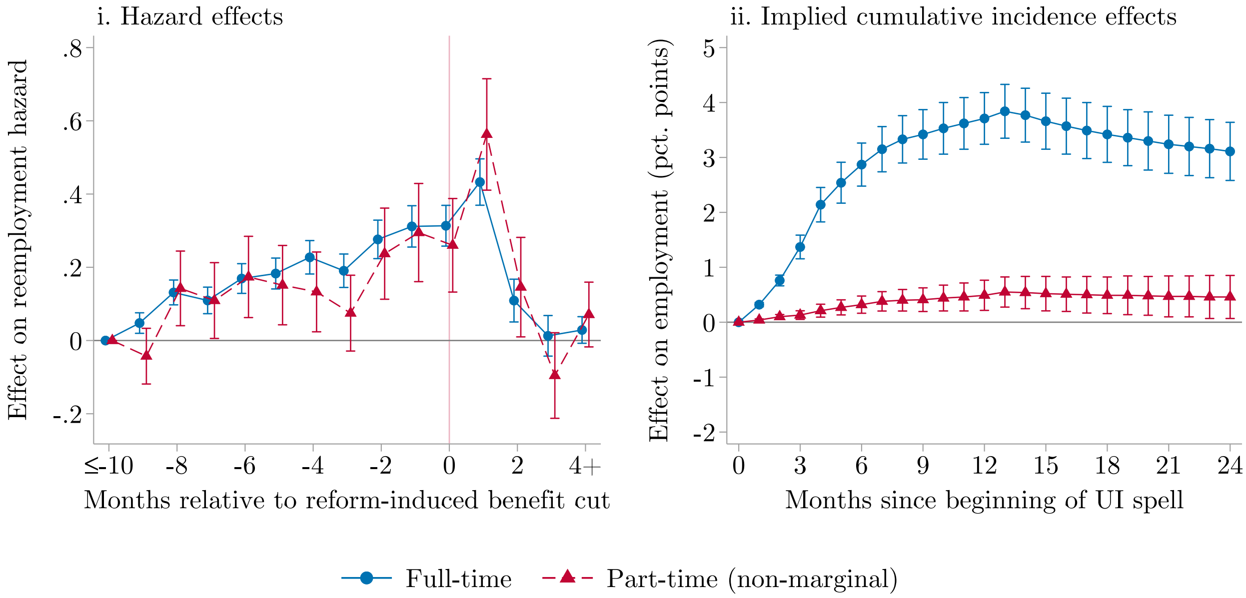
Notes: Mean pre-/post-UI wage changes experienced by claimants in my core estimation sample, binning workers by completed jobless duration. Each claimant's wage change is defined as the difference in log monthly earnings between the job held prior to UI entry and the first socially insured job obtained thereafter.

Figure 12: Benchmark effects of benefit step-downs on the log ratio of post-UI to pre-UI wages



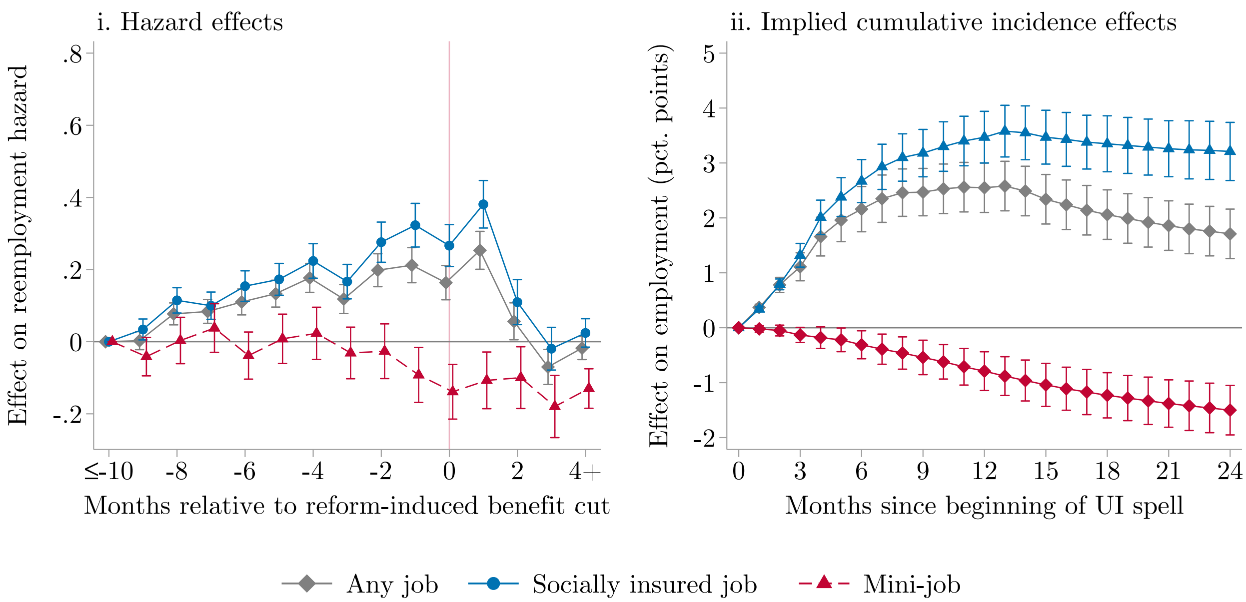
Notes: Estimated effects of short-term benefit exhaustion and reform-induced long-term benefit cuts on the log ratio of monthly earnings in the first socially insured job after UI to monthly earnings in the job that preceded entry to UI, from the OLS regression specified in Equation 8.1. The explanatory variables are the same as those described in the notes to Figure 6. Capped spikes denote 95 percent confidence intervals, clustering on individual.

Figure 13: Effects of long-term benefit cuts on competing risks of full-time vs. part-time jobs



Notes: Left panel: proportional effects of reform-induced benefit cuts on cause-specific hazard rates of transitioning into socially insured full-time vs. part-time jobs (see text for details). Right panel: implied effects of Hartz IV on the cumulative incidence of reemployment into each job type, as computed using UI claims initiated in 2005. Points are offset horizontally for visual clarity. Capped spikes denote 95 percent confidence intervals, clustering on individual.

Figure 14: Effects of long-term benefit cuts on competing risks of regular jobs vs. mini-jobs



Notes: Left panel: the grey series plots the proportional effects of reform-induced benefit cuts on the hazard rate of entry into a job of any kind, redefining employment to include "mini-jobs" alongside regular jobs. The blue and red series plot proportional effects of benefit cuts on cause-specific hazard rates of entry into regular vs. mini-jobs (see text for details). Right panel: implied effects on individual reemployment rates (grey) and cumulative incidence functions (blue/red) for the 2005 cohort. (Equation 9.2 holds only approximately in the estimated model, so the single-risk series does not exactly equal the sum of the cause-specific series.) Points are offset horizontally for visual clarity. Capped spikes denote 95 percent confidence intervals, clustering on individual.

Table 1: Summary statistics for the estimation sample and for a comparison group of employed workers

Baseline characteristics	Estimation sample	Comparison group
Basic demographics		
Female	37.7	43.8
Age	38.8	39.9
	(8.3)	(8.0)
East German resident	34.6	20.3
Non-German native	10.4	8.2
Education		
No vocational training, no university qual. exam	8.3	7.7
Vocational training and/or university qual. exam	77.4	76.4
University degree (incl. *Fachhochschulen*)	14.4	16.0
Household type		
Unmarried	48.1	*
Married without children	25.9	*
Married with children	26.0	*
Monthly wage prior to job loss (estimation sample) or at quarterly snapshots (comparison group), 2005 EUR	1860.9	2521.1
	(881.0)	(1179.4)
Initial monthly UI benefit, 2005 EUR	806.9	*
	(304.1)	
Employed 4+ of last 7 years	68.8	83.7
Claimant outcomes		
Reemployed into socially insured job within ...		
6 months	46.9	*
12 months	61.9	*
24 months	73.3	*
36 months	78.3	*
Monthly wage upon reemployment, 2005 EUR (among claimants reemployed within 24 months)	1776.1	*
	(819.7)	
Sample size		
Number of UI claims	336,634	*
Number of distinct individuals	244,666	*

Notes: The estimation sample consists of prime-age workers (ages 25–54) who initiate a UI claim during 2001–2005 within 30 days of being displaced from a socially insured job. The comparison group is a 2 percent sample of prime-age workers who are employed in socially insured jobs and not claiming UI, based on quarterly snapshots reweighted to match the temporal distribution of the estimation sample. Household structure is reported in the unemployment register, not in the employment records, and hence cannot be constructed for the comparison group. Values are percentages except where indicated.

Table 2: Effect of long-term benefit cuts on reemployment rates by year of entry into UI

	Year of UI entry	Month	% reemp. w/o reform	% reemp. w/reform	Effect of Hartz IV
de facto unexposed	2001	6	47.17	47.17	0.00
		12	61.74	61.74	0.00
		24	72.42	72.42	0.00
partially exposed	2002	6	46.17	46.17	0.00
		12	60.56	60.56	0.00
		24	71.43	71.75	0.31
	2003	6	45.66	45.99	0.33
		12	59.49	60.57	1.08
		24	69.81	71.87	2.07
	2004	6	42.97	45.72	2.75
		12	57.20	61.26	4.06
		24	69.58	73.23	3.65
fully exposed	2005	6	47.49	50.69	3.20
		12	62.74	66.95	4.21
		24	74.71	78.28	3.58

Notes: Implied effects of the Hartz IV reform on the likelihood that an individual is reemployed within 6, 12, or 24 months of claiming UI. To construct the table, I use the benchmark specification to predict the probability that a given jobless spell ends via reemployment by each time horizon. I then repeat this prediction after setting the time-to-Hartz variable to the omitted category ($\geq$ 10 months away). The difference between these predicted values gives the implied effect of Hartz IV on an individual's reemployment rate. I average these implied effects by year of entry into UI.

Table 3: Implied effects of UI reform on mean log reemployment wages for the fully exposed 2005 UI cohort

	Benchmark specification	Time-varying baseline hazard
Overall wage effect	–1.14	1.01
	(0.25)	(0.32)
Primary decomposition		
Selectivity effect	–2.18	–0.17
	(0.24)	(0.30)
Time-out-of-work effect	1.04	1.18
	(0.09)	(0.10)
Alternative decomposition		
Selectivity effect	–2.00	–0.30
	(0.24)	(0.28)
Time-out-of-work effect	0.87	1.31
	(0.08)	(0.11)

Notes: Estimated effects of reform-induced benefit cuts on log reemployment wages for workers entering UI in 2005 (expressed in 100 $\times$ log points). Column (1) is based on my benchmark duration and wage specifications. Column (2) is based on specifications that add a full set of interactions between 3-month duration bins and quarter $\times$ year effects. The overall effect and the primary decomposition are computed as in Equations 8.5 and 8.6. The alternative decomposition is computed as in Footnote 48. Standard errors in parentheses are based on 500 draws from the estimated variance-covariance matrix, which is in turn clustered on individual.

ONLINE APPENDIX
(NOT FOR PUBLICATION)

The Duration and Wage Effects of Long-Term Unemployment Benefits: Evidence from Germany's Hartz IV Reform*

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UC Davis

December 26, 2019

Appendix A Simulating Changes in Household Income

In Section 2.2, I adapt the OECD Tax-Benefit Model to simulate the (potential) change in each UI claimant's post-exhaustion household income induced by Hartz IV. This appendix describes my methodology and summarizes pertinent features of Germany's tax-and-transfer system.[1]

The Tax-Benefit Model provides a rich characterization of the German welfare state. Given a vector of household attributes, the model applies statutory rules to calculate income tax liability, social insurance contributions, short- and long-term unemployment benefits, means-tested social assistance, and a variety of other transfers. The IAB data report many of the key inputs into these calculations, including gross earnings, region, marital status, the number of dependent children, and the age of the youngest child. Spousal earnings—another critical input—are not directly available, but I can back out a proxy for them from a claimant's implied income tax liability. Since I do not observe assets, I follow the OECD default and assume that households have negligible assets, as is likely to be true for most households that have experienced a protracted jobless spell culminating in UI exhaustion. Additional assumptions (described below) are needed for rental expenses, the ages of older children, and other inputs. Following the OECD, I assume that households apply for any public benefits for which they are eligible.

My basic simulation procedure is as follows:

1. Impute spousal earnings by exploiting two features of the German tax/benefit code.
2. Using the tax/benefit rules in effect in 2004, simulate each new UI claimant's net household income (and the components thereof) just after the exhaustion of short-term UI benefits.
3. Repeat this process under the rules in effect in 2005.

Once I have simulated household income under both pre- and post-Hartz IV rules, I calculate reform-induced changes in post-exhaustion cash benefits (defined in Equation 2.2 in the main text) and net household income (defined in Equation 2.3). These measures capture, for each claimant, the financial impact of Hartz IV in the event that she exhausts her short-term benefits and ends up relying on long-term transfers.

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[1] My simulations are adapted from the OECD's publicly available Stata programs, downloadable at http://www.oecd.org/els/soc/Models.zip. I have also drawn on code from Alexandre Desbuquois's Stata package TAXBENEXTRACT. Information in this section is taken from the German Social Code, the series *Social Security at a Glance* published by the German Federal Ministry of Labour and Social Affairs, and the annual OECD publication *Taxing Wages*.

Imputing spousal earnings

Spousal earnings are a key determinant of long-term benefit levels, but spouses are not linked in my data.[2] To circumvent this problem, I infer spousal earnings by exploiting two provisions of Germany's tax/benefit system. First, UI benefits are indexed to *after-tax* earnings in the lead-up to job loss, enabling me to infer each claimant's income tax liability. Second, an individual's tax liability is an explicit function of both own and spousal earnings, so I can invert the tax formula to approximate spousal earnings.

Step 1: inferring income tax liability. A worker's initial monthly short-term UI benefit is calculated as

$$\text{UI benefit} = \text{RR} \times (\text{gross monthly earnings} - \text{income tax} - \text{social insurance contributions}) \tag{A.1}$$

I observe the initial UI benefit level as well as gross monthly earnings prior to UI receipt. Benefits are indexed to average earnings over the 1 or 2 years preceding job loss, whichever is more favorable to the claimant, so I compute prior earnings on this basis. Social insurance contributions are a simple function of earnings and hence easily calculated.[3] The replacement rate (RR) equals 60 or 67 percent, depending on whether the claimant has dependent children (which I also observe). I can therefore invert Equation A.1 to express a claimant's income tax liability as a function of observables.

Step 2: inferring spousal earnings. Income taxes are levied on gross household earnings, net of deductions. Married couples typically file jointly, and—crucially for my purposes—tax calculations employ the so-called "income splitting" method: the spouses' earnings are added together and divided by two to determine each spouse's taxable income. Tax liability is then computed as a piecewise quadratic function of taxable income (plus a "solidarity surcharge"). Because of income splitting, married individuals face tax bills of the form

$$\text{income tax} = f(0.5 \times [\text{own earnings} + \text{spousal earnings} + \text{other taxable income} - \text{deductions}]) \tag{A.2}$$

where $f(\cdot)$ is a known function. I observe own gross earnings and, as noted above, I can deduce income tax liability. I can directly calculate deductions for social insurance contributions and dependent children; following the OECD, I also assume that individuals claim deductions for work-related and special expenses.[4] After inverting Equation A.2 and netting out these terms, I am left with the sum of spousal earnings and other taxable income. I assume that other taxable income is zero, yielding a proxy for spousal earnings.[5]

Given the intricacy of this procedure, it is important to verify that—though approximate—my measure of spousal earnings contains real signal. In Price (2017), I report the following validity checks:

1. Imputed spousal earnings are much higher for female claimants (with male spouses) than for male claimants, consistent with Germany's substantial gender gap in employment and earnings.
2. The gender gap in imputed spousal earnings is larger in the West, where fewer women are employed.
3. Pre-UI claimant earnings are positively correlated with imputed spousal earnings, as expected given some degree of assortative matching between husbands and wives.
4. Among male claimants (with female spouses), spousal earnings are decreasing in the number of children present, with an additional drop if the youngest child is under age 5.
5. As a placebo, calculating "spousal earnings" for unmarried claimants yields much smaller numbers.

For claimants who were married at entry into UI, I therefore impute spousal earnings on this basis. For unmarried claimants, I set spousal earnings equal to zero.

[2] Goldschmidt et al. (2017) match cohabiting spouses in IAB data on the basis of surnames and residential addresses, but only a minority of German couples are successfully matched and the matched sample is not fully representative. Furthermore, only in a small and non-random share of cases would both partners appear in the data extracts I am able to access.

[3] In 2004, workers owed 9.75 percent of gross earnings for pension contributions, 3.25 percent for UI, 7.00 percent for health insurance, and 0.85 percent for long-term care insurance, up to ceilings that are seldom reached by claimants in my sample. Employers make equal-sized contributions. Mini-jobs (and so-called "midi-jobs") are partially exempt from these contributions.

[4] Households with dependent children may claim either a child tax allowance or a child tax credit. Since the federal tax office makes this choice on households' behalf, acting to minimize their tax liability, I select the tax-minimizing option.

[5] The other main sources of taxable income are investment returns and self-employment. Since the workers in my sample are adversely selected on baseline earnings, they are unlikely to have much investment income; since they have separated from socially insured jobs, they are unlikely to have had much much self-employment income during the lookback period.

Calculating benefits under the 2004 rules

Long-term unemployment assistance

Upon exhausting short-term benefits (*Arbeitslosengeld*), UI claimants could apply for long-term unemployment assistance (*Arbeitslosenhilfe*) financed out of general revenues. Long-term assistance was means tested on the basis of both household assets and household income. Workers could claim these benefits indefinitely, provided they continued to satisfy annual means testing.

Exhaustees could not transition to long-term benefits unless (or until) household assets fell below an exemption level. Prior to 2003, the asset exemption equaled €520 times the sum of the claimant's age and (if married) spouse's age. Under Hartz I, the asset limit fell to €200 times own + spousal age for long-term benefit claims initiated after January 1, 2003. Certain protected assets, such as "reasonable" owner-occupied houses, automobiles, and some pension accounts, did not count against these limits. While I do not observe asset holdings in the IAB data, tabulations in the German Socioeconomic Panel (SOEP) suggest that only a small share of UI claimants would face a binding asset test, even under the post-2003 rules.[6] As noted above, my simulations assume that claimants have negligible assets by the time of short-term benefit exhaustion, so that the asset test never binds.

Conditional on satisfying the asset test, long-term benefits replaced up to 53 percent of prior net earnings for childless claimants and 57 percent for claimants with one or more children. Benefits are reduced one-for-one for earnings above the UI earnings disregard, as well as for alternate sources of income such as rentals.[7] In all simulations, I assume that claimants have zero earned income while unemployed. Benefits are also reduced one-for-one for spousal earnings net of taxes, work-related expenses, and a disregard level. I use imputed spousal earnings to apply this income-based means test.

Other benefits

Sufficiently poor households could also apply for supplemental means-tested welfare benefits, known as social assistance (*Sozialhilfe*). The level of social assistance was calculated as

$$\text{social assistance} = \max(0, \text{assessed need} - \text{household income net of means testing}), \tag{A.3}$$

where a household's assessed need is the sum of individual allowances and housing/heating allowances.

Individual allowances were revised annually and varied across municipalities. For 2004, the OECD reports that the base allowance paid to household heads averaged €295 in the West and €285 in the East; additional payments were made for dependent spouses (80 percent of the base rate) and children (50–90 percent depending on age), with extra assistance available to single parents. I impute the ages of older children by assuming a two-year birth interval between children, then compute base benefits using these OECD averages. Households eligible for social assistance were reimbursed for all "reasonable" housing and heating expenditures. Since local definitions of reasonable cost are not readily available, I use a schedule of allowed housing/heating costs as a function of household size, promulgated by the Berlin Senate in 2005.

Once need had been assessed, social assistance was means tested on the basis of household income—specifically, gross earnings (net of a disregard) plus unemployment benefits plus alimony minus income taxes minus social security contributions. I use the OECD's alimony rate for single parents with children under age 6, and I calculate the other components of the means test as described earlier in this appendix.

Both before and after Hartz IV, German households could also apply for means-tested housing benefits (*Wohngeld*) distinct from social assistance. The benefit level is a function of both household income and housing expenditures, up to a ceiling that depends on household size and the municipality's rent designation. I follow the OECD in using the highest rent designation for all households, regardless of where they reside.

[6] The 2002 SOEP wave asked respondents whether they held financial assets exceeding €2500 and, if so, the value of these assets. Among prime-age respondents receiving short-term UI benefits on the survey date, I calculate that 11.7 percent had own + spousal financial assets in excess of the age-based limit applicable to post-2003 claims. Asset holdings are likely further decumulated by the time these claimants reach benefit exhaustion.

[7] The earnings disregard is the larger of €165 or 20 percent of the full benefit amount; workers are also limited to 15 hours of work per week. Hartz IV reduced the earnings disregard for some workers by eliminating the 20 percent minimum disregard.

Calculating benefits under the 2005 rules

Hartz IV replaced long-term unemployment assistance and social assistance with a single, means-tested benefit (*Arbeitslosengeld II*). Unlike the old system of long-term unemployment assistance, the new benefits are not indexed to prior earnings, but rather based (like social assistance) on the household's assessed need. The core benefits consist of (i) a basic cash payment, (ii) assistance with housing and utility expenses, and (iii) a temporary supplement to cushion the transition to the new regime.

In 2005, the base monthly allowance equaled €345 in the West and €331 in the East. Single adults receive 100 percent of the base allowance; married couples receive 90 percent per spouse. Each dependent child below [above] age 15 receives a benefit equal to 60 [80] percent of the base allowance. I use the claimant's region and household characteristics to assign these cash benefits. Next, the level of housing assistance is not explicitly stated in the law; instead, as under the previous system of social assistance, municipalities are instructed to cover "reasonable" costs. I again use Berlin's 2005 schedule of allowed housing costs.

To ease the transition to the new system, Hartz IV includes a temporary supplement for workers who exhaust short-term UI benefits. The supplement depends on the difference between the value of short-term benefits on the eve of benefit exhaustion and the value of long-term benefits thereafter.[8] In the first year after short-term exhaustion, the supplement equals $\frac{2}{3}$ of the assessed difference, up to a ceiling that depends on household structure (€160 for singles and €320 for couples, plus €60 for each child). The supplemental payment and the payment ceiling are cut in half after one year and expire completely after two years.[9] I compute net replacement rates just after short-term benefit exhaustion, when the supplement is maximized.

Appendix B Derivation of Theoretical Results

I now prove the theoretical claims made in Section 3:

Proposition.

(a) A long-term benefit cut increases search intensity and decreases the reservation wage throughout the unemployment spell. That is, $\frac{ds(R)}{d\underline{b}} < 0$ and $\frac{d\underline{w}(R)}{d\underline{b}} > 0$ for all $R \geq 0$. It follows that $\frac{d\lambda(R)}{d\underline{b}} < 0$, so that a benefit cut increases the hazard rate of reemployment at all durations.

(b) These behavioral responses tend to zero for benefit cuts lying arbitrarily far in the future. That is,

$$\lim_{R\to\infty} \frac{ds(R)}{d\underline{b}} = \lim_{R\to\infty} \frac{d\underline{w}(R)}{d\underline{b}} = \lim_{R\to\infty} \frac{d\lambda(R)}{d\underline{b}} = 0$$

(c) There are offsetting effects on mean accepted wages. Observed wages decline conditional on completed duration, but workers accept jobs at earlier durations when reservation wages are higher.

Proof of (a). There is no closed-form solution for the optimal policies, but I can characterize the solution using standard dynamic programming techniques. Given the Inada conditions on $\psi(\cdot)$, an optimizing worker exhausts her short-term benefits with positive probability, so that $U(R)$ is strictly increasing in $\underline{b}$ for all R. Furthermore, the (weakly) declining benefit schedule ensures that $U(R)$ is increasing in R. The net present value of unemployment declines as a worker approaches benefit exhaustion and is constant thereafter.

[8] Departing from the OECD Tax-Benefit Model, I include housing/heating allowances in the post-reform benefit level when performing this calculation. Informational materials published by the Federal Employment Agency during the mid-2000s state explicitly that these allowances belong in the supplement formula.

[9] The two-year time limit begins on the date of short-term benefit exhaustion, even if exhaustion predated the onset of Hartz IV. For example, a claimant who exhausted short-term benefits on December 31, 2002 would not be eligible for the supplement even if she were still unemployed in 2005. A claimant who exhausted short-term benefits on June 30, 2003 would be eligible for a one-half supplement from January 1, 2005 through June 30, 2005 if unemployed during that period. For simplicity, I compute the supplement as though the claimant were at the beginning of the two-year grace period. This is a conservative assumption that maximizes the value of the temporary supplement (and hence the generosity of the post-reform system).

The first-order conditions for the optimal reservation wage and search intensity are, respectively,

$$J(\underline{w}(R)) = U(R) \tag{B.1}$$
$$\psi'(s(R)) = (1 - G(\underline{w}(R)))(\mathbb{E}(J(w) \mid w \geq \underline{w}) - U(R)) \tag{B.2}$$

I next derive a useful intermediate expression showing how the value of unemployment responds to marginal changes in $\underline{b}$. After the exhaustion of short-term benefits, the Bellman simplifies to

$$U(0) = \frac{u(\underline{b}) - \psi(s(0)) + s(0)(1 - G(\underline{w}(0)))\mathbb{E}(J(w) \mid w \geq \underline{w}(0))}{\delta + s(0)(1 - G(\underline{w}(0)))} \tag{B.3}$$

so that, invoking the envelope theorem,

$$\frac{dU(0)}{d\underline{b}} = \frac{u'(\underline{b})}{\delta + s(0)(1 - G(\underline{w}(0)))} > 0$$

Using this property, and again applying the envelope theorem,

$$\begin{aligned}
\frac{dU(R)}{d\underline{b}} &= \exp(-\delta R)\Pr(\text{reach exhaustion})\frac{dU(0)}{d\underline{b}} \\
&= \exp(-\delta R)\exp\left(-\int_0^R \lambda(x)dx\right)\frac{dU(0)}{d\underline{b}} \\
&= \exp\left(-\int_0^R (\delta + \lambda(x))dx\right)\frac{dU(0)}{d\underline{b}} \\
&> 0
\end{aligned} \tag{B.4}$$

This expression implies that $\frac{d}{dR}\left(\frac{dU(R)}{d\underline{b}}\right) < 0$. The intuition is straightforward: to first order, small changes in the long-term benefit level affect utility only once benefits are exhausted, and post-exhaustion utility is discounted at the effective rate $\delta + \lambda(\cdot)$. When R is greater, the future is discounted more heavily due to both pure time preference and the increased likelihood of interim reemployment.

Applying the implicit function theorem to Equations B.1 and B.2, and using $J(w) = \frac{u(w)}{\delta}$, yields

$$\begin{aligned}
\frac{d\underline{w}(R)}{d\underline{b}} &= \frac{\delta}{u'(\underline{w}(R))}\frac{dU(R)}{d\underline{b}} > 0 \quad \text{for all } R \\
\frac{ds(R)}{d\underline{b}} &= -\frac{1}{\psi''(s(R))}(1 - G(\underline{w}(R)))\frac{dU(R)}{d\underline{b}} < 0 \quad \text{for all } R
\end{aligned} \tag{B.5}$$

The effect of a benefit cut on the hazard rate is immediate:

$$\frac{d\lambda(R)}{d\underline{b}} = \frac{ds(R)}{d\underline{b}}(1 - G(\underline{w}(R))) - s(R)g(\underline{w}(R))\frac{d\underline{w}(R)}{d\underline{b}} < 0 \quad \text{for all } R \tag{B.6}$$

This establishes (a): long-term benefit cuts depress reservation wages and increase job-finding at all durations.

Proof of (b). To prove that behavioral responses limit to zero for far-off benefit cuts, consider a hypothetical benefit scheme that pays out $\bar{b}$ in perpetuity. This is a stationary problem with value U^* defined by

$$\delta U^* = \max_{s,\underline{w}} \; u(\bar{b}) - \psi(s) + s(1 - G(\underline{w}))(\mathbb{E}(J(w) \mid w \geq \underline{w}) - U^*), \tag{B.7}$$

and with associated (stationary) policies s^* and $\underline{w}^*$ and associated hazard rate $\lambda^* \equiv s^*(1 - G(\underline{w}^*))$.

A revealed-preference argument shows that $U(R)$ limits to U^*. Under the true benefit schedule $b(R)$, let $\widetilde{U}(\cdot)$ denote the payoff from adopting strategies s^* and $\underline{w}^*$ for all $R > 0$ and then switching to the true optimal policies $s(0)$ and $\underline{w}(0)$ after the benefit step-down. Since the flow payoffs and hazard rates that result coincide with those in the hypothetical problem until benefit exhaustion and with those in the true problem thereafter,

$$U^* - \widetilde{U}(R) = \exp(-(\delta + \lambda^*)R)(U^* - U(0)). \tag{B.8}$$

This utility gap decays exponentially, so that $\lim_{R\to\infty} \widetilde{U}(R) = U^*$. But by revealed preference, $\widetilde{U}(R) \leq U(R) < U^*$ for all R. Hence $\lim_{R\to\infty} U(R) = U^*$.

Returning to the FOCs in Equations B.1 and B.2, take limits as $R \to \infty$. By continuity of u, ψ, and G, the policy functions approach well-defined limits ($\lim_{R\to\infty} s(R) = s^*$, $\lim_{R\to\infty} \underline{w}(R) = w^*$). Now take the limit as $R \to \infty$ in Equation B.5. By continuity, both expressions limit to zero, proving (b).

Proof of (c). How do benefit cuts affect accepted wages? Let $\widetilde{w}(R(D)) \equiv \mathbb{E}(w \mid w \geq \underline{w}(R(D))$ denote the average accepted wage among workers reemployed D months into their unemployment spell, with $R(D) \equiv \max(0, P - D)$ denoting the remaining duration until benefit exhaustion at this time. Let $f(D) \equiv \exp\left(-\int_0^D \lambda(x)dx\right)\lambda(R(D))$ denote the pdf of completed jobless duration. It is instructive to write the mean accepted wage as a weighted average of duration-specific average wages:

$$\mathbb{E}(w) = \int_0^\infty f(D)\widetilde{w}(R(D))dD. \tag{B.9}$$

Differentiating with respect to $\underline{b}$,

$$\frac{d\mathbb{E}(w)}{d\underline{b}} = \int_0^\infty f(D)\frac{d\widetilde{w}(R(D))}{d\underline{b}}dD + \int_0^\infty \frac{df(D)}{d\underline{b}}\widetilde{w}(R(D))dD. \tag{B.10}$$

The first term is the reservation wage effect, which can be shown to equal

$$\int_0^\infty f(D)\left\{\frac{g(\underline{w}(R(D)))}{1 - G(\underline{w}(R(D)))}(\widetilde{w}(R(D)) - \underline{w}(R(D))\frac{d\underline{w}(R(D))}{d\underline{b}}\right\}dD > 0. \tag{B.11}$$

The second term is an indirect effect. Since benefit cuts increase job-finding at all durations, they shorten jobless durations in the sense of first-order stochastic dominance. This puts more weight on mean wages at short durations, when $\underline{w}(R(D))$ and hence $\widetilde{w}(R(D))$ are higher. Thus the second term is negative. □

Appendix C Computing Potential Benefit Durations

My research design hinges on my ability to accurately measure the potential duration of each worker's short-term benefits at the onset of a new UI claim. Although potential benefit duration (PBD) is not recorded directly in the administrative data, I can impute it in two ways. Here I describe both imputation procedures in detail, assess the validity of my preferred approach, and show that my hazard results are robust both to using the alternative procedure and to restricting attention to workers whose PBD is easiest to gauge.

Preferred measure ("ex post")

My preferred approach, which I call the *ex post measure*, imputes PBD based on the realized duration of a UI spell plus any residual UI entitlement remaining at the end of the spell. In the administrative data, each period of benefit receipt consists of one or more "notifications", with a variety of events (such as adjustments in benefit level) triggering a new notification. Critically, the data report the number of days of unused short-term UI benefits (*Restanspruch*), if any, remaining at the end of each notification period.[10]

Let D denote the completed duration of the first notification period corresponding to a new UI claim, let R denote the duration of unused benefits at the end of this period, and let $\bar{P}(a)$ denote the claimant's

[10] Nagl and Weber (2016) report finding data irregularities in the *Restanspruch* variable, relating to mid-claim updates to a claimant's residual benefit duration. Using a correction module graciously provided by the IAB, I find that using the corrected variable developed by Nagl and Weber never alters my PBD calculation, evidently because the issues they discovered never apply to the *initial* UI notifications required for my procedure. My PBD measure is thus immune to the concerns they raise.

age-specific maximum PBD, with all variables expressed in days. I compute start-of-claim PBD, in days, as

$$P^{\text{ex post}} \equiv \min(\bar{P}(a), D + R) \tag{C.1}$$

That is, I set PBD equal to the (observed) completed duration plus any time remaining in the worker's claim, overriding the result if it exceeds the legal maximum.[11] The resulting PBD distribution makes sense given the eligibility rules: in particular, the distribution of $D + R$ has large point masses at 180 days (the starting duration for a newly established UI entitlement) and at the age-specific maximums $\bar{P}(a)$. To conduct the analysis at monthly frequency, I express $P^{\text{ex post}}$ in months by rounding it to the nearest 30-day increment.

Alternative measure ("ex ante")

A potential concern with the ex post measure is that $P^{\text{ex post}}$ may deviate from start-of-claim PBD in the event that benefit entitlements are modified mid-claim. For example, participating in certain vocational training programs can slow by one-half the rate at which UI benefits are consumed, effectively prolonging the potential duration of UI benefits. In such cases, the ex post measure of PBD may be endogenous to post-claim worker behavior, which may in turn vary with effective exposure to the Hartz IV benefit cuts.[12]

To ensure that my results are not driven by such mid-stream benefit changes, I also impute PBD using an alternative *ex ante measure*. To construct this measure, I use a worker's observed employment and unemployment record—together with the programmatic rules applicable to UI claims initiated during 2001–2005—to calculate PBD solely on the basis of information observed by the start of a worker's UI claim. For each new UI claim beginning on date t_0, I do the following:

1. Determine whether the worker received short-term UI benefits at any time in the 7 years preceding t_0. If so, let $R^{\text{prev}} \geq 0$ denote the number of days of short-term benefits remaining at the end of the previous claim (and thus recyclable within the present claim).

2. Determine whether the worker was employed in socially insured jobs for at least 360 days out of the 3 years preceding t_0 (counting only jobs held since the previous UI claim, if any). Let $E \in \{0, 1\}$ be an indicator for satisfying this criterion and thereby establishing a fresh UI entitlement.

3. Let x denote the number of insured days worked during the last 7 years (again counting only jobs held since the previous UI claim). Since UI-entitled workers accrue 60 days of benefits for every 120 days employed, define $X \equiv 60 \cdot \text{floor}(x/120)$ as benefit-days accrued within a new entitlement.

4. Record each worker's age a as of t_0. Since I observe year of birth but not the exact date, I calculate a as though every worker were born on July 1. This assumption minimizes the number of classification errors, since workers who claim UI early [late] in the year tend to be younger [older] on t_0.

5. Seasonal workers with $E = 0$ may qualify for special 90- or 120-day UI entitlements if they have worked in socially insured jobs for at least 180 or 240 days, respectively. Although I cannot identify seasonal eligibility directly, I assign seasonal benefits $S \in \{0, 90, 120\}$ to all workers (i) who have worked sufficiently many days within the preceding 3 years and (ii) whose UI claims begin between October and January, when seasonal layoffs are concentrated in these data.

6. Adapting Equation 4.1 from the main text, I compute start-of-claim PBD, in days, as

$$P^{\text{ex ante}} \equiv \min(\bar{P}(a), E \cdot X + (1 - E) \cdot S + R^{\text{prev}}) \tag{C.2}$$

(rounding to the nearest 30 days when using a monthly version of this variable). This formula reflects the idea that workers can accrue UI benefits through a standard entitlement or a seasonal entitlement, augmented with any prior unused benefits, up to the age-determined cap.

[11] Because I observe year but not date of birth, I cannot perfectly determine $\bar{P}(a)$ for workers who turn 45, 47, or 52 in the year of initial UI receipt. In these ambiguous cases, I make the conservative assumption that a claimant's birthday occurs before the beginning of the UI claim. This assumption minimizes the number of benefit durations that I override.

[12] In practice, the notification logic militates against this concern: the ex post procedure relies only on the *first* UI notification period, but subsequent changes in program participation should appear as fresh notifications not used in my procedure.

7. In some cases, the above procedure suggests that a worker is entitled to zero days of UI benefits. Since the very fact of initiating a UI claim belies this result, I recode $P^{\text{ex ante}} = 180$ days when this occurs. Doing so makes sense if, for example, slight inaccuracies in employment dates result in workers appearing to just fall short of the threshold for establishing a new entitlement.

There are several reasons to expect that this procedure will not work perfectly. First, while I account for residual benefits from any previous UI claim, I cannot capture the full complexity of the rules mapping individual employment records into new benefit accruals. Although the above procedure is likely to perform well for workers who are "new to UI", Schmieder et al. (2012) caution that calculating PBD in Germany "is not as clear cut for workers with intermittent unemployment spells because of complex carry-forward provisions in the law".[13] Second, workers can accrue UI benefits through military service, civil service, and other activities that are not recorded in the IAB data. This may cause me to underestimate PBD for some workers. Third, I can impute seasonal benefits only imperfectly. Fourth, $\bar{P}(a)$ is mismeasured for claimants in the vicinity of ages 45, 47, and 52. Finally, the discontinuous nature of benefit accrual means that slight errors in recorded employment dates can generate large errors in imputed PBD.

For all of these reasons, I view ex post PBD as a better measure of true start-of-claim PBD. Loosely speaking, the observed benefit duration D and residual benefit duration R serve as sufficient statistics for the complex (and not fully observable) administrative procedures that determine benefit eligibility. Consistent with this reasoning, I show below that hazard estimates using ex ante PBD are qualitatively similar to—but, as we should expect, attenuated relative to—those based on ex post PBD.

Comparing the two measures

Online Appendix Table 5 reports summary statistics for the ex post and ex ante PBD measures, using the daily versions of these measures prior to rounding to the nearest month. The measures have very similar means and standard deviations, and the correlation between the measures across all claimants is 0.90. The two measures agree exactly, in days, for 68 percent of claimants; they differ by at most 30 days for 76 percent of claimants. Although we should not expect a perfect correspondence between the two measures (given the difficulties noted above in the calculation of ex ante PBD), the similarities between them are encouraging.

Because much of the difficulty in computing ex ante PBD stems from carryover of residual benefit entitlements from past UI claims, I also report these statistics separately for "UI veterans"—claimants who received short-term UI benefits sometime in the 7 years preceding the present claim—and "UI rookies", who have not done so. As expected, the two measures coincide much more often for rookies (81 percent of cases) than for veterans (62 percent). Since the ex ante measure is free from any potential endogeneity concerns, the high level of agreement within this subsample suggests there is at most very modest scope for the ex post measure to be contaminated by mid-claim changes in benefit duration.

Robustness to method of computing potential duration

I conclude this appendix by showing that my core hazard results are robust to changing the sample and the PBD measure to minimize any threat from endogeneity in my preferred, ex post PBD measure. The first series in Online Appendix Figure 11 replicates my benchmark hazard specification using the full estimation sample. The second series reruns this specification on the subsample of UI claimants whose ex ante PBD and ex post PBD coincide exactly. Reassuringly, the estimates remain quite similar both qualitatively and quantitatively for this subpopulation, for which my preferred PBD measure is less likely to be influenced by mid-claim changes in entitlement duration.

As a final check, the third series reestimates my benchmark specification on the full sample, but uses the ex ante PBD measure to construct the event-time variables τ_{id}^{E} and τ_{id}^{H} that capture time relative to

[13] For example, suppose that a new labor market entrant loses her job after paying into the UI system for 13 months. My algorithm will correctly assign her 6 months of benefits for this initial UI claim, effectively giving her credit for 12 of these 13 months employed. If she then exits UI after 3 months, she will return to work not only with 3 months of residual benefits, which I capture, but also with 1 month of residual *employment* that may be credited towards a new UI entitlement. It is not clear how to carry forward such residual employment. Since accounting for all possible carry-forward scenarios requires knowledge of a worker's complete employment record, the problem is worsened by the absence of East German data prior to reunification.

benefit step-downs. The results are again qualitatively similar to my main estimates, but they are noticeably attenuated, consistent with my prior expectation that the ex ante measure is a noisier proxy for true PBD.

In sum, the results in this appendix indicate that my results are unlikely to be materially influenced by any endogenous feedback whereby mid-claim claimant behavior influences my preferred measure of PBD.

Appendix D Dynamic selection and employment stability

While my preferred hazard approach neatly accounts for the complex and variable timing of the Hartz IV treatment, it has two important limitations. First, hazard models are subject to concerns about dynamic selection, as interventions may lead to unobserved compositional changes in the set of workers who remain in the risk set at each duration (Kiefer, 1988). Second, because the hazard formulation focuses on each worker's *first* transition back into work, it leaves open the question of whether the workers whom Hartz IV induced to find jobs enjoyed stable employment spells or instead quickly lost their new jobs.

To address these issues, I estimate linear probability models that chart Hartz IV's impact on a worker's point-in-time employment status at each duration since the initial entry into UI. First, I assign each UI entrant i to one of seven bins $b(i) \in \{1, 2, \ldots, 7\}$ based on his or her start-of-spell potential benefit duration.[14] Next, restricting the sample to UI claims initiated in either 2001 ("pre-Hartz IV") or 2005 ("post-Hartz IV"), I estimate the following OLS specification separately for each duration $d \in \{1, 2, \ldots, 24\}$:

$$y_{id} = \alpha_d + \mathbf{z}'_{id}\beta_d + \theta_d \mathbf{1}\{\text{Post}_i\} + \sum_{b=1}^{6} \mathbf{1}\{b(i) = b\}(\gamma_d^b + \delta_d^b \mathbf{1}\{\text{Post}_i\}) + \varepsilon_{id} \tag{D.1}$$

Here, the outcome variable y_{id} is an indicator either for *having ever been reemployed by month* d or for *currently being employed as of month* d. $\mathbf{z}_{id}$ includes controls for sex, region, age, experience, nationality, education group, household type, and deciles of prior wage, the effects of which are all allowed by vary with the outcome horizon. The coefficients of interest, δ_d^b, capture pre-/post-Hartz IV differences in reemployment outcomes for workers in potential benefit duration bin b, relative to differences observed among workers entitled to 18 or more months of short-term benefits (who form the omitted $b = 7$ group). As with my main research design, the logic of this exercise is that groups with briefer initial UI entitlements should respond more strongly to Hartz IV. Because the same estimation sample is used at all durations, however, this alternative specification is immune to concerns about dynamic selection.

Online Appendix Figure 12 plots Hartz IV's implied effect on employment outcomes for workers in the 2005 cohort, based on estimates of Equation D.1. Relative to the hazard-based estimates plotted in Figure 9, the "ever reemployed" estimates from the linear probability model indicate modestly larger peak effects of Hartz IV, as well as somewhat faster fadeout of these effects at longer durations. Nonetheless, the overall pattern is qualitatively and quantitatively similar to that implied by my preferred hazard specification, suggesting that dynamic selection does not account for my results. The implied effects on point-in-time employment status are remarkably similar to those for ever having been reemployed, consistent with persistent rather than fleeting employment gains under Hartz IV.

Appendix E Additional Empirical Analyses

E.1 Further discussion of the placebo exercise

In Section 6.4, I model the effects of pseudo-Hartz IV reforms taking effect on January 1 of each year over 1998–2004. As shown in Figure 8, the 1998–2003 pseudo-reforms have no clear effects of the job-finding hazard, but the 2004 pseudo-reform has positive effects. What explains these modest placebo effects?

One possibility is that, despite truncating this placebo analysis in June 2004, I may still be picking up anticipation of Hartz IV itself. Although final passage of Hartz IV did not occur until July of that year, the law passed the lower house of parliament in December 2003. If UI claimants were sufficiently attentive, patient, and concerned about potentially draconian benefit cuts, they might have begun reacting to Hartz

[14] The bins are 0–3 months, 4–6 months, 7–9 months, 10–11 months, exactly 12 months, 13–17 months, and 18+ months, with potential benefit duration rounded to the nearest month. Alternative partitions yield similar results.

IV far in advance of the actual implementation date. If so, these placebo coefficients might be regarded as causal effects of the policy change itself.

Second, I may be detecting lagged effects from a stricter asset test applicable to long-term benefit claims initiated after January 1, 2003. The IAB data do not report assets, but I observe a small decline in the share of UI exhaustees who transition to long-term benefits after this date, suggesting that the tighter asset limits did bind for some claimants. Interacting the main effect of benefit exhaustion (τ_{id}^{E}) with a dummy for being subject to the new asset rules has little impact on the 2004 placebo effects, weighing against this explanation. Adding these interactions also has little impact on my estimated Hartz IV effects.

Third, the placebo effects may reflect a mid-2003 increase in the frequency of benefit sanctions. Hartz I narrowed the grounds on which claimants could turn down job offers, and in April 2003 the Federal Employment Agency instructed caseworkers to apply sanctions more vigorously (Müller and Steiner, 2008). The Agency's data show that the number of sanctions imposed for refusing job offers tripled over the course of 2003, peaking in September at 16,900, but it then declined by half through the end of 2004. Since sanction rates were *falling* in the run-up to Hartz IV, they are unlikely to explain my core results.

To my knowledge, sanction risk was not directly tied to time remaining until benefit exhaustion.[15] But a spurious correlation between sanctions and the placebo-reform event-time variable could potentially arise if the likelihood of being sanctioned varies with duration since entry into UI. This is quite possible, since the set of jobs that a claimant is expected to accept broadens with time spent out of work (Ebbinghaus and Eichhorst, 2009). That fact motivates the right panel of Figure 8, where I augment my benchmark specification by allowing the shape of the baseline hazard rate to vary flexibly over time. These additional controls absorb any temporal changes in job-finding that are linked to *duration since entry*, as distinct from *duration until benefit cuts*. The fact that adding these controls dampens the placebo coefficients (while strengthening the effects of the true 2005 reform) is consistent with such a spurious correlation.

E.2 Allowing for unobserved heterogeneity in post-UI wage recovery

As noted in Section 8.1, a potential concern with my wage estimates is that they may partly reflect unobserved heterogeneity in post-UI wage dynamics among the workers who respond to Hartz IV. Although my research design differences out persistent earnings gaps across individuals, compositional changes associated with realized jobless duration, and differential wage recovery along many observable dimensions, my benchmark estimates may still be confounded by idiosyncratic differences in workers' wage recovery after job loss. For instance, the marginal job-finders who find jobs in the face of Hartz IV may be less comfortable in job interviews and hence disinclined to negotiate their starting salaries, so that they exhibit below-expected post-UI wages regardless of when they are reemployed.

To assess this concern, I use repeat UI spells to test—and control—for time-invariant unobserved heterogeneity in post-UI wage recovery. For each UI claim in my estimation sample, I use employment records dating back to 1993 to identify the worker's *previous* UI claim (if any). Let w_i^{prev} denote the log pre/post wage change associated with this earlier claim. I reestimate Equation 8.1 with w_i^{prev} as the outcome variable, leaving unchanged all of the explanatory variables associated with the current claim i. If the workers who find jobs when Hartz IV binds are prone to experiencing worse-than-average wage losses in the aftermath of job loss, then we should see a similar pattern of declining δ_k^H coefficients in this modified specification.

The left panel of Online Appendix Figure 8 plots the estimated δ_k^H coefficients from my benchmark wage specification alongside the corresponding coefficients from this prior-claim regression, with both models estimated on the subset of UI claims for which I can identify a valid prior claim. The current-claim coefficients are quite similar to those estimated in the full sample. The prior-claim coefficients tend to have negative point estimates, but the estimates are small in magnitude and most are statistically indistinguishable from zero. An alternative is to subtract the prior wage change from the current wage change (putting $w_{id} - w_i^{\text{prev}}$ on the left-hand side), which differences out any time-invariant individual wage-loss effect. The right panel of Online Appendix Figure 8 shows that this double-difference specification yields coefficients very similar to my benchmark estimates, albeit less precise. To the extent that unobserved heterogeneity takes this time-invariant form, accounting for it does not alter the conclusions from my wage analysis.

[15] Using an RD design, Schmieder and Trenkle (2016) show that—at least during their 2008–2010 sample period—the hazard rate of being sanctioned is invariant to potential benefit duration throughout a UI spell, suggesting that caseworkers do not disproportionately sanction claimants whose benefits are running out.

E.3 Auxiliary evidence of worse outside options: the rising hazard rate of recall

To provide auxiliary evidence that jobseekers become less selective when faced with long-term benefit cuts, I distinguish recalls to the previous employer from transitions to a new employer. Many unemployment spells are temporary layoffs that end in recall, and recall expectations are an important determinant of job-search effort (Katz and Meyer, 1990; Nekoei and Weber, 2015). Although I do not observe workers' recall expectations at the moment of layoff, I can identify ex post recalls by matching unique establishment identifiers between pre- and post-UI jobs. Recalls are common in my estimation sample: of the claimants reemployed within 24 months of UI entry, 31.7 percent return to their previous employer. Insofar as recall is "a process not requiring search" (Katz and Meyer, 1990)—because workers are contacted by their former employer and simply exercise or decline the option to return—a rise in the recall hazard in response to benefit cuts would reveal a reduction in workers' perceived continuation value of remaining unemployed.[16]

Online Appendix Figure 9 plots the effects of Hartz IV on the competing risks of being hired by either the previous employer or a new one. Both the recall hazard and the new-employer hazard display the same hump-shaped effect pattern familiar from earlier figures, with the recall hazard rising by 38.3 percent and the new-employer hazard rising by 50.1 percent in the month after the benefit cut. Modulo the need to assume independent risks, these results indicate that still-unemployed workers are more likely to obtain both types of jobs when faced with benefit cuts. Consistent with my wage evidence, the increased recall hazard suggests that Hartz IV made workers less selective about the jobs they accept, providing additional confirmation that long-term benefit cuts have worsened the outside option of remaining unemployed.

Appendix F Implied Effect on Steady-State Unemployment

In Section 6.5, I use my estimated causal effects of Hartz IV on individual job-finding rates to assess the (partial equilibrium) impact of Hartz IV on Germany's steady-state unemployment rate, as follows.

Let u_{ST} and u_{LT} denote the short- and long-term unemployment rates, defined in Germany as the number of workers unemployed for, respectively, under/over 12 months as a share of the overall labor force. Let D_{ST} be the expected number of months that a newly unemployed worker spends in short-term unemployment: hence $D_{\mathrm{ST}} \equiv \sum_{d=1}^{12} S_d$, where S_d is the probability of being unemployed for at least d months. Likewise, let $D_{\mathrm{LT}} \equiv \sum_{d=13}^{\infty} S_d$ denote the expected number of months that such a worker will spend in long-term unemployment. It is convenient to decompose this expression into $D_{\mathrm{LT}} \equiv D_{13\text{–}24} + D_{25\text{–}\infty}$, distinguishing the second year of unemployment from what follows. Next, let q be the exogenous monthly rate of job separation, assumed to be constant throughout this exercise. Abstracting from flows in and out of the labor force, steady-state unemployment equals:[17]

$$u_{\mathrm{ST}} \equiv \frac{D_{\mathrm{ST}}}{1/q + D_{\mathrm{ST}} + D_{\mathrm{LT}}}, \qquad u_{\mathrm{LT}} \equiv \frac{D_{\mathrm{LT}}}{1/q + D_{\mathrm{ST}} + D_{\mathrm{LT}}} \tag{F.1}$$

On the eve of Hartz IV, Germany's overall unemployment rate was about 10 percent, divided equally between short-term and long-term unemployment. I therefore begin by setting $u_{\mathrm{ST}} = u_{\mathrm{LT}} = 5.0$ percent. In the absence of Hartz IV, my hazard estimates imply that $D_{\mathrm{ST}}^{cf} = 6.76$ months and $D_{13\text{–}24}^{cf} = 3.55$ months. Given these values, I calibrate $q = .82$ percent and $D_{25\text{–}\infty}^{cf} = 3.20$ months to satisfy Equation F.1, so that $D_{\mathrm{LT}}^{cf} = 6.76$ months. With Hartz IV in place, my hazard estimates imply smaller values $D_{\mathrm{ST}} = 6.41$ months and $D_{13\text{–}24} = 3.08$ months. Due to censoring, my hazard model is silent as to how Hartz IV affects job-finding after 24 months. If, conservatively, I assume that Hartz IV has no impact on the hazard rate of reemployment beyond this point, then $D_{25\text{–}\infty}$ falls to 2.75 months. The steady-state unemployment rates

[16] If jobs are fully characterized by the wage, then reservation wages are a sufficient statistic for the continuation payoff from staying unemployed. If jobs differ along multiple dimensions, however, workers adopt a more general "reservation utility" cutoff. Although accepting a recall offer is associated with smaller ex post wage losses, a worker may decline even a high-paying recall offer if the wage premium is a compensating differential for the elevated risk of recurrent layoff in the recall job.

[17] To see this, imagine an infinitely lived worker toggling between employment spells of average duration $1/q$ and unemployment spells of average duration $D_{\mathrm{ST}} + D_{\mathrm{LT}}$. Each job-layoff cycle lasts an average of $1/q + D_{\mathrm{ST}} + D_{\mathrm{LT}}$ months, of which D_{ST} are spent in short-term unemployment and D_{LT} are spent in long-term unemployment. If the job-finding rate f is constant, then $D_{\mathrm{ST}} + D_{\mathrm{LT}} = 1/f$, implying the usual steady-state formula $u \equiv u_{ST} + u_{LT} = \frac{q}{q+f}$.

fall to $u_{ST} = 4.79$ percent and $u_{LT} = 4.36$ percent. This implies a 0.85 percentage point fall in aggregate unemployment, comprising 0.21 and 0.64 point declines in the short- and long-term components, respectively.

Soon after Hartz IV, a companion measure reduced the maximal duration of short-term benefits for older workers entering UI after February 1, 2006. Cuts to short-term benefit durations and cuts to long-term benefit levels are likely to have complementary effects, as the former cause the latter to bind earlier in a worker's spell. Such an interaction would magnify the implied partial equilibrium impact of Hartz IV.

Appendix G Correcting for Selection in the Wage Analysis

As explained in Section 8.2, estimating the impact of Hartz IV on individual post-UI wages is complicated by two kinds of selection effects. To correct for selection, I modify Equation 8.4 to gauge how Hartz IV affected wage realizations that would have been observed whether or not Hartz IV had been implemented.

My selection correction exploits the fact that—according to my estimates—Hartz IV accelerated cumulative job-finding for all workers in my sample, so that $\hat{F}_{id} \geq \hat{F}^{cf}_{id}$ for all i and all d. This enables me to define a *truncated* distribution of jobless durations under Hartz IV:

$$\widetilde{F}_{id} \equiv \min(F_{id}, F^{cf}_{i,24}) \quad \text{for } d \in \{1, 2, \ldots, 24\} \tag{G.1}$$

with corresponding truncated pdfs $\widetilde{p}_{id} \equiv \widetilde{F}_{id} - \widetilde{F}_{i,d-1}$ and $\widetilde{q}_{id} \equiv {}^{\widetilde{p}_{id}}/_{\widetilde{F}_{i,24}}$. Intuitively, the modified distribution $\widetilde{F}_{id}$ traces out each worker's reemployment trajectory under Hartz IV, then discards the "excess" probability mass that would have been allocated past the censoring horizon in the absence of Hartz IV.[18] By construction, $\widetilde{F}_{i,24} \equiv F^{cf}_{i,24}$, which in turn implies that $\widetilde{\pi}_i = \pi^{cf}_i$ for all i, where $\widetilde{\pi}_i \equiv {}^{\widetilde{F}_{i,24}}/_{\sum_{j=1}^{N} \widetilde{F}_{j,24}}$ denotes a worker's share of the reemployed pool under $\widetilde{F}_{id}$. Using the modified distribution places the same weight on each claimant in the factual and counterfactual scenarios, thereby eliminating compositional bias.

Employing this correction, I estimate the quantity

$$\Delta\widetilde{\mathbb{E}}(w_i \mid D_i \leq 24) \equiv \sum_{i=1}^{N} \widetilde{\pi}_i \sum_{d=1}^{24} \widetilde{q}_{id}\mu_{id} - \sum_{i=1}^{N} \pi^{cf}_i \sum_{d=1}^{24} q^{cf}_{id}\mu^{cf}_{id} = \sum_{i=1}^{N} \pi^{cf}_i \left(\sum_{d=1}^{24} \widetilde{q}_{id}\mu_{id} - \sum_{d=1}^{24} q^{cf}_{id}\mu^{cf}_{id} \right), \tag{G.2}$$

which can be interpreted as the effect of Hartz IV on reemployment wages among workers who, probabilistically, would have been reemployed within 24 months even if Hartz IV had not been implemented.

[18] We can think of job search as a strategy that maps states of the world ω into realized jobless durations. Letting $D_i(\cdot)$ and $D^{cf}_i(\cdot)$ denote i's Hartz IV and no-Hartz strategies, define the crossover date $\widetilde{D}_i$ such that $\Pr(D_i(\omega) \leq \widetilde{D}_i) = \Pr(D^{cf}_i(\omega) \leq 24)$. My estimates imply that $\widetilde{D}_i \leq 24$ for all i. Let $\Omega^{cf}_{i,24} \equiv \{\omega \mid D^{cf}_i(\omega) \leq 24\}$ be the set of uncensored states under the no-Hartz strategy, and let $\Omega_{i,\widetilde{D}_i} \equiv \{\omega \mid D_i(\omega) \leq \widetilde{D}_i\}$ be the set of states yielding reemployment before $\widetilde{D}_i$ under Hartz IV. Suppose that $D^{cf}_i(\omega) \leq D^{cf}_i(\omega') \iff D_i(\omega) \leq D_i(\omega')$ for all ω, ω'. Under this rank-preservation assumption, $\Omega^{cf}_{i,24} = \Omega_{i,\widetilde{D}_i}$. The modified distribution $\widetilde{F}_{id}$ seeks to compare wages over this common set of states, for which wages are observed under both strategies.

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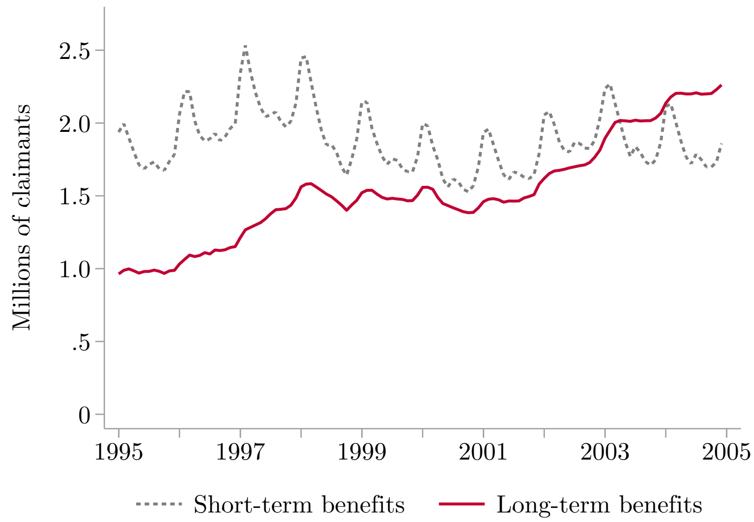
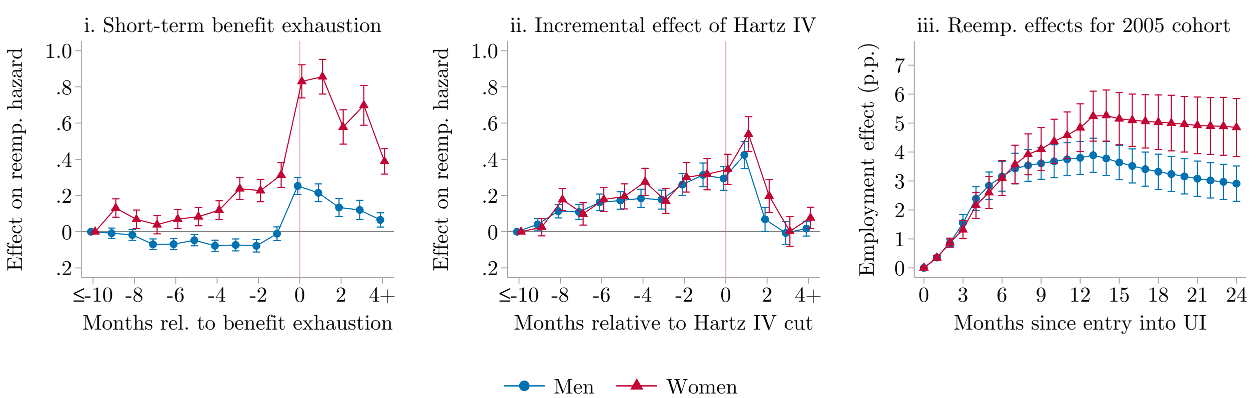
Appendix Figures and Tables

Appendix Figure 1: The rise of Germany's unemployment benefit caseload in the lead-up to Hartz IV



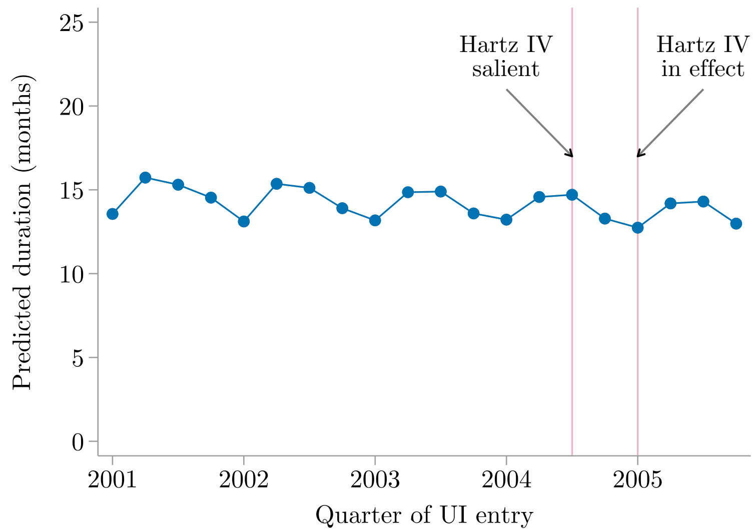
Notes: Monthly aggregate *Arbeitslosengeld* and *Arbeitslosenhilfe* caseloads from Germany's Federal Employment Agency. I truncate the figure in December 2004 because Hartz IV consolidated the existing systems of long-term unemployment assistance and social assistance into a single income-support program (precluding comparisons between the pre- and post-reform caseloads).

Appendix Figure 2: Benchmark hazard effects (first two panels) and path of implied reemployment effects for the fully exposed 2005 UI cohort (third panel), estimated separately for men and women



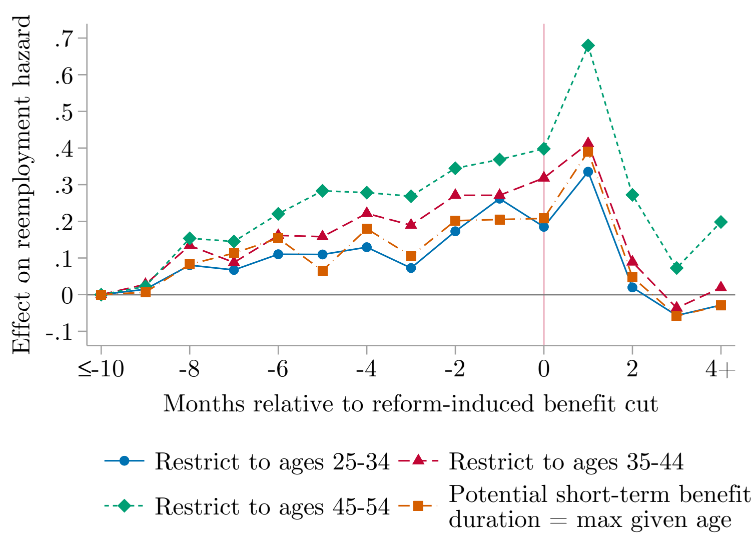
Notes: The left and middle panels plot hazard effects of UI benefit changes, estimated separately for men and women. See notes to Figure 6. The right panel plots the implied effects of Hartz IV on individual reemployment rates. See notes to Figure 9.

Appendix Figure 3: Predicted jobless duration by quarter of UI entry



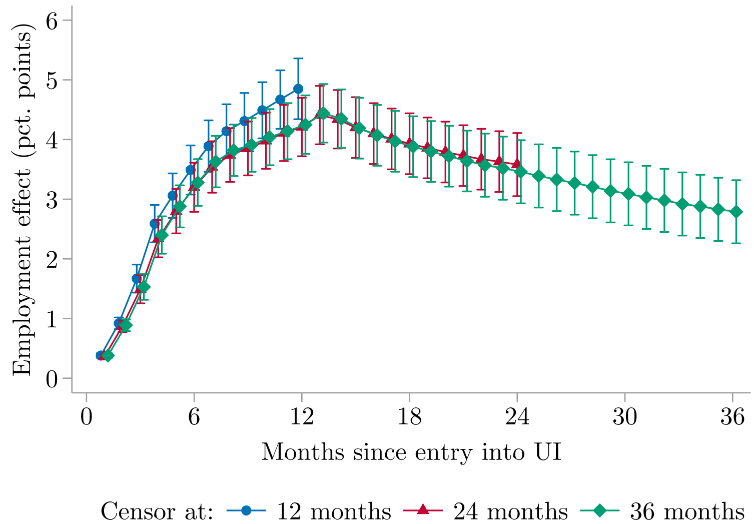
Notes: Mean predicted jobless durations for claimants entering UI in each quarter. The predictions are fitted values from a Weibull model of the reemployment hazard using UI claims that began in 2001. I estimate the model separately by sex × West/East residence, then aggregate up based on the sex and regional composition of each entering UI cohort. The explanatory variables are seven age bins, one-year bins of time worked in the seven years preceding UI receipt, deciles of prior wage, three education groups, a dummy for German nationality, and three household types. I also control for quarter × year effects but, when forming predictions, set these interactions to the reference category. By construction, temporal variation in each series reflects only compositional changes in the characteristics of new UI claimants.

Appendix Figure 4: Isolating two sources of variation in potential benefit duration



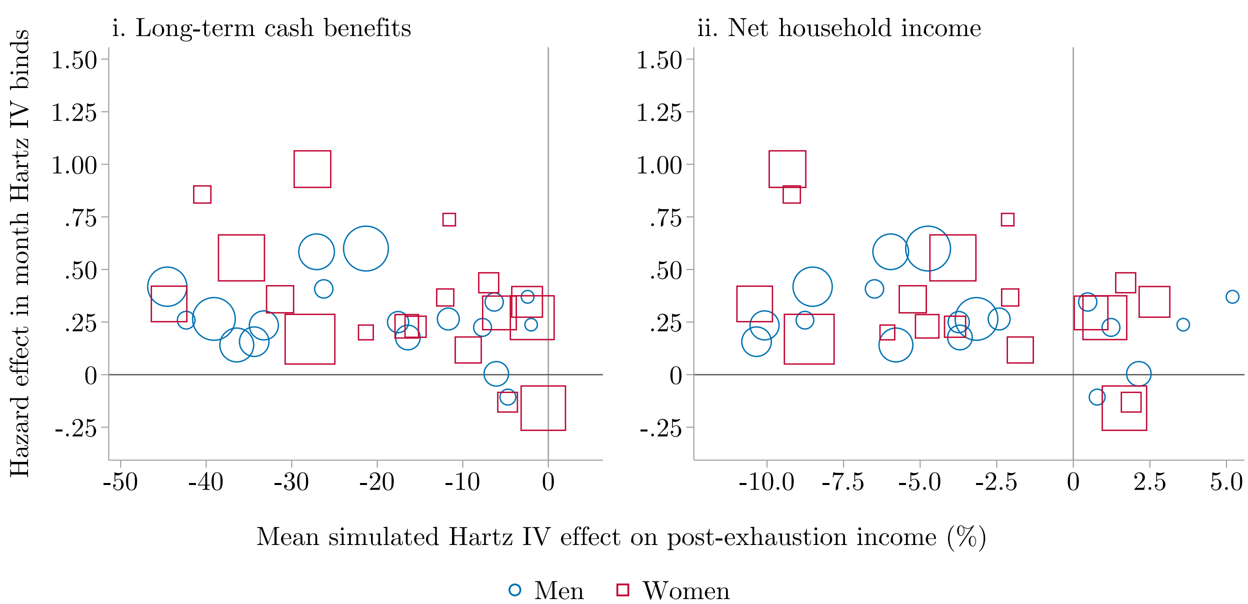
Notes: Potential short-term benefit duration depends on both age and experience. Because all workers under 45 are subject to the same age-determined duration ceiling, variation in potential duration within the 25–34 (blue) and 35–44 (red) age groups is driven solely by differences in labor force attachment in the 7 years preceding UI entry. The orange series instead restricts attention to claimants with the maximum possible benefit duration given their age, so that the variation in duration is driven only by age. For completeness, I also include estimated effects for ages 45–54 (green series), a group for whom benefit duration varies for both reasons. Each specification includes the same controls used in the benchmark specification of Figure 6.

Appendix Figure 5: Implied reemployment effects for the 2005 cohort under alternative censoring horizons



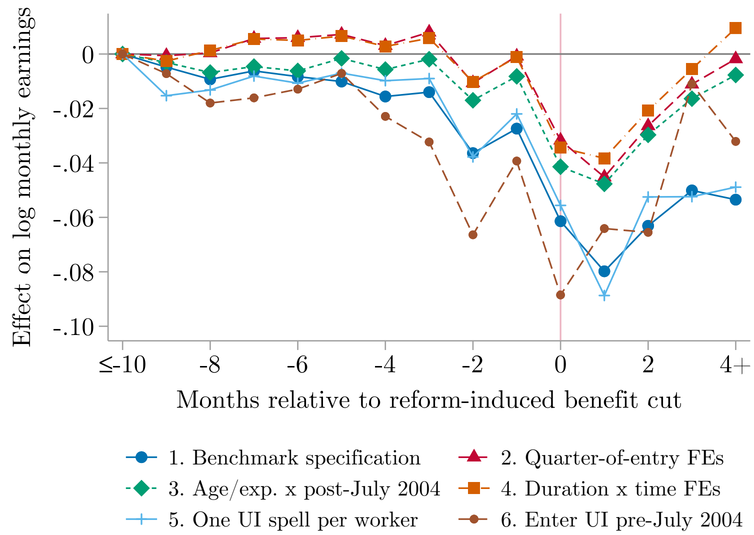
Notes: Implied effects of Hartz IV on reemployment rates for the fully exposed 2005 cohort, obtained by reestimating the benchmark specification with incomplete spells censored at either 1 year, 2 years, or 3 years. See Section 6.5 for details on the quantification procedure. Points are offset horizontally for visual clarity. Capped spikes denote 95 percent confidence intervals, based on 500 draws from the estimated variance-covariance matrix.

Appendix Figure 6: Cross-group relationship between the severity of benefit cuts and the increase in job-finding in the month Hartz IV binds



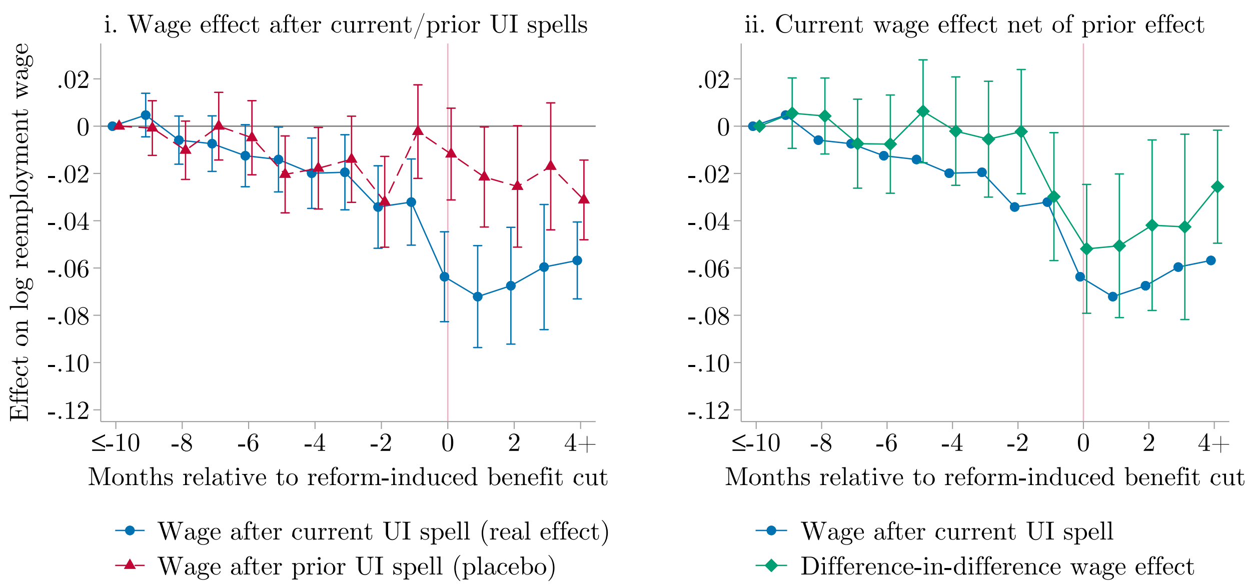
Notes: Each point corresponds to one of 36 cells defined by sex × West/East residence × three household types × terciles of the initial short-term UI benefit level. I estimate my benchmark hazard model separately for each cell, pooling the event months $\{-9,-8,-7\}$, $\{-6,-5,-4\}$, $\{-3,-2,-1\}$, $\{0\}$, $\{1,2,3\}$, and $\{4,5,6,\ldots\}$ to improve precision in these subgroup models. For each cell, I plot the estimated hazard effect for the month of the Hartz IV benefit cut $(\exp(\hat{\delta}_0^H)-1)$ against the mean simulated change in either long-term cash benefits (left panel) or post-exhaustion net household income (right panel), based on the OECD Tax-Benefit Model. Marker sizes are weighted by the number of UI claims in each subsample.

Appendix Figure 7: Robustness of wage effects to alternative specifications



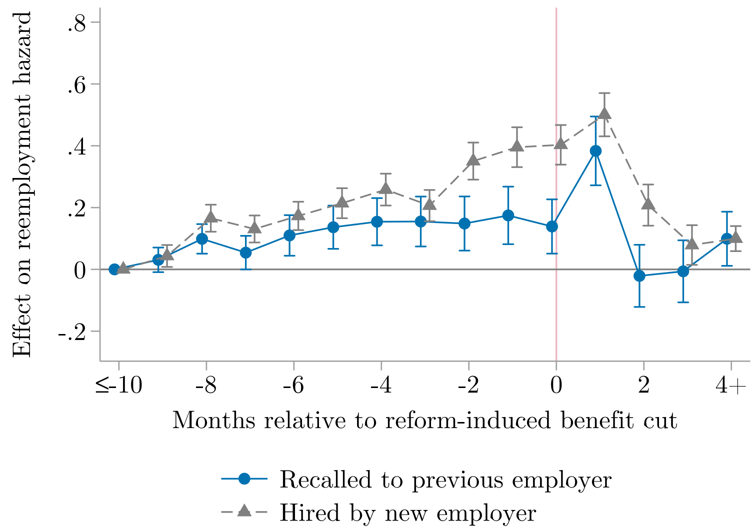
Notes: Estimated effects of reform-induced long-term benefit cuts on the log ratio of monthly earnings in the first socially insured job after UI to monthly earnings in the job that preceded entry to UI, from variants of the regression estimated in Figure 12. See the notes to Figure 7 for a discussion of the control variables and sample restrictions used in each specification. The same estimates are presented (with standard errors) in Online Appendix Table 2.

Appendix Figure 8: Robustness of wage effects to (persistent) unobserved heterogeneity



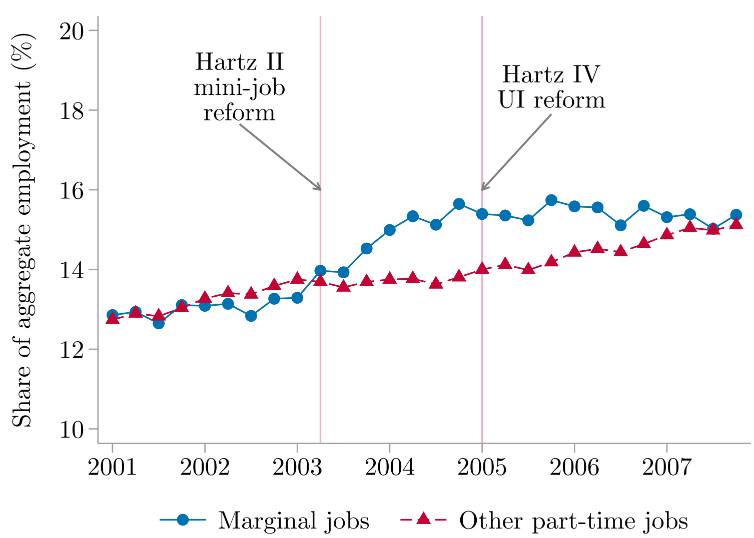
Notes: This figure restricts attention to UI claims for which I observe a *prior* claim by the same worker over 1994–2005. In both panels, the blue series plot the estimated effects of the Hartz IV benefit cuts from my benchmark wage specification (Figure 12) for this restricted subsample. In the left panel, the red series replaces the outcome variable by the log ratio of post-UI to pre-UI wages in the *prior* UI claim, keeping all explanatory variables coded at their original values. In the right panel, the green series replaces the outcome variable by the difference between the log wage changes in the two UI spells. Points are offset horizontally for visual clarity. Capped spikes denote 95 percent confidence intervals, clustering on individual.

Appendix Figure 9: Effects of long-term benefit cuts on competing risks of recalls vs. new employers



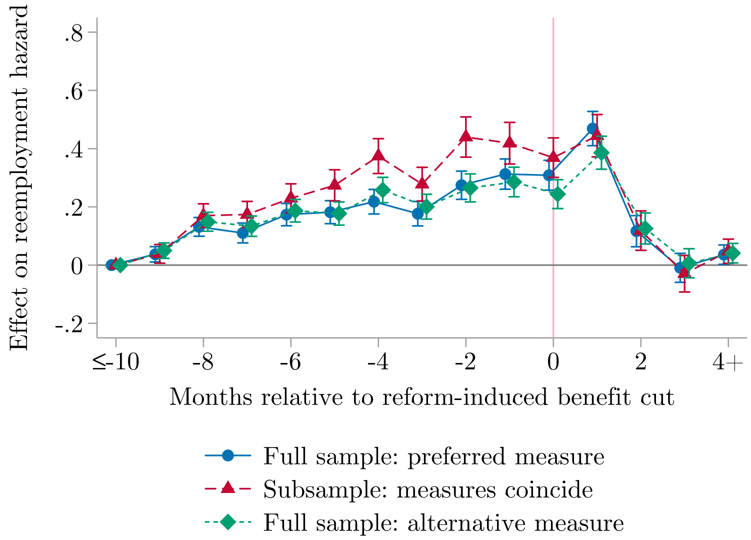
Notes: Estimated proportional effects of reform-induced long-term benefit cuts on the competing risks of recall and of being hired by a new employer (based on whether the pre-UI and post-UI employer identifiers coincide). I include the same explanatory variables used in the benchmark hazard specification. Points are offset horizontally for visual clarity. Capped spikes denote 95 percent confidence intervals, clustering on individual.

Appendix Figure 10: Aggregate growth in part-time marginal and non-marginal employment



Notes: Part-time marginal and non-marginal employment as a share of the aggregate, based on public-use data from the Federal Employment Agency. The April 2003 Hartz II reform expanded the definition of marginal jobs ("mini-jobs") by eliminating the hours ceiling, raising the earnings ceiling, and permitting socially insured workers to hold mini-jobs on the side without incurring extra tax liability. I exclude such side-jobs from both the numerator and the denominator.

Appendix Figure 11: Hazard effects using "ex post" vs. "ex ante" measures of potential benefit duration



Notes: The first series replicates my benchmark specification on the full estimation sample. The second series restricts attention to UI claimants for whom my preferred ("ex post") and alternative ("ex ante") measures of potential benefit duration (PBD) coincide exactly. The third series returns to the full sample but now uses my alternative PBD measure, which relies solely on information observed by the start of a new UI claim.

Appendix Figure 12: Path of implied reemployment effects based on an alternative linear probability model

Notes: Implied effects of Hartz IV on individual reemployment rates, based on the linear probability model given in Equation D.1. The outcome variable is an indicator either for having *ever* been reemployed by the indicated horizon or for *currently* being employed as of that date. I use the fitted values to predict the mean of each outcome variable both under under Hartz IV and under a counterfactual scenario where Hartz IV does not occur. I compute the mean gap between these predicted values for the 2005 cohort of UI entrants, who are fully exposed to the post-reform benefit schedule. Spikes denote 95 percent confidence intervals, based on 500 draws from the estimated variance-covariance matrix.

Appendix Table 1: Effects of long-term benefit cuts on reemployment hazards

	Omitted: ≥ 10 months before	(1)	(2)	(3)	(4)	(5)	(6)
Months relative to reform-induced benefit cut	9 months before	0.04***	0.05***	0.04***	0.04***	0.05***	0.02
		(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
	8 months before	0.13***	0.17***	0.14***	0.13***	0.15***	0.09***
		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
	7 months before	0.11***	0.13***	0.11***	0.10***	0.16***	0.12***
		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
	6 months before	0.17***	0.21***	0.18***	0.17***	0.24***	0.23***
		(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
	5 months before	0.18***	0.24***	0.24***	0.21***	0.23***	0.23***
		(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
	4 months before	0.22***	0.27***	0.28***	0.25***	0.31***	0.24***
		(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
	3 months before	0.18***	0.24***	0.24***	0.21***	0.27***	0.21***
		(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)
	2 months before	0.27***	0.36***	0.36***	0.34***	0.37***	0.33***
		(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)
	1 month before	0.31***	0.42***	0.41***	0.39***	0.43***	0.39***
		(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)
	Month of change	0.31***	0.43***	0.40***	0.39***	0.40***	0.37***
		(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.06)
	1 month after	0.47***	0.62***	0.61***	0.60***	0.55***	0.45***
		(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)
	2 months after	0.12***	0.25***	0.23***	0.23***	0.19***	0.09*
		(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)
	3 months after	–0.01	0.13***	0.11***	0.12***	0.04	–0.14***
		(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
	4+ months after	0.04**	0.24***	0.20***	0.25***	0.08***	0.09**
		(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.05)
	Quarter-of-entry FEs		X				
	Age/experience x post-July 2004			X			
	Duration x time interactions				X		
	One UI spell per individual					X	
	Enter UI prior to July 2004						X
	Number of UI claims	336,634	336,634	336,634	336,634	244,666	240,463
	Number of distinct claimants	244,666	244,666	244,666	244,666	244,666	190,527
	Log likelihood	–847354.27	–846265.75	–847174.01	–847046.79	–604911.65	–601402.84

Notes: Each column reports normalized hazard ratios for the periods preceding and following reform-induced benefit cuts, relative to the omitted category of being observed 10 or more months before these cuts occur. The benchmark hazard specification is replicated in column 1, and estimates from these same specifications are plotted in Figure 7. Standard errors in parentheses are clustered on individual. * $p \leq .10$, ** $p \leq .05$, *** $p \leq .01$.

Appendix Table 2: Effects of long-term benefit cuts on log reemployment wages

Omitted: ≥ 10 months before	(1)	(2)	(3)	(4)	(5)	(6)
9 months before	–0.0048	–0.0006	–0.0031	–0.0026	–0.0153**	–0.0072
	(0.0043)	(0.0043)	(0.0044)	(0.0044)	(0.0061)	(0.0055)
8 months before	–0.0093**	0.0004	–0.0068	0.0013	–0.0132**	–0.0180***
	(0.0047)	(0.0048)	(0.0051)	(0.0051)	(0.0067)	(0.0064)
7 months before	–0.0062	0.0057	–0.0046	0.0055	–0.0081	–0.0161**
	(0.0054)	(0.0054)	(0.0057)	(0.0057)	(0.0072)	(0.0076)
6 months before	–0.0083	0.0060	–0.0062	0.0050	–0.0107	–0.0129
	(0.0058)	(0.0059)	(0.0063)	(0.0062)	(0.0078)	(0.0088)
5 months before	–0.0101	0.0072	–0.0016	0.0066	–0.0070	–0.0071
	(0.0062)	(0.0064)	(0.0071)	(0.0066)	(0.0081)	(0.0106)
4 months before	–0.0156**	0.0031	–0.0057	0.0028	–0.0098	–0.0229*
	(0.0066)	(0.0068)	(0.0075)	(0.0070)	(0.0086)	(0.0118)
3 months before	–0.0140**	0.0080	–0.0019	0.0058	–0.0090	–0.0323**
	(0.0072)	(0.0073)	(0.0080)	(0.0075)	(0.0093)	(0.0129)
2 months before	–0.0364***	–0.0106	–0.0170*	–0.0102	–0.0378***	–0.0664***
	(0.0077)	(0.0079)	(0.0087)	(0.0082)	(0.0099)	(0.0157)
1 month before	–0.0274***	–0.0008	–0.0082	–0.0011	–0.0220**	–0.0393**
	(0.0080)	(0.0083)	(0.0091)	(0.0085)	(0.0103)	(0.0165)
Month of change	–0.0614***	–0.0317***	–0.0414***	–0.0344***	–0.0556***	–0.0885***
	(0.0085)	(0.0087)	(0.0095)	(0.0089)	(0.0108)	(0.0191)
1 month after	–0.0798***	–0.0452***	–0.0477***	–0.0383***	–0.0887***	–0.0641***
	(0.0093)	(0.0097)	(0.0105)	(0.0100)	(0.0115)	(0.0215)
2 months after	–0.0631***	–0.0264**	–0.0297**	–0.0208*	–0.0525***	–0.0655***
	(0.0108)	(0.0112)	(0.0118)	(0.0114)	(0.0134)	(0.0235)
3 months after	–0.0501***	–0.0111	–0.0164	–0.0055	–0.0524***	–0.0110
	(0.0117)	(0.0121)	(0.0127)	(0.0123)	(0.0146)	(0.0235)
4+ months after	–0.0535***	–0.0018	–0.0077	0.0095	–0.0489***	–0.0321*
	(0.0070)	(0.0088)	(0.0094)	(0.0092)	(0.0085)	(0.0186)
Quarter-of-entry FEs		X				
Age/experience x post-July 2004			X			
Duration x time interactions				X		
One UI spell per individual					X	
Enter UI prior to July 2004						X
Number of UI claims	246,679	246,679	246,679	246,679	162,704	173,293
Number of distinct claimants	169,166	169,166	169,166	169,166	162,704	131,474
Log likelihood	–106787.08	–106623.19	–106630.68	–106555.06	–81633.70	–74295.49

Months relative to reform-induced benefit cut

Notes: Each column reports log wage effects for the periods preceding and following reform-induced benefit cuts, relative to the omitted category of being reemployed 10 or more months before these cuts occur. The benchmark wage specification is replicated in column 1, and estimates from these same specifications are plotted in Online Appendix Figure 7. Standard errors in parentheses are clustered on individual. * $p \leq .10$, ** $p \leq .05$, *** $p \leq .01$.

Appendix Table 3: Hazard effects of real (2005) and placebo (pre-2005) Hartz IV reforms (benchmark specification, estimated using a 2 percent sample of claimants)

Months relative to reform-induced benefit cut	Assumed year of placebo/real Hartz IV reform							
Omitted: ≥ 10 months before	1998	1999	2000	2001	2002	2003	2004	2005
9 months before	0.20***	–0.10***	–0.10***	–0.07***	–0.06***	–0.01	0.01	0.06***
	(0.06)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
8 months before	0.09	–0.06**	–0.02	–0.03	–0.02	–0.03	0.01	0.12***
	(0.06)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
7 months before	0.03	0.03	–0.03	0.06**	–0.02	–0.05*	0.03	0.12***
	(0.06)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
6 months before	0.10	0.08**	0.00	–0.04	0.03	0.07**	0.16***	0.23***
	(0.07)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
5 months before	0.06	0.05	–0.04	–0.06*	–0.02	–0.03	0.19***	0.24***
	(0.07)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
4 months before	0.10	–0.00	–0.08**	0.01	–0.00	0.03	0.08**	0.18***
	(0.07)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
3 months before	0.10	–0.05	–0.03	–0.07**	–0.06*	0.05	0.09***	0.25***
	(0.07)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
2 months before	–0.02	0.05	–0.05	–0.06*	–0.04	–0.04	0.15***	0.21***
	(0.07)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
1 month before	0.10	0.08*	–0.06*	–0.10***	–0.06	–0.09**	0.11***	0.30***
	(0.09)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)
Month of change	–0.15**	0.06	–0.01	–0.06	–0.10**	–0.10**	0.18***	0.40***
	(0.07)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)
1+ months after	–0.06	0.00	–0.08***	–0.03	–0.13***	–0.15***	0.11***	0.12***
	(0.07)	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Number of UI claims	46,337	73,827	101,641	128,051	127,916	131,954	135,271	136,719
Number of distinct claimants	41,425	60,834	78,174	93,599	93,902	97,969	100,449	101,161
Log likelihood	83163.6	–156512.0	–228848.6	–295700.7	–289575.4	–293604.3	–302256.6	–308088.6

Notes: Estimated proportional hazard effects of real/placebo UI reforms, using either the actual reform date (January 1, 2005) or a placebo date (January 1 of 2001, 2002, 2003, or 2004). For each "reform" year Y, I construct a 2 percent sample of new UI claims begun between January 1 of year $Y-4$ and June 30 of year Y, then recode the time-to-Hartz event-time variable to measure time until the supposed reform binds. The same estimates are plotted in the left panel of Figure 8. Standard errors in parentheses are clustered on individual. * $p \leq .10$, ** $p \leq .05$, *** $p \leq .01$.

Appendix Table 4: Hazard effects of real and placebo Hartz IV reforms
(augmented with duration × time effects, estimated using a 2 percent sample of claimants)

Months relative to reform-induced benefit cut		Assumed year of placebo/real Hartz IV reform							
	Omitted: ≥ 10 months before	1998	1999	2000	2001	2002	2003	2004	2005
	9 months before	0.20***	–0.10***	–0.10***	–0.07***	–0.06***	0.00	0.01	0.06***
		(0.06)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
	8 months before	0.09*	–0.07**	–0.04	–0.03	–0.05*	–0.06**	–0.05**	0.10***
		(0.06)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)
	7 months before	0.03	0.03	–0.05*	0.06**	–0.04	–0.07***	–0.02	0.11***
		(0.06)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
	6 months before	0.10	0.07**	–0.00	–0.03	0.00	0.04	0.09***	0.22***
		(0.07)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
	5 months before	0.06	0.05	–0.03	–0.05	–0.04	–0.09***	0.14***	0.25***
		(0.07)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
	4 months before	0.10	0.00	–0.07**	0.02	–0.02	–0.04	0.03	0.20***
		(0.07)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
	3 months before	0.10	–0.04	–0.03	–0.05	–0.08**	–0.02	0.04	0.27***
		(0.07)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
	2 months before	–0.03	0.07	–0.05	–0.04	–0.06	–0.12***	0.06	0.26***
		(0.07)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)
	1 month before	0.09	0.10**	–0.06	–0.08*	–0.08*	–0.17***	0.04	0.36***
		(0.09)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)
	Month of change	–0.16**	0.07	–0.02	–0.04	–0.11**	–0.18***	0.11**	0.46***
		(0.07)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.06)
	1+ months after	–0.08	0.05	–0.07*	0.02	–0.07*	–0.26***	0.12***	0.30***
		(0.07)	(0.05)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.05)
	Number of UI claims	46,337	73,827	101,641	128,051	127,916	131,954	135,271	136,719
	Number of distinct claimants	41,425	60,834	78,174	93,599	93,902	97,969	100,449	101,161
	Log likelihood	83160.7	–156482.7	–228786.9	–295585.2	–289514.3	–293527.9	–302156.9	–307990.5

Notes: Estimated proportional hazard effects of real/placebo UI reforms, using either the actual reform date (January 1, 2005) or a placebo date (January 1 of 2001, 2002, 2003, or 2004). For each "reform" year Y, I construct a 2 percent sample of new UI claims begun between January 1 of year $Y-4$ and June 30 of year Y, then recode the time-to-Hartz event-time variable to measure time until the supposed reform binds. I augment the benchmark specification with a full set of interactions between 3-month duration bins and quarter × year effects. The same estimates are plotted in the right panel of Figure 8. Standard errors in parentheses are clustered on individual. * $p \leq .10$, ** $p \leq .05$, *** $p \leq .01$.

Appendix Table 5: "Ex post" and "ex ante" measures of potential UI benefit duration (PBD)

	All	"Veterans"	"Rookies"
Ex post PBD measure	344.58	321.26	388.40
	153.95	148.82	153.85
Ex ante PBD measure	345.73	316.95	399.82
	154.74	147.02	154.40
Corr(ex post, ex ante)	0.90	0.90	0.89
Ex post PBD = ex ante PBD	0.68	0.62	0.81
Discrepancy <= 30 days	0.76	0.72	0.84
Number of UI claims	336,634	219,725	116,909

Notes: My preferred, "ex post" measure of potential UI benefit duration calculates PBD based on the completed duration of each UI claim, together with a variable indicating the number of unused benefit-days remaining at the end of each spell. My alternative, "ex ante" measure calculates PBD based solely on information observed by the start of a new UI claim. I subdivide the sample into UI "veterans"—claimants who have received short-term UI benefits sometime in the 7 years preceding the current claim—and UI "rookies", who have not. My ex ante measure of PBD is likely to perform better for rookies than for veterans, as complex carry-forward provisions make it difficult to accurately determine UI eligibility for recurrent UI claimants.