Labor Market Adjustment to Globalization, Automation, and Institutional Reform

by

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B.A. Economics–Political Science, Columbia University, 2009

Submitted to the Department of Economics
in partial fulfillment of the requirements for the degree of

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Abstract 

This thesis analyzes how national and local labor markets reequilibrate after shocks to labor market institutions (Chapter I), demand (II–III), and supply (IV). 

Chapter I analyzes Germany’s 2005 Hartz IV reform, which lowered the generosity of long-term UI benefits available once short-term benefits run out. Using administrative records, I exploit cross-worker heterogeneity in the timing of when Hartz IV bites to estimate how long-term benefit cuts affect jobless durations, wages, and job characteristics. The job-finding hazard starts rising several months before cuts bind, culminating in a larger “spike at UI exhaustion” under Hartz IV. I find that UI reform reduced the probability of a one-year jobless spell by 12.4 percent, with employment gains driven by full-time jobs. Consistent with lower reservation utility, workers experiencing benefit cuts accept lower-paying jobs. 

Chapter II (joint with Daron Acemoglu, David Autor, David Dorn, and Gordon Hanson) argues that Chinese import competition, which surged after 2000, was a major force behind both recent reductions in US manufacturing employment and—through input-output linkages and other general equilibrium channels—weak overall job growth. Our central estimates suggest import-induced job losses over 1999–2011 in the range of 2.0–2.4 million. 

Chapter III (joint as above) reassesses the conventional wisdom that IT is revolutionizing productivity while making workers redundant. Examining IT usage in US manufacturing, we find only mixed evidence of faster productivity growth in IT-intensive industries. Surprisingly, output in IT-intensive industries falls relative to other manufacturing industries. Productivity increases, when detectable, reflect even faster employment declines. 

Chapter IV exploits German high school reforms to estimate the labor market effects of sharp fluctuations in cohort size. These reforms, which eliminated grade 13 at upper-track high schools, led to an idiosyncratically timed “double cohort” in each reforming state, as students graduated under both old and new curricula. Consistent with the fact that a modest share of upper-track students enter firm-based apprenticeships after graduation, new training contracts jump by about 2 percent in double-cohort years. This increase is driven by upper-track graduates; I find no clear evidence that other graduates are crowded out, but the results are imprecise.

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Graduate school has been a trying experience, and I learned early and often how ill-equipped I would have been to go it alone. I am deeply indebted to everyone who gave me intellectual, emotional, financial, and logistical support during my time at MIT Economics.

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Chapters II and III are coauthored with Daron Acemoglu, David Autor, David Dorn, and Gordon Hanson. From David Dorn I learned about local labor markets, as well as many unglamorous but essential aspects of data construction. Gordon taught me much about international trade and graciously shared his thoughts on the PhD job market. It has been a privilege to work with them.

I am grateful to the MIT Schultz Fund, the Hewlett Foundation, and the Alfred P. Sloan Foundation for funding much of my graduate education. Stefan Bender and Daniela Hochfellner, together with many personnel at Germany’s Institute for Employment Research (IAB), facilitated access to the administrative data used in Chapter I. I am also grateful to the Harvard Economics staff, especially Peter Brown, Clare Dingwell, Edward Silva, and Katherine Zuccala, for providing reliable access to the Harvard IAB data room. They brightened otherwise dreary days. At MIT, Gary King and Beata Shuster put out countless small fires over the years. Thomas Dattilo, Lauren Fahey, Eva Konomi, and Aaron Sullivan all provided warm and timely help throughout the chaos of the job market. As placement officer, Ben Olken gave sound counsel from start to merciful end.

Throughout my time in Cambridge, I have been surrounded by an amazing set of likeminded labor and public-finance colleagues. Alex Bartik has been a thought partner and friend since the very beginning; we’re drawn to the same ideas. My thinking about labor markets has been colored by countless conversations with Sydnee Caldwell, John Coglianese, Colin Gray, Sally Hudson, Simon Jäger, Christina Patterson, and Melanie Wasserman. Colin, Simon, and Christina generously read early drafts of Chapter I and the paper is much the better for it. Simon was a boon companion during long data sessions in what we affectionately called the Besenkammer, and he helped enormously with German translation, institutional questions, and job market strategy. More broadly, it has been a tremendous honor to meet and befriend so many first-rate applied microeconomists, among them Enrico Cantoni, Ludovica Gazzè, Kyle Greenberg, Peter Hull, Angela Kilby, Ray Kluender, Scott Nelson, Michael Stepner, Yufei Wu, and many others. Their stars are bright.

I remain keenly aware of how much I owe to John Beshears, James Choi, David Laibson, and Brigitte Madrian, who hired me as a research assistant right after college. They were devoted mentors and prepared me well for the business of independent research. Lalith Munasinghe, my undergraduate adviser, sparked my interest in labor economics and put me on the primrose path to a PhD. All errors can ultimately be traced back to him.
Many close friends kept me grounded as I wrestled, and struggled, with the hardest thing I have ever done. Ruth Aronoff and I traded war stories from the PhD trenches, though it’s hard to compete with a geologist. Elizabeth Cronson gave untold advice and support at many difficult moments, and I’ve semi-consciously modeled my sense of judgment after hers. Matt Moran got me to run a half-marathon, see the Southwest, and keep up some semblance of a social calendar. Sarah Armitage has taught me something of “how to be idle and blessed”, and much else besides. Andrew Scheineson has for over a decade provided running commentary on my myriad faults, the better to keep them under wraps. Richard Corcoran, my onetime English teacher, has continued to read and refine the poetry I write on the side. I am lucky to know them.

Some portion of this thesis belongs to my dear late friend and fellow social scientist Natasha Chichilnisky-Heal, whose own dissertation would have been brilliant. I think of you often, Tasha, and remember lines by Millay: “I think our heart-strings were, like warp and woof / In some firm fabric, woven in and out; / Your golden filaments in fair design / Across my duller fibre.”

Last but first, my loving parents—together with my brothers, aunt and uncle, and the rest of my family—have endured the blow-by-blow of my doctoral ups-and-downs. May they someday forgive me for choosing this over law school.

***

The research process, with all its stress, uncertainty, sublimity, and promise, calls to my mind a passage I like from T.S. Eliot’s “East Coker”:

So here I am, in the middle way, having had twenty years—
Twenty years largely wasted, the years of l’entre deux guerres—
Trying to use words, and every attempt
Is a wholly new start, and a different kind of failure
Because one has only learnt to get the better of words
For the thing one no longer has to say, or the way in which
One is no longer disposed to say it. And so each venture
Is a new beginning, a raid on the inarticulate,
With shabby equipment always deteriorating
In the general mess of imprecision of feeling,
Undisciplined squads of emotion. And what there is to conquer
By strength and submission, has already been discovered
Once or twice, or several times, by men whom one cannot hope
To emulate—but there is no competition—
There is only the fight to recover what has been lost
And found and lost again and again: and now, under conditions
That seem unpropitious. But perhaps neither gain nor loss.
For us, there is only the trying. The rest is not our business.

I’ve given this my all, and here’s what I have to show for my efforts. The rest is not my business.

Brendan Price
May 15, 2017
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Chapter I

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

1 Introduction

Many displaced workers exhaust their unemployment benefits before returning to work. This is especially true during recessions, when jobs are hardest to find (Schmieder et al., 2012). 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 unemployment benefits loom especially large for workers at the greatest risk of experiencing lengthy jobless spells, which erode employment prospects, deplete savings, and impose fiscal externalities through transfer payments and foregone tax revenue (Kroft et al., 2013; Ganong and Noel, 2016; Nekoei and Weber, 2016). Yet despite the widespread use of two-tiered benefit schedules, and despite renewed interest in long-term unemployment in the wake of the Great Recession, little is known about how long-term UI benefits affect jobless durations and subsequent earnings.

This paper analyzes the employment and wage effects of Germany’s 2005 Hartz IV reform, a prominent and controversial measure that reduced long-term benefit levels for both new and incumbent UI claimants. On January 1, 2005, existing long-term UI recipients—who numbered 2.2 million and comprised 5.3 percent of the civilian labor force on the eve of reform (Figure 1)—were switched overnight to the new, typically lower, post-reform benefit level. Subsequent inflows into long-term UI were subject to the new rules upon exhausting their initial stream of short-term benefits. The lack of grandfathering for incumbent claimants, together with concurrent changes in labor market conditions and institutions, poses difficulties for some of the standard quasi-experimental methods.
that are used to evaluate UI reforms. To overcome these challenges, I exploit cross-worker and cross-cohort variation in the timing of Hartz IV’s effective onset—based on individual heterogeneity in the potential duration of short-term benefits—to identify the causal effects of policy-induced benefit cuts. Using administrative data on over 336,000 UI claims made by prime-age displaced workers during 2001–2005, I find that exposure to the reform increases the hazard rate of reemployment beginning several months before the cut takes 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 post-reform benefit schedule reduces the likelihood of experiencing a one-year jobless spell by 12.4 percent.

I then analyze how benefit cuts affect the wages workers receive upon being reemployed. To motivate this analysis, Figure 2 plots the mean difference in log monthly earnings before and after UI as a function of completed jobless duration for workers in my estimation sample. 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 after modal short-term exhaustion points. To assess whether Hartz IV has contributed to this pattern, I extend the empirical framework to estimate the effects of long-term benefit cuts on the wages paid in a worker’s first job after entering UI. As prior research has noted, these effects are theoretically ambiguous (Nekoei and Weber, 2016; Schmieder et al., 2016). 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 continued unemployment. Consistent with falling reservation wages, I find that workers who accept jobs after exhausting short-term benefits earn 4 to 8 percent lower wages than they would have absent the reform, conditional on their completed duration. On the other hand, if time out of work reduces earnings capacity through stigma or skill depreciation, then benefit cuts may actually mitigate wage declines by shortening jobless durations. Furthermore, since post-UI wages are only observed if a claimant finds work within the sample period, benefit cuts introduce a standard selection problem by influencing transitions into reemployment. To quantify the net wage effect accounting for this selection margin, I jointly estimate the impact of Hartz IV on jobless durations and reemployment wages. I then decompose the overall effect into three components: direct effects at a given duration, indirect effects associated with changes in duration, and a compositional effect due to changing selection into reemployment. The negative direct effect dominates: adjusting for compositional changes in the observable characteristics of successful jobseekers, I estimate that
Hartz IV reduced mean reemployment wages by 1.9 percent.

To shed additional light on the mechanisms underlying these wage effects, I next develop a competing-risks framework to examine how Hartz IV 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, a bounding exercise using the part-time share of employment suggests that my wage results likely reflect reductions in hourly wages rather than shifts along the intensive margin of labor supply. Second, I distinguish transitions into new jobs from recalls to the previous employer and find large, positive effects on both hazards. Insofar as recalls amount to options that workers choose to execute—rather than “search” as traditionally conceived—the increased recall hazard supports my interpretation that reform-induced benefit cuts have worsened jobseekers’ outside options in the event of remaining out of work. Third, I track transitions into a legally defined class of low-paid, part-time 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 reemployment, I find that Hartz IV diverted claimants from mini-jobs into jobs covered by social insurance. This result adds an important nuance to the received wisdom that Germany’s UI reforms have fueled the growth of mini-jobs.

This paper makes two main contributions. First, I conduct the first unified analysis of the effects of reductions in long-term UI benefit levels on jobless durations, subsequent wages, and job characteristics. Although an extensive literature has examined changes in the level and potential duration of short-term UI benefits, much less is known about the long-term benefits at the heart of my study.1 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 hazard analysis confirms that cutting long-term benefits leads to economically significant increases in individual job-finding rates. My

---

1 Building on earlier work by Shavell and Weiss (1979) and Hopenhayn and Nicolini (1997), Kolsrud et al. (2016) show that the optimal slope of the benefit schedule depends on how benefits paid at different durations affect incentives for job search. Applying a regression-kink design to Swedish data, they find that workers respond throughout their spells to changes in long-term benefit levels, but less so than they do to comparable changes in short-term benefits. I complement Kolsrud et al. by using a different design applied to a different policy change in a different institutional setting. Unlike their study, I also analyze the impact of long-term benefits on subsequent wages and job characteristics—outcomes with first-order welfare effects in the presence of fiscal externalities (Chetty, 2006).
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, 2016). I find that reductions in long-term UI generosity have negative effects on net wages, echoing similar findings for short-term generosity obtained by Nekoei and Weber (2016) in their study of Austrian benefit extensions.

Second, I present the first quasi-experimental evaluation of the microeconomic effects of Hartz IV, one of the most prominent social insurance reforms in recent memory. Throughout Europe, Hartz IV—the centerpiece of a broader package of “Hartz reforms” enacted in the mid-2000s—has become a byword for policies designed to strengthen job-search incentives among the unemployed. But despite intense interest from both academics and policymakers, previous studies have not credibly isolated variation in exposure to Hartz IV, which was rolled out simultaneously and uniformly throughout Germany. Nagl and Weber (2014) find that claimants return to work faster after the reform than before it, but their estimates are identified by time-series variation and do not account for other labor market reforms enacted during this period. Engbom et al. (2015) show that wages among previously displaced workers fell sharply relative to those of non-displaced workers during and after the Hartz reforms, 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 calibrated 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). These papers reach disparate conclusions about the impact of benefit cuts on steady-state unemployment, with estimates ranging from 0.1 to 2.8 percentage points. Although my paper is silent on general equilibrium mechanisms such as

---

2 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) observes, “In many respects, 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.

3 Both of these papers also exclude transitional cohorts that were partly exposed to UI reform, leaving protracted gaps between the pre- and post-reform comparison groups. As discussed in Section 4, retaining these interim cohorts allows me to control more flexibly for aggregate time effects, including both changes in the macroeconomic environment and other elements of the Hartz reforms, and to obtain estimates valid for the full population impacted by the reform.

4 In contemporaneous work, Bradley and Kuegler (2016) calibrate the aggregate effects of the successive Hartz reforms on unemployment and wages among male German workers using a structural macroeconomic search and matching model. Whereas the current paper identifies the effects of Hartz IV by exploiting quasi-experimental cross-worker and over-time variation in the effective timing of benefit cuts relative to the start of workers’ unemployment spells, the Bradley-Kuegler model is fit to high-level aggregate moments including monthly job-finding rates, separation rates, and wages among new hires, and does not leverage within-period policy variation in effective exposure to
congestion externalities or job creation, credible estimates of the partial equilibrium effects are an important input into understanding Hartz IV's aggregate impact. A back-of-the-envelope calculation suggests that these partial equilibrium effects—if not augmented or offset by general equilibrium forces—may have lowered Germany's steady-state unemployment rate by 0.9 percentage points. This calculation is merely illustrative: a careful reckoning of Hartz IV's aggregate effects is beyond the scope of this paper.

The rest of the paper is structured as follows. Section 2 describes Germany's UI system and explains how Hartz IV altered the benefit schedule. Section 3 uses a model of job search to motivate the empirical strategy. Section 4 describes the data and research design. Section 5 estimates the effects of benefit cuts on job-finding and jobless durations. Section 6 analyzes wages. Section 7 uses a competing-risks approach to distinguish among transitions into full-time, part-time, and mini-jobs, as well as between new jobs and recalls. Section 8 concludes.

2 Reform of the German UI System

Prior to 2005, Germany had a two-tiered UI system consisting of time-limited unemployment benefits (Arbeitslosengeld) which, when exhausted, could be followed by means-tested unemployment assistance (Arbeitslosenhilfe). I refer to these sequential benefit streams as “short-term” and “long-term” benefits, respectively.5

Hartz IV left short-term benefits unaltered. To establish an entitlement to short-term benefits, an individual must have worked at least 12 months over the preceding 3 years. In the event of job loss, eligible workers are entitled to benefits equal to 60 percent of their prior after-tax earnings (67 percent for claimants with dependent children), averaged over the past year or, if more favorable, two years. Benefit payments are not taxed, and claimants may work at most 15 hours per week under a net earnings disregard equal to the larger of €616.5 per month or 20 percent of prior earnings. The potential duration of short-term benefits, $P$ (in months), is a step function that depends on age at claim initiation ($a$) and on months of covered work experience accrued over the

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5 "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 with notions of "long-term unemployment" that depend only on duration since the start of unemployment, without regard to UI eligibility.
preceding seven years \((x)\):

\[
P = \min(\bar{P}(a), 2 \cdot \text{floor}(x/4)),
\]

where \(\bar{P}(a)\) is an age-specific maximum duration. Hence 12 months of work experience yield 6 months of benefits, and every 4 months of additional work experience extend short-term duration by 2 months. For workers under 45, benefits cannot exceed 12 months. From age 45 onward, the maximum duration rises first to 18 months, then to 22 months (at age 47), then to 26 months (at age 52), and finally to 32 months (at age 57). Figure 3 summarizes the benefit accrual rules applicable to workers in my estimation sample (ages 25–54 at claim initiation). Although new entitlements established under these rules last at least 6 months, shorter potential durations are possible in cases of seasonal employment or the resumption of an unexhausted benefit claim.\(^6\) Figure 4, discussed further in the data section, plots the observed variation in 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 the initial benefit stream, long-term benefits were means-tested on the basis of household assets and income. For a worker passing the means test, the benefit level equaled 53 percent of former net earnings (57 percent with dependent children), with benefit payments falling one-for-one with spousal earnings. Long-term benefits were unlimited in duration, provided that claimants continued to satisfy the means test. Poor households could also apply for supplemental means-tested welfare to top off their UI benefits. The combination of a generous long-term UI benefit level—in many cases only slightly below the short-term level—and indefinite duration set Germany apart from its European neighbors (Wunsch, 2006). The UI caseload grew steadily in the early 2000s: by June 2004, 2.2 million workers were claiming long-term UI benefits, on top of another 1.7 million claiming short-term benefits (Figure 1). The growing fiscal burden, together with a widespread belief that the safety net was too generous, created political pressure for labor market reform.

In March 2002, the center-left government convened a commission led by former Volkswagen director Peter Hartz to recommend a reform package. The Hartz Commission’s report, released in August 2002, proposed a wide range of measures intended to put the unemployed back to work

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\(^6\) Seasonal workers who do not satisfy the normal eligibility criteria are entitled to 3 months of benefits if they have worked at least 6 months, or to 4 months of benefits for 8+ months of work. Furthermore, a claimant who exits UI without having used up her short-term benefits remains entitled to her remaining benefits in the event of a new job loss. If she establishes a fresh entitlement in the interim, she is entitled to 6 months of benefits plus her residual benefits from the previous entitlement, with the sum capped at the age-specific maximum. Due to these carryover provisions, potential duration (in days) can assume any integer value from 1 to the maximum.
through a mixture of carrots and sticks. The first reform measures, which came into effect 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 the UI system is widely regarded as the centerpiece of the entire reform package. 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 welfare payments into a single, means-tested income-support program. In contrast to the old regime, long-term benefits are no longer indexed to prior wages. Instead, each long-term claimant now receives 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 rent and heating. To cushion the drop in benefit level, some long-term UI recipients were also eligible for a temporary supplemental payment. Means testing was tightened relative to pre-reform criteria, and changes in benefit levels were accompanied by tightening of job search requirements and sanctions for those who violated program rules.

Although the post-reform UI system was broadly less generous than its predecessor, some claimants saw their benefit levels—inclusive of supplemental payments and in-kind assistance with rent and utilities—go up after the reform. To establish that Hartz IV had bite, and to gauge the “first-stage” 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 for each claimant in my estimation sample (which is described in Sections 4.1 and 4.2). Some important inputs, notably household assets and spousal earnings, are not reported in the microdata. Appendix A describes my simulation algorithm, together with the imputations I perform to account

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7 See Ebbinghaus and Eichhorst (2009) for a lengthier discussion of the overall reform package, which had many components. Tompson (2009) provides a detailed account of the political context.
8 Writing about the Hartz IV benefit cuts in The New York Times, Landler (November 26, 2004) noted, “Economists say these will be the most important measures in the whole package.”
9 The supplement lasted up to two years and was halved in the second year. Many workers did not qualify for the supplement, however, and those who did faced further step-downs as the supplement waned and then expired. I include this supplement in the benefit simulations described below and calculate reform-induced changes in potential household income just after short-term benefit exhaustion, when the supplement is maximized. See Appendix A for details.
10 Around the time of implementation, Bloš and Rudolph (2005) estimated that one-sixth of long-term recipients would lose their long-term benefits entirely, but that roughly half of remaining claimants would receive a benefit increase. Bloš and Rudolph find, as do I, that benefit cuts among those facing cuts were generally larger in magnitude than benefit gains among those receiving gains.
for unreported inputs. I feed each claimant’s observed and imputed characteristics through the 2004 and 2005 tax and benefit rules. To account for 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 financial impact of Hartz IV as the hypothetical change in post-exhaustion income that a household would experience as a result of switching from 2004 to 2005 rules:

\[
\% \Delta \text{ (household income)} = \frac{\Delta (\text{net household income, 2005 vs. 2004 rules})}{\text{net household income, 2004 rules}}
\] (2)

Given the imputations needed to simulate net income in these data, this measure should be interpreted with caution. Nonetheless, it provides a useful window into how Hartz IV altered household income after benefit exhaustion.

Figure 5 plots the simulated distribution of reform-induced income changes for the claimants in my sample. About 80 percent of men and 70 percent of women incur apparent drops in (potential) post-exhaustion household income as a result of Hartz IV. The median claimant faces a prospective decline in net household income of about 5 percent, with nearly one-quarter of claimants confronting declines of over 10 percent. There is a large point mass near zero, mainly representing married claimants who are ineligible for long-term UI under both old and new rules by virtue of having a high-earning spouse. Fewer than 5 percent of claimants experience benefit gains exceeding 5 percent of prior net income. Several additional features of Hartz IV made long-term benefit receipt less attractive than these numbers imply. The reform included stricter means testing of household assets as well as stricter application of benefit sanctions for long-term UI recipients. Many claimants faced additional step-downs at lengthier durations, as the temporary supplement declined and then expired. On the whole, it appears that Hartz IV made long-term unemployment less appealing for most workers, with countervailing benefit increases generally modest and transitory. 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 additional aspects of Hartz IV warrant emphasis. 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, generally lower benefit level.11 My research design in Section 4 is expressly designed to account for this policy feature. Second, the reform became publicly salient in July 2004. Shortly after final passage of the reform act, the

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11 Legal challenges casting the benefit cuts as an illegal seizure of property were ultimately dismissed by the courts.
government mailed existing claimants a 16-page questionnaire meant to gauge their eligibility for long-term benefits under the new, stricter means test (Tompson, 2009). The questionnaire sparked considerable confusion and led to protests against Hartz IV in dozens of German cities (BBC News, August 17, 2004). The publicity surrounding these events helps rationalize the forward-looking behavioral responses I find throughout the paper.12 Third, because Hartz IV replaced a wage-indexed benefit with a flat, standardized payment, individuals with greater prior earnings tended to receive higher long-term 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 be correlated with prior earnings, which in turn may be correlated with responsiveness to a given benefit cut. My core identification strategy sidesteps this concern by relying on the timing of benefit changes rather than their simulated magnitude. I discuss heterogeneous benefit changes further in Sections 5.4 and 5.6.

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.13 Critically for my purposes, existing claims were not subject to these subsequent reforms. I restrict my sample to UI spells beginning prior to 2006, so that my analysis is not confounded by changes in short-term UI eligibility or duration.14

3 Theoretical Framework

To clarify how reductions in long-term UI benefits might affect individual jobless durations and subsequent wages, I use a continuous-time job search model in the spirit of Mortensen (1977) to

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12 Consistent with this timing, Google Trends shows a sharp uptick in searches for the terms “Hartz IV” and “Arbeitslosengeld” in the summer of 2004 (Appendix Figure 1). “Hartz IV” was also the 2004 German word of the year. As the chairman of Germany’s language association stated at the time, “No term has been used so often this year, at least in politics. Even someone who does not know about Mr. Hartz understands something by the term Hartz IV” (Frankfurter Allgemeine Zeitung, December 10, 2004, own translation). Although July 2004 was clearly an information event, attentive claimants may have suspected that benefit cuts were coming well before this date. I discuss the timing of anticipatory behavior in the context of a placebo exercise in Section 5.5 and Appendix B.

13 Under a deferred provision of Hartz III, the lookback period for establishing a UI entitlement was reduced from 3 years to 2 years; special provisions for seasonal workers were eliminated as well. Under the Labour Market Reform Act—a law adopted simultaneously with Hartz IV but technically separate from it—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 proved deeply unpopular and were partly reversed in 2008 (Lichter, 2016).

14 Dlugosz et al. (2014) show that, in the weeks preceding February 1, some workers strategically timed job losses so as to be covered under the old rules. Although the number of retimers was small, strategic retiming could lead to compositional changes that would threaten identification of Hartz IV’s effects. I show in Section 5.4 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.
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 approach zero in the limit as cuts lie increasingly far in the future. This limiting result offers a theoretical justification for my research design, which presumes that 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 entails disutility \( \psi(s) \), where \( \psi(\cdot) \) is convex and satisfies Inada-type 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 \( R \geq 0 \) denote the remaining duration of benefits at a given point in time (so that \( R = P \) at the beginning of the claim), UI benefits equal

\[
b(R) = \begin{cases} 
\bar{b} & \text{if } R > 0 \\
\tilde{b} & \text{if } R = 0
\end{cases}
\]

with \( 0 < \bar{b} < \tilde{b} \). (3)

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 \( \bar{b} \), 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 \( J(w) = \frac{u(w)}{s} \) denote the indirect utility from being employed forever at wage \( w \). As in Mortensen (1977), the optimal policy entails a cutoff strategy with reservation wage \( \underline{w} \), so that \( U(R) \) admits a Bellman representation:

\[
\delta U(R) = \max_{s, \underline{w}} u(b(R)) - \psi(s) + s(1 - G(\underline{w}))(E(J(w) \mid w \geq \underline{w}) - U(R)) - \dot{U}(R)
\]

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

(a) A long-term benefit cut increases search intensity and decreases the reservation wage throughout the unemployment spell. That is, \( \frac{ds(R)}{db} < 0 \) and \( \frac{dw(R)}{db} > 0 \) for all \( R \geq 0 \). It follows immediately that \( \frac{d\lambda(R)}{db} < 0 \), 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,

\[
\lim_{R \to \infty} \frac{ds(R)}{db} = \lim_{R \to \infty} \frac{dw(R)}{db} = \lim_{R \to \infty} \frac{d\lambda(R)}{db} = 0
\]

(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 unemployment less attractive, and workers try harder to escape unemployment before exhausting short-term benefits. Part (b) reflects discounting: when \( R \) is very large, search intensity and reservation wages asymptote to the values workers would choose if benefits remained perpetually at the elevated level \( b \). These limiting values are invariant to \( b \). Part (c) captures the ambiguous effect of UI generosity on post-UI earnings, noted previously by Schmieder et al. (2016) and Nekoei and Weber (2016).

Proof of (a). There is no closed-form solution for the optimal policies without functional form assumptions, 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 \( b \) 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.

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

\[
J(w(R)) = U(R) \tag{5}
\]

\[
\psi'(s(R)) = (1 - G(w(R)))(E(J(w) \mid w \geq w) - U(R)) \tag{6}
\]

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

$$U(0) = \frac{u(b) - \psi(s(0)) + s(0)(1 - G(w(0)))E(J(w) | w \geq w(0))}{\delta + s(0)(1 - G(w(0)))}$$

(7)

so that, invoking the envelope theorem,

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

Using this property, and again applying the envelope theorem,

$$\frac{dU(R)}{db} = \exp(-\delta R)Pr(\text{reach exhaustion})\frac{dU(0)}{db}$$

$$= \exp(-\delta R) \exp \left( - \int_0^R \lambda(x)dx \right) \frac{dU(0)}{db}$$

$$> 0$$

(8)

This expression implies that $\frac{d}{dR} \left( \frac{dU(R)}{db} \right) < 0$. 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.

Next, applying the implicit function theorem to (5) and (6), and using $J(w) = \frac{u(w)}{\delta}$, yields

$$\frac{dw(R)}{dU(R)} = \frac{\delta}{u'(w(R))} \frac{dU(R)}{db} > 0$$

for all $R$

(9)

and

$$\frac{ds(R)}{db} = -\frac{1}{\psi''(s(R))}(1 - G(w(R))) \frac{dU(R)}{db} < 0$$

for all $R$

(10)

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

$$\frac{d\lambda(R)}{db} = \frac{ds(R)}{db}(1 - G(w(R))) - s(R)g(w(R)) \frac{dw(R)}{db} < 0$$

for all $R$

(11)

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 the generous amount \( \bar{b} \) in perpetuity. This is a stationary problem with value \( U^* \) defined by

\[
\delta U^* = \max_{s, w} \left( u(b) - \psi(s) + s(1 - G(w)) (E(J(w) \mid w \geq w) - U^*) \right),
\]

(12)

and with associated (stationary) policies \( s^* \) and \( w^* \) and associated hazard rate \( \lambda^* = s^*(1 - G(w^*)) \).

A revealed-preference argument shows that \( U(R) \) limits to \( U^* \). Under the true benefit schedule \( b(R) \), let \( \tilde{U}(\cdot) \) denote the payoff from adopting strategies \( s^* \) and \( w^* \) for all \( R > 0 \) and then switching to the true optimal policies \( s(0) \) and \( w(0) \) 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,

\[
U^* - \tilde{U}(R) = \exp(-\delta \lambda^*) R (U^* - U(0)).
\]

(13)

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

Returning to the first-order conditions in (5) and (6), take limits as \( R \to \infty \). By continuity of \( u, \psi, \) and \( G \), the policy functions approach well-defined limits: \( \lim_{R \to \infty} s(R) = s^* \) and \( \lim_{R \to \infty} w(R) = w^* \). Now take the limit as \( R \to \infty \) in (9) and (10). By continuity, both expressions limit to zero, proving (b).

Proof of (c). Finally, consider how benefit cuts affect accepted wages, i.e. the wage measure observed by the econometrician. Let \( \bar{w}(R(D)) = \mathbb{E}(w \mid w \geq w(R(D))) \) denote the average accepted wage among workers reemployed \( D \) months into their unemployment spell, with \( R(D) = \max(0, P - D) \) denoting the remaining duration until benefit exhaustion at this time. Let \( f(D) = \exp \left( -\int_0^D \lambda(x)dx \right) \lambda(R(D)) \) 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:

\[
\mathbb{E}(w) = \int_0^\infty f(D) \bar{w}(R(D)) dD.
\]

(14)

Differentiating with respect to \( b \),

\[
\frac{d\mathbb{E}(w)}{db} = \int_0^\infty f(D) \frac{d\bar{w}(R(D))}{db} dD + \int_0^\infty \frac{df(D)}{db} \bar{w}(R(D)) dD.
\]

(15)
The first term is the reservation wage effect, which can be shown to equal

\[ \int_0^\infty f(D) \left\{ \frac{g(w(R(D)))}{1 - G(w(R(D)))} (\bar{w}(R(D)) - w(R(D))) \frac{dw(R(D))}{db} \right\} dD > 0. \]  

(16)

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 the reservation wage and hence \( \bar{w}(R(D)) \) are higher. Thus the second term is negative, proving (c).\(^{16}\)

In a more general model with duration-dependent earnings potential, the duration effect may be amplified because shorter durations lead to less stigma or loss of skills. An additional selection term emerges if, as in practice, workers are heterogeneous and not everyone is reemployed within the sample period. I pursue this idea in Section 6, where I decompose the net wage effect of Hartz IV into reservation wage, duration, and selection components.

4 Data and Research Design

4.1 Worker-level administrative data

To study the effects of UI reform on unemployed workers’ jobless durations and eventual 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. The source data are used by federal tax and benefit authorities for the assessment of social insurance contributions and for the calculation of UI benefits. I accessed two anonymized extracts under agreement with the data custodian, the Institute for Employment Research (IAB).

Most of my analysis relies on 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 information (including average daily earnings, part-time/full-time status, establishment ID, and industry), and detailed information about periods of unemployment and UI receipt. These work

\(^{16}\) In a closely related model, Nekoei and Weber (2016) prove that marginal changes in the level of short-term benefits have indeterminate effects on reemployment wages: under different functional forms, either force may dominate.
and 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, some parts of my analysis rely on the SIAB, a 2 percent random sample of all individuals who appear in the underlying data universe anytime during 1975–2010 (vom Berge et al., 2013). For each worker, the SIAB reports all of the data elements listed above.

These datasets have several limitations relevant to my analysis. First, I do not observe all of the inputs into the means-testing procedure that determines eligibility for long-term benefits under the old or new rules. Notable omissions include household assets and spousal earnings. These missing inputs make it impossible to know precisely how an individual’s net benefit schedule was changed by Hartz IV, and they preclude me from studying the effect of benefit cuts on asset drawdowns, spousal labor supply, and similar outcomes. 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 and 2006 as a result of administrative transitions during the rollout of Hartz IV. These data gaps prevent me from analyzing the realized durations of long-term benefit receipt, but they pose no other challenges to my analysis. Other limitations include minimal detail about hours worked, topcoding of the earnings variable at the contribution limit for social security, and a lack of direct information about UI eligibility for workers who never initiate a UI claim. Finally, the underlying social security data exclude civil servants and the self-employed. The source data cover about 80 percent of total German employment.

4.2 Constructing the estimation sample

My core estimation sample consists of prime-age displaced workers who entered UI during 2001–2005. Restricting attention to UI claimants excludes both individuals who are ineligible for UI and those who, though eligible, choose not to take up benefits. This restriction is needed for the accurate calculation of potential short-term benefit duration, the key source of identifying variation used in this paper.

Using the AED, I first select UI claims initiated between January 1, 2001 and December 31, 2005. To ensure that I am capturing new unemployment spells, rather than the resumption of benefits after brief interruptions, I drop claimants who received short-term benefits in the 90 days preceding the new claim. Second, I restrict to claimants aged 25–54 at entry into UI to abstract from

17 These endpoints are 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 (see Footnote 13). My sample includes workers who entered UI (i) before Hartz IV was enacted, (ii) between enactment and implementation, and (iii) in the year after Hartz IV took effect.
apprenticeship training, higher education, and retirement decisions. Third, I restrict to claimants who separated from a job covered by social insurance sometime in the 30 days preceding UI receipt. About three-quarters of new UI spells satisfy this criterion, with the remainder preceded either by unrecorded statuses like self-employment, civil service, and university enrollment or by voluntary quits, which preclude a worker from claiming benefits for 12 weeks after separation. Requiring an observed separation allows me to better measure the start of nonemployment, and it ensures that I always observe the features of the worker’s previous job. Since quitters are excluded, the sample consists of 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 later that my results are robust to randomly selecting one UI claim per individual.

Unemployment, employment, and benefit receipt are recorded at daily frequency. Since benefit eligibility accrues in 60-day increments (not calendar months), I divide each UI spell into 30-day periods that I call “months”. I follow each spell until the exact date of reemployment. In Sections 5 and 6, 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 are legally constrained to pay at most €325–400 per month and which are often held concurrently with benefit receipt (Tazhditinova, 2016). To the extent that workers seek 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 workers to find gainful employment (and of their earnings potential upon doing so). I revisit mini-jobs in Section 7, where I distinguish transitions into regular jobs vs. mini-jobs. I censor unfinished spells at 24 months. Using a two-year horizon ensures that all spells are censored prior to the 2008 financial crisis, which may have had important effects on claimant behavior. I also report specifications that instead censor spells at 12 or 36 months.

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18 The specific age cutoffs that I choose correspond to special rules governing benefit sanctions 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).

19 Individuals who transition to UI from an unobserved state are also presumably at the greatest risk of transitioning back into such a state. For instance, self-employed workers may be more likely than salaried workers to return to self-employment after exiting UI. Consistent with this reasoning, excluding untraceable spells increases the share of workers for whom I observe post-UI jobs.

20 I drop the few claimants who hold regular jobs when entering UI, since “jobless duration” is ill-defined in these cases. Furthermore, since earnings in regular jobs typically exceed the earnings disregard for UI recipients, these cases may reflect erroneous recording of employment or benefit receipt dates. By contrast, it is not unusual for new UI claimants to hold mini-jobs on the day they enter UI. Since holding a mini-job need not preclude UI receipt, I retain such claimants when analyzing transitions into regular employment.

21 I do not censor spells for any other reason (such as withdrawal from the labor force) because (i) in most cases
Employers are required by law to report each worker's average daily earnings at least once per year. I deflate earnings to 2005 EUR and 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. 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. Except where noted, I control for underlying earnings potential by assigning workers to deciles of prior wage within cells defined by sex × West/East German residence × year of UI entry.

I assign each worker to one of three education groups using an algorithm from Fitzenberger et al. (2006) to impute missing levels of education, which employers often fail to report. I code each worker as a German native or non-native based on the earliest reported nationality. I partition claimants into seven age bins (25–29, 30–34, 35–39, 40–44, 45–46, 47–51, and 52–54, where the over-45 age bins mirror the age-specific ceilings in the potential short-term benefit schedule) and three household types (unmarried, married without children, married with children) based on their age and household structure at the beginning of the claim. To control for heterogeneity in labor force attachment, I compute days of regular employment during the seven years preceding UI receipt and assign workers to one-year work-history bins, which I call "experience".

Table 1 presents summary statistics both for the core estimation sample and for a comparison group of prime-age employed workers drawn from the representative 2 percent SIAB extract. The estimation sample comprises roughly 210,000 new UI claims among 144,000 distinct men and 127,000 new claims among 101,000 distinct women. Relative to the typical employed worker, claimants are adversely selected along a number of margins. About one-third of claims originate in the economically distressed states of the former East Germany (inclusive of Berlin), which represent just one-fifth of German employment. Mean pre-tax monthly earnings prior to job loss were €2,051 for men and €1,546 for women, considerably lower than mean earnings in the comparison group (€2,904 and €2,030, respectively), and claimants are less likely to have been employed for at least four of the preceding seven years. Claimants are younger than their employed counterparts, less likely to be German natives, and more likely to belong to the lowest education group (though female such people are still at risk of reappearing in the employment records later on; (ii) deregistration from unemployment may be endogenous to future UI benefits; and (iii) the 2005 UI reform created a seam in the unemployment rolls by obligating welfare recipients to register as unemployed for the first time. This data seam does not affect the employment and short-term UI records used in my analysis, but it would confound measurement of labor force exit.

Using unadjusted wage reports yields very similar, but slightly noisier estimates.
claimants are also more likely to be university educated). The mean initial UI benefit, equal to 60 or 67 percent of prior post-tax earnings, was €898 per month among men and €656 per month among women. Nearly a quarter of men and one-third of women exhaust their short-term benefits. After UI receipt, the distribution of completed jobless durations is markedly bimodal: among men, 52.3 percent of spells end within the first six months, but fully 22.4 percent last over two years; reemployment rates among women are uniformly lower but similarly bimodal. Among claimants returning to regular work within two years of UI onset, average monthly earnings in the first new job were €1,936 for men and €1,465 for women, about 5 percent lower than average pre-UI earnings in the full sample.

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. Figure 4 plots the distribution of potential short-term benefit durations across new UI spells. About half of all claimants are located at their age-specific duration ceilings, especially the 12-month ceiling that applies to workers under age 45. Just under one-fifth of claimants (necessarily ages 45–54) have potential durations exceeding 12 months. A similar fraction have durations less than or equal to six months. I exploit variation in benefit eligibility between and within age groups to identify the causal effects of Hartz IV.

4.3 Hazard specification

In this section, I develop a dynamic econometric model that separately identifies the pre-reform “main effect” of benefit exhaustion and the incremental effect of the benefit changes induced by Hartz IV. The model allows me to control flexibly for calendar time effects, compositional changes, and other important determinants of individual employment prospects. Later, I will extend the model to look at wages and job types in conjunction with durations.

23 Half the sample is unmarried (with or without children), and about half of married claimants have at least one dependent child. Household structure is reported in the unemployment register and hence is generally not observed for the comparison group.

24 Let $R$ denote the duration of these residual benefits, let $D$ denote the completed duration of a short-term benefit spell, and let $P(a)$ denote the age-specific maximum benefit duration, with all variables now expressed in days. I compute start-of-spell potential benefit duration as $P = \min(P(a), D + R)$. That is, I set the benefit duration equal to the (observed) completed duration plus any time remaining in the worker’s claim, overriding the result if it exceeds the legal maximum. This procedure yields sensible results; for instance, the distribution of $D + R$ has large point masses at the age-specific maximums $P(a)$, as expected given the eligibility rules. Because I observe year but not date of birth, I cannot perfectly determine $P(a)$ 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.
Let worker $i$ begin a UI spell on date $u_i$. I group the data into 30-day increments, indexed by $d$. For each worker, I define two key durations corresponding to changes in the UI benefit level (superscripted with $E$ for exhaustion or $H$ for Hartz). The first of these, $d_i^E$, is the earliest duration at which the worker has exhausted short-term benefits. Formally,

$$d_i^E \equiv \min\{d \in \mathbb{N} \mid 30d \geq P_i\},$$

where $P_i$ denotes potential days of short-term benefits at the outset of the spell. The second event, $d_i^H$, denotes the earliest duration at which the worker receives long-term benefits under the post-reform rules. Let $d_i^{2005} \equiv \min\{d \in \mathbb{N} \mid u_i + 30d \geq \text{January 1, 2005}\}$ denote the first duration observed after the reform legislation takes effect. Then

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

i.e., the larger of short-term exhaustion and the Hartz IV implementation date. 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 6 plots hypothetical examples of these events for successive cohorts of claimants with 12 months of potential benefits at baseline.

Following common practice in the UI literature, I estimate a discrete-time proportional hazard model using the complementary log-log link (e.g., Prentice and Gloeckler 1978; Meyer 1990).\(^{26}\) Letting $D_i$ denote the completed jobless duration, I specify the conditional probability of being reemployed during the 30-day interval $(d - 1, d]$ as

$$\lambda_{id} \equiv \Pr(D_i = d \mid D_i > d - 1) = 1 - \exp(-\exp(x_{id}'\beta)),$$

where the instantaneous log hazard rate is given by

$$x_{id}'\beta = \alpha_d + \gamma_t + \phi' + \sum_{k=-9}^{4} \delta_k^E 1\{r_{id}^E = k\} + \sum_{k=-9}^{4} \delta_k^H 1\{r_{id}^H = k\}$$

and $t \equiv u_i + 30d$ denotes the calendar date at the end of the interval. I estimate the model via

\(^{25}\) A disproportionate share of jobs begin on the first of the month, giving rise to an inherent periodicity that makes 30-day intervals a natural choice. Using 15-day periods yields similar results.\(^{26}\) The complementary log-log formulation can be derived directly from the survival function under the assumption that the instantaneous hazard rate is constant within 30-day intervals. I estimate all hazard models in Stata using the command cloglog.
maximum likelihood, with standard errors clustered by individual to allow for arbitrary correlation for workers with repeated spells.

In (20), \( \alpha_d \) represents a full set of duration dummies, allowing job-finding rates to vary freely as a function of months since beginning a claim—in the parlance of survival analysis, I allow for a nonparametric baseline hazard. The shifters \( \gamma_t \) control for a variety of effects linked to calendar time. First, I include a full set of quarter \( \times \) year interactions, allowing the hazard rate to evolve flexibly 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 calendar effects, due for example to hiring associated with the fiscal year. Third, with slight abuse of notation, I also take \( \gamma_t \) 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.\(^{27}\) \( z_{id} \) is a set of additional control variables, specified below.

The key explanatory variables are flexible functions of event time relative to each benefit cut, defined as

\[
\begin{align*}
\tau_{id}^E & \equiv \min\{d - d_i^E, 4\} \quad \text{(months relative to short-term benefit exhaustion)} \\
\tau_{id}^H & \equiv \min\{d - d_i^H, 4\} \quad \text{(months relative to reform-induced benefit cut)}
\end{align*}
\]

(21)

The associated coefficients \( \delta_k^E \) and \( \delta_k^H \) allow the hazard rate to vary flexibly in a window around each benefit change. Note that each omitted group comprises periods 10 or more months before the benefit change, and that I pool periods 4 or more months after the change into a single coefficient.\(^{28}\) I report the normalized hazard ratios \( \exp(\delta_k^E) - 1 \) and \( \exp(\delta_k^H) - 1 \), which represent the predicted proportional change in the instantaneous reemployment hazard associated with event time \( \tau_{id}^E \) or \( \tau_{id}^H \) with respect to the corresponding benefit change, relative to the predicted hazard 10 or more months before this event occurs. In using observations for which benefit changes lie far in the future as the reference group, 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 behavioral

\(^{27}\)Recalls are a simple example: they are concentrated in the spring, and they typically occur within six months of layoff. Including these seasonal controls yields smoother estimates, but my results are qualitatively and quantitatively robust to omitting them.

\(^{28}\)The chosen endpoints are to some degree arbitrary. Note, however, that if the “left” endpoint were to exceed 12 months pre-exhaustion, then some of the coefficients would be identified solely by claimants with over 12 months of benefits (who comprise under one-fifth of the sample). 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.
responses to future benefit cuts tend to zero at sufficiently long horizons.\textsuperscript{29} To the extent that claimants already start responding to future benefit cuts as early as 10 months beforehand, my estimates will be a 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.\textsuperscript{30} In all specifications, therefore, \( z_{id} \) 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 \( z_{id} \) account for other demographic characteristics that are correlated with job-finding rates. 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 the onset of UI receipt, so that they are fixed within each spell.

I conduct the analysis separately for men and women. Jobless durations differ markedly by sex, and disparities in average spousal earnings provide an a priori reason why the effects of Hartz IV might differ by sex.

5 Effects on Jobless Durations

5.1 Descriptive evidence

Before proceeding to the main hazard specification, I begin with a descriptive comparison of job-finding rates among claimants entering UI in 2001 ("pre-reform") vs. those entering UI in 2005 ("post-reform"). The 2001 cohort is de facto not exposed to Hartz IV, since all of these claims conclude (either through reemployment or through censoring at 24 months) prior to passage of the reform legislation. The 2005 cohort is fully exposed: upon exhausting short-term benefits, these claimants are immediately subject to the new long-term benefit level.

As a starting point, Figure 7 plots the raw empirical hazard functions for claimants under 45 with exactly 12 months of potential short-term benefits (the maximum possible in this age range).

\textsuperscript{29}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.

\textsuperscript{30}For example, a worker who is entitled to only 6 months of benefits by virtue of limited experience must have \( \tau^b_{id} \geq -6 \). Similarly, older workers are less likely to be observed with high values of \( \tau^b_{id} \) and \( \tau^b_{id} \), since their short-term benefits typically last longer.
The hazard functions are typical of those found in studies of unemployment: job-finding rates initially rise with duration (reflecting recalls from temporary layoffs, especially among men) and subsequently decline, with an uptick in the vicinity of short-term benefit exhaustion, when claimants face benefit step-downs under both pre- and post-reform rules. At short durations, claimants belonging to the pre-reform cohort have a higher reemployment hazard, perhaps reflecting the tighter labor market of 2001. As short-term benefit exhaustion approaches, however, the post-reform hazard rate overtakes the pre-reform hazard. This simple comparison offers prima facie evidence that post-reform claimants are more responsive to short-term benefit exhaustion under the (less generous) post-reform benefit schedule.

Without variation in potential benefit duration, one cannot distinguish time since UI entry from time until exhaustion. To show that Figure 7 reflects greater responsiveness to exhaustion per se—rather than changes in the baseline hazard rate irrespective of benefit duration—I next expand the sample to include all workers who entered UI in either 2001 or 2005, regardless of their potential benefit duration. Because all of these claimants are subject to a fixed benefit schedule during the sample window, I estimate a simplified version of my hazard specification that is suitable for claimants facing only a single benefit step-down. Concretely, for each cohort \( Y \in \{2001, 2005\} \), I run a discrete-time hazard model that replaces the instantaneous log hazard rate from (20) with

\[
x'_{id} \beta = \alpha_d + \gamma_t + \sum_{k=-9}^{4} \delta_k^Y \mathbf{1}\{\tau_{id} = k\},
\]

where the event-time coefficients \( \delta_k^{2001} \) and \( \delta_k^{2005} \) capture changes in job-finding as workers approach benefit exhaustion under the old and new UI rules, respectively. This exercise is in the spirit of difference-in-differences, with variation both across cohorts and, within each cohort, across claimants with different exhaustion points.

Figure 8 plots the normalized hazard ratios \( \exp(\delta_k^Y) - 1 \) obtained by estimating this model separately on the 2001 and 2005 cohorts. The blue and green series replicate a classic finding from research on UI: prior to reform, there is a clear “spike” in the reemployment hazard at the point of benefit exhaustion (Moffitt, 1985; Meyer, 1990; Katz and Meyer, 1990a). More striking is the pre/post change: the orange and red series reveal a much stronger spike at exhaustion for claimants

\footnote{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 the reemployment hazard at the time of benefit exhaustion. The estimates plotted in Figures 7 and 8—as with similar plots throughout the paper—reflect true job-finding, not simply an artifact of deregistration from unemployment.}
subject to Hartz IV.

5.2 Benchmark estimates

By design, Figures 7 and 8 abstract 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, either directly (if they remained unemployed as of 2005) or indirectly (through forward-looking changes in search intensity or reservation wages). This applies especially to claimants who entered UI during 2002-2004, between the “pre” and “post” cohorts analyzed above. The lack of grandfathering presents a difficult identification challenge that has not been addressed by prior studies: a “clean” comparison between cohorts that are either fully exposed or not exposed to the new benefit schedule obligates the econometrician to compare cohorts spaced several years apart, even if (as in Figure 8) one can isolate within-cohort variation in exposure to benefit cuts. The long gap between pre- and post-periods amplifies the potential for intervening changes in labor demand, credit supply, or institutional reforms—such as other components of the Hartz package that were adopted in 2003 and 2004—to confound identification of the causal effects of benefit cuts. 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.

The benchmark specification laid out in Section 4.3 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 9 plots the estimated effect of these benefit step-downs on the hazard rate of reemployment, separately for men and women. Focusing first on men, the blue series in Figure 9a shows that the hazard rate of reemployment first declines slightly in the months leading up to short-term exhaustion, then rises sharply. 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. The point estimate for $\tau_{id}^E = 0$ indicates that the hazard rate at exhaustion is about 25 percent higher than the hazard rate 10 or more months prior to exhaustion. These estimates mirror the pre-reform exhaustion effects previously shown in Figure 8.

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32 If incumbent claimants had been shielded from the reform, one could use a regression discontinuity design to compare claimants who enter UI just before/after January 1, 2005. These groups would face sharp differences in long-term UI generosity but similar labor market conditions and institutions. Absent grandfathering, however, these groups are equally exposed to the new benefit schedule.
The orange series shows the estimated causal effect of Hartz IV on the reemployment hazard. As workers approach reform-induced benefit cuts, the normalized hazard ratio rises steadily, peaking at 0.48 in the month that straddles the benefit step-down ($r_{id}^H = 0$). This pattern of positive hazard effects in the months preceding a benefit cut is indicative of forward-looking behavior on the part of UI claimants, as predicted by the search model in Section 3. The behavioral response is large: relative to claimants for whom Hartz IV lies 10 or more months in the future, claimants experiencing benefit cuts are 48 percent more likely to return to regular employment over a short time interval. The coefficients decline in the wake of the Hartz IV benefit change, then stabilize close to zero. 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 at exhaustion is estimated to be 85 percent greater than the hazard rate when exhaustion lies 10 or more months away ($\exp(\hat{\delta}_0^F + \hat{\delta}_0^H) - 1$), conditional on duration since UI entry and other observables. This is in the same ballpark as the exhaustion spike measured for post-reform male claimants in the simpler specification used in Figure 8.

Figure 9b replicates these plots for women. The results are broadly similar to those for men, with the incremental hazard effect peaking at 0.54 in the month that straddles the reform-induced benefit change. A stark difference, however, is the much stronger main effect of short-term benefit exhaustion (green series) for women than for men. This likely reflects the role of spousal income in means testing: on average, married women have higher earning spouses than married men do, and many married women are ineligible for long-term benefits under both the pre- and post-reform regimes. The pronounced exhaustion spike appears to be driven by especially sharp benefit reductions for women at this juncture. As with men, the post-reform exhaustion spike depends on the exponentiated sum of the main and incremental effects: under the new schedule, my estimates imply that the hazard rate at exhaustion is 181 percent greater than the hazard rate when exhaustion occurs far in the future, again conditioning on observables.

A question that arises here is why the Hartz IV coefficients (and those pertaining to benefit exhaustion itself) decline post-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 hazard rate of exit rises in the run-up to a benefit cut and then stays constant. Even if this model perfectly describes individual behavior, however, unobserved individual heterogeneity can readily generate

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33 Consistent with this hypothesis, the spike at short-term exhaustion is much more pronounced for married women than for singles. Male claimants, by contrast, show fairly similar main effects of short-term exhaustion regardless of marital status—a plausible result given Germany's relatively low rate of female labor force participation.
attenuation over time. Intuitively, if the workers who are most sensitive to benefit reductions return to work in advance of exhausting benefits, the claimants who remain will be the ones least responsive to benefit cuts. Such heterogeneity could stem from differences either in individual preferences (e.g., the cost of job search) or in the change in benefit generosity experienced under Hartz IV. In either case, my estimates reflect the local average treatment effect within a dynamically changing risk set. Dynamic selection is inherent to duration models (Kiefer, 1988). Perhaps for this reason, studies of benefit exhaustion generally show a falling-off of the hazard effect: labor economists speak of an exhaustion “spike”, not an exhaustion “plateau”.

5.3 Effect sizes

How do these 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. To see this, write the cumulative reemployment rate recursively as $F(d) = \sum_{k=1}^{d} f(k) = \sum_{k=1}^{d} S(k-1)\lambda(k)$, where $S(\cdot) = 1 - F(\cdot)$ is the survival function. Letting $S^c(\cdot)$ and $\lambda^c(\cdot)$ denote counterfactual survival and hazard rates absent reform, a useful approximation to the change in reemployment is

$$dF(d) \approx \sum_{k=1}^{d} S^c(k-1)\lambda^c(k)\log\lambda(k).$$

(23)

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 benefit generosity matters more when workers tend to reach long-term unemployment and when, upon doing so, they are on the margin of finding work.

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. I explain the procedure in detail, as similar exercises appear later in the paper.

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34 The UI literature has advanced alternative explanations for the exhaustion spike. 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. (2016) 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 don’t search as hard.
For each UI spell, I predict the discrete-time hazard rate at each duration as

\[ \hat{\lambda}_{id} \equiv \Pr(D_i = d \mid D_i > d - 1) \equiv 1 - \exp(-\exp(x'_{id} \hat{\beta})). \]  

(24)

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

\[ \hat{F}_{id} \equiv \Pr(D_i \leq d) \equiv 1 - \prod_{s=1}^{d} (1 - \hat{\lambda}_{is}). \]  

(25)

To predict reemployment rates with the reform-induced benefit cuts shut off, I recompute this expression with the time-to-Hartz IV variable \( \tau^{H}_{id} \) recoded to the omitted category at all durations. That is, I replace \( x'_{id} \hat{\beta} \) with

\[ x'^{ef}_{id} \hat{\beta} \equiv \hat{\alpha}_{id} + \hat{\gamma}_t + x'_{id} \hat{\phi} + \sum_{k=-9}^{4} \hat{\delta}_k \mathbf{1}\{\tau^{F}_{id} = k\}, \]  

(26)

which partials out the estimated effect of long-term benefit cuts induced by Hartz IV. Using this expression, I compute counterfactual reemployment rates \( \hat{F}_{id}^{cf} \) using the same procedure as above. Finally, I compute the average (partial equilibrium) effect of the reform on claimants entering UI in year \( y \) as

\[ \hat{\Delta} F^{y}_{d} \equiv \frac{1}{N^y_d} \sum_{i\mid u \in y} (\hat{F}_{id} - \hat{F}_{id}^{cf}). \]  

(27)

Table 2 reports \( \hat{\Delta} F^{y}_{d} \) by entry cohort for \( d \in \{6, 12, 18, 24\} \) months, separately for men and women. For claimants who enter UI in 2001, the predicted effect of Hartz IV is mechanically zero: two years into a UI spell, all workers in this cohort are still at least 10 months away from experiencing benefit cuts under Hartz IV, so that \( \hat{F}_{id}^{ef} = \hat{F}_{id} \) by construction. At the opposite extreme, claimants who enter UI in 2005 are fully exposed to Hartz IV: they encounter the post-reform regime as soon as they exhaust short-term benefits. Workers belonging to interim cohorts are affected to a greater or lesser degree depending on the timing of UI entry coupled with their potential short-term benefit duration. As expected, the effects of Hartz IV cumulate steadily for successive cohorts.

The 2005 cohort offers my best estimate of the long-run, steady-state impact of Hartz IV on jobless durations. For this cohort, Figure 10 plots the full path of estimated reemployment effects for 24 months following entry into UI. The effects accrue rapidly for the first 12 months, then decline slightly thereafter as the counterfactual series partly catch up to the factual series. The employment gains are largely persistent at 24 months, however, suggesting that benefit cuts have

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enduring effects on cumulative job-finding even at lengthy durations.

For statistical purposes, Germany defines long-term unemployment—not to be confused with long-term UI, whose timing varies across individuals—as a jobless spell lasting over one year. I estimate that Hartz IV increased the probability of being reemployed within 12 months of beginning a claim by 4.0 percentage points for men (relative to a counterfactual probability of 68.8 percent) and by 5.9 points for women (relative to a counterfactual probability of 51.0 percent). In proportional terms, these estimates imply that Hartz IV reduced the likelihood of entering long-term unemployment by 12.8 percent for men and by 12.0 percent for women (or by 12.4 percent overall, pooling men and women). 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. The net employment effects are precisely estimated and easily differ from zero at the 5 percent level.

My core analyses censor incomplete spells at 24 months. To verify that my results are not sensitive to the censoring horizon, and to see whether Hartz IV’s pro-employment effects persist at even longer durations, I reestimate my benchmark specification with incomplete spells censored at either 12 months or 36 months. Table 3 reports net employment effects for the fully exposed 2005 cohort under these alternative horizons. The bottom-line magnitudes are very similar (as are the hazard effects, which I omit to conserve space). The factual-counterfactual employment gap continues to contract beyond 24 months but remains sizable even 36 months after UI entry.

5.4 Robustness

The results from the benchmark specification are robust to a variety of control strategies and sample modifications. I present these robustness checks graphically in Figure 11. Results from the same specifications (including standard errors) are also presented in Appendix Table 1. All specifications are precisely estimated, with highly significant increases in the reemployment hazard in the months preceding the change in benefit.

Specification 1 (in dark blue) replicates the benchmark Hartz IV effects from Figure 9. Specification 2 (orange) allows for compositional changes in the pool of new UI claimants by adding fixed effects 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 successive cohorts. Reassuringly, this specification yields similar (and in fact somewhat larger) effects. The scope for composition bias appears to be limited, given that there is little observable time-series variation in claimant characteristics.

The next two series stress-test the logic underlying my identification strategy. First, in specification 3 (green), I allow the age x duration and experience x duration interactions already included in \( z_{id} \) 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 potential short-term UI durations (due to the 12-month cap for workers under 45), such an improvement might spuriously lead me to overstate the pro-employment effects of exposure to benefit cuts. Adding three-way interactions between age bins, duration bins, and a post-reform dummy addresses this concern. The three-way experience interactions serve a similar function. Second, on top of these three-way interactions, specification 4 (red) allows the shape of the baseline hazard to change over time. In place of quarter x year dummies, which allow the baseline hazard function to shift up or down proportionally over time, I interact each 3-month duration bin with the full set of quarter x year dummies. This more flexible specification allows the reemployment hazards at short, medium, and long durations to vary independently of one another from quarter to quarter. In effect, this demanding specification compares the reemployment hazards of individuals who are observed at similar durations at a given point in time, but who differ in the timing of benefit changes. In both of these specifications, the results are strikingly stable, with the point estimates increasing modestly in magnitude.

Some individuals in my estimation sample appear in multiple, disjoint spells. Although it is not obvious that repeat spells present any problems—especially since I cluster on individual throughout—specification 5 (light blue) 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

\[ 35 \text{ For example, workers laid off during 2004-2005—when unemployment was at its peak—are likely to be positively selected on unobservables (e.g., work ethic) relative to workers laid off during the tighter labor market of 2001-2002 (Nakamura, 2008; Mueller, 2015). Since these late-sample workers are more exposed to Hartz IV, failing to control for unmeasured cohort effects could bias me towards overstating the pro-employment effects of the reform.}\]

\[ 36 \text{ Appendix Figure 2 plots the mean predicted jobless duration by quarter of entry into UI, based on a Weibull model of reemployment hazards estimated on UI claims that begin in 2001. Men exhibit no trend in predicted duration, apart from seasonal fluctuations that likely reflect the greater incidence of temporary layoffs in the first and fourth quarters of each year. Women exhibit a gradual decline in predicted duration, but there is no discontinuity or trend break at the time of Hartz IV.}\]

\[ 37 \text{ Two-way interactions between age and the post-reform dummy would address the same concern, but interacting with duration as well is more flexible.}\]
similar estimates.\textsuperscript{38}

The final series, specification 6 (brown), addresses a more specific selection concern. Under a standard model of UI take-up (Anderson and Meyer, 1997), reductions in the benefit level should deter some individuals from claiming UI. Moreover, the take-up “compliers” (who would claim benefits under the pre-Hartz rules but not the post-Hartz rules) may differ unobservably from those who would claim benefits under both scenarios.\textsuperscript{39} To limit the scope for such differential take-up, I restrict the sample to workers whose UI claims begin before July 2004. This restriction leads to somewhat larger estimates, especially for men. One possible explanation for this result is that, insofar as Hartz IV did deter UI take-up, the post-July 2004 job losers most sensitive to the onset of the reform may not enter my sample at all.\textsuperscript{40} Another is that Hartz IV may have had stronger effects on workers who were caught by surprise than on those who entered UI after the new regime was already in place. Regardless, there is no indication that differential take-up is causing me to overstate the pro-employment effects of UI reform.

These specifications exploit variation in potential benefit duration stemming from both age and experience. This fact invites an additional test: by isolating each source of variation in turn, I can gauge if they yield similar results, as my story would suggest. I explore this idea in Appendix Figure 4. The first two specifications, in blue and orange, reestimate the benchmark specification using workers ages 25–34 and 35–44, respectively. Since short-term benefits max out at 12 months for workers under 45, variation in potential duration within these subsamples is driven entirely by differences in prior employment and benefit histories. Conversely, the red series restricts attention to claimants (of any age) with maximal potential duration given their age bracket. Here, the identifying variation stems solely from differences in age. Results are qualitatively and quantitatively similar across specifications.

\textsuperscript{38} A related concern is that individuals with very limited benefit eligibility—either because they qualified through seasonal employment or because they are drawing down a prior, partially exhausted claim—may be driving the increased exhaustion spike. Such individuals are disproportionately represented among observations for which $r_{iH} = 0$, since their benefit exhaustion occurs before many of them have had time to exit the risk set. I obtain very similar results if I restrict to claimants with at least 6 months of potential short-term benefits, the starting-point for a fresh UI entitlement.

\textsuperscript{39} Although Anderson and Meyer consider a one-tier, fixed-length UI regime (as in the United States), their comparative static that benefit cuts deter UI take-up is easily extended to the case of reductions in a second benefit tier. Using the envelope theorem, it can also be shown that the utility change from a (small) cut to long-term benefits is increasing in the likelihood of reaching long-term unemployment and decreasing in the potential duration of short-term benefits. Given these differential welfare consequences, it is plausible that Hartz IV would lead to differential selection into UI.

\textsuperscript{40} I attempted to estimate the effect of Hartz IV on UI takeup, exploiting variation in imputed potential benefit duration. The results were too imprecise to be informative, reflecting both measurement error in the imputation of UI eligibility and the smaller 2 percent SIAB sample available for this exercise. Seasonality in job loss poses additional challenges for studying UI takeup.
As a final robustness check, I estimate the benchmark specification separately for 36 cells defined by sex, region, three household types, and terciles of initial short-term benefit level. Appendix Figure 5 reports the effect of Hartz IV on each group's job-finding hazard in the month of the Hartz IV benefit change \((r_{t\alpha} = 0)\). Almost every cell experiences a positive effect, suggesting that the Hartz IV benefit cuts—though difficult to measure accurately given data limitations—were pervasive enough to spur reemployment for a wide range of claimants. Exposure to the new benefit schedule increases job-finding in both West and East Germany, weighing against region-specific alternative explanations, and for both single claimants and childless couples. I find more mixed results among married claimants with children, consistent with benefit simulations suggesting that many such claimants were ineligible for long-term UI under both pre- and post-reform rules.

### 5.5 A placebo exercise

Is there anything special about January 2005, when Hartz IV took effect? 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 decreased as labor market conditions worsened in the early 2000s. Second, suppose that the earlier components of the Hartz package—implemented in January 2003 and January 2004—differentially affected reemployment rates among jobless workers who were close to exhausting benefits. Either phenomenon could conceivably result in spurious “Hartz IV” effects even if the UI reform itself had no causal effect on job finding. The reason, heuristically, is that in hazard models allowing a structural break in 2005, both of these forces could potentially load onto the post-break variables, even if the break itself is

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41 Much of the complexity of changes in the benefit schedule stems from spousal income, which is included in the means test, and from the presence of children, who yield supplementary benefits. For a given household structure, prior earnings—which map one-to-one into the short-term benefit level, after taxes—are a key determinant of the reform-induced change in UI generosity.

42 Because sample sizes are considerably smaller when I split the sample by household type and benefit tercile, I pool the event months \([-9, -8, -7], [-6, -5, -4], [-3, -2, -1], \{0\}, \{1, 2, 3\}, \{4, 5, 6, 7\}\) to improve precision. The subgroups exhibit the same hump-shaped hazard effects seen in Figure 9: for 29 of the 36 cells, the peak hazard effect occurs when \(r_{t\alpha} = 0\) (and for 5 of the remaining 6 cells, the peak effect occurs 1-3 months prior to the benefit change).

43 These estimates offer partial support for the idea that claimants exposed to steeper cuts should exhibit larger behavioral responses. Across these cells, the peak hazard effect is negatively correlated with the mean simulated change in potential household income induced by Hartz IV \((r = -0.45\) among men, \(r = -0.37\) among women). Several factors may contribute to this fairly weak relationship. First, my simulated benefit changes may suffer from classical measurement error, which would attenuate these correlations. Second, even if claimants expected to incur benefit cuts under the new rules, they may not have known precisely how idiosyncratically steep their own cuts would be, again attenuating these correlations. Third, changes in benefit cuts may be correlated with responsiveness to a given cut. For example, high-earners tended to face steeper cuts under Hartz IV, but they may be less responsive to marginal changes in long-term benefits because they are at lower risk of experiencing very long jobless spells.
mistimed.

To assess these threats, I estimate placebo specifications in which I alter the assumed date of the UI reform. Concretely, for each year $Y \in \{2001, 2002, 2003, 2004, 2005\}$, I use the 2 percent SIAB dataset to construct a sample of new UI claims initiated between January 1 of year $Y - 4$ and June 30 of year $Y$.

I then reestimate the benchmark hazard model with the true Hartz IV event-time variable $\tau_{id}^H$ replaced by $\tau_{id}^{H,Y}$, where $\tau_{id}^{H,Y}$ is computed as though Hartz IV took effect on January 1 of year $Y$. Note that $\tau_{id}^{H} = \tau_{id}^{H,2005}$, so that for $Y = 2005$ this model reestimates the causal effects of Hartz IV using the SIAB dataset. I censor incomplete spells on June 30 of year $Y$ to avoid misattributing the causal effects of Hartz IV itself to the placebo reforms. I pool all post-event periods into a single coefficient, since the abbreviated post-"reform" period—lasting only six months—makes it difficult to separately identify the event-time coefficients for $\tau_{id}^{H,Y} \geq 1$.

Figure 12 shows that 2005 was indeed different. The blue series in each panel—corresponding to the true 2005 reform—closely parallels my benchmark estimates, with the hazard effect peaking at about 60 percent for men and 45 percent for women. For pseudo-reform years 2001, 2002, and 2003, the estimated placebo effects are close to zero, providing no evidence of a longstanding trend towards greater sensitivity to short-term exhaustion. The stability of the placebo estimates is especially encouraging because Germany’s labor market picture was changing rapidly in these years, with steep increases in unemployment throughout the early 2000s. For 2004, I do find positive (and significant) placebo effects, though they are reassuringly smaller than the Hartz IV effects themselves in the months leading up to benefit cuts.

In Appendix B, I consider three possible explanations for the modest 2004 placebo effects: anticipatory reactions to Hartz IV prior to July 2004, delayed responses to an earlier tightening of the asset means-test for long-term benefits, and changes in the frequency of UI benefit sanctions. As explained in the appendix, allowing the baseline hazard to evolve flexibly over time—which more stringently partials out changes in duration dependence unrelated to benefit step-downs—strengthens the 2005 Hartz IV effects while attenuating the 2004 placebo effects (Appendix Figure 3), lending further support to a causal interpretation of my estimates.

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44 Recall that the AED is representative only of spells that begin in 2001 or later. For that reason, the SIAB is better suited to this exercise. Slightly different age cutoffs for the maximum potential benefit duration applied to claimants who entered UI before April 1999 (some of whom appear in the 2001–2003 placebo specifications). When computing potential benefit duration, I use whichever cutoff was in effect at the onset of the claim. Placebo results are robust to excluding pre-April 1999 entries into UI.
5.6 Timing of responses among incumbent long-term claimants

I have shown that reemployment hazards rise as UI claimants approach benefit changes induced by Hartz IV. To provide further evidence of the temporal link between the implementation of Hartz IV and increased job-finding among reform-exposed claimants, I now examine job-finding rates among incumbent long-term claimants, all of whom were subject to immediate benefit changes if they were still unemployed on January 1, 2005. Among workers receiving long-term benefits prior to the reform, post-reform changes in relative reemployment hazards are strongly correlated with a proxy for the size of the benefit cut induced by Hartz IV.45

Because Hartz IV replaced a wage-indexed benefit with a uniform benefit unrelated to pre-UI earnings, claimants with greater prior earnings generally received steeper benefit cuts under the new rules. To explore this idea, I use the SIAB dataset to construct a representative 2 percent sample of new long-term UI claims originating between 2000 and 2004. To ensure that I observe baseline covariates, I restrict to workers who held a regular job sometime within the 3 years preceding long-term UI receipt. To proxy for the benefit change that occurs on January 1, 2005, I partition claimants into terciles based on their initial long-term benefit level (which I observe net of means testing), stratifying by sex, region, household type, and year of entry into long-term UI. Finally, I estimate a complementary log-log model of duration until reemployment. The explanatory variables are a full set of duration effects, allowing the baseline hazard rate to evolve nonparametrically with time since entry to long-term UI (stratified by West/East residence); dummies for age bins, German nationality, and three household types; calendar time effects at quarterly frequency; and interactions of these time effects with indicators for the second and third benefit terciles. Because the sample becomes thinner at long durations and at later calendar dates, I censor incomplete spells 36 months after entry to long-term UI or at the end of 2006, whichever comes first.

Figure 13 plots the time effect x benefit tercile interactions, which show how the reemployment hazards of medium- and high-benefit claimants evolve over time relative to those of low-benefit claimants. Between 2000 and mid-2004, reemployment hazards for the second and third benefit terciles closely track those for the lowest tercile. Towards the end of 2004, however, there is a clear divergence: consistent with the hypothesis that cuts were steeper for those with generous pre-reform

45 As noted in Footnote 43, such comparisons may be confounded by (i) imperfect comprehension among claimants, (ii) measurement error in my benefit simulations, or (iii) treatment effect heterogeneity across groups experiencing different benefit cuts. These three potential confounds should be less severe in a sample of claimants who reach long-term UI, since (i) such claimants have greater familiarity with the UI system, (ii) I observe their pre-reform long-term benefits net of means testing, and (iii) conditioning on long-term unemployment should truncate the distribution of unobserved heterogeneity that gives rise to heterogeneous treatment effects.
benefits, the second and (especially) third terciles show employment gains relative to the first.\textsuperscript{46} These interactions reveal only the relative effect of Hartz IV as a function of baseline benefits, with the main effect difficult to separate from any background time trends.\textsuperscript{47} It is also possible that the differential trajectories of workers in each tercile partly reflect different elasticities of job-finding to a given change in hazard, rather than being driven solely by differences in the benefit changes they encounter. Nonetheless, the stark time patterns in Figure 13 establish a tight temporal link between the post-Hartz IV shift in job-finding dynamics and concurrent changes in UI generosity.

5.7 Partial equilibrium impact on the steady-state unemployment rate

Having argued that my estimates represent the causal effects of long-term benefit cuts on individual jobless durations, I conclude this section with a brief discussion of their possible implications for the broader German labor market. 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 estimates, taken at face value, imply for the steady-state unemployment rate, and in particular for its long-term component.

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 $D_{ST} = \sum_{d=1}^{12} S_d$, where $S_d$ is the probability of being unemployed for at least $d$ months. Likewise, let $D_{LT} = \sum_{d=13}^{\infty}$ denote the expected number of months that such a worker will spend in long-term unemployment. It is convenient to decompose this expression into $D_{LT} = D_{13-24} + D_{25-\infty}$, 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

\textsuperscript{46} Of the 60 time $\times$ tercile interactions that precede 2004Q3 (30 each for men and women), only five are significantly positive at the 5 percent level in a two-sided test (three are significantly negative). By contrast, more than half of the interactions are positive at the 5 percent level from 2004Q3 onward (none are significantly negative). I omit confidence intervals to avoid cluttering the figure.

\textsuperscript{47} The calendar-time coefficients are negative after Hartz IV takes effect, so that the lowest tercile exhibits lower reemployment rates after the reform than before it. The decline could represent benefit increases for this group of claimants, congestion externalities due to Hartz IV's impact on aggregate labor supply, or other, unrelated changes in labor market conditions.
exercise. Abstracting from flows in and out of the labor force, steady-state unemployment equals:

\[
\begin{align*}
  u_{\text{ST}} &= \frac{D_{\text{ST}}}{1/q + D_{\text{ST}} + D_{\text{LT}}} \\
  u_{\text{LT}} &= \frac{D_{\text{LT}}}{1/q + D_{\text{ST}} + D_{\text{LT}}}
\end{align*}
\] (28) (29)

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 \( u_{\text{ST}} = u_{\text{LT}} = 5.0 \) percent. In the absence of Hartz IV, my hazard estimates imply that \( D_{13-24}^{ST} = 6.79 \) months and \( D_{13-24}^{LT} = 3.62 \) months. Given these values, I calibrate \( q = .82 \) percent and \( D_{25-\infty}^{ST} = 3.17 \) months to satisfy (28) and (29), so that \( D_{25-\infty}^{LT} = 6.79 \) months. With Hartz IV in place, my hazard estimates imply smaller values \( D_{\text{ST}} = 6.43 \) months and \( D_{13-24} = 3.10 \) 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 \( D_{25-\infty} = 2.68 \) months. The steady-state unemployment rates fall to \( u_{\text{ST}} = 4.8 \) percent and \( u_{\text{LT}} = 4.3 \) percent.

Though stylized, this back-of-the-envelope calculation suggests that Hartz IV may have reduced Germany’s unemployment rate by about 0.9 percentage points—a measurable, if not decisive, share of the dramatic 6.6 percentage point decline in unemployment that Germany experienced between December 2004 and December 2015.\(^{49}\) Strikingly, the steady-state impact of Hartz IV is driven almost entirely by a 0.7 percentage point reduction in the long-term unemployment rate. This finding underscores the point that long-term UI 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.

My research design identifies partial equilibrium impacts of benefit cuts on individual job-finding. In general equilibrium, the direct effect of increased individual search effort may be either mitigated by congestion externalities or amplified by job creation. In equilibrium search-and-

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\(^{48}\)To see this, imagine an infinitely lived worker toggling between employment spells of average duration \( 1/q \) and unemployment spells of average duration \( D_{\text{ST}} + D_{\text{LT}} \). Each job-layoff cycle lasts an average of \( 1/q + D_{\text{ST}} + D_{\text{LT}} \) months, of which \( D_{\text{ST}} \) are spent in short-term unemployment and \( D_{\text{LT}} \) are spent in long-term unemployment. If the job-finding rate \( f \) is constant, then \( D_{\text{ST}} + D_{\text{LT}} = 1/f \) so that \( u \equiv u_{\text{ST}} + u_{\text{LT}} = \frac{1}{q + f} \); that is, these expressions nest the familiar steady-state formula given constant transition probabilities.

\(^{49}\)Seasonally adjusted unemployment rates published by Germany’s Federal Statistical Office, based on the International Labor Organization’s definition of unemployment. Soon after Hartz IV, a companion measure reduced the maximal duration of short-term benefits for older workers entering UI after February 1, 2006 (see Footnote 13). There is a complementarity between reducing long-term UI benefit levels and reducing short-term benefit durations, as the latter causes the former to apply earlier in a worker’s spell. Accounting for this interaction effect would magnify the estimated partial equilibrium impact of Hartz IV.
matching models, these indirect effects depend on the intensity of changes in individual search effort. Credible estimates of the direct effect can therefore help researchers gauge the possible importance of the congestion and job-creation channels. In addition to their effects on the unemployed, benefit cuts may also affect employed workers by worsening their outside options in the event of job loss. Reductions in UI generosity may deter separations or put downward pressure on wages through shifts in bargaining power. Although my partial equilibrium estimates are only one part of the puzzle, I regard them as a useful input into future efforts to calibrate the macroeconomic effects of Hartz IV.

6 Effects on Reemployment Wages

I have shown that Hartz IV spurred UI claimants to find jobs faster. But how does cutting long-term UI benefits affect the wages they receive on those jobs? An oft-cited rationale for UI is that, in addition to smoothing consumption, it enables liquidity-constrained jobseekers to prolong their job searches and thereby obtain higher-paying positions. In keeping with this view, critics of Hartz IV allege that cutting UI benefits has contributed to rising German wage inequality and to falling real wages at the left end of the earnings distribution (Dustmann et al., 2009). As the model in Section 3 suggests, however, reducing long-term UI generosity has theoretically ambiguous effects on post-UI wages: benefit cuts can indeed 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 (Nekoei and Weber, 2016; Schmieder et al., 2016). Identification of wage effects is further complicated by a standard selection challenge: wages are only observed for those who eventually become reemployed (Heckman, 1979; Ham and Lalonde, 1996). This section develops an empirical framework for quantifying the net wage impact and for disentangling it into these three channels.

6.1 Accepted wages in the lead-up to benefit changes

I begin by estimating the effects of benefit cuts on the wages workers receive in their first regular job after UI, conditional on their completed jobless duration. 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

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Though absent from the theoretical framework in Section 3, bargaining effects might arise either in general or in partial equilibrium, depending on whether employers can identify how UI reform affects particular applicants’ outside options. In the wage specification, market-wide bargaining effects will be absorbed by calendar time dummies, whereas idiosyncratic effects will register as lower wages for workers approaching benefit cuts.
analogy with the benchmark hazard specification laid out in Section 4.3, I run OLS regressions of log wages on remaining benefit durations at the moment of hiring:

\[
w_{id} = \tilde{\alpha}_d + \tilde{\gamma}_t + x_{id}'\tilde{\phi} + \sum_{k=-9}^{4} \tilde{\delta}_k^E 1\{r_{id}^E = k\} + \sum_{k=-9}^{4} \tilde{\delta}_k^H 1\{r_{id}^H = k\} + \epsilon_{id}. \tag{30}
\]

The explanatory variables are identical to those used in the hazard model (with coefficients distinguished by tildes). As before, the event-time coefficients \(\tilde{\delta}_k^E\) and \(\tilde{\delta}_k^H\) allow wages to evolve flexibly as workers approach and then pass the two benefit changes. The duration dummies \(\tilde{\alpha}_d\) allow wages to vary with completed unemployment duration, as a result of either structural duration dependence or dynamic selection. The time effects \(\tilde{\gamma}_t\) control both for shifts in aggregate wage dynamics and for seasonal patterns in wage offers. As before, the vector of demographics \(x_{id}\) includes indicator variables for each decile of pre-Unemployment Insurance (UI) wages, allowing the ratio of wages before/after unemployment 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.

Figure 14a and Figure 14b plot, for men and women respectively, estimated changes in log reemployment wages as workers approach the two step-downs in the benefit schedule. The lefthand panel in each figure shows the main effect of benefit exhaustion, which is active both before and after the reform. Men exhibit a gradual deterioration of reemployment wages as they approach short-term benefit exhaustion, with a sharp decline in the month of exhaustion. Women initially show no change in wages, but wages fall sharply a month prior to exhaustion. Men (women) who accept jobs in the month following benefit exhaustion, when \(r_{id}^E = 1\), receive 6 percent (10 percent) lower wages than those reemployed 10 or more months prior to exhaustion, conditional on previous earnings, completed duration, and a rich set of observable characteristics. These negative effects—identified by cross-sectional variation in potential benefit durations among observationally similar claimants with the same ex post jobless duration—offer suggestive evidence that reservation wages fall sharply as workers approach benefit cuts. My finding of lower wages in the vicinity of a benefit step-down is consistent with Schmieder et al. (2016), who find that workers reemployed in the month of short-term UI exhaustion accept lower wages.\(^{51}\)

The righthand panels show the additional impact of benefit cuts induced by Hartz IV.

\(^{51}\)See their Figure 6, which uses a regression discontinuity at age 45 to compare the path of reemployment wages for German workers eligible for either 12 or 18 months of short-term benefits. My results differ from Schmieder et al.’s in two ways: first, I find that wage responses among men begin several months prior to exhaustion; second, for both men and women, reemployment wages remain depressed after exhaustion.
Among claimants with more than four months remaining until the new rules kick in, wages are insensitive to future benefit cuts. Wages decline sharply thereafter, however, with effects peaking around 8 percent for both men and women taking jobs in the month after the benefit cut. The pattern of falling wages in advance of the benefit cut is indicative of anticipatory behavior on the part of jobseekers.\footnote{Even if reservation wages do not change, contracted wages could still decline as workers approach benefit cuts if there are rents to bargain over and if firms can accurately identify which job applicants are on the verge of benefit reductions. Given the complexity of the benefit calculation—which requires precise information on the exact timing of prior employment spells and UI receipt—this story would place a heavy information burden on potential employers. In Section 7, I find that benefit cuts increase the hazard rate of being recalled to a previous employer—suggesting that benefit cuts affect which job offers claimants choose to accept, rather than (exclusively) lowering wage offers within employment relationships that would have been formed anyway.} Interestingly, however, changes in the reemployment hazard precede changes in reemployment wages by several months, suggesting that workers faced with long-term benefit cuts may first increase their search effort, then lower their reservation wages if they still remain unemployed. As with the hazard effects in Section 5, the total impact of benefit exhaustion under the new rules is given by the sum of the main and incremental effects: for fully exposed claimants, jobs accepted just after benefit exhaustion pay roughly 15 percent lower wages than jobs accepted 10 or more months before benefits run out.

Appendix Figure 6 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 time-varying duration effects—attenuates the effects by up to about one-third for men and, in some specifications, by over one-half for women, but the qualitative patterns are similar.\footnote{As reported in Appendix Table 2, the peak effect in each specification remains statistically significant at the 5 percent level, with one exception: for women, the wage effects become statistically insignificant when I control for entry-cohort effects, though the point estimates remain negative.} A balanced reading of the evidence is that Hartz IV lowered reemployment wages by about 4 to 8 percent for men accepting jobs in the immediate aftermath of benefit cuts, with qualitatively similar but noisier and perhaps smaller effects for women. These effects augment the "drop at exhaustion" already evident in wage offers accepted prior to the reform.

### 6.2 Quantifying and decomposing the net wage effect

As noted above, Figure 14 and Appendix Figure 6 give only a partial picture of the overall wage effect, since Hartz IV may also impact wages indirectly by shortening durations and altering selection into work. Much as I previously used the fitted duration model to quantify effects on
jobless durations for claimants fully exposed to the new benefit schedule, I now translate these point-in-time wage impacts into average effects on the wage paid in a Hartz-exposed worker's first post-UI job. To unpack the underlying mechanisms and to deal with observable changes in the pool of reemployed workers, I then decompose the overall impact into direct, indirect, and selection terms.

Following Schmieder et al. (2016), it is useful to distinguish changes in the path of reemployment wages from shifts along the path. Formally, let $D_i$ denote the realized duration at which claimant $i$ becomes reemployed, let $p_{id} \equiv \Pr(D_i = d)$, and define $\lambda_{id} \equiv \Pr(D_i = d \mid D_i > d - 1)$ as the (discrete-time) hazard rate of reemployment. Let $w_i$ denote the log ratio of the reemployment wage to the pre-UI wage (without conditioning on completed duration), and define $\mu_{id} \equiv \mathbb{E}(w_i \mid D_i = d)$ as the average value of $w_i$ among workers who find jobs $d$ months into their spells. I take $p_{id}$, $\lambda_{id}$, and $\mu_{id}$ to be conditioned on $x_{id}$, the explanatory variables used in the hazard and wage equations. I keep the conditioning implicit to simplify the notation.

By the law of iterated expectations, the mean log wage ratio (conditional on being reemployed) equals

$$
\mathbb{E}(w_i \mid D_i \leq 24) = \frac{1}{F_{i,24}} \sum_{d=1}^{24} p_{id} \mu_{id} = \frac{1}{F_{i,24}} \sum_{d=1}^{24} \left( \lambda_{id} \prod_{s=1}^{d-1} (1 - \lambda_{is}) \right) \mu_{id},
$$

(31)

where $F_{i,24}$ is the probability of being reemployed within 24 months of UI entry. Holding hazard rates constant, a benefit cut may affect reemployment wages by changing mean wages $\mu_{id}$ at each possible duration. I refer to this as the reservation wage effect. Holding $\mu_{id}$ constant, a benefit cut may affect reemployment wages by raising the hazard rate of reemployment, so that this expression places greater weight on earlier periods when wage offers tend to be higher. I call this the duration effect. A third effect emerges if benefit cuts affect $F_{i,24}$, thereby altering the set of workers for whom I observe post-UI wages. This is the selection effect.

These distinct effects motivate a joint econometric model of reemployment hazards and reemployment wages (as in, for example, Caliendo et al., 2013). I specify the reemployment hazard as in the benchmark specification estimated in Section 5.2. I specify the log wage as $w_{id} = \mu_{id} + \varepsilon_{id}$, where $\mu_{id}$ is the fitted value of wages in (30) 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

\[
L_i(\theta) = \begin{cases} 
\lambda_i D \prod_{s=1}^{D-1} (1 - \lambda_{is}) \frac{1}{\sqrt{2\pi}} \exp \left( -\frac{(w_{i,s} - \mu_{iD})^2}{2\sigma^2} \right) & \text{if } D_i \leq 24 \\
\prod_{s=1}^{24} (1 - \lambda_{is}) & \text{if } D_i > 24 
\end{cases}
\] (32)

The first case represents workers reemployed \( D_i \) months after entering reemployment (so that \( w_i = w_{i,D} \) by construction). The second case represents workers for whom I don’t observe a post-UI job (or wage) within 24 months. I estimate the model by maximum likelihood.

Adapting the quantification procedure from Section 5.3, I use the fitted model to predict counterfactual reemployment wages in the absence of Hartz IV. First, I denote the fitted (factual and counterfactual) wages at a given duration by \( \tilde{\mu}_{id} \) and \( \tilde{\mu}_{id}^{cf} \), where the latter sets the Hartz IV event-time variable \( \tau_{id}^H \) to its omitted value, implicitly assuming that Hartz IV lies far enough in the future not to influence wages. Second, let \( \hat{\lambda}_{id} \) be the fitted value of the reemployment hazard, and let \( \hat{\lambda}_{id}^{cf} \) be the counterfactual hazard when I again set \( \tau_{id}^H \) to its omitted value. Using these fitted and counterfactual hazards, I estimate the pdf of completed jobless durations:

\[
\hat{p}_{id} = \hat{\lambda}_{id} \prod_{s=1}^{d-1} (1 - \hat{\lambda}_{is})
\]
\[
\hat{p}_{id}^{cf} = \hat{\lambda}_{id}^{cf} \prod_{s=1}^{d-1} (1 - \hat{\lambda}_{is}^{cf})
\] (33)

The corresponding probabilities of being reemployed within 24 months are \( \hat{F}_{i,24} = \sum_{d=1}^{24} \hat{p}_{id} \) and \( \hat{F}_{i,24}^{cf} = \sum_{d=1}^{24} \hat{p}_{id}^{cf} \). To simplify notation, I also define the conditional pdf \( \hat{q}_{id} = \hat{F}(D_i = d \mid D_i \leq 24) = \frac{\hat{p}_{id}}{\hat{F}_{i,24}} \) with \( \hat{q}_{id}^{cf} \) defined similarly.

Under Hartz IV, the predicted mean wage for worker \( i \), conditional on reemployment within 24 months, is \( \sum_{d=1}^{24} \hat{q}_{id} \hat{\mu}_{id} \). In expectation, \( \sum_{i=1}^{N} \hat{F}_{i,24} \) workers are reemployed, so that each worker’s expected share of the reemployed sample is \( \hat{\pi}_i = \frac{\hat{F}_{i,24}}{\sum_{j=1}^{N} \hat{F}_{j,24}} \), with \( \hat{\pi}_i^{cf} \) defined analogously. The predicted average wage in the full set of reemployed workers is therefore

\[
E(w) = \sum_{i=1}^{N} \hat{\pi}_i \sum_{d=1}^{24} \hat{q}_{id} \hat{\mu}_{id},
\] (34)

with the counterfactual average wage defined similarly. Taking the difference, the predicted effect
of Hartz IV is
\[
\Delta \bar{E}(w) \equiv \sum_{i=1}^{N} \sum_{d=1}^{24} \left( \hat{\pi}_i^e d \mu_id - \hat{\alpha}_i^e d \hat{\mu}_id \right).
\]

(35)

I can decompose this expression to quantify the three effects described above:
\[
\Delta \bar{E}(w) = \sum_{i=1}^{N} \sum_{d=1}^{24} \hat{\pi}_i^e d \hat{\mu}_id - \hat{\alpha}_i^e d \hat{\mu}_id + \sum_{i=1}^{N} \sum_{d=1}^{24} (\hat{\pi}_i^e d - \hat{\alpha}_i^e d) \hat{\mu}_id + \sum_{i=1}^{N} (\hat{\pi}_i^e - \hat{\alpha}_i^e) \sum_{d=1}^{24} \hat{\mu}_id
\]

(36)

The first term shows the wage impact holding jobless durations, as well as the composition of the pool of reemployed workers, constant at their counterfactual values. I interpret it as a reservation wage effect (see Footnote 52). The second term shows the wage impact due to changes in the timing of reemployment, again holding composition constant. The third term reflects selection on observables: it captures any covariance between workers' predicted wages and the change in their likelihood of being reemployed as a result of Hartz IV. I compute these expressions using all workers entering UI in 2005, the first cohort of UI entrants fully exposed to the new benefit schedule.

Results from this decomposition are reported in Table 4, under the heading "benchmark decomposition". Overall, I estimate that exposure to Hartz IV reduced the mean initial reemployment wage by 1.6 percent for male claimants and by 2.0 percent for female claimants. The overall impact masks offsetting reservation wage and duration effects: the decline in reemployment wages due to the point-in-time effects of Hartz IV on accepted wages (1.96 percent for men, 2.27 percent for women) is only slightly offset by wage gains from shorter durations (0.27 percent for men, 0.14 percent for women). The selection component is positive but small (0.07 percent for men, 0.12 percent for women). The positive value suggests that the marginal claimants who become reemployed within 24 months as a result of Hartz IV are positively selected on predicted wages. Subtracting this selection term yields an estimated wage effect for a fixed group of workers, namely those who are predicted to have been reemployed in the absence of Hartz IV. For these workers, I estimate the negative causal effect of benefit cuts on average reemployment wages to be 1.7 percent for men and 2.1 percent for women. These losses amount to a sizable share of the wage losses incurred by displaced workers in my sample (which equal, on average, 6.7 percent among male claimants reemployed with 24 months, and 8.8 percent among female claimants).

As usual with Oaxaca-Blinder expressions, this decomposition is not unique: in general, the effect attributed to each channel depends on the order in which the terms are decomposed. One
alternative decomposition would weight the reservation wage effects by the factual reemployment shares \( \hat{\pi}_t \) and the factual conditional probabilities \( \hat{q}_{id} \). The alternative formula yields very similar results (presented in the lower panel of Table 4), reflecting the fact that the second-order interactions between the duration and wage effects are small in magnitude.

7 What Kind of Jobs?

To shed additional light on workers' search behavior and on the mechanisms through which long-term benefit cuts depress initial post-UI wages, this section develops a competing-risks framework to track transitions into different kinds of jobs. I partition jobs in three ways. First, I examine transitions into full-time vs. part-time jobs to gauge whether the negative wage effects found in Section 6 reflect shifts along the intensive margin of labor supply, rather than declines in hourly wages. Net employment gains are mostly driven by full-time jobs, with little change in the part-time share of employment. Second, I distinguish brand-new employment relationships from recalls to the previous employer. I find positive effects on both new-job and recall hazards. The increased recall hazard provides corroborating evidence that benefit cuts worsen workers' outside options, as my wage results suggest. Third, I broaden the employment concept to encompass low-paid mini-jobs often held during UI receipt. Contrary to received wisdom, the Hartz IV benefit cuts reduced the share of displaced workers who transition into mini-jobs.

7.1 Full time vs. part-time

In Section 6, I found that long-term benefit cuts reduce monthly earnings in a worker's first regular job after entering UI. Although I have attributed this negative effect to a decline in reservation wages, an alternative possibility is that it reflects a reduction in hours worked rather than a reduction in hourly wages. The IAB data do not report hours worked, but I can explore the intensive margin of labor supply by looking at full-time/part-time status. To answer this question, I distinguish transitions between full-time and part-time regular jobs to bound the component of the earnings response explainable by shifts into part-time employment.

Formally, I adapt my dynamic 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. As is standard in competing-risk
models, I treat each job type as an absorbing state; that is, I ignore subsequent transitions between job types, such as “promotions” from part-time to full-time jobs. 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 specification.

The left-hand panel of Figure 15 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 the hazard rates of entry into both job types. But cause-specific hazard rates 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 initial 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 type of job.

I therefore use the fitted model to estimate cumulative incidence functions, which measure the share of workers absorbed into each job type at a given duration since baseline (Fine and Gray, 1999).\(^{54}\) Let \(D_i\) denote completed jobless duration (in continuous time), and let \(J_i \in \{\text{full-time job, part-time job, no job}\}\) denote the 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

\[
I_{id}^j = P(D_i \leq d \cap J_i = j).
\] (37)

The overall reemployment rate \(F_{id}\) can be expressed as the sum of the type-specific cumulative incidences:

\[
F_{id} = \sum_{j \neq \text{no job}} I_{id}^j.
\] (38)

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 5.3, I obtain predicted and counterfactual cumulative incidence functions for each UI claim. I compute the gap between these functions at durations \(d \in \{1, 2, \ldots, 24\}\), and I average this gap across all UI claims begun in 2005 as a measure of how

\(^{54}\) The cumulative incidence function is interpretable without assuming independent risks. To understand why, consider an analogy to a randomized controlled trial in which an experimental program assists unemployed workers applying to different kinds of jobs. Under randomized assignment, the net effect of the treatment on reemployment into a given job category at any given duration is identified by a simple difference of means. This is the cumulative incidence. By contrast, without imposing additional structure, the econometrician cannot consistently estimate how the treatment impacted individual search intensity towards each type of job: in the presence of unobserved heterogeneity, observed changes in cause-specific hazards reflect not only average changes in individual transition probabilities, but also compositional changes in the risk set induced by dynamic selection out of unemployment.
Hartz IV affected job types for workers fully exposed to the new benefit schedule. The gap is plotted in the righthand panel of Figure 15. Net employment gains predominantly represent full-time jobs—an unsurprising result given the cross-sectional fact that most regular jobs in my sample are full-time. For men, my estimates imply that the part-time share of employment within the fully exposed 2005 cohort of UI entrants rose slightly, from 4.1 percent to 4.4 percent, as a result of Hartz IV. For women, my estimates imply that the part-time share of employment fell from 30.0 to 29.9 percent.

A bounding exercise suggests that shifts to part-time jobs are unlikely to explain much of the overall wage effect I find. Suppose, for sake of argument, that part-time jobs pay the same hourly wages as full-time jobs but entail only half as many hours. Then, holding hourly wages constant, the positive impact on men’s part-time share would induce only a 0.2 percent decline in mean male earnings—an order of magnitude smaller than the net effect I estimate in Section 6. For women, benefit cuts slightly reduce the part-time share of employment, so that an increased incidence of part-time employment cannot explain my wage results. 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 negative wage effects do not stem from intensive changes in labor supply.

7.2 New jobs vs. recalls

A second partition of interest is the distinction between new jobs versus recalls to the previous employer. Many unemployment spells, especially in cyclical sectors like construction and hospitality, are temporary layoffs that frequently end in recall, and recall expectations are an important determinant of job-search effort (Katz and Meyer, 1990b; 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. Insofar as returning to a former employer 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—the recall hazard offers an additional window into how benefit cuts affect the continuation value of remaining unemployed. Recalls are common in my estimation sample: among claimants reemployed within 24 months of UI entry, 35.8 percent of men and 23.8 percent of women return to their previous employer.

The lefthand panel in Figure 16 plots the effects of Hartz IV on the competing risks of being hired by either a new employer or the previous employer. Both the recall hazard and the new-job
hazard display the same hump-shaped effect pattern familiar from the benchmark specification, with the recall hazard rising by about 30 to 35 percent and the new-job hazard rising by about 60 percent in the month of the benefit cut. On face, these results indicate that still-unemployed workers are more likely to find both types of jobs when faced with benefit cuts. Modulo the need to assume independent risks, the increased recall hazard suggests that Hartz IV made workers less selective about the jobs they accept, provide additional confirmation that long-term benefit cuts have worsened the outside option of remaining unemployed.\footnote{If jobs are fully characterized by the wages they offer, 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, some workers may decline even high-paying recall offers if these wage premia reflect a compensating differential for the elevated risk of subsequent layoff in jobs that extend recall offers.}

Using the cumulative incidence functions, the righthand panel plots the corresponding impact on net transitions into both types of jobs. Consistent with the descriptive fact that recalls typically happen either early in unemployment or not at all, recalls account for a large share of women’s net job gains during the first four months after UI entry but flatline thereafter. Among men, the elevated recall hazard translates into only negligible net gains in recall employment: new jobs account for essentially all of the net employment gains induced by Hartz IV.\footnote{This is counterintuitive but entirely possible. Some male claimants are presumably being diverted into new jobs from recalls they would counterfactually have accepted, and these diversions may exactly offset increased recalls among claimants who would counterfactually have found new jobs or remained unemployed.}

7.3 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 legally distinct class of low-paid, part-time jobs that are subject to an earnings cap and partly exempt from these contributions.\footnote{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. Hartz II also created a class of intermediate jobs (“midi-jobs”) paying €400–800 per month, with the social security exemption phased out gradually.} In June 2004, mini-jobs accounted for 9.8 percent of aggregate male employment and fully 21.2 percent of female employment.\footnote{Own calculation, public-use IAB data. Mini-jobs are even more common after job loss: setting aside workers already holding a mini-job at entry into UI, 17.5 percent of men in my sample and 29.1 percent of women took a mini-job prior to finding regular work.} Critics of Hartz IV allege that it has fueled the growth of such marginal positions by compelling the unemployed to accept any work they can find, potentially at the expense of job security and other amenities historically provided by the German labor market (Jarosch,
But a priori, reductions in long-term benefits 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 the alternative strategy of seeking socially insured employment. Which effect dominates is an empirical question.

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 claimants who hold a mini-job on the date they enter UI (5.0 percent of men and 8.8 percent of women). Figure 17 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, however, providing an initial indication that Hartz IV promotes transitions into regular jobs, rather than mini-jobs. The blue and brown 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 for men and change little for women. As noted previously, however, cause-specific hazards are difficult to interpret without a strong independence assumption. Absent independence, the declining mini-job hazard may reflect dynamic selection out of the risk set as some workers enter regular jobs, rather than changes in individual mini-job hazards. Independence

59 By worsening outside options among displaced workers, benefit cuts may also have contributed to the rise of contingent work arrangements—such as domestic outsourcing, temporary help, and independent contracting—that have been implicated as a proximate source of rising wage inequality in both Europe and the United States (Goldschmidt and Schmieder, 2015; Katz and Krueger, 2016).

60 To see this, consider a static model of sector choice where workers choose among pure unemployment with UI benefit \( b \), benefit receipt coupled with a mini-job at wage \( w_M \) below the earnings disregard, or a regular job at wage \( w_R \). The corresponding payoffs are

\[
\begin{align*}
u_1 &= u(b) \\
u_2 &= u(b + w_M) - c_M \\
u_3 &= u(w_R) - c_R
\end{align*}
\]

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 \( u() \) 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.

61 I omit confidence intervals for visual clarity. The any-job and regular-job hazard effects are precisely estimated and are positive at the 5 percent level for all \( \tau_{id}^H \in (-8, 1) \) for men and for all \( \tau_{jd}^H \in (-9, 2) \) for women. The mini-job hazard is significantly negative for men from \( \tau_{id}^H = -2 \) onward and generally indistinguishable from zero for women (with significant positives for \( \tau_{id}^H = -5 \) and \( \tau_{id}^H = 1 ) \).
is unlikely to hold in this setting: finding rates for regular and mini-jobs may be either complements (if some applicants obtain more job offers across the board) or substitutes (if searching for one type of job crowds out time spent searching for others).

Figure 18 plots the implied effects on the cumulative incidence functions, which are easier to interpret (see Footnote 54). The blue series show that regular jobs more than account for the net employment gains. Contrary to much of the political discourse surrounding the reform, the brown series shows Hartz IV had a modestly negative causal effect on the share of workers drawn into mini-jobs. Among men, these negative causal impacts accrue steadily with duration, reaching 1.4 percentage points two years after UI entry. Among women, absorption into mini-jobs initially rises slightly but then declines, with a net decline of 0.6 percentage points two years after entry. Though not a foregone conclusion, this result is quite plausible, since Hartz IV made it less attractive for workers to claim long-term UI benefits while keeping their earnings low to remain eligible for benefits. These results reveal only the net change in the share of workers who end up in each state; they say nothing about how individual workers’ outcomes were affected. What is clear, however, is that the UI reform reduced net transitions into mini-jobs among former UI claimants, at least at longer durations.

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. Although this positive effect is in apparent tension with my earlier finding that Hartz IV depressed wages in regular jobs, the sign reversal 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,

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62 The grey series in each panel plots the reform-induced change in overall reemployment rates implied by a single-risk model using a broader employment concept that includes mini-jobs. The reform-induced employment gap peaks about 12 months after entry into UI at 2.4 percentage points for men and 4.1 points for women, with partial convergence thereafter. These magnitudes are about one-third smaller than those obtained when restricting attention to regular jobs (Figure 10).

63 Due to estimation error, the identity in (38) holds only approximately in the estimated model. Hence the blue and brown series do not exactly sum to the grey series corresponding to the single-risk model.

64 Consider two possibilities. Hartz IV may have drawn some counterfactual non-workers into regular jobs while also causing some counterfactual mini-job holders to seek regular work. Alternatively, it may have induced some non-workers to obtain mini-jobs but also induced some (larger number of) counterfactual mini-job holders to enter regular jobs. These stories are difficult to disentangle.

65 Although Hartz IV appears to have acted as a brake on transitions from UI into marginal employment, it is quite possible that the broader package of Hartz reforms did foster marginal employment, especially given the mini-job reform of April 2003. My analysis also abstracts from any general equilibrium mechanisms (e.g., market-wide changes in bargaining) through which benefit cuts may have altered the aggregate composition of jobs.

66 Descriptively, I observe considerable bunching of mini-job earnings at the UI disregard of €165 per month. So closely are mini-jobs entwined with the UI system that the tax subsidies accorded to mini-jobs are sometimes likened to an active labor market policy.
my preferred employment concept—which excludes mini-jobs—should better capture how Hartz IV affected earnings potential upon reemployment. Taken together, the patterns in Figures 14, 17 and 18 suggest that Hartz IV induced workers to seek gainful employment in lieu of mini-jobs that are implicitly subsidized by UI—but that it also pushed down wages for those who did so.

8 Conclusion

Many countries offer long-term unemployment assistance for claimants who have exhausted their initial stream of short-term 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 lengthy spells may depress wages, discourage jobseekers, 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 UI 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 12.4 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 only slightly offset by wage gains due to shorter jobless spells and by observable selection into reemployment. Holding constant the composition of the pool of successful jobseekers, I estimate that being subject to the Hartz IV benefit schedule reduced monthly earnings on a worker's first socially insured post-UI job by 1.9 percent. These negative impacts suggest that generous long-term benefits may promote productive job search and not merely moral hazard.

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 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 0.9 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 are a useful input into calibrating the market-wide impact of Hartz IV. Gauging the aggregate impact of Hartz IV—including its possible contributions to rising wage inequality (Dustmann et al., 2009), changes in equilibrium job composition (Tazhitdinova, 2016), and the “German employment miracle” of the late 2000s (Burda and Hunt, 2011)—is an important avenue for future research.67

Focusing purely 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 UI in June 2004, who experienced (often steep) benefit cuts overnight if still unemployed on January 1, 2005. Prior to the Hartz era, Germany’s generous safety net had historically been 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 rationally have expected to claim generous, wage-indexed benefits until entering 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. The causal estimates I estimate encompass both the existing stock of incumbent claimants and subsequent inflows into the UI system. In future work, I plan to further explore how benefit cuts impacted these long-standing UI beneficiaries. At a time of declining labor force participation and lackluster wage growth in the United States and elsewhere, 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.

67 A related literature has debated the macroeconomic impact of UI benefit extensions during the US Great Recession (Hagedorn et al., 2015, 2016; Coglianese, 2015; Chodorow-Reich and Karabarbounis, 2016).
Figures and Tables

Figure 1: Germany's growing long-term UI caseload in the lead-up to Hartz IV

Note: Aggregate UI caseloads at monthly frequency from Germany's Federal Employment Agency. Short-term and long-term UI refer to German Arbeitslosengeld and Arbeitslosenhilfe, respectively. I truncate the figure in December 2004 because the 2005 Hartz IV reform replaced the existing long-term UI system with a new benefit regime that encompassed both long-term UI claimants and non-UI welfare recipients (rendering the pre- and post-reform caseloads incomparable).

Figure 2: Pre-/post-UI changes in log monthly earnings by completed jobless duration

Note: Mean difference in log monthly earnings between the job held prior to UI entry and the first social-insurance-covered job obtained after UI entry, binning workers by completed jobless duration. I use a representative 4.7 percent sample of new UI claims initiated by prime-age displaced workers during 2001-2005. I winsorize pre- and post-UI wages at the 0.5th and 99.5th percentiles of the pre-UI wage distribution. Throughout the paper, I deflate wages to 2005 EUR using the consumer price index published by Germany's Federal Statistical Office.
Figure 3: Potential short-term benefit duration for new UI claims

Note: Calculation of potential short-term benefit duration for UI claimants in my estimation sample (ages 25-54 at the start of the claim). To establish an initial entitlement, a worker had to be employed in a socially insured job for at least 12 months out of the preceding 3 years. Potential duration is an increasing step function of months worked over the past seven years (with 2 months of benefits accruing for every 4 months of work), up to a maximum value determined by age at entry into UI. The duration ceilings for ages 45+ were lowered in February 2006, but claims already in progress were not subject to this change.

Figure 4: Empirical variation in potential short-term benefit duration

Note: Observed distribution of potential short-term benefit duration for UI claimants in my estimation sample (rounding up to the nearest month). Mass points at 12, 18, 22, and 26 months correspond to the age-specific ceilings shown in Figure 3. The remaining mass represents claimants who have not maxed out the potential duration attainable given their age. Although a standard UI entitlement lasts at least 6 months, seasonal workers not meeting the usual eligibility criteria are entitled to 3–4 months of short-term benefits if they have paid into the system for at least 6–8 months. In addition, unused benefits are carried forward in the event of a subsequent job loss, so that potential duration (in days) can take on any integer value from 1 to the age-specific ceiling.
Figure 5: Simulated reform-induced changes in household income after exhaustion of short-term UI

![Graph showing kernel density distribution of reform-induced change in post-exhaustion income for Men and Women.]

Note: Kernel-smoothed probability distribution functions for the simulated change in potential household income induced by Hartz IV (defined in (2)). I simulate the change in net income under the 2005 vs. 2004 tax-benefit system for each claimant in my estimation sample by running claimant characteristics through programmatic rules adapted from the OECD Tax-Benefit Model. I measure potential household income just after exhaustion of short-term benefits. See Appendix A for details on the simulation procedure.

Figure 6: Hypothetical benefit changes for pre-reform, interim, and post-reform cohorts

i. Enters UI in July 2001

![Diagram showing benefit changes for July 2001 cohort.]

ii. Enters UI in July 2003

![Diagram showing benefit changes for July 2003 cohort.]

iii. Enters UI in July 2005

![Diagram showing benefit changes for July 2005 cohort.]

Note: Hypothetical 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 sign and magnitude of the subsequent, reform-induced benefit change depend on a complex set of household characteristics and programmatic rules and vary across individuals.
Figure 7: Empirical job-finding hazards among claimants entitled to 12 months of short-term UI

Note: Raw empirical hazard rates of reemployment among new UI claimants ages 25–44 with exactly 12 months of potential short-term benefits (the maximum possible given their age, as well as the modal potential duration in the estimation sample). The “pre-reform” and “post-reform” cohorts consist of claims beginning in 2001 and 2005, respectively. By construction, all pre-reform spells are completed or censored no later than December 2003, prior to final passage of Hartz IV, and all post-reform spells are immediately subject to the post-Hartz long-term benefit level upon exhaustion of short-term benefits.

Figure 8: Relative job-finding hazards before and after short-term benefit exhaustion

Note: Estimated proportional effects of short-term benefit exhaustion on transitions to employment using the discrete-time hazard specification of (19) and (22). 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). Control variables are listed in the notes to Figure 9. See discussion in Section 5.1.
Figure 9: Benchmark effects of benefit step-downs on the hazard rate of reemployment

(a) Men

i. Short-term benefit exhaustion

ii. Incremental effect of Hartz IV

(b) Women

i. Short-term benefit exhaustion

ii. Incremental effect of Hartz IV

Note: Estimated proportional effects of UI benefit changes on transitions to employment using the discrete-time hazard specification of (19) and (20). The lefthand panels report normalized hazard ratios corresponding to the main effect of short-term benefit exhaustion ($\exp(\delta^F_I) - 1$). The righthand panels report the incremental hazard effect of Hartz IV ($\exp(\delta^H_I) - 1$). To allow for duration dependence, aggregate time effects, and seasonality, the model includes a nonparametric baseline hazard, a dummy for each quarter $\times$ year interaction, a dummy for each month (not interacted with year), and interactions between four quarter dummies and 3-month duration bins (allowing seasonal effects to vary over the course of an unemployment spell). I also include controls for East German residence, seven age bins, and one-year bins of time worked in the seven years preceding UI receipt ("experience"), as well as interactions between these controls and 3-month duration bins. Finally, I include dummies for deciles of prior wage, three education groups, German nationality, and three household types. Dashed lines represent 95 percent confidence intervals, obtained by clustering on individual. See discussion in Section 5.2.
Figure 10: Path of implied reemployment effects for the fully exposed 2005 UI cohort

Note: Implied effects of Hartz IV on individual reemployment rates, estimated separately for men and women. I use the benchmark specification from Figure 9 to predict the probability that an individual returns to work within 1–24 months of entering UI under both factual and counterfactual scenarios, where the latter sets the time-to-Hartz variable to the omitted category (≥ 10 months away). 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. Dashed lines represent 95 percent confidence intervals, based on 500 draws from the estimated variance-covariance matrix. See Section 5.3.

Figure 11: Robustness of hazard effects to alternative specifications

Note: Incremental effects of reform-induced benefit cuts on the hazard rate of reemployment. Specification 1 replicates the benchmark estimates from Figure 9. Specification 2 absorbs cohort effects by adding quarter-of-entry fixed effects at quarterly frequency. Specifications 3 and 4 allow the effects of age and experience to differ before/after Hartz IV became salient by including three-way interactions between age/experience, 3-month duration bins, and a post-July 2004 dummy. Specification 4 further adds a full set of interactions between 3-month duration bins and quarter × year effects, letting the shape of the hazard function vary freely over time. Specifications 5 and 6 limit the sample as indicated. The same estimates are presented (with standard errors) in Appendix Table 1. See Section 5.4.
Figure 12: Hazard effects of real and placebo UI reforms (benchmark specification)

Note: Estimated effects of real/placebo UI reforms on the reemployment hazard, using either the actual reform date (January 1, 2005) or a placebo date (January 1 of 2001, 2002, 2003, or 2004). For each specification, I construct a 2 percent sample of new UI claims begun between January 1 of year Y - 4 and June 30 of year Y, where Y is the assumed reform year. For the placebo specifications, I recode the time-to-Hartz event-time variable to measure time until the placebo reform. I modify the benchmark specification by censoring ongoing spells as of June 30 of year Y and by pooling all post-event periods into a single coefficient. See discussion in Section 5.5. See Appendix Figure 3 for additional specifications that allow the shape of the hazard rate to vary freely over time.

Figure 13: Relative job-finding rates among long-term UI claimants by tercile of baseline benefits

Note: Relative job-finding rates for a 2 percent sample of new long-term UI claimants during 2000–2004. I measure duration starting at entry into long-term UI, follow workers until reemployment, and censor incomplete spells 36 months after baseline or at the end of 2006 (whichever is earlier). I assign claimants to terciles of long-term benefit level within cells defined by sex × region × household type × year of entry into long-term UI. I specify the reemployment hazard as a function of quarter effects as well as a full set of quarter × benefit tercile interactions, plus a nonparametric hazard stratified by West/East as well as controls for age bins, German nationality, and household type. The plotted series report normalized hazard effects for the time × benefit tercile interactions.
Figure 14: Benchmark effects of benefit step-downs on the log ratio of post-UI to pre-UI wages

(a) Men

i. Short-term benefit exhaustion

ii. Incremental effect of Hartz IV

(b) Women

i. Short-term benefit exhaustion

ii. Incremental effect of Hartz IV

Note: 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 (30). I winsorize wages at the 0.5th and 99.5th percentiles of the pre-UI wage distribution within the estimation sample. The explanatory variables are the same as those described in the notes to Figure 9. Dashed lines are 95 percent confidence intervals, clustering on individual. See Section 6.1 for additional details.
Figure 15: Effects of long-term benefit cuts on transitions into full-time vs. part-time jobs

i. Hazard effects

[Graph showing hazard effects for full-time and part-time jobs for men and women, with effect size values (e.g., 0.8, 0.6, 0.4, 0.2, 0)]

ii. Implied cumulative incidence effects

[Graph showing cumulative incidence for full-time and part-time jobs for men and women, with effect size values (e.g., 5, 4, 3, 2, 1)]

Note: Lefthand panel: proportional effects of reform-induced benefit cuts on transition rates into socially insured full-time and part-time jobs, estimated using a competing-risks model that treats each job type as an absorbing state. The explanatory variables match those used in the benchmark hazard specification. Righthand panel: estimated effect of Hartz IV on the cumulative incidence of reemployment into full-time or part-time jobs. To compute these effects, I use the fitted model to predict the cumulative incidence of each reemployment risk for UI claims initiated in 2005 (so that all such claims are “fully exposed” to the reform), with and without the time-to-Hartz IV effect turned on. I then plot the gap between predicted and counterfactual cumulative incidence as a function of months since entry into UI. See Section 7.1 for details.

Figure 16: Effects of long-term benefit cuts on transitions into new jobs vs. recalls

i. Hazard effects

[Graph showing hazard effects for new jobs and recalls for men and women, with effect size values (e.g., 0.8, 0.6, 0.4, 0.2)]

ii. Implied cumulative incidence effects

[Graph showing cumulative incidence for new jobs and recalls for men and women, with effect size values (e.g., 5, 4, 3, 2, 1)]

Note: Lefthand panel: proportional effects of reform-induced benefit cuts on transitions into recalls (where the pre-UI and post-UI employer identifiers coincide) or new jobs (where they do not). I include the same explanatory variables used in the benchmark hazard specification. Righthand panel: estimated effects of Hartz IV on the cumulative incidence of reemployment into recalls or new jobs. See Section 7.2 for details.
Figure 17: Effects of long-term benefit cuts on competing risks of regular jobs vs. mini-jobs

Note: The grey series plot estimated proportional effects of reform-induced benefit cuts on the hazard rate of entry into a job of any kind, using a broader definition of employment that includes low-paid "mini-jobs" alongside regular jobs. The model is otherwise identical to the benchmark specification estimated in Figure 9. The blue and brown series plot proportional effects of reform-induced benefit cuts on cause-specific hazard rates of entry into regular jobs or mini-jobs (excluding spells in which the worker held a mini-job at UI entry). See Section 7.3 for details.

Figure 18: Effects of long-term benefit cuts on net transitions into regular jobs vs. mini-jobs

Note: The grey series plots the estimated impact of Hartz IV on cumulative reemployment based on a single-risk model in which the employment concept is broadened to include mini-jobs. The blue and brown series plot the estimated effect of Hartz IV on the cumulative incidence of reemployment into regular or mini-jobs. Due to estimation error, the single-risk series does not exactly equal the sum of the cause-specific series. See Section 7.3 for details.
Table 1: Summary statistics for the estimation sample and for an employed comparison group

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<tr>
<td>6 months</td>
<td>52.3</td>
<td>37.9</td>
</tr>
<tr>
<td>1 year</td>
<td>67.1</td>
<td>53.3</td>
</tr>
<tr>
<td>2 years</td>
<td>77.6</td>
<td>66.1</td>
</tr>
<tr>
<td>3 years</td>
<td>82.1</td>
<td>71.9</td>
</tr>
<tr>
<td>Exhausts short-term benefits</td>
<td>23.5</td>
<td>32.7</td>
</tr>
<tr>
<td>Monthly wage upon reemployment, 2005 EUR</td>
<td>1,935.8</td>
<td>1,465.2</td>
</tr>
<tr>
<td>Obtains mini-job prior to socially insured job (conditional on no mini-job at baseline)</td>
<td>(795.1)</td>
<td>(776.3)</td>
</tr>
<tr>
<td>Recalled to previous employer</td>
<td>35.8</td>
<td>23.8</td>
</tr>
<tr>
<td>Sample size</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of UI claims</td>
<td>209,896</td>
<td>126,738</td>
</tr>
<tr>
<td>Number of distinct individuals</td>
<td>143,629</td>
<td>101,037</td>
</tr>
</tbody>
</table>

Note: The estimation sample consists of prime-age workers (ages 25–54) who initiate a UI claim during 2001–2005 within 30 days of leaving 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. To match the temporal structure of the estimation sample, I first take quarterly snapshots of these workers on January 1, April 1, July 1, and October 1 for each year 2001–2005. I then reweight each observation so that the weight placed on each quarter equals the share of UI claims originating in that quarter. 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

<table>
<thead>
<tr>
<th>Year of UI entry</th>
<th>Month</th>
<th>% reemployed with reform</th>
<th>% reemployed w/o reform</th>
<th>Effect of Hartz IV</th>
<th>% reemployed with reform</th>
<th>% reemployed w/o reform</th>
<th>Effect of Hartz IV</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: Implied effects of the Hartz IV reform on the likelihood that an individual is reemployed within 6, 12, 18, 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 at each time horizon. I then repeat this prediction after setting the time-to-Hartz variable to the omitted category (> 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. See Section 5.3 for details.

Table 3: Implied reemployment effects for the 2005 cohort under alternative censoring horizons

<table>
<thead>
<tr>
<th>Censor at 1 year</th>
<th>Censor at 2 years</th>
<th>Censor at 3 years</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Men</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>At 12 Months</td>
<td>4.23</td>
<td>3.98</td>
</tr>
<tr>
<td>At 24 Months</td>
<td>2.97</td>
<td>2.86</td>
</tr>
<tr>
<td>At 36 Months</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Women</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>At 12 Months</td>
<td>6.69</td>
<td>5.86</td>
</tr>
<tr>
<td>At 24 Months</td>
<td>5.74</td>
<td>5.53</td>
</tr>
<tr>
<td>At 36 Months</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: 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 (the rest of the paper censors at 2 years). See notes to Table 2 for details.
Table 4: Implied effects of UI reform on mean log reemployment wages for the 2005 UI cohort

<table>
<thead>
<tr>
<th></th>
<th>Men</th>
<th>Women</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Overall wage effect</strong></td>
<td>-1.62</td>
<td>-2.02</td>
</tr>
<tr>
<td></td>
<td>(0.30)</td>
<td>(0.57)</td>
</tr>
<tr>
<td><strong>Benchmark decomposition</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reservation wage effect</td>
<td>-1.96</td>
<td>-2.27</td>
</tr>
<tr>
<td></td>
<td>(0.30)</td>
<td>(0.56)</td>
</tr>
<tr>
<td>Duration effect</td>
<td>0.27</td>
<td>0.14</td>
</tr>
<tr>
<td></td>
<td>(0.04)</td>
<td>(0.07)</td>
</tr>
<tr>
<td>Selection effect</td>
<td>0.07</td>
<td>0.12</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Netting out selection</td>
<td>-1.69</td>
<td>-2.14</td>
</tr>
<tr>
<td><strong>Alternative decomposition</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reservation wage effect</td>
<td>-1.92</td>
<td>-2.26</td>
</tr>
<tr>
<td></td>
<td>(0.30)</td>
<td>(0.57)</td>
</tr>
<tr>
<td>Duration effect</td>
<td>0.22</td>
<td>0.11</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.06)</td>
</tr>
<tr>
<td>Selection effect</td>
<td>0.08</td>
<td>0.13</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Netting out selection</td>
<td>-1.70</td>
<td>-2.15</td>
</tr>
</tbody>
</table>

Note: Estimated effects of reform-induced benefit cuts on log reemployment wages for workers entering UI in 2005 (expressed in log points). The overall effect and its components are computed as in (35) and (36). The top panel reports results from my benchmark decomposition, which weights the direct wage effects by the estimated counterfactual distribution of jobless duration in the absence of Hartz IV. The lower panel reports results from an alternative decomposition that instead weights these effects by the factual duration distribution. Standard errors in parentheses are based on 500 draws from the estimated variance-covariance matrix.
Supplementary Figures and Tables

Appendix Figure 1: UI-related Google searches spike in the summer of 2004

Note: Google Trends data showing the relative frequency of searches for “Arbeitslosengeld” (English: unemployment insurance) and “Hartz IV” within Germany between 2004 and 2006. Each series is rescaled to reach a maximum value of 100 in this period. Google Trends is unavailable prior to 2004.

Appendix Figure 2: Predicted jobless duration by quarter of UI entry

Note: 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. 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 3: Hazard effects of real and placebo UI reforms (time-varying duration effects)

i. Men

ii. Women

Note: See notes to Figure 12, which plots the effects of real and placebo UI reforms on the reemployment hazard. This figure augments each specification with a full set of interactions between 3-month duration bins and quarter \times year effects. These interactions allow the shape of the hazard rate to vary freely over time. See discussion in Section 5.5.

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

i. Men

ii. Women

Note: Each specification includes the same controls used in the benchmark specification of Figure 9. The potential duration of short-term benefits depends on both age and experience. Because all workers under 45 are subject to the same age-determined duration ceiling, variation in potential benefit duration within the 25–34 and 35–44 age groups (blue and orange series, respectively) is driven solely by differences in labor force attachment in the 7 years preceding UI entry. The red 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.
Appendix Figure 5: Heterogeneity of hazard effects in the month of reform-induced benefit cuts

Note: Each point corresponds to one of 36 cells defined by sex × West/East residence × three household structures × terciles of the initial short-term UI benefit level. I estimate the benchmark specification separately for each cell; because sample sizes are considerably smaller in this subgroup analysis, I pool the event months {−9, −8, −7}, {−6, −5, −4}, {−3, −2, −1}, {0}, {1, 2, 3}, and {4, 5, 6,...} to improve precision. For each cell, I plot the estimated hazard effect for the month of the reform-induced benefit cut (τ_i^H = 0) against the mean simulated change in potential post-exhaustion household income, computed as described in Appendix A. Numbers denote benefit terciles. Error bars indicate 95 percent confidence intervals, clustering by individual.

Appendix Figure 6: Robustness of wage effects to alternative specifications

Note: 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 14. See the notes to Figure 11 for a discussion of the control variables and sample restrictions used in each specification. The same estimates are presented (with standard errors) in Appendix Table 2.
Appendix Table 1a: Effects of long-term benefit cuts on reemployment hazards: men

<table>
<thead>
<tr>
<th>Months before</th>
<th>Omitted: ≥ 10 months before</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td>9 months before</td>
<td>0.01 0.03* 0.02 0.02 0.08*** -0.03</td>
<td>(0.02) (0.02) (0.02) (0.02) (0.02) (0.02)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>8 months before</td>
<td>0.11*** 0.11*** 0.12*** 0.12*** 0.22*** 0.09***</td>
<td>(0.02) (0.02) (0.02) (0.02) (0.03) (0.03)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>7 months before</td>
<td>0.14*** 0.15*** 0.15*** 0.15*** 0.23*** 0.18***</td>
<td>(0.02) (0.02) (0.02) (0.02) (0.03) (0.03)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>6 months before</td>
<td>0.14*** 0.17*** 0.17*** 0.17*** 0.23*** 0.28***</td>
<td>(0.02) (0.02) (0.03) (0.03) (0.03) (0.04)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>5 months before</td>
<td>0.16*** 0.19*** 0.22*** 0.22*** 0.23*** 0.27***</td>
<td>(0.02) (0.03) (0.03) (0.03) (0.03) (0.05)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4 months before</td>
<td>0.18*** 0.21*** 0.25*** 0.24*** 0.30*** 0.22***</td>
<td>(0.03) (0.03) (0.03) (0.04) (0.04) (0.05)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3 months before</td>
<td>0.21*** 0.25*** 0.28*** 0.27*** 0.34*** 0.39***</td>
<td>(0.03) (0.03) (0.03) (0.04) (0.04) (0.07)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2 months before</td>
<td>0.32*** 0.38*** 0.42*** 0.42*** 0.49*** 0.52***</td>
<td>(0.03) (0.04) (0.04) (0.04) (0.05) (0.08)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1 month before</td>
<td>0.34*** 0.41*** 0.43*** 0.44*** 0.49*** 0.51***</td>
<td>(0.03) (0.04) (0.04) (0.04) (0.05) (0.09)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Month of change</td>
<td>0.48*** 0.56*** 0.59*** 0.59*** 0.65*** 0.71***</td>
<td>(0.04) (0.04) (0.05) (0.05) (0.05) (0.11)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1 month after</td>
<td>0.23*** 0.32*** 0.32*** 0.32*** 0.32*** 0.24***</td>
<td>(0.04) (0.04) (0.04) (0.04) (0.04) (0.08)</td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>2 months after</td>
<td>0.05 0.14*** 0.14*** 0.14*** 0.16*** -0.03</td>
<td>(0.03) (0.04) (0.04) (0.04) (0.04) (0.07)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3 months after</td>
<td>-0.08** 0.02 0.01 0.06 -0.01 -0.09</td>
<td>(0.03) (0.04) (0.04) (0.04) (0.04) (0.07)</td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>4+ months after</td>
<td>0.03 0.15*** 0.15*** 0.20*** 0.09*** 0.19***</td>
<td>(0.02) (0.03) (0.03) (0.04) (0.03) (0.07)</td>
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</tr>
</tbody>
</table>

Note: 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. See Section 4.3 for a discussion of the benchmark hazard specification, which is replicated in column 1. See Section 5.4 for an explanation of the control variables and sample restrictions used in these specifications. The same estimates are plotted in Figure 11. Standard errors in parentheses are clustered on individual. * p ≤ .10, ** p ≤ .05, *** p ≤ .01.
Appendix Table 1b: Effects of long-term benefit cuts on reemployment hazards: women

<table>
<thead>
<tr>
<th>Months relative to reform-induced benefit cut</th>
<th>Omitted: ≥ 10 months before</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>9 months before</td>
<td>0.13***</td>
<td>0.15***</td>
<td>0.14***</td>
<td>0.14***</td>
<td>0.14***</td>
<td>0.13***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.04)</td>
<td>(0.04)</td>
<td></td>
</tr>
<tr>
<td>8 months before</td>
<td>0.17***</td>
<td>0.19***</td>
<td>0.22***</td>
<td>0.21***</td>
<td>0.21***</td>
<td>0.24***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.05)</td>
<td></td>
</tr>
<tr>
<td>7 months before</td>
<td>0.14***</td>
<td>0.18***</td>
<td>0.19***</td>
<td>0.17***</td>
<td>0.14***</td>
<td>0.20***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.05)</td>
<td></td>
</tr>
<tr>
<td>6 months before</td>
<td>0.18***</td>
<td>0.26***</td>
<td>0.31***</td>
<td>0.27***</td>
<td>0.26***</td>
<td>0.35***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.07)</td>
<td></td>
</tr>
<tr>
<td>5 months before</td>
<td>0.25***</td>
<td>0.34***</td>
<td>0.41***</td>
<td>0.37***</td>
<td>0.30***</td>
<td>0.28***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.04)</td>
<td>(0.06)</td>
<td></td>
</tr>
<tr>
<td>4 months before</td>
<td>0.25***</td>
<td>0.35***</td>
<td>0.42***</td>
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<td>0.31***</td>
<td>0.27***</td>
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</tr>
<tr>
<td></td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.07)</td>
<td></td>
</tr>
<tr>
<td>3 months before</td>
<td>0.31***</td>
<td>0.46***</td>
<td>0.53***</td>
<td>0.50***</td>
<td>0.41***</td>
<td>0.41***</td>
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<td>(0.04)</td>
<td>(0.05)</td>
<td>(0.06)</td>
<td>(0.06)</td>
<td>(0.05)</td>
<td>(0.09)</td>
<td></td>
</tr>
<tr>
<td>2 months before</td>
<td>0.32***</td>
<td>0.48***</td>
<td>0.58***</td>
<td>0.57***</td>
<td>0.39***</td>
<td>0.42***</td>
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<tr>
<td></td>
<td>(0.04)</td>
<td>(0.05)</td>
<td>(0.06)</td>
<td>(0.06)</td>
<td>(0.05)</td>
<td>(0.09)</td>
<td></td>
</tr>
<tr>
<td>1 month before</td>
<td>0.45***</td>
<td>0.66***</td>
<td>0.74***</td>
<td>0.73***</td>
<td>0.49***</td>
<td>0.38***</td>
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</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.06)</td>
<td>(0.07)</td>
<td>(0.07)</td>
<td>(0.06)</td>
<td>(0.09)</td>
<td></td>
</tr>
<tr>
<td>Month of change</td>
<td>0.54***</td>
<td>0.81***</td>
<td>0.86***</td>
<td>0.90***</td>
<td>0.60***</td>
<td>0.74***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.06)</td>
<td>(0.07)</td>
<td>(0.07)</td>
<td>(0.06)</td>
<td>(0.12)</td>
<td></td>
</tr>
<tr>
<td>1 month after</td>
<td>0.44***</td>
<td>0.74***</td>
<td>0.75***</td>
<td>0.78***</td>
<td>0.51***</td>
<td>0.56***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.06)</td>
<td>(0.07)</td>
<td>(0.08)</td>
<td>(0.06)</td>
<td>(0.12)</td>
<td></td>
</tr>
<tr>
<td>2 months after</td>
<td>0.14***</td>
<td>0.41***</td>
<td>0.41***</td>
<td>0.44***</td>
<td>0.20***</td>
<td>0.14</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.04)</td>
<td>(0.06)</td>
<td>(0.06)</td>
<td>(0.07)</td>
<td>(0.05)</td>
<td>(0.09)</td>
<td></td>
</tr>
<tr>
<td>3 months after</td>
<td>0.03</td>
<td>0.33***</td>
<td>0.31***</td>
<td>0.35***</td>
<td>0.07</td>
<td>0.01</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.06)</td>
<td>(0.07)</td>
<td>(0.07)</td>
<td>(0.05)</td>
<td>(0.09)</td>
<td></td>
</tr>
<tr>
<td>4+ months after</td>
<td>0.10**</td>
<td>0.53***</td>
<td>0.47***</td>
<td>0.58***</td>
<td>0.14***</td>
<td>0.28***</td>
<td></td>
</tr>
<tr>
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<td>(0.06)</td>
<td>(0.06)</td>
<td>(0.07)</td>
<td>(0.04)</td>
<td>(0.09)</td>
<td></td>
</tr>
</tbody>
</table>

Quarter-of-entry FEs

Age/experience x post-July 2004

Time-varying duration effects

One UI spell per individual

Enter UI prior to July 2004

Number of UI claims

Number of distinct claimants

Log likelihood

Note: See notes to Appendix Table 1a.
Appendix Table 2a: Effects of long-term benefit cuts on log reemployment wages: men

Omitted: ≥ 10 months before

<table>
<thead>
<tr>
<th>Months before</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>9 months</td>
<td>-0.69</td>
<td>-0.20</td>
<td>-0.40</td>
<td>-0.07</td>
<td>-1.55**</td>
<td>-1.12*</td>
</tr>
<tr>
<td></td>
<td>(0.47)</td>
<td>(0.48)</td>
<td>(0.49)</td>
<td>(0.51)</td>
<td>(0.69)</td>
<td>(0.65)</td>
</tr>
<tr>
<td>8 months</td>
<td>-0.63</td>
<td>0.02</td>
<td>0.08</td>
<td>0.78</td>
<td>-1.50**</td>
<td>-1.07</td>
</tr>
<tr>
<td></td>
<td>(0.52)</td>
<td>(0.53)</td>
<td>(0.57)</td>
<td>(0.61)</td>
<td>(0.74)</td>
<td>(0.78)</td>
</tr>
<tr>
<td>7 months</td>
<td>-0.07</td>
<td>0.78</td>
<td>0.62</td>
<td>1.39**</td>
<td>-0.53</td>
<td>-0.52</td>
</tr>
<tr>
<td></td>
<td>(0.59)</td>
<td>(0.60)</td>
<td>(0.65)</td>
<td>(0.68)</td>
<td>(0.83)</td>
<td>(0.93)</td>
</tr>
<tr>
<td>6 months</td>
<td>-1.14*</td>
<td>0.16</td>
<td>0.02</td>
<td>0.66</td>
<td>-1.62*</td>
<td>0.15</td>
</tr>
<tr>
<td></td>
<td>(0.67)</td>
<td>(0.68)</td>
<td>(0.75)</td>
<td>(0.77)</td>
<td>(0.92)</td>
<td>(1.20)</td>
</tr>
<tr>
<td>5 months</td>
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<td>0.31</td>
<td>0.85</td>
<td>-0.89</td>
<td>-0.64</td>
</tr>
<tr>
<td></td>
<td>(0.71)</td>
<td>(0.73)</td>
<td>(0.82)</td>
<td>(0.83)</td>
<td>(0.96)</td>
<td>(1.34)</td>
</tr>
<tr>
<td>4 months</td>
<td>-1.83**</td>
<td>-0.40</td>
<td>-0.52</td>
<td>0.03</td>
<td>-2.44**</td>
<td>-3.02**</td>
</tr>
<tr>
<td></td>
<td>(0.78)</td>
<td>(0.80)</td>
<td>(0.88)</td>
<td>(0.89)</td>
<td>(1.06)</td>
<td>(1.52)</td>
</tr>
<tr>
<td>3 months</td>
<td>-2.52***</td>
<td>-0.74</td>
<td>-0.81</td>
<td>-0.56</td>
<td>-2.84**</td>
<td>-4.05**</td>
</tr>
<tr>
<td></td>
<td>(0.87)</td>
<td>(0.90)</td>
<td>(0.97)</td>
<td>(0.99)</td>
<td>(1.15)</td>
<td>(1.91)</td>
</tr>
<tr>
<td>2 months</td>
<td>-3.38***</td>
<td>-1.57*</td>
<td>-1.18</td>
<td>-0.93</td>
<td>-3.41***</td>
<td>-4.96**</td>
</tr>
<tr>
<td></td>
<td>(0.91)</td>
<td>(0.93)</td>
<td>(1.02)</td>
<td>(1.04)</td>
<td>(1.20)</td>
<td>(2.06)</td>
</tr>
<tr>
<td>1 month</td>
<td>-4.61**</td>
<td>-2.64***</td>
<td>-2.32**</td>
<td>-2.04*</td>
<td>-5.44***</td>
<td>-6.16***</td>
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<tr>
<td></td>
<td>(0.99)</td>
<td>(1.02)</td>
<td>(1.11)</td>
<td>(1.12)</td>
<td>(1.32)</td>
<td>(2.39)</td>
</tr>
<tr>
<td>Month of change</td>
<td>-5.82***</td>
<td>-3.49***</td>
<td>-3.25***</td>
<td>-3.04***</td>
<td>-5.79***</td>
<td>-5.11*</td>
</tr>
<tr>
<td></td>
<td>(1.00)</td>
<td>(1.04)</td>
<td>(1.12)</td>
<td>(1.14)</td>
<td>(1.31)</td>
<td>(2.61)</td>
</tr>
<tr>
<td>1 month after</td>
<td>-7.98***</td>
<td>-5.51***</td>
<td>-4.70***</td>
<td>-4.29***</td>
<td>-6.94***</td>
<td>-4.13</td>
</tr>
<tr>
<td></td>
<td>(1.22)</td>
<td>(1.26)</td>
<td>(1.35)</td>
<td>(1.37)</td>
<td>(1.52)</td>
<td>(2.99)</td>
</tr>
<tr>
<td>2 months after</td>
<td>-5.34***</td>
<td>-2.72**</td>
<td>-1.82</td>
<td>-1.46</td>
<td>-5.12***</td>
<td>-2.89</td>
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<td></td>
<td>(1.31)</td>
<td>(1.36)</td>
<td>(1.44)</td>
<td>(1.48)</td>
<td>(1.64)</td>
<td>(2.95)</td>
</tr>
<tr>
<td>3 months after</td>
<td>-7.65***</td>
<td>-4.53***</td>
<td>-3.57**</td>
<td>-2.77*</td>
<td>-8.44***</td>
<td>-2.46</td>
</tr>
<tr>
<td></td>
<td>(1.45)</td>
<td>(1.52)</td>
<td>(1.57)</td>
<td>(1.62)</td>
<td>(1.86)</td>
<td>(3.18)</td>
</tr>
<tr>
<td>4+ months after</td>
<td>-4.63***</td>
<td>-1.25</td>
<td>0.14</td>
<td>0.22</td>
<td>-4.15***</td>
<td>-2.54</td>
</tr>
<tr>
<td></td>
<td>(0.86)</td>
<td>(1.10)</td>
<td>(1.15)</td>
<td>(1.33)</td>
<td>(1.05)</td>
<td>(2.42)</td>
</tr>
</tbody>
</table>

Quarter-of-entry FEs
Age/experience x post-July 2004
Time-varying duration effects
One UI spell per individual
Enter UI prior to July 2004

Number of UI claims                       162,963  162,963  162,963  162,963  101,460  114,245
Number of distinct claimants              105,282  105,282  105,282  105,282  101,460  82,899

Note: 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. See Section 6.1 for a discussion of the benchmark wage specification, which is replicated in column 1. The same estimates are plotted in Appendix Figure 6.
Appendix Table 2b: Effects of long-term benefit cuts on log reemployment wages: women

<table>
<thead>
<tr>
<th>Months relative to reform-induced benefit cut</th>
<th>Months of change</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Omitted: ≥ 10 months before</td>
<td>Omitted:</td>
<td>-0.96</td>
<td>-0.13</td>
<td>-1.14</td>
<td>-0.45</td>
<td>-1.25</td>
<td>-2.13</td>
</tr>
<tr>
<td>9 months before</td>
<td>9 months</td>
<td>-0.26</td>
<td>0.94</td>
<td>-0.69</td>
<td>0.87</td>
<td>0.07</td>
<td>-2.47</td>
</tr>
<tr>
<td>8 months before</td>
<td>8 months</td>
<td>-0.06</td>
<td>1.44</td>
<td>-0.71</td>
<td>0.83</td>
<td>0.08</td>
<td>-1.35</td>
</tr>
<tr>
<td>7 months before</td>
<td>7 months</td>
<td>-1.37</td>
<td>0.74</td>
<td>-1.15</td>
<td>-0.40</td>
<td>-0.26</td>
<td>-2.58</td>
</tr>
<tr>
<td>6 months before</td>
<td>6 months</td>
<td>-1.24</td>
<td>1.06</td>
<td>-0.47</td>
<td>0.26</td>
<td>-0.25</td>
<td>-1.26</td>
</tr>
<tr>
<td>5 months before</td>
<td>5 months</td>
<td>-0.64</td>
<td>1.94</td>
<td>0.36</td>
<td>1.03</td>
<td>0.23</td>
<td>-1.54</td>
</tr>
<tr>
<td>4 months before</td>
<td>4 months</td>
<td>-1.87</td>
<td>1.49</td>
<td>-0.51</td>
<td>0.32</td>
<td>-1.61</td>
<td>-5.05*</td>
</tr>
<tr>
<td>3 months before</td>
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<td>-2.80*</td>
<td>0.80</td>
<td>-1.06</td>
<td>-0.22</td>
<td>-2.25</td>
<td>-7.04**</td>
</tr>
<tr>
<td>2 months before</td>
<td>2 months</td>
<td>1.49</td>
<td>2.18</td>
<td>0.02</td>
<td>0.73</td>
<td>0.84</td>
<td>-1.41</td>
</tr>
<tr>
<td>1 month before</td>
<td>1 month</td>
<td>-7.54***</td>
<td>-2.25</td>
<td>-3.61*</td>
<td>-1.98</td>
<td>-8.20***</td>
<td>-5.84</td>
</tr>
<tr>
<td>Month of change</td>
<td>Month of change</td>
<td>-6.21***</td>
<td>-0.92</td>
<td>-2.75</td>
<td>-1.41</td>
<td>-5.61**</td>
<td>-12.34***</td>
</tr>
<tr>
<td>1 month after</td>
<td>1 month</td>
<td>-3.23</td>
<td>2.74</td>
<td>0.54</td>
<td>1.82</td>
<td>-1.24</td>
<td>-0.55</td>
</tr>
<tr>
<td>2 months after</td>
<td>2 months</td>
<td>-5.65***</td>
<td>2.36</td>
<td>-0.98</td>
<td>1.51</td>
<td>-5.57***</td>
<td>-1.19</td>
</tr>
<tr>
<td>3 months after</td>
<td>3 months</td>
<td>83,716</td>
<td>83,716</td>
<td>83,716</td>
<td>83,716</td>
<td>61,218</td>
<td>59,048</td>
</tr>
<tr>
<td>4+ months after</td>
<td>4+ months</td>
<td>63,884</td>
<td>63,884</td>
<td>63,884</td>
<td>63,884</td>
<td>61,218</td>
<td>48,575</td>
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</table>

Note: See notes to Appendix Table 2a.
### Appendix Table 3: Cross-validation of imputed spousal earnings

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1{Spouse is male}</td>
<td>2250.11***</td>
<td>(15.04)</td>
<td>2421.70***</td>
<td>(15.91)</td>
<td></td>
</tr>
<tr>
<td>1{Spouse is male} x 1{West Germany}</td>
<td>3034.09***</td>
<td>(19.30)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1{Spouse is male} x 1{East Germany}</td>
<td>868.42***</td>
<td>(21.63)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Claimant's monthly earnings</td>
<td>0.24***</td>
<td>(0.01)</td>
<td>0.38***</td>
<td>(0.01)</td>
<td>0.38***</td>
</tr>
<tr>
<td>1{At least one child}</td>
<td>-5.83</td>
<td>(17.02)</td>
<td>877.11***</td>
<td>(33.39)</td>
<td></td>
</tr>
<tr>
<td>Number of additional children</td>
<td>-202.47***</td>
<td>(10.09)</td>
<td>75.58***</td>
<td>(28.55)</td>
<td></td>
</tr>
<tr>
<td>1{At least one child &lt;5}</td>
<td>-368.93***</td>
<td>(18.94)</td>
<td>148.54***</td>
<td>(51.82)</td>
<td></td>
</tr>
<tr>
<td>Demographic controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sex of claimant</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Number of distinct claimants</td>
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<td>127,094</td>
<td>127,094</td>
<td>73,415</td>
<td>53,679</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.20</td>
<td>0.24</td>
<td>0.20</td>
<td>0.04</td>
<td>0.14</td>
</tr>
</tbody>
</table>

Note: Results from OLS regressions of imputed monthly spousal earnings on claimant characteristics. All specifications control for calendar-year dummies, region (West or East), seven age bins, and German nationality. Robust standard errors in parentheses are clustered on individual. See Appendix A for details.
Appendix A  Details on the Tax-Benefit Simulations

This appendix describes the methodology I use to simulate changes in the net long-term replacement rate induced by Hartz IV (as discussed in Section 2). My simulations are adapted from the publicly available Stata programs provided by the OECD Tax-Benefit Model (downloadable at http://www.oecd.org/els/soc/Models.zip).68

The IAB data report many of the key inputs into the tax and benefit calculations, including gross earnings, region, marital status, and the number of dependent children (along with the age of the youngest child). Another critical input—spousal earnings—is not directly available, but I can back out a proxy for spousal earnings 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, so that the asset means test for long-term UI receipt binds under neither pre- nor post-reform rules. Additional assumptions are needed for rental expenses, the ages of older children, and other inputs. I assume that households apply for any public benefits for which they are eligible, including unemployment assistance, housing benefits, and childcare benefits.

The basic simulation procedure is as follows:

1. Impute missing data inputs, notably spousal earnings.
3. Calculate the potential change in post-exhaustion household income defined in (2).

Income taxation and social insurance contributions

Federal income taxes are levied on gross household earnings, after deductions for social security contributions, work-related expenses, and special expenses. Married couples typically file jointly, and tax calculations employ the so-called “income splitting” method, under which gross earnings are assigned equally to each spouse for purposes of computing tax liability. Households with dependent children may claim either a child tax allowance or a child tax credit; personnel in the federal tax office make this choice on the household’s behalf, acting to minimize the household’s tax liability. Following the OECD, I therefore select the tax-minimizing option. Tax liability is computed as a piecewise quadratic function of taxable income, plus an additional “solidarity surcharge” levied on earnings above a statutory exemption. There are no state or local income taxes.

In addition to income taxes, individuals earning above the mini-job threshold are subject to mandatory social insurance contributions.69 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, so that payroll taxes amount to roughly 40 percent of gross earnings. Note that, unlike the US system, the German UI system does not experience-rate firms when assessing their UI contributions.

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68 I have also adapted Stata code from the TAXBENEXTRACT package published by Alexandre Desbuquois. Information in this section is taken from the German Social Code, the publication series Social Security at a Glance published by the German Federal Ministry of Labour and Social Affairs, and the annual OECD publication Taxing Wages (particularly the 2004 and 2005 editions).

69 Mini-jobs are exempted from worker-side social insurance contributions; employers, however, are subject to an omnibus 25 percent contribution rate for mini-jobs. Effective April 1, 2003, the Hartz II reform created an additional class of intermediate (“midi”) jobs with gross monthly earnings between €400–800. These jobs pay partial social insurance contributions at an intermediate rate.
Imputation of spousal earnings

Spousal earnings are a key determinant of long-term UI eligibility, but spouses are not linked in my data. I circumvent this problem by inferring spousal income from a claimant’s implied income tax liability prior to job loss. The initial monthly short-term UI benefit is calculated as

\[ \text{UI benefit} = RR \times (\text{gross monthly earnings} - \text{income tax} - \text{social insurance contributions}) \] (39)

I observe the initial UI benefit level as well as gross monthly earnings prior to UI receipt. As described above, social insurance contributions are a simple function of earnings. The replacement rate (RR) equals 60 or 67 percent, depending on whether the claimant has dependent children (also observed). I can therefore express a claimant’s income tax liability as a function of observables.

Next, I exploit the German tax system’s reliance on full income splitting for married couples. That is,

\[ \text{income tax liability} = f(0.5 \times [\text{own gross earnings} + \text{spousal earnings} + \text{unearned income}]) \] (40)

Given income tax liability and the claimant’s own gross earnings, I can back out the sum of spousal earnings and unearned income (such as dividends). For simplicity, I refer to this measure as a proxy for spousal earnings.

In Appendix Table 3, I cross-validate this proxy by testing a number of propositions one would expect to hold for a measure of spousal earnings:

1. Imputed spousal earnings are much higher for female claimants (with male spouses) than male claimants.
2. The gender gap in spousal earnings is larger in West Germany, 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. Calculating “spousal earnings” for single claimants yields much smaller numbers (not shown in table).

These checks provide some reassurance that—though imperfect—my proxy for spousal earnings contains meaningful information. I use this proxy when simulating income changes for married claimants.

Benefits under the 2004 rules

Unemployment benefits

Upon job loss, individuals who have established a UI entitlement can register as unemployed and begin receiving benefits. Short-term unemployment benefits (Arbeitslosengeld) replace a fixed fraction of a claimant’s prior earnings, net of income and payroll taxes. For dual-earning couples, I assign income taxes in proportion to each spouse’s gross earnings. The reference net earnings used

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70 Goldschmidt et al. (2014) match cohabiting spouses in the IAB based on surnames and residential addresses, but the match rate is fairly low and the matched sample is not fully representative.
71 German law did not permit same-sex couples to file their taxes jointly until 2013. Marital status in the IAB data pertains to husband-wife pairs, since same-sex marriage has not been legalized.
in the UI calculation are capped at €61,440 (annual), which almost never binds in my data. The net replacement rate is 60 percent for childless claimants and 67 percent for claimants with at least one dependent child. Benefits are paid for 30 days out of each month, and checks are issued at the end of the month. Unemployment benefits are not taxed.

The earnings disregard is the larger of €165 or 20 percent of the full benefit amount; earnings above this threshold reduce UI benefits one-for-one. Workers are also limited to 15 hours of work per week. In all simulations, I assume that claimants have zero earned income while unemployed.72

Upon exhausting short-term benefits, UI claimants could apply for tax-financed, means-tested long-term unemployment assistance (Arbeitslosenhilfe). 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. Furthermore, benefits are reduced one-for-one for spousal earnings net of taxes, work-related expenses, and a spousal earnings exemption.73

Finally, long-term UI benefits cannot be claimed unless (or until) household assets fall below an asset exemption. Prior to 2003, the asset exemption equaled €520 times the sum of the claimant’s age and (if married) spouse’s age, up to a maximum of €3,800. Under Article 11 of Hartz I, the asset limit fell to €200 per year of age for long-term UI claims initiated after January 1, 2003.74

Prior to Hartz IV, workers who continued to satisfy the means test (conducted once annually for continuing claims) could claim long-term UI benefits indefinitely until reaching retirement age.

Other benefits

Regardless of their employment status, sufficiently poor households could apply for additional means-tested welfare benefits, known as social assistance (Sozialhilfe). Social assistance is calculated by computing each household’s assessed need—the sum of individual allowances and housing/heat allowances—and deducting net income above a modest earnings disregard.

Individual allowances were revised annually and varied somewhat 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. Households applying for social assistance are reimbursed for all “reasonable” housing and heating expenditures. For simplicity, I set household rents (and corresponding benefit ceilings) based on benchmark values for Berlin in 2005.

Once household need has been computed, social assistance is means tested on the basis of household income—specifically, gross earnings (net of a disregard) plus unemployment benefits plus alimony minus income and payroll taxes.75 The level of social assistance then equals

\[
social\ assistance = \max(0, \text{assessed need} - \text{household income net of means testing}).
\]

72 Hartz IV reduced the earnings disregard for some workers by eliminating the 20 percent minimum disregard. After the reform, everyone’s disregard is capped at €165 per month.

73 The exemption level equals the larger of a legislated minimum or the spouse’s hypothetical long-term replacement rate in the event of unemployment. The legislated minimum has changed over time. Prior to 2003, the minimum exemption equaled €633 per month, corresponding to the “subsistence level” recognized by German law, plus a supplemental exemption of €151. In 2003, the minimum exemption fell to €482, which is 80 percent of the subsistence level, and the supplemental exemption was eliminated. The subsistence level itself was increased in 2004, so that the minimum spousal exemption stood at €511 per month in 2004.

74 The higher asset limit remained in place for workers born before 1948, all of whom fall outside my sample.

75 Single parents may claim alimony for up to 72 months for each child under age 12. I follow the OECD in applying the alimony rate for children under age 6, which in 2004 equals €122 per child per month.
In addition to social assistance, German households could apply for means-tested housing benefits (Wohngeld) to assist either with rent or mortgages/home maintenance. The benefit is a complex formula that depends both on household income and on rental expenditures, up to a ceiling that depends on household size and the municipality’s rent level. I follow the OECD in assigning the highest rent level (VI) to all households, regardless of where they reside.

Benefits under the 2005 rules

Hartz IV replaced long-term unemployment assistance and social assistance with a single, means-tested benefit (Arbeitslosengeld II). Rather than being indexed to prior wages, the new benefit level is determined by the household’s assessed living requirements. The core benefits consist of (i) lump-sum payments for basic living expenses, (ii) assistance with housing and utility expenses, and (iii) a temporary supplement to cushion the transition to the new benefit schedule.

In 2005, the standard monthly allowance equaled €345 in the West and €331 in the East. Singles receive 100 percent of the standard allowance; married couples receive 90 percent per spouse. The benefit payment is increased by 60 percent of the standard allowance for each dependent child up to age 14 and by 80 percent of the standard allowance for children 15 or older. The new benefit system also provides for housing and heating expenses. The level of housing assistance is not explicitly stated in the law; rather, municipalities are instructed to cover “reasonable” accommodation and heating costs. I use OECD estimates of housing and heating allowances for Berlin residents.

To ease the transition to the new system, the new benefit system includes a temporary supplement for long-term UI recipients who previously exhausted short-term 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. 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 (equaling €160 for singles and €320 for couples, plus €60 for each child). The supplemental payment and the payment ceiling decline are cut in half after one year and expire completely after two years. In my benefit simulations, I compute net replacement rates just after short-term benefit exhaustion, when the supplement is maximized.

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76 I depart from the OECD’s calculation in that I include housing/heating allowances in the post-reform benefit level for this calculation. Informational materials published by the Federal Employment Agency during the mid-2000s state explicitly that these allowances belong in the supplement formula.

77 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, when imputing the immediate benefit change experienced by a claimant at time $t^*$, 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).
Appendix B Further Discussion of the Placebo Exercise

In Section 5.5, I conduct a falsification exercise to test whether job-finding hazards respond to placebo long-term benefit reforms assumed to take effect on January 1 of 2001, 2002, 2003, or 2004. Although the placebo estimates for 2001, 2002, and 2003 are consistently close to zero, I do estimate modest, but statistically significant, increases in the reemployment hazard as workers approach the January 2004 placebo reform (see Figure 12).

What accounts for the 2004 placebo effects? One possibility is that, despite truncating the 2004 placebo analysis in June 2004, I may nonetheless 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 the scarring effects of long-term unemployment, they might rationally have begun reacting to Hartz IV far in advance of the actual implementation date.\textsuperscript{78} The degree of anticipation in the benchmark estimates of Figure 9—with significant increases in job-finding emerging as early as eight or nine months prior to benefit cuts—renders this conjecture plausible.

A second possibility is that I may be detecting lagged effects from stricter asset means-testing rules that apply to long-term UI claims initiated after January 1, 2003. The IAB data do not report assets, but I observe a small decline in the share of short-term UI exhaustees who transition to long-term UI after this date, suggesting that the new asset limits did bind for some claimants who would previously have been eligible. In unreported specifications, I find that interacting the main effect of benefit exhaustion ($r_{id}^E$) with a dummy for whether exhaustion is projected to occur under the tighter asset rules has little impact on the 2004 placebo effects, casting some doubt on this explanation. Adding these additional interactions to my benchmark specification also has little impact on my estimated Hartz IV effects.

A third possibility is that the placebo effects may stem from an increase in benefit sanctions that occurred in mid-to-late 2003. The Hartz I reform of January 1, 2003 narrowed the grounds on which claimants could turn down job offers without incurring sanctions, and an internal memorandum circulated by the Federal Employment Agency in April 2003 instructed caseworkers to apply sanctions more vigorously (Müller and Steiner, 2008). Nationwide, the number of sanctions imposed each month for refusing suitable job offers tripled over the course of 2003, peaking in September at 16,900, and then declined by half between September 2003 and the end of 2004.\textsuperscript{79} To my knowledge, the risk of being sanctioned during this period was not directly related to time remaining until benefit exhaustion.\textsuperscript{80} 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

\textsuperscript{78} The scope for anticipatory effects is strongest for high values of $r_{id}^{H,2004}$, which are closest in time to the onset of Hartz IV. For example, the positive placebo effects for $r_{id}^{H,2004} \geq 0$ observed among female claimants are identified by women observed in the first half of 2004, after Hartz IV was passed by the Bundestag.

\textsuperscript{79} These sanction counts are taken from aggregate data published by the Federal Employment Agency. Sanctions are difficult to identify in the IAB microdata during my sample period. However, there is considerable geographic variation in the degree to which sanctions rose in mid-2003, stemming both from local labor market conditions and from the policy preferences of the local employment offices (Müller and Oschmiansky, 2006; Müller and Steiner, 2008). I am working to obtain local sanction rates, so as to gauge whether the 2004 placebo effect is stronger in areas with elevated sanction rates. An advantage of this local approach is that it captures not only ex post "penalty effects", but also ex ante "threat effects": though only a small share of claimants are sanctioned in a given month, the risk of sanctions may have important deterrent effects, which should manifest at least partly at the local level.

\textsuperscript{80} Exploiting the discontinuity in potential benefit duration induced by the increased duration ceiling at age 50, Schmieder and Trenkle (2016) show that—at least during their 2008–2010 sample period—sanctioning probabilities over the course of an unemployment spell do not vary with baseline potential benefit duration, suggesting that caseworkers do not disproportionately sanction claimants who are close to benefit exhaustion.
with time spent out of work. To address this possibility, Appendix Figure 3 augments the placebo specifications with a full set of interactions between 3-month duration bins and quarter x year effects, to absorb any temporal changes in job-finding that are linked to duration since entry as distinct from duration until benefit cuts. Adding these controls strengthens the 2005 causal effects while attenuating the 2004 placebo effects, especially for women. In this specification—which most stringently isolates variation related to remaining benefit duration—the effects of the 2005 reform consistently lie above those of the 2004 pseudo-reform, and the peak effect is three times larger for both men and women.

It is difficult to adjudicate among competing explanations for the modest, but positive, 2004 placebo effect. Nonetheless, the placebo estimates weigh against a secular trend towards a stronger exhaustion spike, and they reveal much stronger responses to the true 2005 reform than to the earlier placebos.

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81 Caseworkers assess the suitability of a job offer in light of its compensation relative to prior earnings, alongside other criteria like commuting time and marital status. According to Ebbinghaus and Eichhorst (2009), "During the first three months, the unemployed can reject jobs offering less than 80 percent of prior earnings, thereafter less than 70 percent until the sixth month of unemployment, and finally after six months of unemployment, all jobseekers have to accept jobs providing net earnings equal to or higher than unemployment insurance benefits." Hartz I transferred the burden of proof for declining an "unsuitable" job offer from the caseworker to the claimant.
References for Chapter I


Chapter II

Import Competition and the Great US Employment Sag of the 2000s

With Daron Acemoglu, David Autor, David Dorn, and Gordon H. Hanson. Published in the Journal of Labor Economics, January 2016, 34(S1), S141-S198.

1 Introduction

During the last decade of the twentieth century—christened the “Roaring Nineties” by Krueger and Solow (2002)—the US labor market exhibited a vigor not seen since the 1960s. Between 1991 and 2000, the employment-to-population ratio rose by 1.5 percentage points among men, and by more than 3 percentage points among women. Following five years of rapid wage growth accompanied by minimal inflation, the national unemployment rate in the year 2000 reached a nadir of 4.0 percent, its lowest level since 1969. Just one year later, the US labor market commenced what Moffitt (2012) terms a “historic turnaround” in which the gains of the prior decade were undone. Between 2001 and 2007, male employment rates lost all of their ground attained between 1991 and 2000. The rapid increase of female employment rates halted simultaneously. The growth rate of employment averaged only 0.9 percent between 2000 and 2007—that is, during the seven years before the onset of the Great Recession—versus 2.6 percent between 1991 and 2000 (Figure 1).

This pre-Great Recession US employment “sag” of the 2000s is widely recognized but poorly understood. It coincides with a significant increase in import competition from China. Between

1 See http://www.bls.gov/ilc/#laborforce for data on the size and the employment rate of the working-age population.
2 The employment series plotted in Figure 1, as well as the employment statistics provided later in this section, are derived from the County Business Patterns. As detailed below, the County Business Patterns covers all US employment except for self-employed individuals, employees of private households, railroad employees, agricultural production employees, and most government employees.
3 Moffitt (2012) studies potential causes for the sag including wage levels, age structure, family structure, taxes, transfers, minimum wage policies, and population health. Only declining male wage rates are found to have substantial explanatory power. Yet, this explanation leaves unanswered the question of why male wages fell. The concurrence of
1990 and 2011, the share of world manufacturing exports originating in China increased from 2 percent to 16 percent (Hanson, 2012). China’s export surge is the outcome of deep economic reforms in the 1980s and 1990s, which were reinforced by the country’s accession to the World Trade Organization in 2001 (Naughton, 2007). The country’s share in US manufacturing imports has shown an equally meteoric rise from 4.5 percent in 1991 to 10.9 percent in 2001 before surging to 23.1 percent in 2011. Simultaneously, after staying relatively constant during the 1990s, US manufacturing employment declined by 18.7 percent between 2000 and 2007 (Figure 1).4

In this paper, we explore how much of the US employment sag of the 2000s can be attributed to rising import competition from China. Our methodology builds on recent work by Autor et al. (2013) and Autor et al. (2015), as well as related papers by Autor et al. (2014), Bloom et al. (2015), and Pierce and Schott (2015). Akin to Pierce and Schott (2015), we begin our analysis with industry-level empirical specifications.5 This approach enables us to estimate the direct effect of exposure to Chinese import competition on industry employment at the US national level. Our direct industry-level employment estimates come from comparing changes in employment across four-digit manufacturing industries from 1991 to 2011 as a function of industry exposure to Chinese import competition. The first part of our paper shows that there is a sizable and robust negative effect of growing Chinese imports on US manufacturing employment.

Quantitatively, our direct estimates imply that had import penetration from China not grown after 1999, there would have been 560,000 fewer manufacturing jobs lost through the year 2011. Actual US manufacturing employment declined from 17.2 million workers in 1999 to 11.4 million in 2011, making the counterfactual job loss from the direct effect of greater Chinese import penetration amount to approximately 10 percent of the realized job decline in manufacturing.

These direct effects do not, however, correspond to the full general equilibrium impact of growing Chinese imports on US employment, which also encompasses several indirect channels through which rising exposure to import competition may impact employment levels. One source of indirect effects, also studied by Pierce and Schott (2015), is industry input-output linkages. These falling wages and falling employment-to-population ratios suggests an inward shift in labor demand.

4 Using County Business Patterns data, we calculate that US manufacturing employment was 17.0 million in 1991, 17.1 million in 2000, 13.9 million in 2007, and 11.4 million in 2011.
linkages can create both positive and negative changes in US industry labor demand, generating a net employment change that is ambiguous in sign. If an industry contracts because of Chinese competition, it may reduce both its demand for intermediate inputs produced in the United States and its supply of inputs to other domestic industries. An industry may thus be negatively affected by trade shocks either to its domestic suppliers or to its domestic buyers. The sign of the "downstream effect"—through which import exposure propagates downstream from a supplying industry to its customers—is theoretically ambiguous: while trade competition may reduce the domestic supply of certain inputs, such reductions may be offset by the increased supply of imported inputs. By contrast, the "upstream effect"—whereby import exposure within an industry propagates upstream to its suppliers—should have unambiguously contractionary consequences for the upstream industry.

We use the US input-output table for 1992 to estimate the effects of upstream and downstream import exposure for both manufacturing and non-manufacturing industries. Our initial measure of the upstream (respectively, downstream) effect for an industry, which sums over the direct import exposure experienced by all other industries using as weights their share in the total output demands of (respectively, their input supplies to) the industry in question, captures this notion. Estimates from this exercise indicate sizable negative upstream effects while, consistent with the anticipated ambiguity of downstream effects, the downstream magnitudes are imprecisely estimated and unstable in sign. Our preferred measure of indirect trade shocks further accounts not only for shocks to an industry's immediate buyers or suppliers, but also for the full set of input-output relationships among all connected industries (e.g., shocks to an industry's buyers, its buyers' buyers, etc). Applying this direct plus full input-output measure of exposure increases our estimates of trade-induced job losses for 1999–2011 to 985,000 workers in manufacturing alone and to 1.98 million workers in the entire economy. Thus, interindustry linkages magnify the employment effects of trade shocks, doubling the size of the impact within manufacturing and producing an equally large employment effect outside of manufacturing.

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6 Trade shocks to an industry's suppliers will have negative effects on that industry if, due to specific investments, existing supply relationships are more productive or are able to provide highly customized inputs as generally presumed in the industrial organization literature on vertical integration (e.g., Williamson, 1975; Hart and Moore, 1990).

7 An earlier version of this paper (Acemoglu et al., 2014a) reversed the terminology of upstream and downstream effects. We have adopted the present terminology for consistency with common usage in the literature on input-output effects.

8 See Long and Plosser (1983) and Acemoglu et al. (2012) for the reasoning behind this value share definition, which also corresponds to the relevant entries in the input-output tables. A detailed derivation is provided in Appendix A.
Our second empirical strategy, which focuses on local labor markets, is motivated by the fact that analysis at the level of national industries fails to capture two other potentially important and opposing general equilibrium channels. One such additional channel is a reallocation effect from growing trade with China, which works through the movement of factors of production from declining sectors to new opportunities and potentially counteracts any negative direct or industry linkage effects. In both Heckscher-Ohlin and Ricardo-Viner models of international trade, stronger import competition for one sector reduces the relative price of its final good and induces the reallocation of labor and capital to sectors whose relative prices have increased (Feenstra, 2003). Under fully inelastic labor supply, no labor market frictions, and other neoclassical assumptions that ensure that the aggregate economy is always at full employment, reallocation effects would, by definition, exactly offset direct, upstream, and downstream effects so as to restore full employment. However, with imperfections in labor and other markets, there is no guarantee that reallocation effects will be sufficient to restore employment to the same level that would have emerged in the absence of trade growth from China.

An additional general equilibrium channel operates through aggregate demand effects, multiplying the negative direct and indirect effects of import growth from China. Through familiar Keynesian-type multipliers, domestic consumption and investment may be depressed, extending employment losses to sectors not otherwise exposed to import competition. A negative effect of increased import competition on aggregate demand necessarily requires that employment reallocation in response to a negative trade shock is incomplete, such that aggregate earnings decline and this decline is multiplied throughout the economy via demand linkages.

We jointly estimate reallocation and aggregate demand effects (in net) at the level of local labor markets by exploiting the impact of trade shocks within US commuting zones (CZs). If the reallocation mechanism is operative, then when an industry contracts in a CZ as a result of Chinese competition, some other industry in the same labor market should expand. Some component of aggregate demand effects should also take place within local labor markets, as shown by Mian and Sufi (2014) in the context of the recent US housing bust: if increased trade exposure lowers aggregate employment in a location, reduced earnings will decrease spending on non-traded local goods and services, magnifying the impact throughout the local economy. Because aggregate demand effects also have a national component, which our approach does not capture, focusing on local labor
markets is likely to provide a lower bound on the sum of reallocation and aggregate demand effects.\textsuperscript{9}

Empirically, our second strategy examines changes in employment in CZs that have different levels of exposure to Chinese competition by virtue of differences in their initial pattern of industrial specialization, a strategy also used by Autor et al. (2013). The reallocation effect should result in a greater expansion of employment in non-exposed industries—meaning non-tradable industries as well as tradable industries not significantly exposed to trade with China. Surprisingly, we find no robust evidence for this effect: the estimated impact of import competition on employment in non-exposed industries is very modest in magnitude and statistically indistinguishable from zero. The reallocation of employment into non-exposed industries appears to be swamped by the adverse effect of the aggregate demand channel, which presumably inhibits labor reabsorption.

Our estimates of local general equilibrium effects imply that import growth from China between 1999 and 2011 led to an employment reduction of 2.4 million workers, inclusive of employment changes within non-exposed sectors. Consistent with the idea that import competition may have negative general equilibrium effects on local employment, this figure exceeds our national-industry-level estimate of the direct and indirect disemployment effects of rising import exposure mentioned above. As noted below, neither the CZ-level nor the national estimate fully incorporates all of the adjustment channels encompassed by the other. The national-industry estimates exclude reallocation and aggregate demand effects, whereas the CZ estimates exclude the national component of these two effects, as well as the non-local component of input-output linkage effects. Because the CZ-level estimates suggest that general equilibrium forces magnify rather than offset the effects of import competition, we view our industry-level estimates of employment reduction as providing a conservative lower bound.

Our analysis of the aggregate employment consequences of import competition builds on the recent work of Autor et al. (2013) and Autor et al. (2015) by expanding their CZ-level analysis to include analysis at the level of national industries, a dimension they do not consider, and by characterizing the alternative mechanisms—reallocation versus changes in aggregate demand—

\textsuperscript{9} Of course, reallocation effects may also have a national component due to the movement of labor across regions. As we discuss in Section 2, in practice there appears to be little response of local labor supply to location-specific increases in import competition from China (Autor et al., 2013, 2014), leading us to view reallocation effects as being primarily local in nature. Another complicating factor is that, in the presence of labor and product market imperfections, the decline of an industry in the local labor market may lead to the expansion of some tradable industries in other labor markets, making the local reallocation effects a lower bound on the aggregate reallocation effects.
through which trade induces employment decline at the local level. Our national-industry approach is similar in spirit to that of Bloom et al. (2015) and Pierce and Schott (2015). Pierce and Schott, in particular, explore how China’s 2001 World Trade Organization (WTO) accession affected US manufacturing employment. Our paper, while complementary to theirs, differs in two respects. The first is in terms of identification strategy. Whereas Pierce and Schott seek to identify the growth in China trade that resulted from the post-2001 removal of uncertainty surrounding China’s most-favored-nation access to the US market, our identification strategy captures China’s trade growth due to broader productivity-driven changes in its export supply. Further, our paper expands the analysis to include the transmission of trade shocks to non-manufacturing sectors and the estimation of employment effects resulting from reallocation across sectors and changes in aggregate demand.

We begin in Section 2 by outlining the conceptual framework that motivates our empirical analysis. Section 3 describes our empirical approach to estimating the effects of exposure to trade shocks and briefly discusses the data. Section 4 gives our primary OLS and 2SLS estimates of the impact of trade shocks on employment, and also considers additional labor market outcomes. Section 5 expands the analysis to include intersectoral linkages. Section 6 presents estimation results for data on local labor markets. Section 7 concludes. Appendix A contains the derivation of our upstream and downstream import exposure measures from a simple general equilibrium model with input-output linkages.

2 Conceptual Framework

We start with a brief outline of the conceptual framework that motivates our empirical work. Consider a simple decomposition of the total national employment impact of increased Chinese trade exposure:\(^\text{10}\)

\[
\text{National employment impact} = \text{Direct impact on exposed industries} + \text{Indirect impact on linked industries} + \text{Aggregate reallocation effects} + \text{Aggregate demand effects}
\]

\(^\text{10}\) We follow the standard practice in such decompositions and fold the “covariance” terms into the “main effects” (so that the magnitudes are not independent of the order in which these different terms are evaluated).
Here, the direct impact is the reduction in employment in industries whose outputs compete with imports from China. Added to this direct effect is an indirect effect arising because other industries linked to the impacted industry through the input-output matrix are also likely to see changes in output. For example, the chemical and fertilizer mining industry—which is in non-manufacturing—sells 74 percent of its output to the manufacturing sector. Its largest single manufacturing customer is industrial organic chemicals not elsewhere classified, which accounts for 15 percent of its sales. Similarly, the iron and ferroalloy ores industry sells 83 percent of its output to the manufacturing sector, two-thirds of which goes to the blast furnace and steel mill industry. Accordingly, a shock to the demand for a given domestic manufactured good is likely to indirectly impact demand for, and reduce employment in, industries, whether in manufacturing or non-manufacturing, that supply inputs to the affected industry. We refer to these linkages as upstream effects, by which industries exposed to import competition indirectly affect industries that are located upstream of them in input-output space.

Conversely, a trade shock to the suppliers of a given industry (e.g., the suppliers of tires to the automobile industry) may also affect the industries that are its customers. The direction of this effect is generally ambiguous. On the one hand, from the perspective of purchasing industries, the trade shock expands input supply and puts downward pressure on input prices, and thus may tend to expand employment in the industries that consume these inputs (Goldberg et al., 2010). On the other hand, the trade shock may destroy existing long-term relationships for specialized inputs as domestic input suppliers are driven out of business, creating a force towards contraction in the industries that were their customers. We refer to such linkages as downstream effects, since they propagate from an import-exposed industry to industries located downstream in the production chain. We estimate these effects on linked industries using the input-output matrix of the US economy as described below.

We begin our empirical analysis with industry-level regressions that estimate the direct im-

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11 See, among others, Long and Flosser (1983) and Acemoglu et al. (2012) on the propagation of shocks through the input-output network of the economy.
12 Unfortunately, the terminology of upstream and downstream effects is open to confusion, since upstream effects—that is, effects that propagate upstream—work through the import exposure experienced by downstream industries, and similarly for downstream effects.
13 Consistent with this reasoning, De Loecker et al. (2014) find substantial negative domestic product price effects from trade liberalization in India, and Goldberg et al. (2010) document that greater availability of imported intermediate inputs is associated with more rapid introduction of new product varieties by domestic firms, also in the Indian context.
pact of import competition on employment in exposed industries (Section 4) and subsequently add the indirect employment impacts arising from input-output linkages between industries (Section 5). The industry-level analysis thus captures the first two components of the aggregate national employment effect, the direct impact on exposed industries plus the indirect impact on linked industries. The industry-level regressions do not, however, encompass the third and the fourth components of the national employment effect: the reallocation effect, which captures the potential increase in employment from the expansion of other industries to absorb the factors of production freed by contracting industries, and the aggregate demand effect, which corresponds to the impact of Keynesian-type multipliers operating through local or national shifts in consumption and investment.\(^\text{14}\)

To obtain estimates of the magnitudes of these two additional effects, we turn in Section 6 to local labor market analysis, focusing on the employment impact of increased import competition from China at the commuting zone level. The total employment effect observed in a local labor market can be decomposed as:

\[
\text{Local employment impact} = \text{Direct impact on exposed industries} + \text{Local impact on linked industries} + \text{Local reallocation effects} + \text{Local demand effects}
\]

We hypothesize that the direct impact at the local level, when scaled appropriately by the size of the industry in the local labor market, is comparable to the direct impact estimated at the national level. The other three effects could potentially differ between the local and the aggregate levels. For instance, even though linked industries tend to co-locate (e.g., Ellison et al., 2010), only part of the input-output linkages will be within the same local labor market, and the local impact on linked industries may thus be much smaller than the aggregate effect.

What makes our local labor market analysis informative is that local reallocation and local demand effects are linked to their aggregate counterparts. Consider the reallocation effects first.

\(^{14}\) It is in theory possible for the aggregate demand effect to be positive; for instance, aggregate demand may increase because the aggregate price level declines as a result of the lower costs of imported products from China. We view this positive channel as second-order and in general presume that the aggregate demand effect, working in the standard Keynesian fashion, amplifies the potential negative direct impact of trade shocks. This is consistent with the results from our local labor market analysis, which indicate that the sum of reallocation and demand effects is negative.
Local labor markets are a plausible unit of analysis for the study of this channel. As a local labor market experiences a loss of jobs when local industries contract in response to rising import competition, there should be an adjustment of quantities within the same labor market, despite the fact that prices are, at least in part, determined in the national or the international equilibrium. If the extent of worker migration between local labor markets in response to these labor market shocks is modest, as suggested by the evidence in Autor et al. (2013), Notowidigdo (2013), and Autor et al. (2014), this adjustment will take the form of reallocation from declining industries to others within this locale. 15

An important component of aggregate demand effects also plausibly takes place within local labor markets. Mian and Sufi (2014) show that during the Great Recession, US counties suffering large wealth losses because of particularly severe declines in housing values also saw large declines in employment, consistent with local transmission of shocks to aggregate demand. Components of the aggregate demand effect that operate at the national level will not be captured by our analysis, however, as they will be common across locations. Our empirical strategy seeks to identify the combined impact of reallocation and aggregate demand effects by quantifying how trade-induced shocks impact a commuting zone’s employment in non-exposed industries—defined as industries that are not exposed to imports from China either through direct product market competition or through interindustry purchases of intermediate inputs.

Overall, this discussion suggests that our local labor market strategy will provide an informative alternative estimate of the aggregate employment impact of greater import competition from China, though this is likely to be an underestimate of the aggregate effects because it ignores part of the impact on linked industries and also excludes demand effects that have no counterpart at the local level. In what follows, we will separately compute the implied aggregate effects consisting of the sum of the direct impact and the impact on linked industries from our national-industry-level analysis, and the total employment impact from the local analysis.

15 Complementing this US-based evidence, Balsvik et al. (2014) and Dix-Carneiro and Kovak (2015) document weak labor mobility responses to trade-induced employment shocks in Norway and Brazil, respectively. As discussed in Footnote 9, there are some components of reallocation that might take place outside the local labor market.
3 Empirical Approach

Sweeping economic reforms initiated in the 1980s and extended in the 1990s permitted China to experience rapid industrial productivity growth (Naughton, 2007; Hsieh and Ossa, 2011; Zhu, 2012), rural to urban migration flows in excess of 150 million workers (Li et al., 2012), and massive capital accumulation (Brandt et al., 2012), which together caused manufacturing to expand at a breathtaking pace. What did this growth mean for US employment inside and outside manufacturing? We seek to capture the changes in US industry employment induced by shifts in China’s competitive position and the subsequent increase in its exports, accounting for input-output linkages between industries and other indirect channels of transmission. We subsequently consider how these labor demand shifts can be aggregated to national totals.

3.1 Industry trade shocks

Our baseline measure of trade exposure is the change in the import penetration ratio for a US manufacturing industry over the period 1991–2011, defined as

\[
\Delta IP_{jt} = \frac{\Delta M_{jt}^{UC}}{Y_{jt,91} + M_{jt,91} - E_{jt,91}},
\]

where for US industry \( j \), \( \Delta M_{jt}^{UC} \) is the change in imports from China over the period 1991–2011 (which in most of our analysis we divide into two subperiods, 1991–1999 and 1999–2011) and \( Y_{jt,91} + M_{jt,91} - E_{jt,91} \) is initial absorption (measured as industry shipments, \( Y_{jt,91} \), plus industry imports, \( M_{jt,91} \), minus industry exports, \( E_{jt,91} \)). We choose 1991 as the initial year as it is the earliest period for which we have the requisite disaggregated bilateral trade data for a large number of country pairs that we can match to US manufacturing industries.\(^{16}\) The quantity in (1) can be motivated by tracing export supply shocks in China—due, e.g., to productivity growth—through to demand for US output in the markets in which the United States and China compete. Supply-driven changes in China’s exports will tend to reduce demand for and employment in US industries.

One concern about (1) as a measure of trade exposure is that observed changes in the import

\(^{16}\)Our empirical approach requires data not just on US trade with China but also on China’s trade with other partners. Specifically, we require trade data reported under Harmonized System (HS) product codes in order to match with US Standard Industrial Classification (SIC) industries. The year 1991 is the earliest in which many countries began using the HS classification.
penetration ratio may in part reflect domestic shocks to US industries that affect US import demand. Even if the dominant factors driving China’s export growth are internal supply shocks, US industry import demand shocks may still contaminate bilateral trade flows. To capture this supply-driven component in US imports from China, we instrument for trade exposure in (1) with the variable

$$\Delta IPO_{j,\tau} = \frac{\Delta M^{OC}_{j,\tau}}{Y_j,88 + M_j,88 - X_j,88},$$

where $\Delta M^{OC}_{j,\tau}$ is the growth in imports from China in industry $j$ during the period $\tau$ (in this case 1991–2011 or some subperiod thereof) in eight other high-income countries excluding the United States. The denominator in (2) is initial absorption in the industry in 1988. The motivation for the instrument in (2) is that high-income economies are similarly exposed to growth in imports from China that is driven by supply shocks in the country. The identifying assumption is that industry import demand shocks are uncorrelated across high-income economies, and that there are no strong increasing returns to scale in Chinese manufacturing (which might imply that US demand shocks will increase efficiency in the affected Chinese industries and induce them to export more to other high-income countries).

Appendix Figure 1 plots the value in (1) against the value in (2) for all US manufacturing industries at the four-digit level, as defined below, which is equivalent to the first-stage regression in our subsequent estimation without detailed controls. The coefficient is 0.98 and the t-statistic and $R^2$ are 7.0 and 0.62 respectively, indicating the strong predictive power of import growth in other high-income countries for US import growth from China.

A potential concern about our analysis is that we largely ignore US exports to China, focusing primarily on trade flows in the opposite direction. This is for the simple reason that our instrument, by construction, has little predictive power for US exports to China. Nevertheless, to

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17 These countries are Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland, which represent all high-income countries for which we can obtain disaggregated bilateral trade data at the Harmonized System level back to 1991.
18 See Autor et al. (2013) and Autor et al. (2014) for further discussion of threats to identification using this instrumentation approach.
19 Modeling the China trade shock as in (1) does not exclude the role of global production chains. During the 1990s and 2000s, approximately half of China’s manufacturing exports were produced by export processing plants, which import parts and components from abroad and assemble these inputs into final export goods (Feenstra and Hanson, 2005). Our instrumental variable strategy does not require China to be the sole producer of the goods it ships abroad; rather, we require that the growth of its gross manufacturing exports is driven largely by factors internal to China (as opposed to shocks originating in the United States), as would be the case if, plausibly, the recent expansion of global production chains involving China is primarily the result of its hugely expanded manufacturing capacity.
the extent that our instrument is valid, our estimates will correctly identify the direct and indirect
effects of increased import competition from China (this is in particular because there is no reason
for trade to balance at the industry or region level, so we do not need to simultaneously treat exports
to China in our analysis). We also take comfort from the fact that imports from China are much
larger—approximately five times as large—than manufacturing exports from the United States to
China (Figure 2).20

3.2 Data sources

Data on international trade for 1991–2011 are from the UN Comtrade Database (http://comtrade.un.org/db/default.aspx), which gives bilateral imports for six-digit Harmonized Commodity Description and Coding System (HS) products. To concord these data to four-digit Standard Industrial Classification (SIC) industries, we first apply the crosswalk in Pierce and Schott (2012), which assigns 10-digit HS products to four-digit SIC industries (at which level each HS product maps into a single SIC industry), and aggregate up to the level of six-digit HS products and four-digit SIC industries (at which level some HS products map into multiple SIC industries). To perform this aggregation, we use data on US import values at the 10-digit HS level, averaged over 1995–2005. The crosswalk assigns HS codes to all but a small number of SIC industries. We therefore slightly aggregate the four-digit SIC industries so that each of the resulting 397 manufacturing industries matches to at least one trade code, and none is immune to trade competition by construction. To ensure compatibility with the additional data sources below, we also aggregate together a few additional industries such that our final data contains 392 manufacturing industries. All import amounts are inflated to 2007 US dollars using the Personal Consumption Expenditure (PCE) deflator.

Our main source of data on US employment is the County Business Patterns (CBP) for the years 1991, 1999, 2007, and 2011. CBP is an annual data series that provides information on employment, firm size distribution, and payroll by county and industry. It covers all US employment except self-employed individuals, employees of private households, railroad employees, agricultural

20 A second rationale for our import focus is data constraints. Much of US exports to China are in the form of indirect exports via third countries or embodied services of intellectual property, management expertise, or other activities involving skilled labor. These indirect and service exports are difficult to measure because the direct exporter may be a foreign affiliate of a US multinational or because they occur via a chain of transactions involving third countries. As such exports tend to be intensive in highly skilled labor, they may have only modest direct impacts on the employment of production workers—though their indirect impacts are difficult to gauge with available data.
production employees, and most government employees.\textsuperscript{21}

To supplement the employment and establishment count measures available from the CBP, we utilize the NBER-CES Manufacturing Industry Database for the years 1971–2009 (the latter being the latest year available).\textsuperscript{22} These data allow us to explore labor market outcomes not reported in the CBP, as well as to perform a falsification exercise not possible in the CBP. We additionally draw on the NBER-CES data to compute measures of the production structure in each industry, subsequently used as controls, including production workers as a share of total employment, the log average wage, the ratio of capital to value added, computer investment as a share of total investment, and high-tech equipment as a share of total investment. Additionally, we create industry pretrend controls for the years 1976–1991, including the changes in industry log average wages and in the industry share of total US employment.

A final data source used in our analysis is the 1992 input-output table for the US economy (from the US Bureau of Economic Analysis, \url{http://www.bea.gov/industry/io_benchmark.htm}), which we use to trace upstream and downstream demand linkages between industries both inside and outside of US manufacturing. We discuss our application of input-output tables in more detail below.

\textsuperscript{21} CBP data are extracted from the Business Register, a file of all known US companies that is maintained by the US Census Bureau; see \url{http://www.census.gov/econ/cbp/index.html}. To preserve confidentiality, CBP information on employment by industry is sometimes reported as an interval instead of an exact count. We compute employment in these cells using the fixed-point imputation strategy developed by Autor et al. (2013).

\textsuperscript{22} The NBER-CES database contains annual industry-level data from 1958–2009 on output, employment, payroll and other input costs, investment, capital stocks, TFP, and various industry-specific price indexes (Becker et al., 2013). Data and documentation are at \url{http://www.nber.org/data/nberces5809.html}. 

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4 Estimates of the Direct Impact of Trade Exposure on Employment

We begin by estimating the direct effect of trade exposure on employment over the period 1991–2011 using aggregate, industry-level regressions.

4.1 Baseline results for national industries

Our initial specification is of the following form:

\[ \Delta L_{j\tau} = \alpha_\tau + \beta_1 \Delta IP_{j\tau} + \gamma X_{j0} + \epsilon_{j\tau}, \]  

(3)

where \( \Delta L_{j\tau} \) is 100 times the annual log change in employment in industry \( j \) over time period \( \tau \); \( \Delta IP_{j\tau} \) is 100 times the annual change in import penetration from China in industry \( j \) over period \( \tau \) as defined in (1); \( X_{j0} \) is a set of industry-specific start of period controls (specified later); \( \alpha_\tau \) is a period-specific constant; and \( \epsilon_{j\tau} \) is an error term. We fit this equation separately for stacked first differences covering the two subperiods 1991–1999 and 1999–2011, where in some specifications we shorten the second subperiod to 1999–2007 in order to evaluate employment impacts prior to the onset of the Great Recession. Variables specified in changes (denoted by \( \Delta \)) are annualized since (3) is estimated on periods of varying lengths. The elements in the vector of controls \( X_{j0} \), when included, are each normalized with mean zero so that the constant term in (3) reflects the change in the outcome variable conditional only on the variable of interest, \( \Delta IP_{j\tau} \). Most outcome variables are measured at the level of 392 four-digit manufacturing industries, while later models also estimate spillovers to 87 non-manufacturing industries. Regression estimates are weighted by start-of-period industry employment, and standard errors are clustered at the three-digit industry level to allow for arbitrary error correlations within larger industries over time.\(^{23}\)

Table 1 summarizes the import exposure and employment variables used in initial estimates of (3). The employment-weighted mean industry saw Chinese import exposure rise by 0.5 percentage points per year between 1991 and 2011, with more rapid penetration during 1999–2007 than during 1991–1999: 0.8 versus 0.3 percentage points, respectively. Growth from 2007 to 2011, at 0.3

\(^{23}\) There are 135 three-digit manufacturing industry clusters encompassing the 392 four-digit industries. Because our non-manufacturing data have already been extensively aggregated to 87 industries for concordance with the BEA input-output table, we treat each of the 87 non-manufacturing industries as a single cluster.
percentage points per year, indicates a marked slowdown in import expansion in the late 2000s. The slowdown during that period is the combined effect of a steep decline in US trade in 2008 and 2009 and an equally dramatic recovery in 2010 (Levchenko et al., 2010), which together left import penetration rates modestly higher.\textsuperscript{24}

Changes in import penetration are highly right-skewed across manufacturing industries, with the mean increase exceeding the median by a factor of 3.5. We find a similar pattern of import penetration change and skewness in the other high-income countries used to construct the import penetration instrument, where this skewness reflects China’s strong comparative advantage in labor-intensive industries. Table 1 also shows that the manufacturing decline accelerated throughout the sample: the average industry contracted by 0.3 log points per year between 1991 and 1999, by 3.6 log points per year between 1999 and 2007, and by 5.7 log points per year in the final period 2007–2011. The within-industry growth rate of non-manufacturing employment also slowed across the three subperiods of our sample, but the deceleration was not nearly as pronounced as in manufacturing.

Table 2 presents a simple stacked first-difference model for the two time periods 1991–1999 and 1999–2011, with the change in import penetration and a dummy for each time period as the only regressors. Alongside these estimates, we also present results from stacking the time periods 1991–1999 and 1999–2007, and from fitting the model separately for the three subperiods 1991–1999, 1999–2011, and 1999–2007. These additional specifications permit inspection of results before and after the commencement of the 2000s US employment sag, and allow for comparison of the results for the 2000s with and without including the Great Recession years. We also present results for the single long difference, 1991–2011, for comparison against the stacked first differences.

In column 1, which excludes the import penetration variable, the time dummies reflect the (employment-weighted) mean annual within-industry change in employment in each period. Column 2 adds the observed import exposure measure without instrumentation. This variable is negative and highly significant, consistent with the hypothesis that rising import penetration lowers domestic industry employment. Nevertheless, as noted above, this OLS point estimate could be biased because growth in import penetration is driven partly by changes in domestic supply

\textsuperscript{24}Explanations for the excess sensitivity of trade flows during the Great Recession include the role of shocks to the credit market and trade finance (Amiti and Weinstein, 2011; Chor and Manova, 2012), and to global production networks (Levchenko et al., 2010). Other explanations dwell on the large drop in durable good spending during the crisis (Eaton et al., 2013).
and demand. Column 3 mitigates this simultaneity bias by instrumenting the observed changes in industry import penetration with contemporaneous changes in other-country China imports as specified in (2) above. The estimate in column 3 implies that a one percentage point rise in industry import penetration reduces domestic industry employment by 1.3 percentage points (t-ratio of 3.2).

Column 4, which stacks the periods 1991–1999 and 1999–2007, shows that the coefficient of import penetration is very similar if we restrict attention to the years preceding the Great Recession.

The remaining columns of Table 2 present bivariate estimates of this relationship separately by subperiod. The coefficient on trade exposure is negative and statistically significant in all time periods, and is largest in absolute value for 1991–1999 and smallest for 1999–2007. Even though the sensitivity of employment to import penetration is greater before 2000, the much faster growth in China’s imports after 2000 produces an overall impact of trade on employment that, as we discuss below, is considerably larger in the latter period. The sensitivity of employment to trade for 1999–2011 is similar to the estimate for 1999–2007, despite the onset of the global financial crisis in 2007 and the associated dislocation of worldwide trade patterns.\(^{25}\)

A simple long-difference model for the change in manufacturing employment over the full 1991–2011 period (column 8) also supports a negative relationship between import penetration and US manufacturing employment. The coefficient estimates in column 3, for the stacked first differences, and column 8, for the long time difference, are quite similar, reflecting strong persistence in the growth in China’s import penetration within industries. Replacing stacked first differences with the long difference may remove cyclical variation in the data, accounting for the mildly larger coefficient estimates in the latter case.

Returning to the results in column 3 of Table 2, we evaluate the economic magnitude of these estimates by constructing counterfactual changes in employment that would have occurred absent increases in Chinese import competition. Using (3), we write the difference between actual and counterfactual manufacturing employment in year \(t\) as

\[
\Delta L^c_t = \sum_j L_{jt} \left[ 1 - e^{-\beta_1 \Delta \bar{P}_{jt}} \right],
\]

\(^{25}\)In the United States, imports plus exports divided by GDP fell by a stunning 22 percent from the first quarter of 2008 to the first quarter of 2009. However, imports fully recovered in 2010 and continued to grow in 2011. The exaggerated cyclical swings in trade surrounding the Great Recession thus mix with the continued secular growth in China’s exports to the United States over the period.
where \( \hat{\beta}_1 \) is the 2SLS coefficient estimate from (3) and \( \Delta \hat{IP}_{jt} \) is the increase in import penetration from China that we attribute to China's improving competitive position in industry \( j \) between 1991 (or 1999) and year \( t \). Following Autor et al. (2013), we estimate \( \Delta \hat{IP}_{jt} \) by multiplying the observed increase in import penetration \( \Delta IP_{jt} \) with the partial \( R \)-squared from the first-stage regression of (1) on the instrument in (2), which has a value of 0.56 in our baseline specification in column 3 in Table 2. When our instrument is valid and there is no measurement error, this partial \( R \)-squared adjusted \( \Delta \hat{IP}_{jt} \) variable is a consistent estimate of the contribution of Chinese import supply shocks to changes in import penetration. In constructing the counterfactuals, we further assume that all other factors, including observed covariates and unobserved shocks captured by the error term in (3), would be unaffected by the artificially imposed reduction in the growth of import penetration from China.

We collect these counterfactual estimates in Table 8, where we compare employment estimates across three different estimation strategies. The first row of Table 8 reports counterfactual employment differences implied by the estimates in Table 2, where we evaluate changes for 1991–1999, 1999–2011, and the entire 1991–2011 period. Using coefficient estimates from column 3, we calculate that had import penetration from China remained unchanged between 1991 and 2011, manufacturing employment would have fallen by 837,000 fewer jobs over the full 1991–2011 span and by 560,000 fewer jobs during the employment sag era of 1999–2011. Observed manufacturing employment changes over these time periods were minus 5.6 million workers (11.4 million – 17.0 million) and minus 5.8 million workers (11.4 million – 17.2 million), respectively. The larger quantity for the second period is indicative of the modest growth in manufacturing employment of 200,000 workers that occurred between 1991 and 1999. By shutting down China's import growth, the contraction of US manufacturing employment suggested by our estimates would have been 14.9 percentage points smaller over 1991–2011, and 9.7 percentage points smaller for the period after 1999. It is also worth noting that counterfactual reductions in employment for the period 1991–2007—based on the specification in column 4 of Table 2—amount to 853,000, quite similar to our estimates for 1991–2011.
4.2 Comparison to other estimates in the literature

How do our estimates of the direct effect of import competition on manufacturing employment compare with those found in the literature? There are few estimates to consider, as the majority of work on the labor market implications of globalization addresses not the absolute employment effects of trade, but its impact on relative wages and relative employment levels by skill (e.g., Harrison et al., 2011). Trade impacts on absolute employment levels are a less common object of study, perhaps reflecting modeling conventions that impose inelastic labor supply and full employment.

In an influential treatment of trade impacts on US manufacturing, Bernard et al. (2006) estimate that import penetration from low-income countries—with China being the largest member of this group by far—accounts for 14 percent of the total decline in manufacturing employment of 675,000 workers that occurred between 1977 and 1997.26 Their specification differs from ours, making a direct comparison of the two sets of results difficult to perform. They regress the change in log employment at the level of the manufacturing plant (rather than industry) on the initial level (rather than change) of the share of low-income countries in industry imports (rather than the import penetration rate). Despite these differences, Bernard et al. find a relatively high sensitivity of employment to import competition. But over their period of study, the annual increase in import penetration from low-income countries in US manufacturing was only 0.09 percentage points,27 whereas over our sample period the annual increase in import penetration from China alone was 0.50 percentage points (Table 1). Had their much lower level of import growth obtained over our sample period, the reduction in manufacturing job loss implied by our coefficient estimates would have been only one-fifth as large.28 One reason why Bernard et al.'s analysis may produce higher estimates of the impact of imports on employment than ours is that they study plant-level data as compared to our industry-level regressions. Aggregating across plants within an industry is preferable in this instance because it avoids confounding aggregate effects with within-industry reallocation, which

26 In related work, Artuç et al. (2010) evaluate how costs to workers of moving between sectors dampen the employment response to changes in trade barriers, and Muehler and Becker (2010) and Harrison and McMillan (2011) estimate the responsiveness of employment in multinational companies to changes in foreign wages. This work tends to emphasize the elasticity of employment with respect to changes in trade barriers or foreign production costs, rather than producing estimates of aggregate impacts of foreign competition on employment.

27 This figure comes from information provided in Table 2 of Bernard et al. (2006).

28 This ratio is based on the calculation, \((1 - e^{-1.30 \times .56 \times .09}) / (1 - e^{-1.30 \times .56 \times .50}) = 0.21\), where the value -1.30 is the coefficient from column 3 of Table 2 and the value .56 discounts observed changes in import penetration by the partial R-squared of the first stage.
take place as some workers may exit declining plants to take jobs with establishments in their same sector (consistent with the results in Autor et al., 2014).

Pierce and Schott (2015) use a difference-in-difference strategy to test whether after 2001 manufacturing employment fell by more in industries that were more exposed to China’s WTO accession. They measure this potential increase in exposure to China trade using the difference between the US MFN (most favored nation) tariff and the US non-MFN tariff—to which China was potentially subject prior to becoming a WTO member and whose level was substantially higher than the MFN duty. Pierce and Schott thus identify the growth in China trade after 2001 using the notional reduction in US trade barriers confronting China. A complication with this approach is that the US granted China MFN status on a renewable basis in 1980, two decades prior the country joining the WTO. The US non-MFN tariff is only a meaningful predictor of China’s pre-2001 trade to the extent that there was genuine risk the US government would choose not to renew China’s MFN privileges, an eventuality that Congress discussed annually but that never materialized. Pierce and Schott estimate that China’s WTO accession reduced post-2001 manufacturing employment by 15.1 log points in exposed industries relative to non-exposed industries.29 Our estimates, which identify the impact of growth in China’s imports based on the common component of the country’s export expansion across high-income markets, imply that had there been no increase in import penetration from China after 1999, the 2011 level of employment would have been 4.9 percent higher (0.560m/11.4m) than it otherwise would have been. Comparing our results in Table 2 to Bernard et al. (2006) and to Pierce and Schott (2015) thus suggests that our estimates for the direct industry-level employment effects of China trade are relatively modest.

4.3 Controlling for industry confounds and pretrends

A challenge for our analysis is that industries subject to greater import competition may be exposed to other economic shocks that are correlated with China trade. We begin to address this concern in Table 3 by incorporating controls for potential industry confounds. We additionally offer a set of falsification tests.

We consider three groups of control variables. First, we probe the robustness of our results by including dummies for 10 one-digit manufacturing sectors. Since our regressions are in first

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29 This estimate is from column 3 of Table 1 of their paper, which we view as closest in spirit to the specifications in our paper.
differences, the inclusion of these dummies amounts to allowing for differential trends across these one-digit sectors. Regressions including these dummies therefore identify the industry-level impacts of trade exposure while purging common trends within the one-digit sectors and using only variation in import growth across industries with relatively similar skill intensities.

Technological progress within manufacturing has been most rapid in recent decades in computer and skill-intensive sectors (Doms et al., 1997; Autor et al., 1998). To capture the extent to which industries are exposed to technical change, we next add a second set of control variables, drawn from the NBER-CES database, measuring the intensity of their use of production labor and capital. These variables, summarized in Appendix Table 1, include the share of production workers in total employment, the log of the average wage, the ratio of capital to value added (all measured in 1991), as well as computer and high-tech equipment investment in 1990, each expressed as a share of total 1990 investment.

US manufacturing as a share of employment has been declining since the 1950s, and the number of manufacturing employees has also trended downward since the 1980s. This long-standing secular trend highlights a concern that the correlation we document between rising industry trade penetration and contemporaneous, within-industry declines in manufacturing employment during 1991–2011 could potentially predate the recent rise in import exposure. In that case, our estimates would likely overstate the impact of trade exposure in the current period. We therefore finally add measures of pretrends in industry employment and earnings in Table 3, specifically the change in the industry’s share of total US employment, and the change in the log of the industry average wage, both measured over the interval 1976–1991 (Appendix Table 1).

The first seven columns of Table 3 permute among combinations of these three groups of industry controls: the one-digit sector dummies, industry-level controls for production structure, and industry-level controls for pretrends. Column 1 replicates results from column 3 of Table 2 to serve as a benchmark. Among the additional groups of covariates, only the one-digit sector dummies have a substantial impact on the point estimates, reducing the (instrumented) estimates by about 40 percent.\textsuperscript{30} Though the inclusion of the sectoral dummies is an important robustness check for our results, there are two reasons why these specifications may underestimate the impact

\textsuperscript{30} Quantitatively, the specification in column 2 of Table 3 implies that had import penetration from China remained unchanged between 1991 and 2011, manufacturing employment would have fallen by 463,000 jobs over the full 1991–2011 span, and by 307,000 jobs between 1999 and 2011, which are about 45 percent lower than our baseline numbers.
of Chinese import competition. First, trade exposure at the four-digit industry level is likely to be measured with error, and the inclusion of the one-digit sector dummies will then cause significantly greater attenuation of our estimates of the impact of Chinese import growth. Second, if there is a significant increase in imports in some industries within a one-digit sector (say, in women’s dresses within textiles), then employers in other similar industries within this broad sector (say, women’s blouses and shirts, also within textiles) may anticipate greater competition both from the substitutes already being imported from China and also from future waves of Chinese imports, and thus will be more likely to downsize and close existing plants and less likely to open new plants. By contrast, neither the production nor the pretrend variables have an important effect on the magnitude or precision of the coefficient of interest. As a further robustness test, column 8 includes a full set of dummies for the 392 four-digit manufacturing industries in our data. These variables serve as industry-specific trends in our stacked first-difference specification, so the effect of import competition on industry employment in this specification is identified by changes in the growth rates of industry employment and import penetration in 1999–2011 relative to 1991–1999. Remarkably, relative to specifications that include one-digit sector dummies, the addition of an exhaustive set of industry-specific trends only modestly reduces the point estimate and precision of the coefficient of interest, thus highlighting the robustness of the relationship. In summary, while our preferred industry-level model from column 3 of Table 2 allows for an impact of Chinese trade competition on employment both within and across broad manufacturing subsectors, the estimates in Table 3 document that a sizable negative employment effects remains even when focusing only on the within-subsector or within-industry, over-time variation in trade exposure.

As a falsification exercise, Table 4 reports results from a regression of changes in industry employment in earlier decades on the instrumented change in industry import exposure between 1991 and 2011. It would be problematic for our identification strategy if future growth in Chinese import exposure predicted industry employment declines in the era prior to China’s trade opening.\textsuperscript{31} Panel A performs this exercise without additional covariates, while panel B controls for 10 one-digit sector

\textsuperscript{31}To carry the analysis back to 1971, we employ the NBER-CES data, which covers a longer time horizon than the County Business Patterns data used in our main estimates. A disadvantage is that the NBER-CES database is currently only updated through 2009, two years less than the CBP. To improve comparability, we use the NBER data in all columns of Table 4, including for the post-1990 period (unlike in Tables 2 and 3, where we use CBP data). These estimates also differ from those in Tables 2 and 3 in that the import exposure variable (and its instrument) corresponds to the long 1991–2011 change in all columns.
dummies. In both panels, the estimated relationship between our China trade exposure measure and industry employment is statistically insignificant and close to zero in both the 1970s (1971–1981) and 1980s (1981–1991). The point estimate only becomes economically large and statistically significant after 1990. This pattern of results is consistent with the hypothesis that the within-industry correlation between rising import penetration and declining manufacturing employment in the 1990s and 2000s emanates from contemporaneous trade shocks rather than long-standing factors driving industry decline.

4.4 Additional employment and establishment-level outcomes

We have so far focused on the effects of trade exposure on industry employment, which is but one margin along which industries adjust. Others include the wage bill, establishment size, establishment shutdown, and production versus non-production employment and earnings. Using a combination of CBP and NBER-CES data, we explore these outcomes in Table 5.

Given our findings on how import penetration affects employment in Tables 2 and 3, many of the results in Table 5 are in line with expectations. Stronger import competition reduces the count of establishments (column 2), average employment per establishment (column 3), and total industry wage payments (column 4). Production employment (column 6) declines slightly more than non-production employment (column 7), indicating a larger sensitivity to Chinese import competition on the part of lower-skilled labor, a result consistent with China’s strong comparative advantage in labor-intensive sectors.

The table also contains some informative surprises. Trade exposure predicts a rise in real industry log wages for production workers (column 8)—that is, the real production worker wage bill divided by the production worker headcount. The impact on non-production worker wages (column 9) is negative but small and not statistically significant. Joining these two effects produces the positive but insignificant coefficient estimate for average real wages (column 5). The results for production workers that combine strongly negative employment effects and mildly positive average wage effects are suggestive of trade-induced changes in the composition of employment. Less highly paid workers may be those more likely to be laid off within the subgroup of production employees, leading to an upward shift in wages among those still employed as a result of unobserved changes in composition. This interpretation is consistent with Autor et al.’s (2014) finding that the earnings
of lower wage workers are most adversely affected by greater import competition.\textsuperscript{32}

5 Accounting for Sectoral Linkages

We now expand the scope of the inquiry to encompass the effects of trade shocks on employment in both manufacturing and non-manufacturing industries working through input-output linkages. In Appendix A, we present a simple model of Cobb-Douglas production that yields expressions for changes in industry employment resulting from upstream and downstream import exposure. Here we discuss the empirical implementation of these upstream and downstream effects.

To study these interindustry linkages, we envisage an economy along the lines of that studied by Long and Plosser (1983) and Acemoglu et al. (2012), where each industry uses with different intensities the output of other industries as inputs. We apply this methodology to the Bureau of Economic Analysis’ input-output table for 1992. We choose the 1992 input-output table since it largely predates the China trade shock and hence measures linkages that are unlikely to be endogenous to the subsequent shock.

To estimate the upstream effect—the exposure to import competition that propagates upstream from an industry’s buyers—we calculate the following quantity for each industry $j$,

$$\Delta IP_{j\tau}^U = \sum_g w_{gj} \Delta IP_{g\tau},$$  \hspace{1cm} (5)

which is equal to the weighted average change in import penetration during time interval $\tau$ across

\textsuperscript{32} Complementing these results, Appendix Table 2 reports the impact of Chinese import competition on industry output, measured as the value of shipments. In panel A, we find that import exposure has an economically and statistically significant negative effect on nominal shipments (column 1), but when we decompose this effect into changes in real shipments and changes in the shipments price deflator (columns 2 and 3), we find no effect on real shipments. This surprising pattern turns out to be driven by computer-producing industries, which experienced rapid growth in real value added, precipitous declines in output prices, and substantial increases in Chinese import penetration during our sample period. In panel B, where we exclude 28 computer-producing industries corresponding to NAICS 334, we find comparable effects on nominal shipments, but these effects are now driven primarily by relative declines in real shipments in trade-exposed industries, rather than by relative declines in output prices. We view these results as consistent with a mounting body of evidence that computer-producing industries have an outsized influence on measured output and productivity in the manufacturing sector (Houseman et al., 2015; Acemoglu et al., 2014b).
all industries, indexed by $g$, that purchase from industry $j$. These weights $w_{gj}^U$ are defined as

$$w_{gj}^U = \frac{\mu_{gj}^U}{\sum_{g'} \mu_{g'j}^U}, \quad (6)$$

where $\mu_{gj}^U$ is the 1992 “use” value in the BEA input-output matrix for the value of industry $j$’s output purchased by industry $g$, such that the weight in (6) is the share of industry $j$’s total sales that are used as inputs by industry $g$. Thus, (5) is a weighted average of the trade shocks faced by the purchasers of $j$’s output.\(^3^3\) When industry $j$’s purchasers suffer a negative trade shock, they are likely to reduce demand for $j$’s output. The theoretical justification for these expressions is provided in Appendix A using a simple model of input-output linkages.

Similarly, to compute the downstream effect $\Delta IP^{D}_{j\tau}$ experienced by each industry $j$—that is, the exposure to import competition that propagates downstream from $j$’s suppliers—we make the same calculation after reversing the $j$ and $g$ indexes in the numerator of (6).\(^3^4\) We instrument both the upstream and downstream exposure measures analogously to our main import shock measure: using contemporaneous changes in China imports in eight other high-income countries to calculate predicted upstream and downstream exposure for each industry, where these predictions serve as instruments for the measured domestic values. Concretely, we construct these instruments by replacing the term $\Delta IP_{jr}^V$ with $\Delta IP_{g\tau}$ in (5), while retaining the same weights.

Equation (5) accounts for the direct (first-order) effect on output demand of an industry $j$ stemming from trade-induced changes in demand from its immediate buyers. But it ignores further indirect effects on industry $j$’s demand stemming from changes in demand from its buyers’ buyers, and so on. To account for the full chain of linked downstream and upstream demands, we replace $\Delta IP_{jr}^U$ and $\Delta IP_{jr}^D$ (and their instruments) with the full chain of implied responses from the input-output matrix, which is given by the Leontief inverse of the matrix of upstream and downstream

\(^3^3\) We use the BEA “make” table to assign commodities to the industries that produce them. The summation in the denominator of (6) runs over not only manufacturing industries, but also non-manufacturing industries as well as final demand. Since our direct shock variable only reflects manufacturing trade, all upstream effects experienced by a sector emanate by definition from shocks to their manufacturing purchasers (that is, $\Delta IP_{jr}$ is defined to equal zero for non-manufacturing industries and for final demand). These shocks affect both manufacturing and non-manufacturing industries to the degree that they supply inputs to manufacturing industries $g$ that are directly shocked.

\(^3^4\) When we construct weights for the downstream effect, the summation in the denominator again runs over industry $j$’s total sales. Analogously to the case of upstream effects, downstream effects emanate from trade shocks to these industries’ suppliers in manufacturing (though, as just noted, both manufacturers and non-manufacturers may have suppliers in manufacturing).
linkages (see, e.g., Acemoglu et al., 2012). The details of this computation are given in Appendix A. Upstream and downstream exposure measures are summarized in Appendix Table 3. As expected, the indirect exposure measures are substantially smaller in magnitude, and have far less cross-industry variation, than the direct exposure measures. In the average manufacturing industry, direct trade exposure is five times as large as the first-order downstream exposure measure and over three times as large as the first-order upstream exposure measure. Incorporating higher-order linkages significantly increases the magnitude of the upstream and downstream exposure measures. The full indirect upstream exposure measure (given by the Leontief inverse) is approximately half as large as the direct exposure measure, while the full indirect downstream exposure measure is about one-third as large as the direct exposure measure.

The two panels of Table 6 present instrumental variables estimates of the effects of import exposure on industry employment, akin to those in Table 3 column 1 (without the one-digit sector dummies) and column 2 (with the one-digit sector dummies), here augmented with the upstream and downstream import exposure measures. The upper panel of Table 6 employs the first-order upstream and downstream measures, \( \Delta IP_{j}^{U} \) and \( \Delta IP_{j}^{D} \), while the lower panel uses the full Leontief exposure measures. We present results with and without the one-digit sector dummies introduced earlier.\(^{35}\)

Columns 1 through 3 of Table 6 consider the impact of upstream and downstream linkages on employment in the 392 manufacturing industries; columns 4 and 5 consider these impacts on employment in the 87 non-manufacturing industries; and columns 6–10 present results for manufacturing and non-manufacturing pooled. All regressions employ the stacked first differences specification: columns 1 through 8 and 10 cover the time periods 1991–1999 and 1999–2011, while column 9 shortens the second period to 1999–2007. Downstream import effects are not statistically significant in any specification, and are unstable in sign, showing up as positive in the manufacturing only specification (column 2) and negative in the non-manufacturing and pooled specifications (columns 5 and 7).\(^{36}\) This imprecision may be due to the fact that the downstream effects combine the offset-

\(^{35}\)We do not include the industry production and pretrend controls used in Table 3. These were shown to have little effect conditional on sector dummies but still absorb degrees of freedom, which is problematic in a setting with multiple instrumented endogenous variables that are themselves correlated.

\(^{36}\)Additionally, the downstream effect in manufacturing reverses sign (while remaining insignificant) when the upstream variable is omitted. Observe that there is no 'direct' trade exposure effect in non-manufacturing since our trade measures are confined to manufactured goods.
ting effects of reduced domestic input supply (due to US-based suppliers curtailing shipments in the face of increased import competition) and increased foreign input supply. Given the instability of effects working through downstream linkages, we focus our attention on the upstream effects, which are, in contrast, quite stable across specifications and are qualitatively similar for manufacturing and non-manufacturing sectors.

Consistent with our reasoning above, growth in an industry’s upstream trade exposure is found to reduce industry employment. For manufacturing industries alone, the coefficient of the upstream linkage effect is quite large without the one-digit sector dummies in the regression (column 2), and of similar magnitude to the direct trade shock coefficient as well as more precisely estimated when the one-digit sector dummies are added in column 3. For non-manufacturing industries, upstream linkages are also negative and statistically significant (columns 4 and 5), and larger in magnitude than the estimates for manufacturing. Pooling manufacturing and non-manufacturing, coefficients on upstream linkages are negative and statistically significant either without (columns 6 and 7) or with (column 8) the one-digit sector dummies included in the regression. Results for the period 1991–2007 (column 9) are quantitatively similar.

Finally, in the last specification in Panel B (column 10), we regress changes in industry employment on the sum of the direct and upstream exposure measures, which is the form suggested by our theoretical model in Appendix A. As expected, the estimated coefficient on the combined shock lies between the coefficients on the direct and upstream effects in column 6.

Comparing across the two panels of Table 6, which employ the first-order (panel A) and full (panel B) upstream and downstream measures, we detect a similar pattern of coefficient estimates. In all cases, the coefficients on the full exposure measures are smaller in magnitude than those on the first-order exposure measures, though they are also more precisely estimated. Of course, the full exposure measures are considerably larger in magnitude than the first-order exposure measures, so the smaller coefficients do not imply smaller quantitative effects.

---

37 The non-manufacturing estimates do not include sector dummies (unlike the manufacturing estimates) since our non-manufacturing industry scheme is already highly aggregated and, moreover, does not collapse down readily to a one- or two-digit sector scheme since we had to extensively aggregate four-digit SIC industries for concordance with the input-output tables used by the BEA.

38 We cannot reject the hypothesis that the coefficient on this combined variable is the same as the separate coefficients on the direct and the upstream exposure measures in column 2. The implied quantitative magnitudes (reported below) are also very similar regardless of whether we use this combined measure or separate measures for direct and indirect upstream effects.
Accounting for upstream linkages substantially increases the impact of trade shocks on employment. Using estimates from the regression that pools manufacturing and non-manufacturing together (column 6, the specification without one-digit sector dummies), we evaluate the counterfactual change in employment analogous to the exercise in (4), with the results again shown in Table 8. This new exercise combines the employment impacts of trade shocks working through direct effects and indirect effects associated with upstream linkages.\textsuperscript{39} Had import competition from China remained unchanged between 1991 and 2011, according to our estimates from panel A (using only first-order upstream effects), there would have been 1.33 million additional workers employed in manufacturing and 805,000 additional workers employed in non-manufacturing, for a total employment differential of 2.14 million workers. Examining just the 1999–2011 period, the corresponding counterfactual employment additions are 928,000 in manufacturing and 653,000 in non-manufacturing, for a total of 1.58 million additional workers employed. Accounting for the full set of direct and indirect upstream effects shown in our preferred specification (panel B, column 6), we obtain employment estimates that are larger again: 1.41 million workers in manufacturing, 1.22 million in non-manufacturing, and 2.62 million overall for 1991 through 2011; and 985,000 workers in manufacturing, 994,000 in non-manufacturing, and 1.98 million overall for 1999 through 2011. These combined direct and indirect effects of increased Chinese imports are substantially larger than the direct effects alone (837,000 workers for 1991–2011, and 560,000 workers for 1999–2011). Thus, accounting for upstream linkages inside and outside of manufacturing more than triples the estimated direct employment effects for manufacturing alone.\textsuperscript{40}

These estimated magnitudes do not, however, include the full general equilibrium impact of trade exposure as they fail to capture aggregate reallocation and demand effects as outlined above. We turn to local labor market analysis to obtain estimates of these additional adjustment mechanisms.

\textsuperscript{39} Consistent with the analysis of Section 4, these counterfactuals assume that 56 percent of the observed growth in direct and indirect import exposure is attributable to the Chinese supply shock.

\textsuperscript{40} The specification in column 8, which controls for 10 one-digit manufacturing sector dummies, implies somewhat smaller employment effects. According to our estimates from panel B (accounting for the full set of direct and upstream effects), had import competition from China remained unchanged between 1991 and 2011, there would have been 857,000 additional workers employed in manufacturing and 821,000 additional workers employed outside of manufacturing, for a total employment gain of 1.68 million workers. For the 1999–2011 period, the corresponding counterfactual employment additions are 597,000 in manufacturing and 670,000 in non-manufacturing, yielding total employment gains of 1.27 million. These numbers are about 35 percent smaller than our baseline estimates incorporating the indirect upstream effects.
6 Local General Equilibrium Effects of Trade on Employment

Our industry-level analysis, which compares changes in relative employment among industries with differing levels of trade exposure, is not well-suited to identifying the reallocation and demand effects discussed in the Introduction and Section 2. In this section, we attempt to quantify the reallocation and aggregate demand effects by applying an alternative strategy that focuses on the implications of rising import competition from China for employment in local labor markets.

6.1 Empirical approach

To exposit the logic of our approach, consider a simplified setting in which each commuting zone (CZ) houses up to three sectors that have no input-output linkages: toys, footwear, and construction.41 Toys and footwear experience an increase in imports from China, so we label these sectors as exposed. Construction does not experience this shock and we label it non-exposed. If a particular CZ has many workers employed in toys prior to the rise of import competition from China, it will experience significant worker displacement as this sector contracts.42 Due to the reallocation effect, we would expect displaced workers to gain employment in another sector. This sector is unlikely to be footwear, however, since it is simultaneously facing rising import competition. In this simple setting, labor within the commuting zone should therefore reallocate towards construction. Estimating by how much employment in construction expands in this CZ as toys and footwear decline can help us to assess the positive general equilibrium effects resulting from reallocation.

Employment in construction may be affected by a second channel as well: the potentially negative Keynesian aggregate demand multiplier, stemming from reductions in local economic activity. In our simple example, the initial reduction in employment in exposed industries will reduce local incomes and, via this channel, may depress local demand for new home construction or renovation, further depressing employment.43 The net effect of these reallocation and aggregate demand effects on employment in construction may be positive or negative.

Now suppose that the third industry in this economy is not construction but chemicals, 41The choice of construction as the non-traded sector is motivated in part by Charles et al. (2013), who find that the 2000–2007 housing boom helped local labor markets absorb workers displaced from manufacturing. 42This discussion also makes it clear that empirically, it is appropriate to combine the shocks of all of the local industries using weights related to their local employment shares, which is the strategy employed here and in Autor et al. (2013). 43It is possible for trade-induced price declines to simultaneously contribute to aggregate demand by spurring additional consumption or investment as discussed in Footnote 14.
which unlike construction, is tradable within the United States across local labor markets and, as it happens, has not been subject to significant increases in import competition from China. To make progress in this case, suppose that our local labor markets can be thought of as small open economies within the United States, so that prices of tradables are determined at the US level (or on world markets). This does not change the reallocation effect, but it may alter the aggregate demand effect. Even if aggregate demand for non-tradables in the local labor market is depressed, there might be an increase in local employment in chemicals, the output of which is then sold to residents in other CZs. This is simply a reflection of the fact that the component of the negative aggregate demand effect working at the national level will not be easily identified from variation across local labor markets. An implication of this observation is that our strategy will tend to underestimate the aggregate demand effect, to the degree it operates nationally rather than locally.

6.2 Estimates

The local labor market analysis is based on 722 CZs that cover the entire US mainland. These CZs are clusters of counties with strong internal commuting ties (see Tolbert and Sizer, 1996 and Autor and Dorn, 2013).

We begin by estimating stacked first-difference models for changes in CZ employment-to-population rates of the following form:

\[
\Delta E_{it} = \alpha_t + \beta \Delta IP_{it}^{CZ} + \gamma X_{i0} + e_{it}
\]

Here, the dependent variable \(\Delta E_{it}\) is equal to 100 times the annual change in the ratio of employment to working-age population in CZ \(i\) over time period \(t\); \(X_{i0}\) is a set of CZ-by-sector start-of-period controls (specified later); \(\alpha_t\) is a time effect; and \(e_{it}\) is an error term.\(^{44}\) The key explanatory variable in this model is \(\Delta IP_{it}^{CZ}\), which measures a CZ’s annual change in exposure to Chinese imports over period \(t\). The coefficient \(\beta\) reveals the impact of import exposure on overall employment rates, combining employment shifts in both trade-exposed and non-exposed industries. We define a CZ’s change in import exposure as a local employment-weighted average of changes in

\(^{44}\) Throughout this section, local employment is derived from the County Business Patterns, and local working-age population (ages 15–64) is derived from the Census Population Estimates.
import exposure:
\[
\Delta IP_{i\tau}^{CZ} = \sum_{j} \frac{L_{i\tau}}{L_{i\tau}} \Delta IP_{j\tau}.
\] (8)

In (8), \(\Delta IP_{j\tau}\) is the measure of Chinese import competition used in our industry-level analysis, and \(L_{i\tau}/L_{i\tau}\) is industry \(j\)'s start-of-period share of total employment in CZ \(i\).\(^{45}\) The variation in \(\Delta IP_{i\tau}^{CZ}\) across local labor markets stems entirely from variation in local industry employment structure at the start of period \(\tau\). As with our industry-level estimates, a concern is that realized US imports from China in (8) may be correlated with industry import demand shocks. We again instrument for growth in Chinese imports to the US using the contemporaneous growth of Chinese imports in eight other developed countries as specified in (2).\(^{46}\) Appendix Table 4 summarizes CZ-level changes in exposure to Chinese imports and in employment-to-population rates.

To gauge the differential impact of import exposure on different types of industries within local labor markets, we decompose employment changes into three broad sectoral groupings. Specifically, we interact the CZ’s change in import exposure with indicator variables for exposed industries, non-exposed tradable industries, and other non-exposed industries:

\[
\Delta E_{ik\tau} = \alpha_{k\tau} + \beta_1 \Delta IP_{i\tau}^{CZ} \times 1[\text{Exposed}_k] + \beta_2 \Delta IP_{i\tau}^{CZ} \times 1[\text{Non-Exposed Tradable}_k] \] (9)
\[+ \beta_3 \Delta IP_{i\tau}^{CZ} \times (1 - 1[\text{Exposed}_k] - 1[\text{Non-Exposed Tradable}_k]) + \gamma X_{i0k} + \epsilon_{ik\tau}.
\]

In these regressions, \(\Delta E_{ik\tau}\) is the change in employment of sector \(k\) in CZ \(i\), expressed in percentage points of working-age population. While the specification in (9) is similar to that in Autor et al. (2013), it differs importantly by separating the employment effects of import competition in CZs according to sector import exposure and tradability. To compute \(\Delta E_{ik\tau}\), we assign each industry to one of the three mutually exclusive sectors: exposed industries, non-exposed tradable industries, and other non-exposed industries. First, we define the exposed sector to encompass all manufacturing industries for which predicted import exposure rose by at least 2 percentage points between 1991 and

\(^{45}\)This is similar to Autor et al. (2013) and Autor et al. (2014), except that for consistency with our industry-level analysis, we normalize industry-level imports by initial US market volume instead of initial employment.

\(^{46}\)Our expression for non-US exposure to Chinese imports, which serves as an instrument for \(\Delta IP_{i\tau}^{CZ}\), differs from the expression in (8) in that in place of realized changes in US import exposure (\(\Delta IP_{j\tau}\)), we use the analogous expression based on realized imports from China to other high-income markets (\(\Delta IP_{Oj\tau}\)). In addition, we use 1988 employment counts for the construction of the instrument to reduce the error covariance between the dependent and independent variables.
2011, as well as all industries (both within and outside of manufacturing) for which the predicted full upstream import exposure measure increased by at least 4 percentage points over 1991-2011. Relative to an exposure definition based only on own-industry import exposure, incorporating upstream linkages expands the exposed sector to include additional manufacturing industries, as well as industries outside of manufacturing that sell a sizable portion of their outputs to import-exposed manufacturing firms. For example, the latter group includes forestry, wholesale trade, miscellaneous repair services, and chemical and fertilizer mining. All other industries are designated as non-exposed. Following our simple example of construction versus chemicals as non-exposed industries, we next subdivide the non-exposed sector into tradables and non-tradables. In our nomenclature, tradable industries are those that produce tradable goods or commodities, and specifically comprise the manufacturing, agriculture, forestry, fishing, and mining sectors. We classify all other sectors, including services, as non-tradable, though this approach is admittedly imperfect since some services are also traded.

Table 7 presents our estimates. The first set of specifications in columns 1 through 3 pool employment across all sectors to determine the impact of import exposure in local labor markets on overall employment. Column 1 considers the relationship between CZ import exposure and changes in CZ employment-to-population rates without additional controls. The strongly negative and statistically significant point estimate in this column indicates that a one percentage point increase in the average import penetration of local industries reduces the employment rate among a CZ's working-age population by 1.64 percentage points. We refine the estimates and explore robustness in the next pair of columns by controlling for the initial manufacturing employment share in a local labor market (column 2) and for nine Census divisions (column 3). By controlling for local manufacturing intensity, we allow for differential employment trends in the manufacturing and non-manufacturing sectors, as we do in our industry-level estimates of Table 6. The controls

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47 Predicted import exposure is computed from first-stage estimates of (3) over the single long period 1991-2011.
48 Despite this broad definition of the exposed sector, our regression analysis in this section will only partially capture the indirect effects working through input-output linkages we directly estimated previously. While pairs of industries linked through input-output relationships tend to co-locate (e.g., Ellison et al., 2010), many firms purchase and sell inputs beyond the boundaries of their commuting zone, and thus any local strategy will exclude a potentially sizable fraction of these indirect effects.
49 The exposed sector consists of 293 industries (285 in manufacturing and 8 outside of manufacturing), which together comprised 20.2 percent of 1991 US employment. The non-exposed tradable sector consists of 113 industries (107 in manufacturing, 6 outside of manufacturing), comprising 6.7 percent of 1991 employment. Finally, the non-exposed non-tradable sector consists of 73 industries (all outside manufacturing) accounting for 73.1 percent of 1991 employment.
for Census divisions allow for heterogeneity in regional time trends. Adding these covariates has a modest impact on the trade coefficient, which remains sizable and statistically significant at \(-1.70\) in column 3.

The regressions of columns 4 through 6 disaggregate the overall employment effects of columns 1 through 3 into their sectoral components. Consistent with the results of the industry analysis, column 4 shows a strongly negative and statistically significant effect of import exposure on local labor market employment in trade-exposed industries. The point estimate indicates that a one percentage point increase in local import exposure reduces the share of a CZ’s working-age population employed in exposed industries by 1.95 percentage points. Between 1999 and 2011, mean CZ import exposure rose by 1.21 percentage points, while employment in exposed industries declined by 3.64 percentage points of working-age population. The estimate in column 4 thus implies that 1.32 percentage points (or 36 percent) of this fall can be explained by rising Chinese import competition.\(^5\)

As our conceptual discussion anticipates, the estimate in column 4 also shows some offsetting employment growth in non-exposed industries, corresponding to the net impact of local reallocation and Keynesian demand effects. However, the offsetting employment effect is substantially smaller than the employment reduction in exposed industries and is never statistically significant. These estimates suggest that employment gains through the sectoral reallocation effect are largely offset by negative aggregate demand effects. In parallel with our specifications examining overall employment impacts, we refine the estimates in the next pair of columns by controlling for initial local labor market manufacturing intensity (column 5) and Census divisions (column 6), with the coefficients on these controls allowed to vary by sector. Adding these covariates only modestly changes the estimated negative impact of import exposure on employment in exposed industries, while the small and imprecise estimates for offsetting employment gains decline to almost zero. The final columns replicate the specifications from columns 3 and 6 over the stacked periods 1991–1999 and 1999–2007. The results are similar to those for the full sample period and suggest negative effects of trade competition on employment in exposed industries, combined with small and insignificant effects in non-exposed sectors.

\(^5\) As above, this calculation discounts the growth of imports by the partial \(R\)-squared of 0.56 of the first-stage regression: \(1.32 = 0.56 \times 1.21 \times 1.95\).
While our estimates suggest the presence of strong aggregate demand effects that limit employment gains in the non-exposed sectors of trade-exposed local labor markets, we would anticipate that these local demand effects primarily impact employment in the non-traded sector rather than the non-exposed tradable sector. Our results however provide scant evidence for differential employment impacts in the two non-exposed sectors. In columns 4 and 5, the point estimates for non-tradables exceed the point estimate for non-exposed tradables; in columns 6 and 8, the relationship is reversed.

Why does reallocation fail to accord more clearly with the simple reasoning outlined in Section 6.1? It is conceivable that the small increase in employment in non-tradable sectors detected in columns 4 and 5 (though not in column 6) may be related to the rapid rise in the US aggregate trade deficit during our sample period, a substantial part of which reflects a growing trade imbalance with China (Figure 2). In response to import competition, an open economy normally reallocates resources out of some tradable industries into others, at least under balanced trade. If, however, the trade shock is accompanied by a rise in the trade deficit, then the reallocation from exposed tradables into non-exposed tradables may be delayed, shifting employment into non-tradables instead—that is, the deficit may fuel increasing expenditure in the domestic economy, part of which falls on non-tradable consumption. While this reasoning is not inconsistent with a long-run reallocation towards non-exposed tradables, the large and growing US trade deficit during the period under study may have significantly slowed down such a reallocation. This reasoning is, unfortunately, silent on why a rising US trade deficit coincided with China’s growing import penetration. It nevertheless underscores that shifts in global imbalances may complicate the simple adjustment mechanism we posit.

Quantitatively, the estimates in column 6 of Table 7 encompass four impacts of Chinese trade competition on local labor market employment: direct employment effects in exposed industries, indirect employment effects via local input-output linkages between industries, local reallocation effects, and local aggregate demand effects. As summarized in Table 8, the coefficient estimates imply that had import competition from China not increased after 1999, trade-exposed industries in local labor markets would have avoided the loss of 2.35 million jobs. Comparing this quantity to the outcome of our national-industry analysis, it is modestly larger than the employment effect derived from Table 6, panel B reported above, which incorporated both the direct and the upstream
effects of import competition and tallied employment reductions in trade-exposed manufacturing and non-manufacturing industries at 1.98 million jobs. The fact that employment effects on exposed industries in CZs are slightly larger than the direct and indirect effects of import competition in national industries is suggestive of negative local aggregate demand spillovers. Such spillovers imply that multipliers operating at the local level suppress demand in non-exposed industries as well, inducing further employment declines in trade-exposed industries.

Our estimates imply near zero, though imprecisely estimated, employment effects of trade exposure on non-exposed industries. Absent further increases in import penetration from China after 1999, the results summarized in Table 8 show that non-exposed industries would have shed 18,000 fewer jobs. Combining figures from exposed and non-exposed industries, the overall local impact is 2.37 million jobs whose loss would have been averted absent further increases in Chinese import competition after 1999. With the numerous caveats acknowledged, our conceptual framework in Section 2 suggests that this estimate is a lower bound on the aggregate total impact of increased import competition from China on national employment. In particular, this estimate does not include the components of industry interlinkage effects and aggregate demand effects that work at the national level. This lower bound estimate is relatively close to the jobs lost based on our industry-level analysis in Table 6, panel B (shown in Table 8), which combines direct competition effects and interindustry linkages with non-manufacturing sectors. Recall that Appendix Table 1, panel B’s industry-level estimate of the jobs lost does not include reallocation and aggregate demand effects. Since our analysis in this section indicates that employment losses due to negative aggregate demand effects dominate employment gains due to reallocation effects, our industry-level estimates of employment reduction should indeed be lower bounds. 51

7 Conclusion

In the years leading up to the Great Recession, overall US employment growth was slow and manufacturing employment experienced a steep contraction. In this paper, we investigate the contribution

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51 In particular, recall that the industry-level numbers could underestimate the net employment losses due to aggregate demand effects or overestimate these losses due to reallocation effects. But if reallocation effects are modest and swamped by demand effects at the local level, as suggested by the Table 7 estimates, we would also expect the demand effects to dominate at the aggregate level—especially since these demand effects are themselves underestimated at the local level.
of the rise in import competition from China to this employment "sag".

We begin by estimating the direct effect of trade competition on employment in manufacturing industries that are differentially exposed to growing Chinese import penetration, and then expand the analysis to include multiple general equilibrium channels through which trade exposure may affect employment: other sectors might be impacted because they are related to the affected sectors through input-output linkages; employment may reallocate away from trade-exposed industries toward non-exposed industries; and Keynesian-type aggregate demand spillovers may significantly magnify the direct competition effect.

In our analysis of US national industries, we estimate upstream and downstream trade effects for both manufacturing and non-manufacturing sectors. We expect upstream effects to contribute to further job losses, while the impact of downstream effects is ambiguous. Consistent with these expectations, we find large negative employment responses when an industry's customers are exposed to trade competition and unstable effects when an industry's suppliers are exposed to trade competition.

As a complementary strategy, we assess the impact of Chinese trade on US commuting zones to jointly estimate reallocation and aggregate demand effects at the local level. Theoretically, if an industry contracts in a local labor market because of Chinese competition, then, barring substantial interregional migration, some other industry in the same labor market should expand. In addition, part of any aggregate demand spillovers will also accrue to the local labor market. Our estimates show sizable job losses in exposed industries, and few if any offsetting job gains in non-exposed industries, a pattern that is consistent with substantial job loss due to aggregate demand spillovers.

Our results are a first step in quantifying the employment impact of increasing import competition on the US labor market. Several questions remain unanswered that could be addressed in future work. Using plant-level data to achieve a finer distinction between tradable and non-tradable industries would enable both a sharper test of the implications of local general equilibrium interactions and a separate quantification of reallocation and aggregate demand effects. We should in particular see employment declines in non-tradables due to local aggregate demand spillovers, but no differential decline in tradables except through geographically concentrated input-output linkages. This perspective could elucidate how local and national labor markets respond to growing import competition, in particular allowing us to determine to which degree shocks propagate locally.
or at the national level.

We finally note that, though our paper has focused on the contribution of rising international competition to the US “employment sag” of the 2000s, we have had comparatively less to say about the impact of trade during the Great Recession. As shown in Figure 2, US imports from China dropped sharply in 2009. This might imply that exporters to the United States—China in particular—absorbed part of the demand shock accompanying the Great Recession that would otherwise have further reduced US employment (albeit from a notionally higher base). While this hypothesis is intuitive, additional exploration of US manufacturing data suggests otherwise. We find that US manufacturing industries that were heavily exposed to Chinese import competition during the 1999–2007 period continued to see rapid, differential employment declines during 2007–2011, despite the fact that there was almost no correlation between industry-level changes in trade exposure during 1999–2007 and changes in trade exposure during 2007–2011. This pattern suggests that the trade shocks of the prior decade cast a long shadow over US manufacturing, even when trade pressure eased temporarily. One explanation for this long shadow is that US manufacturers recognized that the loss in comparative advantage in the sectors that China had penetrated in the prior decade was largely permanent whereas the lull in trading activity was temporary. Indeed, as shown in Figure 2, US imports from China more than made up all of their ground lost in 2009 by the following year, and then rose further from there. Thus, trade pressure appears to have contributed to the US employment sag not just before, but also during the Great Recession, despite the temporary drop off of international trading activity during this period. Though much evidence suggests that rising labor costs in China augur a reduction in trade pressure in the years ahead (Li et al., 2012), our analysis suggests that this particular Chinese export has yet to reach US shores.

52 When we regress 100 × the annual log change in manufacturing industry employment between 2007 and 2011 on changes in Chinese import competition between 2007 and 2011 and between 1999 and 2007 (expressed as percentage points of 1991 US market volume), we find

$$\Delta L_{j,07-11} = -5.02 - 1.06 \times \Delta IP_{j,09-07} + 0.59 \times \Delta IP_{j,07-11}.$$  

This substantial impact of Chinese import competition between 1999 and 2007 on 2007–2011 employment growth suggests a pattern of delayed declines in employment in affected industries. We obtain similar results if we control for 10 one-digit sector dummies.
Figures and Tables

Figure 1: Changes in US manufacturing and non-manufacturing employment, 1991–2011

Note: Employment is computed in the County Business Patterns. Employment counts are normalized to 1 in 1991.

Figure 2: Bilateral US-China trade flows and Chinese import penetration, 1991–2011

Note: Trade data are taken from the UN Comtrade Database. Imports and exports are deflated to 2007 US$ using the Personal Consumption Expenditure price index. Chinese import penetration is constructed by dividing US manufacturing imports from China by US domestic manufacturing absorption, defined as US domestic manufacturing output plus imports less exports. Export data are available only from 1992 onwards. The import penetration ratio series ends in 2009 because computing the denominator requires use of the NBER-CES Manufacturing Industry Database, which ends in 2009.
Table 1: Industry-level changes in Chinese import exposure and US manufacturing employment

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<td>Mean/SD</td>
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<td>-0.52</td>
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<td>(0.76)</td>
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<td>100 x annual log Δ in emp. (manufacturing industries)</td>
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<td>-2.05</td>
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<td>100 x annual log Δ in emp. (non-manufacturing industries)</td>
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<td>-5.73</td>
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<td>(1.46)</td>
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Note: For each manufacturing industry, the change in US exposure to Chinese imports is computed by dividing 100 × the annualized increase in the value of US imports over the indicated period by 1991 US market volume in that industry. The instrument is constructed by dividing 100 × the annualized increase in imports from China in a set of comparison countries by 1988 US market volume in the industry. The quantities used in these computations are deflated to constant dollars using the Personal Consumption Expenditures price index. Employment changes are computed in the County Business Patterns. All observations are weighted by 1991 industry employment.

Table 2: Effect of import exposure on log employment in US manufacturing industries: OLS and 2SLS estimates

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<td></td>
<td></td>
<td>(0.94)</td>
<td></td>
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</tr>
<tr>
<td>Instrument for Δ in US exposure to Chinese imports</td>
<td>392</td>
<td>0.44</td>
<td>0.15</td>
<td>-0.52</td>
<td>8.59</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.76)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>100 x annual log Δ in emp. (manufacturing industries)</td>
<td>392</td>
<td>-2.71</td>
<td>-2.05</td>
<td>-38.32</td>
<td>4.62</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(3.07)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>100 x annual log Δ in emp. (non-manufacturing industries)</td>
<td>87</td>
<td>1.33</td>
<td>1.02</td>
<td>-5.73</td>
<td>5.75</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1.46)</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: Columns 1–4 report results from stacking log employment changes and changes in US exposure to Chinese imports over the periods 1991–1999 and either 1999–2011 or 1999–2007, as indicated (n = 784 = 392 four-digit manufacturing industries × 2 periods). Columns 5–8 report results from regressing the employment change over the indicated period on the change in US exposure to Chinese imports over the same period (n = 392). Employment changes are computed in the County Business Patterns and are expressed as 100 × annual log changes. In 2SLS specifications, the change in US import exposure is instrumented as described in the text. In all specifications, observations are weighted by 1991 employment. Standard errors in parentheses are clustered on 135 three-digit industries in all specifications. *: p < .10, **: p < .05, ***: p < .01.
Table 3: 2SLS estimates of import effects on log employment including industry-level controls

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
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<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
</tr>
</thead>
<tbody>
<tr>
<td>100 x annual Δ in US</td>
<td>-1.30***</td>
<td>-0.75***</td>
<td>-1.10***</td>
<td>-1.33***</td>
<td>-0.80***</td>
<td>-0.76***</td>
<td>-0.74***</td>
<td>-0.60***</td>
</tr>
<tr>
<td>exposure to Chinese</td>
<td>(0.41)</td>
<td>(0.22)</td>
<td>(0.35)</td>
<td>(0.43)</td>
<td>(0.25)</td>
<td>(0.22)</td>
<td>(0.23)</td>
<td>(0.29)</td>
</tr>
<tr>
<td>imports (01991–1999)</td>
<td>0.05</td>
<td>-0.09</td>
<td>0.00</td>
<td>0.06</td>
<td>-0.08</td>
<td>-0.09</td>
<td>-0.10</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.36)</td>
<td>(0.32)</td>
<td>(0.37)</td>
<td>(0.36)</td>
<td>(0.30)</td>
<td>(0.32)</td>
<td>(0.30)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.33)</td>
<td>(0.27)</td>
<td>(0.35)</td>
<td>(0.32)</td>
<td>(0.28)</td>
<td>(0.26)</td>
<td>(0.27)</td>
<td>(0.45)</td>
</tr>
<tr>
<td>1-digit mfg sector</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Production controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pretrend controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Industry fixed effects</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>X</td>
</tr>
</tbody>
</table>

Note: Each column reports results from stacking log employment changes and changes in US exposure to Chinese imports over the periods 1991–1999 and 1999–2011 (n = 784 = 392 four-digit manufacturing industries x 2 periods). The dependent variable is 100 x the annual log change in each industry’s employment in the County Business Patterns over the relevant period. The regressor is 100 x the annual change in US exposure to Chinese imports over the same period; it is instrumented as described in the text. Sector controls are dummies for 10 one-digit manufacturing sectors. Production controls for each industry include production workers as a share of total employment, the log average wage, and the ratio of capital to value added (in 1991); and computer investment as a share of total investment and high-tech equipment as a share of total investment (in 1990). Pretrend controls are changes in the log average wage and in the industry’s share of total employment over 1976–1991. In the final column, we include a full set of four-digit industry fixed effects. Covariates are demeaned to facilitate interpretation of the time effects. Observations are weighted by 1991 employment. Standard errors in parentheses are clustered on 135 three-digit industries. *: p < .10, **: p < .05, ***: p < .01.

Table 4: 2SLS estimates of import effects on log employment over 1971–2009

<table>
<thead>
<tr>
<th></th>
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<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>A. Excluding 1-digit mfg sector controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>100 x annual Δ in US exposure to Chinese imports (computed over 1991–2011)</td>
<td>0.34</td>
<td>-0.40</td>
<td>-0.84*</td>
<td>-2.01***</td>
<td>-1.49***</td>
</tr>
<tr>
<td>Constant</td>
<td>(0.33)</td>
<td>(0.28)</td>
<td>(0.45)</td>
<td>(0.66)</td>
<td>(0.51)</td>
</tr>
<tr>
<td></td>
<td>1.15***</td>
<td>-0.68**</td>
<td>0.35</td>
<td>3.09***</td>
<td>2.05**</td>
</tr>
<tr>
<td>Constant</td>
<td>(0.30)</td>
<td>(0.34)</td>
<td>(0.46)</td>
<td>(0.43)</td>
<td>(0.29)</td>
</tr>
<tr>
<td>B. Including 1-digit mfg sector controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>100 x annual Δ in US exposure to Chinese imports (computed over 1991–2011)</td>
<td>0.20</td>
<td>0.03</td>
<td>-0.57*</td>
<td>-0.91***</td>
<td>-0.76***</td>
</tr>
<tr>
<td>Constant</td>
<td>(0.26)</td>
<td>(0.26)</td>
<td>(0.31)</td>
<td>(0.31)</td>
<td>(0.23)</td>
</tr>
<tr>
<td></td>
<td>-0.05</td>
<td>-0.08</td>
<td>0.52</td>
<td>-0.98**</td>
<td>-0.32</td>
</tr>
<tr>
<td>Constant</td>
<td>(0.32)</td>
<td>(0.74)</td>
<td>(0.63)</td>
<td>(0.45)</td>
<td>(0.48)</td>
</tr>
</tbody>
</table>

Note: n = 384 four-digit manufacturing industries (we exclude eight industries for which post-1996 employment data are unavailable in the NBER-CES Manufacturing Industry Database). The dependent variable in each specification is 100 x the annual log employment change over the indicated period, as computed in the NBER-CES data. The regressor in each specification is 100 x the annual change in US exposure to Chinese imports over 1991–2011, instrumented as described in the text. Panel A includes no additional controls. Panel B includes dummies for 10 one-digit manufacturing sectors. Observations are weighted by 1991 employment. Standard errors in parentheses are clustered on 135 three-digit industries. *: p < .10, **: p < .05, ***: p < .01.
Table 5: 2SLS estimates of import effects on additional labor market outcomes

<table>
<thead>
<tr>
<th></th>
<th>Employment</th>
<th>Number of establishments</th>
<th>Employment per establishment</th>
<th>Real wage bill</th>
<th>Real wage</th>
<th>Production employment</th>
<th>Non-production employment</th>
<th>Real production wage</th>
<th>Real non-production wage</th>
</tr>
</thead>
<tbody>
<tr>
<td>CBP (1)</td>
<td>CBP (2)</td>
<td>CBP (3)</td>
<td>CBP (4)</td>
<td>CBP (5)</td>
<td>NBER (6)</td>
<td>NBER (7)</td>
<td>NBER (8)</td>
<td>NBER (9)</td>
<td></td>
</tr>
<tr>
<td>100 x annual Δ in US</td>
<td>-0.75***</td>
<td>-0.23***</td>
<td>-0.52***</td>
<td>-0.67***</td>
<td>0.08</td>
<td>-0.99***</td>
<td>-0.78***</td>
<td>0.24**</td>
<td>-0.05</td>
</tr>
<tr>
<td>exposure to Chinese imports</td>
<td>(0.22)</td>
<td>(0.09)</td>
<td>(0.17)</td>
<td>(0.21)</td>
<td>(0.06)</td>
<td>(0.31)</td>
<td>(0.29)</td>
<td>(0.11)</td>
<td>(0.09)</td>
</tr>
<tr>
<td>1(1991-1999)</td>
<td>-0.09</td>
<td>0.48**</td>
<td>-0.57**</td>
<td>1.53***</td>
<td>1.63***</td>
<td>0.33</td>
<td>-0.20</td>
<td>1.13***</td>
<td>1.81***</td>
</tr>
<tr>
<td></td>
<td>(0.32)</td>
<td>(0.19)</td>
<td>(0.26)</td>
<td>(0.30)</td>
<td>(0.08)</td>
<td>(0.38)</td>
<td>(0.34)</td>
<td>(0.06)</td>
<td>(0.09)</td>
</tr>
<tr>
<td>1(1999-2011) or 1(1999-2009)</td>
<td>-3.82***</td>
<td>-1.51***</td>
<td>-2.31***</td>
<td>-3.42***</td>
<td>0.40***</td>
<td>-4.84***</td>
<td>-3.63***</td>
<td>0.22</td>
<td>0.32***</td>
</tr>
<tr>
<td></td>
<td>(0.27)</td>
<td>(0.19)</td>
<td>(0.18)</td>
<td>(0.30)</td>
<td>(0.10)</td>
<td>(0.37)</td>
<td>(0.31)</td>
<td>(0.14)</td>
<td>(0.11)</td>
</tr>
<tr>
<td>1-digit mfg sector controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>1(1991-1999)</td>
<td>-0.30</td>
<td>0.41**</td>
<td>-0.71***</td>
<td>1.35***</td>
<td>1.65***</td>
<td>0.06</td>
<td>-0.40</td>
<td>1.19***</td>
<td>1.80***</td>
</tr>
<tr>
<td></td>
<td>(0.32)</td>
<td>(0.19)</td>
<td>(0.26)</td>
<td>(0.31)</td>
<td>(0.07)</td>
<td>(0.36)</td>
<td>(0.33)</td>
<td>(0.06)</td>
<td>(0.08)</td>
</tr>
<tr>
<td>1(1999-2011) or 1(1999-2009)</td>
<td>-4.32***</td>
<td>-1.67***</td>
<td>-2.66***</td>
<td>-3.87***</td>
<td>0.46***</td>
<td>-5.38***</td>
<td>-4.06***</td>
<td>0.35***</td>
<td>0.30**</td>
</tr>
<tr>
<td></td>
<td>(0.25)</td>
<td>(0.17)</td>
<td>(0.17)</td>
<td>(0.29)</td>
<td>(0.08)</td>
<td>(0.34)</td>
<td>(0.32)</td>
<td>(0.14)</td>
<td>(0.13)</td>
</tr>
</tbody>
</table>

Note: In the table heading, CBP and NBER indicate the dataset used to compute the indicated outcome (CBP = County Business Patterns, NBER = NBER-CES Manufacturing Industry Database). Each column stacks changes in the indicated outcome and changes in US exposure to Chinese imports over the periods 1991–1999 and either 1999–2011 (for CBP outcomes) or 1999–2009 (for NBER-CES outcomes). In columns 1–5, n = 784 = 392 four-digit manufacturing industries × 2 periods. In columns 6–9, we exclude eight industries for which post-1996 data are unavailable in the NBER-CES, yielding n = 768 = 384 industries × 2 periods. In each column, the dependent variable is 100 × the annual log change in the indicated quantity. Panel A reports 2SLS estimates including the annual change in US exposure to Chinese imports over the relevant period; it is instrumented as described in the text. Panel B reports OLS estimates from a regression including only time effects and sector controls. All specifications include dummies for 10 one-digit manufacturing sectors, which are demeaned to facilitate interpretation of the time effects. Observations are weighted by 1991 employment in the relevant dataset. Standard errors in parentheses are clustered on 135 three-digit industries. *: p < .10, **: p < .05, ***: p < .01.
Table 6: 2SLS estimates of import effects on employment incorporating input-output linkages

<table>
<thead>
<tr>
<th></th>
<th>Manufacturing inds (n = 784)</th>
<th>Non-mfg inds (n = 174)</th>
<th>Pooling mfg and non-mfg inds (n = 958)</th>
</tr>
</thead>
<tbody>
<tr>
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<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
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<tr>
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<td>(6)</td>
</tr>
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<td></td>
<td>(7)</td>
<td>(8)</td>
<td>(9)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(10)</td>
<td></td>
</tr>
<tr>
<td><strong>A. First-order input-output linkages</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Direct import exposure</td>
<td>-1.17*** -1.28*** -0.72***</td>
<td>-1.14*** -1.11** -0.69*** -1.07***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.42) (0.49) (0.22)</td>
<td>(0.42) (0.48) (0.22)</td>
<td>(0.38)</td>
</tr>
<tr>
<td>Upstream import exposure</td>
<td>-2.21* -2.44** -1.03** -6.63** -6.88** -2.70** -2.64** -1.72** -3.06***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.14) (1.13) (0.45) (2.79)</td>
<td>(2.97) (1.26) (0.75)</td>
<td>(1.09)</td>
</tr>
<tr>
<td>Downstream import exposure</td>
<td>2.31</td>
<td>-5.80</td>
<td>-0.67</td>
</tr>
<tr>
<td></td>
<td>(2.66) (7.43) (3.69)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Combined import exposure (direct + upstream)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-1.35***</td>
<td></td>
<td>(0.38)</td>
</tr>
</tbody>
</table>

| **B. Full (higher-order) input-output linkages** |                             |                        |                                        |
| Direct import exposure  | -1.20*** -1.30*** -0.72***  | -1.18*** -1.14** -0.71*** -1.12*** |                                        |
|                         | (0.42) (0.49) (0.22)        | (0.42) (0.48) (0.22)   | (0.38)                                 |
| Upstream import exposure| -1.64* -1.78** -0.85** -3.19 -3.17 -1.90** -1.86** -1.29** -2.10*** |                                      |                                        |
|                         | (0.84) (0.82) (0.37) (2.14) | (2.27) (0.86) (0.91)   | (0.75)                                 |
| Downstream import exposure | 1.74 | -4.26 | -0.68 |                                        |
|                         | (2.10) (5.94) (2.95)        |                        |                                        |
| Combined import exposure (direct + upstream) |                         |                        |                                        |
|                         | -1.32***                    |                        | (0.37)                                 |
| Sector x period effects | X X X X X X X X X X X     |                        |                                        |
| 1-digit mfg sector controls | X                          |                        |                                        |
| Exclude 2007–2011        | X                           |                        |                                        |

Note: The sample consists of 392 manufacturing industries (columns 1–3), 87 non-manufacturing industries (4–5), or both sets of industries pooled (6–10). Each column stacks changes in log employment and changes in import exposure over the periods 1991–1999 and either 1999–2011 (columns 1–8, 10) or 1999–2007 (9). The dependent variable is 100 × the annual log change in employment, as computed in the County Business Patterns. The direct import exposure of industry i equals 100 × the annual change in US exposure to Chinese imports. In panel A, upstream (respectively, downstream) import exposure for a given industry is a weighted average of the direct import exposure experienced by its customers (suppliers), as identified by the Bureau of Economic Analysis’s 1992 input-output table. In panel B, we use the Leontief inverse of the input-output matrix to incorporate higher-order linkages. Direct, upstream, and downstream measures of import exposure are instrumented using changes in comparison countries’ exposure to Chinese imports. See text for details. In column 10, combined import exposure is defined as the sum of the direct and upstream exposure measures used in the other columns; we include separate instruments for the direct and upstream components of the combined measures. Columns 1–5 include dummies for each time period. Columns 6–10 include sector (manufacturing/non-manufacturing) × period interactions. Where indicated, we include dummies for 10 one-digit manufacturing sectors (which equal zero for non-manufacturing industries). Observations are weighted by 1991 industry employment, and standard errors in parentheses are clustered on three-digit industry (with each non-manufacturing industry constituting its own cluster). *: p < .10, **: p < .05, ***: p < .01.
Table 7: 2SLS estimates of import effects on commuting zone employment-to-population ratios

<table>
<thead>
<tr>
<th></th>
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<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Commuting zone import exposure</td>
<td>-1.64***</td>
<td>-1.95***</td>
<td>-1.89***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.46)</td>
<td>(0.62)</td>
<td>(0.78)</td>
<td></td>
</tr>
<tr>
<td>Commuting zone import exposure x 1[exposed sector]</td>
<td>-1.95***</td>
<td>-2.14***</td>
<td>-1.68***</td>
<td>-1.66***</td>
</tr>
<tr>
<td></td>
<td>(0.16)</td>
<td>(0.30)</td>
<td>(0.24)</td>
<td>(0.19)</td>
</tr>
<tr>
<td>Commuting zone import exposure x 1[non-exposed tradable sector]</td>
<td>-0.01</td>
<td>0.04</td>
<td>-0.00</td>
<td>-0.05</td>
</tr>
<tr>
<td></td>
<td>(0.06)</td>
<td>(0.11)</td>
<td>(0.11)</td>
<td>(0.10)</td>
</tr>
<tr>
<td>Commuting zone import exposure x 1[non-exposed non-tradable sector]</td>
<td>0.33</td>
<td>0.15</td>
<td>-0.01</td>
<td>-0.18</td>
</tr>
<tr>
<td></td>
<td>(0.39)</td>
<td>(0.44)</td>
<td>(0.57)</td>
<td>(0.55)</td>
</tr>
<tr>
<td>Sector x time effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
</tr>
<tr>
<td>Sector x mfg emp share at baseline</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
</tr>
<tr>
<td>Sector x Census division dummies</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
</tr>
<tr>
<td>Number of observations</td>
<td>1444</td>
<td>1444</td>
<td>1444</td>
<td></td>
</tr>
</tbody>
</table>

Note: Each column reports results from stacking changes in commuting zone employment rates and exposure to Chinese imports over the periods 1991–1999 and either 1999–2011 (columns 1–6) or 1999–2007 (7–8). In columns 1, 2, 3, and 7, the dependent variable is 100 × the annual change in the ratio of total employment to working-age population (n = 1444 = 722 commuting zones × 2 periods). In the other columns, the dependent variable is 100 × the annual change in the ratio of sectoral employment to working-age population, with industries partitioned into three sectors: industries exposed to trade competition, non-exposed industries that produce tradable goods, and all remaining non-exposed industries (n = 4332 = 722 commuting zones × 3 sectors × 2 periods). See text for details. Commuting zone import exposure is an employment-weighted average of annualized changes in exposure to Chinese within local industries; it is instrumented as described in the text. Employment is computed in the County Business Patterns; population data come from the Census Population Estimates. The manufacturing share of baseline commuting zone employment is computed in 1991 (for the 1991–1999 period) or 1999 (for the 1999–2011 and 1999–2007 periods). Census division dummies control for nine Census divisions. Observations are weighted by 1991 commuting zone population. Standard errors in parentheses are clustered on commuting zone. *: p < .10, **: p < .05, ***: p < .01.
Table 8: Implied employment changes induced by changes in exposure to Chinese imports

<table>
<thead>
<tr>
<th></th>
<th></th>
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<th></th>
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<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Table 2, columns 3/4</td>
<td>Industry</td>
<td>Direct effect of import exposure</td>
<td>Manufacturing</td>
<td>-277</td>
<td>-560</td>
<td>-837</td>
<td>-853</td>
</tr>
<tr>
<td>Table 6A, columns 6/9</td>
<td>Industry</td>
<td>Direct and “first-order” upstream effects of import exposure</td>
<td>Total</td>
<td>-556</td>
<td>-1581</td>
<td>-2137</td>
<td>-2218</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Manufacturing</td>
<td>-404</td>
<td>-928</td>
<td>-1332</td>
<td>-1414</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Non-manufacturing</td>
<td>-152</td>
<td>-653</td>
<td>-805</td>
<td>-804</td>
</tr>
<tr>
<td>Table 6B, columns 6/9</td>
<td>Industry</td>
<td>Direct and &quot;full&quot; (higher-order) upstream effects of import exposure</td>
<td>Total</td>
<td>-645</td>
<td>-1979</td>
<td>-2624</td>
<td>-2669</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Manufacturing</td>
<td>-421</td>
<td>-985</td>
<td>-1406</td>
<td>-1475</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Non-manufacturing</td>
<td>-224</td>
<td>-994</td>
<td>-1218</td>
<td>-1194</td>
</tr>
<tr>
<td>Table 7, column 6/9</td>
<td>Commuting zone</td>
<td>Effect of local import exposure on employment in the commuting zone, controlling for baseline manufacturing share and for Census divisions</td>
<td>Total</td>
<td>-743</td>
<td>-2367</td>
<td>-3110</td>
<td>-3031</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Exposed industries</td>
<td>-737</td>
<td>-2348</td>
<td>-3086</td>
<td>-2663</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Non-exposed tradables</td>
<td>0</td>
<td>-1</td>
<td>-1</td>
<td>-79</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>Other non-exposed</td>
<td>-5</td>
<td>-17</td>
<td>-23</td>
<td>-289</td>
</tr>
</tbody>
</table>

Note: Reported quantities represent the change in employment attributed to instrumented changes in import exposure in each of our preferred specifications. Negative values indicate that import exposure is estimated to have reduced employment. For the industry-level analyses, we first use the estimated coefficients to predict the changes in each industry’s log employment induced by changes in import exposure over the periods 1991–1999 and 1999–2011. Concretely, we multiply the coefficient of interest by the observed change in import exposure, then multiply this product by .56 (the partial R-squared from our baseline first-stage regression). We then use each industry’s observed end-of-period employment to convert these estimates from logs into levels. Upstream effects are handled similarly. For the commuting-zone analyses, we first use observed changes in imports per worker—again discounted by .56—to predict the trade-induced change in each commuting zone’s employment-to-population ratio within the indicated sectors over the periods 1991–1999 and 1999–2011. We then multiply by end-of-period commuting zone working-age population to compute the implied changes in each sector’s employment in each commuting zone. Summing these sectoral estimates across commuting zones yields nationwide estimates. See the text for definitions of the exposed, non-exposed tradable, and non-exposed non-tradable sectors. For both industry-level and commuting-zone-level analyses, predictions for 1991–2011 equal the sum of the predictions for the two subperiods. Predicted employment changes for the period 1991–2007 are computed similarly, using coefficients from models estimated over the stacked periods 1991–1999 and 1999–2007.
Supplementary Figures and Tables

Appendix Figure 1: First-stage regression, 1991–2011

![Graph showing the first-stage regression](image)

Note: Each point represents a four-digit manufacturing industry ($n = 392$). The change in US exposure to Chinese imports is defined as the change in US imports from China divided by 1991 US market volume; the change in the comparison countries’ exposure to Chinese imports is defined as the change in these countries’ imports from China divided by 1988 US market volume. Lines are fitted by OLS regression, weighting by each industry’s 1991 employment in the County Business Patterns. The 95 percent confidence interval is based on standard errors clustered on 135 three-digit industries. The slope coefficient is .98 with standard error .14; the regression has an $R^2$ of .62.
### Appendix Table 1: Industry-level control variables

<table>
<thead>
<tr>
<th>Industry-level control variables</th>
<th>Mean</th>
<th>SD</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td>Production workers' share of employment, 1991</td>
<td>68.43</td>
<td>15.50</td>
<td>18.72</td>
<td>97.62</td>
</tr>
<tr>
<td>Ratio of capital to value added, 1991</td>
<td>0.92</td>
<td>0.55</td>
<td>0.19</td>
<td>3.52</td>
</tr>
<tr>
<td>Log real wage (2007 US$), 1991</td>
<td>10.54</td>
<td>0.29</td>
<td>9.78</td>
<td>11.09</td>
</tr>
<tr>
<td>Computer investment as share of total, 1990</td>
<td>6.56</td>
<td>6.07</td>
<td>0.00</td>
<td>43.48</td>
</tr>
<tr>
<td>High-tech equipment as share of total investment, 1990</td>
<td>8.24</td>
<td>4.84</td>
<td>1.20</td>
<td>18.25</td>
</tr>
<tr>
<td>Change in industry share of total employment, 1976–1991</td>
<td>-0.03</td>
<td>0.07</td>
<td>-0.42</td>
<td>0.07</td>
</tr>
<tr>
<td>Change in log real wage, 1976–1991</td>
<td>3.57</td>
<td>9.94</td>
<td>-32.01</td>
<td>48.06</td>
</tr>
</tbody>
</table>

Note: \( n = 392 \) four-digit manufacturing industries. Observations are weighted by industry employment in 1991, as measured in the County Business Patterns. Production workers' share, the ratio of capital to value added, log real wage, and the changes in industry employment share and in log real wage are computed using the NBER-CES Manufacturing Industry Database; total employment in 1976 and 1991 is computed from the Current Employment Statistics. The remaining control variables are taken from Autor et al. (2014). Share variables are expressed in percentage points.

### Appendix Table 2: Estimates of import effects on log gross output and log price deflators

<table>
<thead>
<tr>
<th></th>
<th>Nominal shipments (1)</th>
<th>Real shipments (2)</th>
<th>Shipments deflator (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>A. All manufacturing industries (n = 768)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>100 x annual ( \Delta ) in US exposure to Chinese imports</td>
<td>-1.08***</td>
<td>-0.17</td>
<td>-0.91**</td>
</tr>
<tr>
<td>1-digit mfg sector controls</td>
<td>(0.32)</td>
<td>(0.44)</td>
<td>(0.42)</td>
</tr>
<tr>
<td><strong>B. Exclude computer industries (n = 712)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>100 x annual ( \Delta ) in US exposure to Chinese imports</td>
<td>-1.00**</td>
<td>-0.86**</td>
<td>-0.14*</td>
</tr>
<tr>
<td>1-digit mfg sector controls</td>
<td>(0.47)</td>
<td>(0.41)</td>
<td>(0.08)</td>
</tr>
</tbody>
</table>

Note: Each column stacks changes in the indicated outcome and changes in US exposure to Chinese imports over the periods 1991–1999 and 1999–2009. In panel A, the sample consists of 384 four-digit manufacturing industries for which data are consistently available in the NBER-CES Manufacturing Industry Database (\( n = 768 = 384 \) industries \( \times 2 \) periods). In panel B, we exclude 28 computer-producing industries corresponding to NAICS 334 (\( n = 712 = 356 \) industries \( \times 2 \) periods). The dependent variable in each column is 100 \( \times \) the annual log change in the indicated outcome, as computed in the NBER-CES. The change in US exposure to Chinese imports is instrumented as described in the text. All specifications include time effects as well as controls for 10 one-digit manufacturing sectors. Observations are weighted by 1991 employment in the NBER-CES. Standard errors in parentheses are clustered on three-digit industries. *: \( p < .10 \), **: \( p < .05 \), ***: \( p < .01 \).
### Appendix Table 3: Direct, upstream, and downstream import exposure, 1991–2011

<table>
<thead>
<tr>
<th></th>
<th>Manufacturing industries (n = 392)</th>
<th>Non-manufacturing industries (n = 87)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean/SD</td>
<td>Median</td>
</tr>
<tr>
<td><strong>Direct import exposure</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Direct exposure</td>
<td>0.50</td>
<td>0.14</td>
</tr>
<tr>
<td>Instrument for direct exposure</td>
<td>0.44</td>
<td>0.15</td>
</tr>
<tr>
<td><strong>First-order indirect exposure</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Upstream exposure</td>
<td>0.16</td>
<td>0.06</td>
</tr>
<tr>
<td>Instrument for upstream exposure</td>
<td>0.12</td>
<td>0.05</td>
</tr>
<tr>
<td>Upstream exposure</td>
<td>0.10</td>
<td>0.07</td>
</tr>
<tr>
<td>Instrument for upstream exposure</td>
<td>0.09</td>
<td>0.07</td>
</tr>
<tr>
<td><strong>Full (higher-order) indirect exposure</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Upstream exposure</td>
<td>0.24</td>
<td>0.09</td>
</tr>
<tr>
<td>Instrument for upstream exposure</td>
<td>0.19</td>
<td>0.10</td>
</tr>
<tr>
<td>Upstream exposure</td>
<td>0.14</td>
<td>0.11</td>
</tr>
<tr>
<td>Instrument for upstream exposure</td>
<td>0.14</td>
<td>0.12</td>
</tr>
</tbody>
</table>

Note: The direct import shock to industry $i$ is defined as the $100 \times$ the annual change in US exposure to Chinese imports in that industry over 1991–2011. The first-order measure of upstream (respectively, downstream) import exposure experienced by $i$ is a weighted average of the direct import exposure experienced by its customers (suppliers) $j$, where the weight on industry $j$ equals $i$'s sales to ($i$'s purchases from) $j$ divided by $i$'s total sales. The full upstream and downstream exposure measures are constructed using the Leontief inverse of the input-output matrix to incorporate higher-order linkages; see text for details. Instruments for the direct, upstream, and downstream exposure measures are constructed analogously, using changes in comparison countries' exposure to Chinese imports in own and linked industries. Observations are weighted by 1991 industry employment in the County Business Patterns.
Appendix Table 4: Changes in commuting zone import exposure and employment rates

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean/SD</td>
<td>Median</td>
<td>Min</td>
<td>Max</td>
<td>Mean/SD</td>
<td>Median</td>
<td>Min</td>
</tr>
<tr>
<td>Δ in local exposure to Chinese imports</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>100 x annual Δ in commuting zone exposure to Chinese imports</td>
<td>0.05</td>
<td>0.04</td>
<td>0.00</td>
<td>0.95</td>
<td>0.10</td>
<td>0.09</td>
<td>0.00</td>
</tr>
<tr>
<td>Instrument for Δ in commuting zone exposure to Chinese imports</td>
<td>(0.05)</td>
<td>(0.07)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>0.04</td>
<td>0.04</td>
<td>-0.06</td>
<td>0.53</td>
<td>0.13</td>
<td>0.12</td>
<td>-0.01</td>
<td>0.79</td>
</tr>
<tr>
<td>Instrument for Δ in commuting zone exposure to Chinese imports</td>
<td>(0.04)</td>
<td>(0.09)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Δ in employment/working-age pop</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>100 x annual Δ in overall emp/pop</td>
<td>0.73</td>
<td>0.73</td>
<td>-1.15</td>
<td>3.48</td>
<td>-0.52</td>
<td>-0.58</td>
<td>-2.16</td>
</tr>
<tr>
<td>0.39</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>100 x annual Δ in emp/pop within exposed industries</td>
<td>-0.03</td>
<td>-0.04</td>
<td>-1.90</td>
<td>1.21</td>
<td>-0.30</td>
<td>-0.30</td>
<td>-1.55</td>
</tr>
<tr>
<td>-0.04</td>
<td>-0.04</td>
<td>-0.70</td>
<td>1.47</td>
<td>-0.07</td>
<td>-0.08</td>
<td>-0.85</td>
<td>1.52</td>
</tr>
<tr>
<td>Instrument for Δ in commuting zone exposure to Chinese imports</td>
<td>(0.16)</td>
<td>(0.17)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>100 x annual Δ in emp/pop within non-exposed tradable industries</td>
<td>-0.04</td>
<td>-0.04</td>
<td>-0.70</td>
<td>1.47</td>
<td>-0.07</td>
<td>-0.08</td>
<td>-0.85</td>
</tr>
<tr>
<td>0.10</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>100 x annual Δ in emp/pop within other non-exposed industries</td>
<td>0.80</td>
<td>0.82</td>
<td>-0.62</td>
<td>3.21</td>
<td>-0.14</td>
<td>-0.14</td>
<td>-1.82</td>
</tr>
<tr>
<td>0.32</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: n = 722 commuting zones. The annual change in commuting zone exposure to Chinese imports is a weighted average of changes in US import exposure in 392 four-digit manufacturing industries, where the weights are start-of-period employment shares within the commuting zone. The instrument is constructed by replacing US imports from China with imports from China by a set of comparison countries, and by using 1988 commuting-zone employment shares as weights; see text for details. Imports are deflated to constant dollars using the Personal Consumption Expenditures price index. In the second panel, each variable describes the annual change in 100 x total or sectoral employment divided by the commuting-zone population between the ages of 15 and 64. Exposed industries include manufacturing industries for which the predicted increase in Chinese import penetration exceeds 2 percentage points between 1991 and 2011, plus industries for which the predicted increase in the measure of full upstream import exposure (incorporating higher-order linkages) exceeds 4 percentage points over 1991-2011. Among non-exposed industries, we define agriculture, forestry, fishing, mining, and manufacturing industries as tradable and all other industries as non-tradable. Employment is computed in the County Business Patterns, and population is computed using the Census Population Estimates. Observations are weighted by total 1991 commuting-zone population.
Appendix A Derivation of the Downstream and Upstream Effects

In this Appendix, we briefly outline the justification for the specifications we use for the upstream and downstream effects in Section 5 of the paper.

Setup

Consider a static perfectly competitive economy with $n$ industries, and suppose that each industry $j = 1, \ldots, n$ has a Cobb-Douglas production function of the form

$$y_j = \sum_{i=1}^{n} x_{ji}^{a_{ji}}.$$ (A1)

Here $x_{ji}$ is the quantity of goods produced by industry $i$ used as inputs by industry $j$. We assume that, for each $j$, $a_j^i > 0$, and $a_{ji} \geq 0$ for all $i$, and that

$$a_j^i + \sum_{i=1}^{n} a_{ji} = 1,$$

so that the production function of each industry exhibits constant returns to scale. (Physical capital can also be introduced without affecting the results, but we omit it to simplify the notation and the discussion.)

The output of each industry is used as input for other industries or consumed in the final good sector. In addition, there are also imports from abroad (say China), and we ignore exports for simplicity (and thus also ignored is the trade balance condition). The market clearing condition for industry $j$ can then be written as

$$y_j = c_j + \sum_{k=1}^{n} x_{kj} - m_j,$$ (A2)

where $c_j$ is final consumption of the output of industry $j$, and $m_j$ denotes total (real) imports.

The preference side of this economy is summarized by a representative household with a utility function

$$u(c_1, c_2, \ldots, c_n).$$

We focus on the competitive equilibrium of this economy.

Main result

First consider the unit cost function of sector $j$:

$$C(p, w) = B_j w^{\alpha_j^i} \prod_{i=1}^{n} p_i^{a_{ji}},$$ (A3)

where $p$ is the vector of prices, $w$ is the wage rate, and

$$B_j = \left( \frac{1}{\alpha_j^i} \right)^{\alpha_j^i} \prod_{i=1}^{n} \left( \frac{1}{a_{ji}} \right)^{a_{ji}}.$$
is a sector-specific constant.

Cost minimization of industry $j$ (given competitive markets) implies that

$$a_{ji} = \frac{p_i x_{ji}}{p_j y_j}, \quad (A4)$$

where $p_j$ is the price of the output of industry $j$. This expression makes it clear that $a_{ij}$'s also correspond to the entries of the input-output matrix, which we denote by $A$.

Next, given the constant returns to scale production function of each sector specified in (A1), prices satisfy the zero profit conditions of the $n$ sectors in the competitive equilibrium. In particular, the price of good $j$ must be equal to the unit cost function of that sector, (A3), and thus

$$p_j = B_j w^w \prod_{i=1}^n p_i^{a_{ji}},$$

Taking logs, we have

$$\ln p_j = \ln B_j + \alpha_j \ln w + \sum_{i=1}^n a_{ji} \ln p_i \quad \text{for all } j \in \{1, \ldots, n\}.$$

Let us choose $w = 1$ as the numeraire. Then, these equations define an $n$ equation system in $n$ prices

$$\ln p = (I - A)^{-1} b,$$

where, as noted above, $A$ is the input-output matrix of the economy, and $b$ is the vector with entries given by $\ln B_i$ (and we are using the fact that $\ln w = 0$). This implies that prices in this economy are determined independently of imports (purely from the supply side). Consequently, there will only be quantity responses to imports.

But from consumer maximization, with unchanged prices relative consumption levels remain unchanged. How total consumption is impacted depends on whether there is trade balance or not. With trade balance, the economy would have to export some goods to make up for the increase in imports. Here for simplicity, we allow for a trade deficit and thus leave the entire consumption vector unchanged. With unchanged consumption levels, we must have from (A2) combined with (A4) that

$$a_{ji} d(p_j y_j) = d(p_i x_{ji}). \quad (A5)$$

For future reference, let us define nominal values (which are more useful for several of the expressions below) with tildes. For example,

$$\tilde{x}_{ji} \equiv p_i x_{ji}, \quad \tilde{y}_{ji} \equiv p_j y_{ji}, \quad \text{and } \tilde{m}_{ji} \equiv p_j m_j.$$

Then (A5) can be equivalently written as

$$a_{ji} d\tilde{y}_{ji} = d\tilde{x}_{ji}. \quad (A6)$$

Now totally differentiating the resource constraint, (A2), for sector $i$, we obtain

$$dy_i = dc_i + \sum_{j=1}^n dx_{ji} - dm_i$$
which, using \((A6)\) and the fact that consumption levels are not changing, can be written as
\[
\frac{d(p_iy_i)}{p_iy_i} = \sum_{j=1}^{n} \alpha_{ji} \frac{d(p_jy_j)}{p_jy_j} - \frac{d(p_im_i)}{p_im_i}.
\]

Writing this in matrix form, and noting that, because prices are constant, \(\frac{d(p_iy_i)}{p_iy_i} = d\ln y_i\), we have
\[
d\ln y = \hat{A}'d\ln y - \Lambda d\hat{m}
= -\left(\mathbf{I} - \hat{\mathbf{A}}'\right)^{-1}\Lambda d\hat{m}
= -\hat{\mathbf{H}}'\Lambda d\hat{m}
\tag{A7}
\]

where \(\hat{m}\) is the vector with entries given by \(p_im_i\), \(\hat{\mathbf{H}} = \left(\mathbf{I} - \hat{\mathbf{A}}'\right)^{-1}\),

\[
\hat{\mathbf{A}} = \begin{pmatrix}
\hat{a}_{11} & \hat{a}_{12} & \cdots \\
\hat{a}_{21} & \hat{a}_{22} & \\
& & \ddots \\
& & & \hat{a}_{nn}
\end{pmatrix}
\]

with entries \(\hat{a}_{ij} = \frac{p_iy_j}{p_jy_j}\) (as opposed to \(\alpha_{ij}\) which is equal to \(\frac{p_iy_j}{p_jy_j}\)) and \(\Lambda\) is the matrix with \(\frac{1}{y_j}\) on the diagonals and zero on the non-diagonals.\(^{53}\) Intuitively, any import shock creates a direct negative effect on the directly impacted sector, which is captured by the matrix \(\Lambda\), and the indirect effects are summarized by the Leontief inverse matrix \(\hat{H}'\).

We can see from this expression that there will only be upstream effects (simply note that it is the transpose of the matrix \(\hat{A}', \hat{\mathbf{A}}',\) which matters in the Leontief inverse, thus corresponding to transmission only in the upstream direction). This is a consequence of the fact that there are no changes in prices and hence quantities will respond to changes in imports, but for each change in the quantity of a sector directly affected by imports from China, the quantities of inputs that it receives from its suppliers will have to adjust, causing upstream propagation.\(^{54}\) In fact, the matrix \(\hat{H}'\) is exactly what we use in Section 5 for computing the full (Leontief inverse) upstream effects.

Equation \((A7)\) gives the output responses to import shocks. It is straightforward to derive from this the employment responses, which are our main focus. In particular, given the Cobb-Douglas form of the production function in \((A1)\), cost minimization for industry \(i\) implies that \(wli = \alpha_i^tp_iy_i\). Since the wage is constant, employment in industry \(i\) is proportional to its nominal output, enabling us to work with an analog of \((A7)\) with employment on the left-hand side.

We next develop a more heuristic derivation of this result, which provides further intuition, shows how the full effects summarized by the Leontief inverse matrix \(\hat{H}'\) come about, and also explains why under more general conditions there might also be some downstream effects.

**Heuristic derivation**

Let us first ignore the second- and higher-order input-output linkages, and focus on first-order impacts. Let us use the notation for nominal variables introduced above and begin by approximating

\(^{53}\) Note that since the largest eigenvalue of \(\hat{A}'\) is less than one, \(\mathbf{I} - \hat{\mathbf{A}}'\) is invertible.

\(^{54}\) There would be further effects if we were to impose trade balance, because some sectors would have to expand in order to compensate for the increase in imports. In that case the matrix \(\Lambda\) would have non-zero off-diagonals.
the impact of the increase in imports in industry $j$ on domestic production in the same industry as $d\bar{y}_j \approx -d\bar{m}_j$. (This is clearly an approximation, since as our derivation in the previous section showed, there will be higher-order effects on the output of sector $j$ as captured by the Leontief inverse matrix $\mathbf{H}'$.)

Note further that from (A4), any reduction in the value of output of an industry translates into a proportionate reduction in all of the inputs, in particular,

$$\frac{d\bar{x}_{ji}}{d\bar{y}_j} = a_{ji}$$

(A8)

for each industry $i$. Then from (A8) we have

$$\frac{d\bar{y}_i}{d\bar{m}_j} \approx -\frac{d\bar{y}_i}{d\bar{y}_j} = -a_{ji}$$

for each industry $i \neq j$, and we have

$$\frac{d\bar{y}_j}{d\bar{m}_j} \approx -(1 + a_{jj})$$

for industry $j$ itself, reflecting both direct import substitution and the resultant decline in $j$'s demand for its own inputs. These two cases can be dealt with succinctly by defining $d_{ij} \equiv 1\{i = j\}$, so that for any industries $i$ and $j$

$$\frac{d\bar{y}_i}{d\bar{m}_j} \approx -(d_{ij} + a_{ji}).$$

For small changes in $m_j$, a first-order Taylor approximation gives the total impact on domestic production in industry $i$ as

$$d\bar{y}_i \approx \frac{d\bar{y}_i}{d\bar{m}_j} \times d\bar{m}_j \approx -(d_{ij} + a_{ji}) \times d\bar{m}_j.$$  

Now turning this into a proportional (log) effect by normalizing the impact on industry $i$ relative to its domestic production, we obtain

$$\frac{d\bar{y}_i}{\bar{y}_i} \approx \frac{d\bar{y}_i}{d\bar{m}_j} \times d\bar{m}_j \times \frac{1}{\bar{y}_i} \approx -(d_{ij} + a_{ji}) \times d\bar{m}_j \times \frac{1}{\bar{y}_i}.$$  

This expression shows how industry $i$ is affected when a single industry $j$ to which it sells inputs is exposed to import competition. We can next compute the total effect on industry $i$ from the full vector of import changes by summing this expression across all of $i$'s customer industries:

$$(d \ln \bar{y}_i)_{\text{first order}} \approx \left( \sum_{j=1}^{n} \frac{d\bar{y}_j}{d\bar{m}_j} \times d\bar{m}_j \times \frac{1}{\bar{y}_i} \right)_{\text{first order}} \approx -\sum_{j=1}^{n} (d_{ij} + a_{ji}) \times d\bar{m}_j \times \frac{1}{\bar{y}_i}.$$  

(A9)

where $\hat{a}_{ij}$'s correspond to the entries of the matrix $\hat{\mathbf{A}}$ used in (A7). Now using the same matrix

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notation as in that equation, this relationship can be written as

$$\frac{d \ln y_{\text{first order}}}{d \ln y_{\text{full}}} \approx -(I + \hat{A})\Lambda \hat{m},$$

which clarifies that first-order effects take exactly the same form as the full effects we just derived, but with only the direct effect working through the transpose of the matrix $\hat{A}$ included (hence the first-order designation rather than the full effects). This expression is what we use to compute first-order downstream effects in Section 5.55

Our more rigorous derivation in the previous subsection makes it clear that the first-order effect cannot be isolated from higher-order effects, since an increase in $m_j$ will have an impact on $y_k$ and from there on the sectors supplying inputs to $k$ and so on. Letting $\hat{A}_i$ denote the $i$th column of $A$, $\hat{A}^2_i$ denote the $i$th column of $\hat{A}^2$, and so on, we can obtain

$$(d \ln y_i)_{\text{full}} = \left(\sum_{j=1}^{n} \frac{d \hat{m}_i}{d \hat{m}_j} \times \frac{1}{y_i}\right)_{\text{full}} = -(\epsilon'_i \cdot \hat{m} \times \frac{1}{y_i} + (A_i)' \cdot \hat{m} \times \frac{1}{y_i} + (A^2_i)' \cdot \hat{m} \times \frac{1}{y_i} + \ldots)
= -((I - A')^{-1}) \cdot \hat{m} \times \frac{1}{y_i}$$

Using the same notation as above, this can be rewritten as

$$d \ln y = -(I - \hat{A}')^{-1} \Lambda \hat{m}$$
$$= -\hat{H}' \Lambda \hat{m},$$

confirming (A7).

### Downstream effects

Downstream effects simply correspond to effects that spread downstream following the input-output matrix $A$, and in our empirical work we construct first-order and full downstream effects as $-(I + A)\Lambda \hat{m}$ and $-(I - A)^{-1} \Lambda \hat{m}$, respectively.

The above derivation confirms that, in our baseline model, there are no downstream effects from changes in imports. This result, however, depends on certain assumptions. First, the focus on competitive equilibrium in which there are no relationship-specific investments between input suppliers and customers is important. Second, the feature that there are no price effects, which will no longer be true with departures from perfectly competitive markets, also plays a major role.

In particular, without the competitive equilibrium assumption, the increase in imports may drive some producers out of the market, and this may have a negative impact on firms that are their customers, creating negative downstream effects. Conversely, if there are declines in the prices of goods being imported more intensively from China, this may create positive downstream effects as customers using these goods as inputs can expand their operations.

Ultimately, whether there are downstream effects or not is an empirical question, and our results do not provide much evidence for sizable downstream effects.

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55 Using (A4), we can rewrite $\sum_{j=1}^{n} \hat{a}_{ij} \times d \hat{m}_j \times \frac{1}{y_j}$ as $\sum_{j=1}^{n} \hat{a}_{ij} \frac{d \hat{m}_j}{y_j}$, which clarifies that the upstream effect on industry $i$ is a sales-weighted average of the proportional import shocks experienced by its customers $j$. In our empirical work, import changes correspond to changes in Chinese import penetration, and the weights are constructed using the 1992 BEA benchmark input-output table. Our empirical measure also denominates import changes by US market volume in each industry (shipments plus imports minus exports), rather than by industry shipments.
References for Chapter II


Chapter III

Return of the Solow Paradox? IT, Productivity, and Employment in US Manufacturing


1 Introduction

An increasingly popular “technological-discontinuity” paradigm, powerfully articulated in Brynjolfsson and McAfee (2011), argues that US workplaces have been, and will continue to be, automated and transformed by information technology (IT) capital. Two implications of this transformation are emphasized. First, all sectors—but particularly IT-intensive sectors—are experiencing major increases in productivity. Thus, Solow’s paradox is long since resolved: computers are now everywhere in our productivity statistics.1 Second, IT-powered machines will increasingly replace workers, ultimately leading to a substantially smaller role for labor in the workplace of the future.

Adding urgency to this argument, labor’s share of national income has fallen in numerous developed and developing countries over roughly the last three decades, a phenomenon that Karabarbounis and Neiman (2014) attribute to IT-enabled declines in the relative prices of investment goods. And many scholars have pointed to the seeming “decoupling” between robust US productivity growth and sclerotic or negligible growth rates of median US worker compensation (Fleck et al., 2011) as evidence that the “race against the machine” has already been run—and that workers have lost.

This paper provides a simple evaluation of this viewpoint using detailed data from the US

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1 Robert Solow’s comment on computers appears in his 1987 New York Times Book Review article: “…what everyone feels to have been a technological revolution, a drastic change in our productive lives, has been accompanied everywhere, including Japan, by a slowing-down of productivity growth, not by a step up. You can see the computer age everywhere but in the productivity statistics.”
manufacturing sector. We find, unexpectedly, that earlier "resolutions" of the Solow paradox may have neglected certain paradoxical features of IT-associated productivity increases, at least in US manufacturing. First, focusing on IT-using (rather than IT-producing) industries, the evidence for faster productivity growth in more IT-intensive industries is somewhat mixed and depends on the measure of IT intensity used. There is also little evidence of faster productivity growth in IT-intensive industries after the late 1990s. Second and more importantly, to the extent that there is more rapid growth of labor productivity (log(Y/L)) in IT-intensive industries, this is associated with declining output (log Y) and even more rapidly declining employment (log L). If IT is indeed increasing productivity and reducing costs, at the very least it should also increase output in IT-intensive industries. As this does not appear to be the case, the current resolution of the Solow paradox does not appear to be what adherents of the technological-discontinuity view had in mind.

2 Information Technology and Labor Productivity

We employ the NBER-CES Manufacturing Industry Database, sourced from the Annual Survey of Manufacturers (Becker et al., 2013), to estimate and plot a set of simple, descriptive regressions that chart the relationship between IT investment and industry-level outcomes for the time period 1980–2009. Our regression model takes the form

\[ \log Y_{jt} = \gamma_j + \delta_t + \sum_{t=81}^{09} \beta_t \times IT_j + e_{jt}, \]

where Y is an outcome variable (expressed in log points), \( \gamma \) is a vector of industry fixed effects, \( \delta \) is a vector of time dummies, \( IT \) is a static measure of industry IT-intensity, and \( e \) is an error term. This specification normalizes the coefficient on the IT variable to zero in the base year, and hence the series \( \{\beta_{81}, \beta_{82}, ..., \beta_{09}\} \) may be read as the level of the coefficient on IT in each subsequent year relative to 1980. Following Berman et al. (1994) and Autor et al. (1998), we measure IT intensity as the ratio of industry computer (IT) expenditures to total capital expenditures.\(^2\)

Figure 1a, which plots the over-time relationship between IT-intensity and the log of real

\(^2\)Specifically, we compute this ratio in 1977, 1982, 1987, 1992, 2002, and 2007 (no 1997 measure is available), take the average across these six data points (placing slightly greater weight on the last two periods to compensate for the absence of the 1997 measure), and standardize the result so that the final measure has zero mean and unit standard deviation across employment-weighted industries.
shipments per worker (our preferred productivity measure),\(^3\) shows a dramatic differential rise in output per worker in IT-intensive industries throughout the entire 1980–2009 period. But crucially, this pattern is almost entirely driven by the computer-producing sector (NAICS 334).\(^4\) Across the entire manufacturing sector, industries that had a one standard deviation higher rate of IT investment over the sample period saw differential productivity gains averaging a remarkable 10 log points per decade between 1980 and 2009. Excluding computer-producing industries, however, results in a murkier picture. There is some differential productivity growth in IT-intensive industries in the late 1990s, but this effect is very small (on the order of a few percentage points at its peak) and subsides after 2001. By 2009, there is no net relative productivity gain in IT-intensive industries over the full sample period.

This productivity growth pattern is unexpected in light of the earlier resolution of the Solow Paradox (e.g., Oliner and Sichel, 2000). One possible explanation is that our focus on manufacturing is misplaced—perhaps the productivity gains from IT investments are taking place elsewhere. While our data do not allow us to exclude this possibility, earlier evidence from Stiroh (2002) suggests that the IT-driven productivity growth in the 1990s was not specific to non-manufacturing and may in fact have been more pronounced in manufacturing. Moreover, productivity growth in US manufacturing has generally exceeded that outside of manufacturing for many decades, and this productivity growth differential rose sharply during the 1990s (Fleck et al., 2011).\(^5\)

A second category of explanation for these unexpected results is that our measure of IT investment, constructed by averaging computer investment data from 1977–2007, misses the mark. Plausibly, an IT investment measure that focused on the most recent years of IT investments—rather than averaging over three decades—might prove more predictive of recent industry-level productivity growth since such a measure would better reflect the current locus of the IT frontier. We explore

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\(^3\) We choose this productivity construct because it is unaffected by the choice of deflators for intermediate inputs: if the productivity of a dollar of IT investment rises over time due to IT quality improvements, this should raise shipments in IT-using industries. By contrast, the effect of rising IT quality on value-added and TFP in IT-using industries is ambiguous. Nevertheless, results using value added measures are very similar and are not shown to conserve space (see Figure A6 of the online appendix).

\(^4\) Our focus on NAICS 334 follows Houseman et al. (2013), who underscore that the relatively robust growth of productivity and value-added in US manufacturing over the last two decades is substantially driven by IT-producing industries. What Figure 1a contributes to this discussion is the finding that outside of the IT-producing industries, there is little relationship between IT investments and productivity growth.

\(^5\) Table 2 of Stiroh (2002) shows somewhat slower differential productivity growth of IT-intensive industries relative to 1987–1995 when durable goods manufacturing is excluded from the sample (compare columns 4 and 5 in the upper panel), though the pattern is reversed when the comparison is to 1977–1995 in the second panel.
this possibility in Figure A1 of the online appendix by plotting the over-time relationship between labor productivity and IT investment in non-computer-producing industries using three different vintages of IT investment measures, corresponding to averages of 1977 and 1982, 1987 and 1992, and 2002 and 2007 investments, as well as our preferred measure from Figure 1a, which is simply the weighted average of all six years of investment data. This analysis does not lend support to the hypothesis that our primary IT measure is “out of date.” Indeed, the strongest predictor of industry relative productivity growth during the 1990s is the 1977/1982 investment measure, whereas the most recent measure (from 2002/2007) is the weakest predictor. Moreover, none of these measures predicts relative productivity growth in IT-intensive industries during the 2000s.

A further concern with our simple IT investment measure is that it may fail to capture recent innovations in IT that are embodied in newer manufacturing technologies, such as computer numerically controlled machinery, pick and place robots, automated guided vehicle systems, material working lasers, etc. To explore this possibility, we exploit data from the Census Bureau’s Survey of Manufacturing Technology (SMT) conducted in 1988 and 1993, and previously used by Doms et al. (1997), which surveyed plants about their use of 17 advanced manufacturing technologies. Specifically, we reestimate (1) while replacing the computer investment measure with an SMT-based measure of the employment-weighted mean fraction of the 17 technologies in use across plants in the 120 four-digit industries for which they are available (averaging over 1988 and 1993). We exclude computer-producing industries from this analysis (and all subsequent analyses), since our focus is on induced productivity gains in IT-using industries. Figure 1b, which plots these estimates, documents that labor productivity rose relatively rapidly in SMT-intensive manufacturing industries during the 1980s and 1990s. As with the computer investment measure, however, the relationship between SMT technology adoption and industry-level labor productivity plateaus in the late 1990s, and shows little further relative gain in labor productivity after 1999.

The SMT survey was only administered to plants in five major high-tech sectors (SICs 34–38)—presumably those sectors where the 17 manufacturing technologies studied were most applicable. To check whether our main results for computer investment carry over to this restricted set of sectors, we reestimate our prior (Figure 1a) model using only these five high-tech sectors

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6 These SICs are fabricated metal products, non-electrical machinery, electrical and electronic equipment, transportation equipment, and instruments and related products industries.
(excluding computer-producing industries), applying two vintages of the computer investment measure: our main measure using data from 1977–2007; and a measure that uses only investment data from 1987 and 1992, chosen to parallel the SMT's survey years of 1988 and 1993. These estimates, also plotted in Figure 1b, indicate that the computer investment measure is a weaker predictor of productivity growth in these five high-tech sectors than is the SMT-based measure. However, neither the SMT nor the computer investment measure predicts a differential rise in productivity in IT-intensive industries after the late 1990s.

In sum, our evidence so far suggests very limited IT-driven productivity growth in computer-intensive manufacturing industries, with the contrasting result of more rapid productivity growth in industries using advanced manufacturing technologies more intensively. Different measures of IT intensity thus appear to give different results. Our result based on advanced technologies may suggest that adoption of high-tech, IT-related capital has contributed to rapid productivity growth in manufacturing, but our subsequent results cast doubt on this interpretation.

3 What Drives Rising $Y/L$—the Numerator or the Denominator?

Since our measure of labor productivity equals the log ratio of gross output to payroll employment, the positive relationship we detect in Figure 1 between industry IT and output per worker during the 1990s implies that industry output is rising proportionately faster than employment in IT-intensive industries. But it does not reveal whether either output is rising faster or employment is falling faster relative to non-IT-intensive industries.

We thus explore these two outcomes (output and employment) separately in the remaining figures. Under the assumption that IT-intensive industries are seeing improvements in technology and automation and reductions in production costs, we would expect them to experience a relative expansion in output. The implications for employment are of course ambiguous—and this could make the labor productivity measure somewhat more difficult to interpret—because these industries may be shedding labor as they automate, but may also increase employment as they expand.

Figure 2a examines the numerator of this ratio, the logarithm of shipments, measured either as real or nominal shipments, using our 1977–2007 measure of computer investments. The relationship between IT-intensity and industry shipments is almost precisely the opposite of expec-
tations: both real and nominal shipments rise at best modestly in IT-intensive industries (relative to non-IT-intensive industries) during the 1980s and then commence a relative decline in the 1990s that accelerates in the 2000s. Thus, relative output growth in IT-intensive industries begins to fall exactly when the IT-productivity payoff is thought to have materialized. While it could be that demand for the output of IT-intensive industries is price inelastic, this would not explain why real shipments decline. If, on the other hand, IT-intensive industries have upgraded their quality relative to other industries and this is not fully captured by the industry price deflators, this mismeasurement could explain the decline in real shipments but not the decline in nominal shipments. The two sets of results together defy a simple explanation.

We repeat this exercise in Figure 2b using the embodied IT capital measures from the SMT database. Though we detected above a more robustly positive relationship between use of advanced manufacturing technologies and growth in output per worker, Figure 2b makes clear that this pattern is not driven by rising relative output in SMT-intensive industries. Instead, real (and nominal) shipments in industries that heavily adopted these technologies also exhibit a sharp relative decline between 1992 and 1996, with no rebound thereafter.

The combination of rising log output per worker and falling log output in IT-intensive industries implies that log employment must have fallen even more rapidly than output in these industries, a reality confirmed by Figure 3. Whether measured by total employment or by the real wage bill, labor input in technology intensive industries declined sharply from the early 1990s through the early 2000s (in relative terms), and then roughly held steady during the 2000s. Thus, the flattening relationship between IT investments—measured either as computer investments (Figure 3a) or usage of advanced manufacturing technologies (Figure 3b)—and labor productivity in the 2000s is proximately explained by the cessation of relative employment declines in these industries. Though one can read this evidence as corroborating the “worker-less workplace” narrative of recent technological change, the timing appears wrong: relative employment declines in technology-intensive industries halted or modestly reversed from 2000 forward, which is inconsistent with the premise that IT has contributed to the slackening of labor demand in the new millennium.
4 Conclusion

This paper documents a pattern of growth among IT-using manufacturing industries that stands in contrast to the powerful and intuitively appealing view that IT is making workers redundant through outsized productivity gains. While we find some evidence of differential productivity growth in IT-intensive manufacturing industries, this depends on the measure of IT intensity and is never visible after the late 1990s. More importantly, when present, it is driven by declining relative output accompanied by even more rapid declines in employment. It is difficult to square these output declines with the notion that computerization and IT embodied in new equipment are driving a productivity revolution, at least in US manufacturing. It may well be that IT-induced technological changes are transforming non-manufacturing, or that they are so widespread as to be taking place rapidly even in non-IT-intensive industries. But at the very least, our evidence suggests that the previously-proposed resolutions of the Solow Paradox need to be critically examined, and that proponents of the technological-discontinuity view need to provide more direct evidence of the IT-induced transformation in the US economy. Prior declarations of the death of the Solow Paradox may have been premature.
Figures

Figure 1: IT intensity and log real shipments per worker, 1980–2009

(a) Entire manufacturing sector (with/without computer producers)

(b) Five high-tech manufacturing subsectors (SIC 34–38)

Note: Panel (a): $n = 387$ manufacturing industries ($n = 359$ when excluding computer-producing industries). IT intensity is defined as the ratio of computer investments to total investments, averaged over 1977–2007. Panel (b): $n = 120$ non-computer-producing industries comprising SIC 34–38. The two computer investment series in (b) define IT intensity as in (a), using data from the indicated years. The technology usage series defines IT intensity as the employment-weighted share of 17 advanced technologies used by plants within the industry. IT measures are normalized to have zero mean and unit standard deviation. Regressions are employment weighted and include industry and year fixed effects. Confidence intervals are based on standard errors clustered by industry.
Figure 2: IT intensity and log real and nominal shipments, 1980–2009

(a) Entire manufacturing sector (excluding computer producers)

(b) Five high-tech manufacturing subsectors (SIC 34–38)

Note: Panel (a): $n = 359$ non-computer-producing manufacturing industries, and IT intensity is based on 1977–2007 computer investments. Panel (b): $n = 120$ non-computer-producing industries in SIC 34–38, and IT intensity is based on 1988/1993 technology usage. Real shipments are computed using industry-specific price deflators.
Figure 3: IT intensity and log employment and real wage bill, 1980–2009

(a) Entire manufacturing sector (excluding computer producers)

(b) Five high-tech manufacturing subsectors (SIC 34–38)

Note: Panel (a): $n = 359$ non-computer-producing manufacturing industries, and IT intensity is based on 1977–2007 computer investments. Panel (b): $n = 120$ non-computer-producing industries in SIC 34–38, and IT intensity is based on 1988/1993 technology usage.
References for Chapter III


Chapter IV

Can Local Labor Markets Absorb Crowded Cohorts? Evidence from German High School Reforms

1 Introduction

Every year, local labor markets must absorb large numbers of new entrants, as students complete their schooling, men and women return from parental leave, and migrants arrive from other cities or countries. The size of these entry cohorts differs both across space and over time. If workers are imperfectly mobile across locations—due to family ties, credit constraints, limited information, or other factors—then shocks to cohort size may alter local labor market equilibria. For a given worker, an increase in cohort size (say due to a baby boom) heightens competition for jobs, university slots, and other scarce resources (Welch, 1979; Bound and Turner, 2007). Cohort-crowding might therefore raise unemployment, deter voluntary separations, and depress wages in new and existing jobs. Supply gluts may also incentivize workers to migrate elsewhere or to delay labor market entry or reentry. By the same token, however, crowded cohorts make it easier for firms to fill vacancies. Firms might respond by creating additional jobs (Hagedorn et al., 2016), or they may simply cherry-pick the best candidates from an oversized applicant pool. If cohort shocks are skill-biased, they may induce additional changes in technology adoption or capital intensity (Lewis, 2012). Analyzing how workers and firms adjust to oversized cohorts can therefore shed light on a host of questions about job search and job creation.

This paper uses sharp fluctuations in local labor supply, driven by policy-induced changes in the number of graduating secondary school students, to identify the effects of labor supply shocks on equilibrium labor market outcomes. My research design exploits state-level high school reforms that compressed the curriculum at upper-track high schools (“gymnasiums”), so that students graduate after grade 12 instead of grade 13. As a byproduct of these reforms, each reforming state experienced a single year in which two cohorts of students graduate simultaneously, as the last cohort subject to the old rules graduates alongside the first cohort subject to the new rules. Although a

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majority of new gymnasium graduates proceed immediately to university, about 20 percent instead pursue apprenticeship positions, most of which are housed at and financed by firms (Schneider and Franke, 2014). To the extent that double cohorts disproportionately impact the states in which they originate—rather than being diffused equiproportionally nationwide—comparing outcomes in double-cohort state-years against single-cohort state-years will reveal the equilibrium impact of these cohort shocks on local apprentice markets.

Empirically, I first show that the gymnasium reforms did lead to substantial increases in the number of school-leavers in double-cohort years, as expected given the institutional rules. Second, I find that the number of newly signed apprenticeship contracts increases by about 2 percent in a double-cohort year. Though modest, this increase is precisely estimated, and I can easily reject the null hypothesis that short-run firm demand for new apprentices is perfectly inelastic: local employers absorb at least some of the increase in local labor supply by hiring more trainees. This result also confirms that local labor supply shocks do have disproportionate local effects, a necessary condition for a regional design to detect such effects. Next, decomposing the increase in apprenticeships by high school degree type, I find that the increase is driven by recipients of the upper-track degree, providing further reassurance that I am picking up the causal impact of the double gymnasium cohorts. I find no clear evidence that students not subject to the curricular reform—as proxied using both high school degree type and age—are crowded out of the apprenticeship market, but the effects are imprecise.

Studying the equilibrium impact of labor supply shifts is difficult for three reasons. First, many such shifts—such as rising female labor force participation or rising collegiate attainment—occur gradually over long periods of time. Relying on slow-moving variation increases the likelihood that other determinants of labor market equilibrium—such as technology, tastes, or institutions—will be changing during the period under study. Second, even sharp cohort shocks may occur in response to labor market conditions, raising doubts about whether they are plausibly exogenous to local conditions. Third, even when sharp, plausibly exogenous cohort shocks can be identified, they often involve increased labor supply among populations (such as immigrants or welfare recipients) that differ considerably from the labor force as a whole. The high school reforms used in this study circumvent all three challenges. I show empirically that these reforms led to sharp, one-time increases in the number of school-leavers in accordance with the timing of curricular changes. The reforms
were enacted several years before the double cohorts graduated (and partly in response to broader European educational reforms), making it unlikely that the timing of these supply changes was endogenous to local conditions. Finally, these reforms effectively duplicated an important segment of the typical entry cohort—namely, graduates from upper-track high schools that comprise a large and growing share of German secondary education.

My paper contributes to an existing literature that measures the equilibrium effects of local labor supply shocks (e.g., Card, 2001; Dustmann and Glitz, 2015). Most closely related is Morin (2015), who studies how Ontario’s 2003 double cohort—which resulted from a high school reform very similar to the ones I study here—impacted labor market outcomes among young high school graduates. Morin finds that the cohort shock decreased both youth employment rates and youth wages, with weekly wages falling by 5 to 9 percent two years after the supply shock. I complement Morin’s paper in several ways. First, rather than analyzing the impact of cohort-crowding on equilibrium wages, I instead focus on equilibrium job creation in the entry-level market for new apprentices. Germany is well-suited to this analysis because its apprenticeship market is legally demarcated and because apprenticeships are the main port of entry for most new German workers. Second, because the high school reforms occurred in different states in different years, my design is more robust to the threat of idiosyncratic state-year factors that might confound analysis of a single cohort shock. Third, because the reforms I study impacted only a subset of German high schools, I am able to analyze spillover effects on students graduating from non-reformed high schools in the year of the double cohort shock. Fourth, I use annual data, enabling a higher-frequency analysis than Morin is able to conduct using available Canadian census and survey rounds.

A separate literature has analyzed the impact of the German high school reforms on educational attainment. Using survey data from Saxony-Anhalt, the first state to implement these reforms, Büttner and Thomsen (2015) show that students subject to the new, compressed curriculum perform worse in mathematics but equally well in literature. Meyer and Stephan (2016), who study the Saxony-Anhalt reform using the same data, find that some female students responded to the shorter curriculum by delaying university enrollment in favor of first pursuing vocational training. Studying the full set of high school reforms, Huebener and Marcus (2017) show that students subject to the new, compressed curriculum are more likely to repeat grades and earn lower GPAs. Using administrative microdata on the universe of German university students, Marcus and Zambre
(2016) find that students educated under the compressed curriculum delay their university enrollments and are more likely to drop out of university or to change their major. Other papers have studied the impact of the reforms on cognitive skills (Andrietti, 2015) and on personality (Dahmann and Anger, 2014). To my knowledge, however, my paper is the first to identify equilibrium impacts of the high school reforms on the German labor market.

The rest of this paper is structured as follows. Section 2 describes the institutional setting, including the high school reforms as well as necessary background on Germany’s apprenticeship system. Section 3 lays out the research design. Section 4 reports my results. Section 5 concludes.

2 Institutional Setting

2.1 High school reforms and the “double cohorts”

German education policy falls mostly within the jurisdiction of the sixteen federal states (Bundesländer), but its basic contours are similar across states.1 Upon completing primary school, German students are assigned to one of three secondary-school tracks, which differ both in their academic rigor and in the duration of their curriculums.2 The three tracks are traditionally housed in entirely separate school facilities—known as the Hauptschule, Realschule, and Gymnasium—though some states have introduced comprehensive schools that combine two or more tracks under one roof. The lower track, which culminates after grade 9 or 10 with a school-leaving certificate known as the Hauptschulabschluss, is often described as preparing students for blue-collar jobs. The middle track, which ends after grade 10 with the Realabschluss qualification, is oriented towards non-professional white-collar jobs. The upper track lasts through grade 12 or 13 and culminates in the Abitur credential, which qualifies students to enroll in university. As of the early 2000s, upper-track (gymnasium) education concluded after grade 13 in all but two states.3

1 A standing conference of the states’ education ministers (the Kultusminister-Konferenz, or KMK) coordinates policy-making across states and promotes uniform educational standards. This section draws heavily on Lohmar and Eckhardt (2015), an annual dossier published by the KMK.
2 Primary school ends after grade 4 in most of Germany but after grade 6 in Brandenburg; in some states, grades 5 and 6 constitute an “orientation phase” in which tracking is not yet finalized. Both schools and parents have a say in the tracking decision, though the final authority differs across states (Dustmann et al., Forthcoming).
3 Prior to German reunification in 1990, East Germany’s gymnasium system lasted only through grade 12. After reunification, the states of Saxony and Thuringia retained their 12-grade systems, but Berlin, Brandenburg, Mecklenburg-Western Pomerania, and Saxony-Anhalt introduced a 13th grade to conform to contemporary West German practice. In Mecklenburg-Western Pomerania and Saxony-Anhalt, the first cohort of 13th graders did not
Between 2001 and 2007, however, almost all of Germany’s states passed laws eliminating the gymnasium’s 13th grade. Incumbent high school cohorts graduated on their original timetables, but, once the new curriculum was in place, subsequent cohorts graduated after grade 12, with the school year and school day lengthened throughout high school to keep total instructional hours constant. The reforms had two main goals: first, to bring Germany’s education system in line with standard European practice, especially in the wake of the 1999 Bologna Declaration (Büttner and Thomsen, 2015); and second, to ease demographic burdens on public finances by extending working lives. But as a byproduct, each reforming state experienced a year in which two cohorts of gymnasium students graduated simultaneously: the last 13th graders subject to the old rules, plus the first 12th graders subject to the compressed curriculum. Figure 1 shows the timing of Germany’s double gymnasium cohorts, of which the first graduated in 2007 and the last in 2016.

To illustrate how these reforms affected the size of school-leaving cohorts, Figure 2 plots the number of gymnasium, non-gymnasium, and total secondary-school-leavers in the large southern state of Bavaria. The state’s 2011 double gymnasium cohort is clearly visible. Because Bavaria’s lower- and middle-track schools were not subject to the curricular reform, the total school-leaving population rises by the amount of the excess gymnasium cohort. After the new system is fully in place, the number of graduates reverts to its pre-reform trend.

Figure 3 generalizes beyond this case study by plotting the evolution of gymnasium and non-gymnasium graduates in each reforming state, with event time normalized to zero in the double-cohort year. Every state experiences a sharp increase in gymnasium graduates, with no similar changes in non-gymnasium graduate counts. Because the double cohorts occur in different states in different years, they offer a compelling source of quasi-experimental variation for analyzing how

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4 A leading proponent of the curriculum reform was Bavaria’s Minister President Edmund Stoiber, who in a 2003 address declared, “Compared with other European countries, the German education system is robbing our young people of valuable time that they could be using to start their families, their careers, and their pensions. If German university graduates are only able to start their working lives at age 28, it’s a waste of resources for the social security system and ultimately for the community as a whole. They ought to start school sooner and start work sooner. ...Our young people should have the best possible starting position for their start into life. They should receive excellent training. But they should also be able to keep up with young people from other countries who start working earlier and hence have better chances in our globalized world. As such, we will shorten the Gymnasium to eight years [from grade 13 to grade 12].” (Bachmeier, 2017, own translation).

5 These figures plot graduates per capita, where the denominator for year t is constructed by projecting forward the number of 14-year-olds residing in the state in t − 5 and t − 4 (for gymnasium students, who typically graduate at age 18 or 19) or t − 2 (for non-gymnasium students, most of whom graduate at age 16). Normalizing by population is essential to account for the East German baby bust that occurred in the wake of German reunification. I discuss these population dynamics further in Section 3.1.
sharp changes in entry-cohort size affect local labor market equilibria.\(^6\)

2.2 The apprenticeship system and transitions to the labor market

Upon completing general schooling, most lower- and middle-track students proceed to part-time or full-time vocational training, often within Germany’s “dual system” of joint classroom and on-the-job training. Many upper-track students immediately enroll at university, but some pursue vocational training: according to a 2012 survey, about 12 percent of new gymnasium graduates were employed as firm-based apprentices six months after graduation, with an additional 8 percent engaged in other forms of vocational education (Schneider and Franke, 2014).\(^7\) As such, the double gymnasium cohorts are likely to increase the supply of applicants to apprentice postings, especially in higher-skilled occupations. This might induce firms to open additional positions, but if the demand for apprentices is not perfectly elastic, it may also crowd some non-gymnasium students out of the apprenticeship market.\(^8\)

Firm-based training is highly regulated. Under the dual system, each apprenticeship is classified into one of about 330 recognized occupations; apprenticeships typically last between 2 and 4 years and culminate in a standardized examination. Wages are collectively bargained, but employers have some ability to set individual wages through bonuses and other mechanisms (Mohrenweiser et al., 2013). The cost of classroom training is borne by government, but employers pay for trainee stipends and for the costs of on-the-job training. Motivated by Becker's (1964) insight that workers should bear the cost of general human capital, several papers (e.g., Acemoglu and Pischke, 1998) have asked why employers participating in the dual system are willing to pay for what appear to be portable skills. Many apprentices are retained by their training firm after completing the course of training (von Wachter and Bender, 2006), and employers may be able to recoup their upfront

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\(^6\) To give a sense of the shock's nationwide impact, Appendix Figure 1 plots the total number of gymnasium graduates nationwide against a counterfactual series constructed by replacing each double cohort with the simple average of graduates in the year preceding and the year following the shock. The two series coincide prior to 2007, diverge slightly in 2007–2010 (when four small states experienced double cohorts), and diverge more strongly in 2011–2013 when larger states experienced double cohorts.

\(^7\) Prior to 2011, male German citizens were in principle subject to a year of mandatory military or civil service, but many men obtained exemptions from the service requirement. Conscription was abolished in 2011. Survey data indicate that the share of graduates engaged in apprenticeship training six months after graduation remained relatively constant before and after the end of conscription (Schneider and Franke, 2014).

\(^8\) To the extent that gymnasium graduates are disproportionately likely to enroll in universities in their home states, the high school reforms will likely produce a second wave of local labor supply shocks when the double cohorts graduate from university. This second shock may not be very sharp, however, since there is considerable variation in time-to-degree both across programs and across students within a given program.
investments by exploiting informational advantages relative to outside employers. In recent years, German firms have also come under political pressure to open additional training slots. Nonetheless, individual employers maintain substantial leeway in deciding how many positions to open, what occupations to train, and whom to hire.

3 Research Design

3.1 Specification

My research design amounts to tracking state-level outcomes before and after the occurrence of each state’s double gymnasium cohort. Concretely, I run event-time specifications of the form

$$y_{st} = \alpha_s + \gamma_t + \sum_{k \neq -1} \delta^k D_{st}^k + X_{st}'\beta + \varepsilon_{st},$$

where $y_{st}$ is an outcome variable for state $s$ in year $t$, $D_{st}^k$ is an indicator variable for observations $k$ periods before/after the double cohort, and $X_{st}$ is a set of state x year-level controls. I include dummies for $k \leq -3$, $k = -2$, $k = 0$, $k = 1$, $k = 2$, and $k \geq 3$. The coefficients $\delta^k$ capture changes in the outcome variable at event-time $k$ relative to the omitted period (one year before the double cohort occurs). I estimate a single event-time coefficient for all periods 3 or more years before the double cohort ($k \leq -3$), and similarly for all periods 3 or more years after it ($k \geq 3$).

My sample period spans 2002–2016, though data limitations dictate a shorter sample period for certain outcomes of interest. Two East German states (Mecklenburg-Western Pomerania and Saxony-Anhalt) experienced a “zero cohort” in 2001, when their oldest gymnasium students proceeded to grade 13 for the first time rather than graduating after grade 12 as previous cohorts had. I begin the analysis in 2002 to exclude these earlier cohort shocks from my sample period.

The East German states of Saxony and Thuringia did not experience double gymnasium cohorts during my sample period, since their gymnasium curriculums already ended after grade 12 and hence did not undergo reform. For these states, I set $D_{st}^k = 0$ for all $k$, so that they do not help to identify the event-time coefficients (but do help identify the year effects and other controls). The West German state of Hesse implemented its reform gradually, so that its double gymnasium
cohort was spread out over 2012–2014. Following Marcus and Zambre (2016), I omit Hesse from the analysis in order to focus on a comparable set of sharp cohort-size shocks. This leaves me with 15 remaining states, including the three city-states of Berlin, Bremen, and Hamburg.

I cluster standard errors at the state level to allow for arbitrary serial correlation in state-level outcomes. Given the small number of clusters, I compute standard errors using the wild-bootstrap method suggested by Cameron et al. (2008). As a sensitivity check, I also perform a permutation test to gauge the statistical significance of my main result on the number of new apprenticeship contracts.

To isolate the policy-induced variation associated with the gymnasium reforms, it is critical to control adequately for contemporaneous changes in state-level school-age population that may be spuriously correlated with the timing of the double cohorts. Of particular importance are the belated effects of the East German baby bust that accompanied German reunification in 1990. To proxy for the size of successive school-leaving cohorts on the eve of their first secondary-school graduations (which begin when Hauptschule enrollees graduate at age 15), Appendix Figure 2 plots the age-14 population in both West and East Germany over 1995–2016. Whereas West German cohorts grew smoothly in the early 2000s and have declined smoothly since 2005, East German cohorts of 14-year-olds fell almost in half between 2005 and 2008. These sharp population swings mirror equally sharp declines in East German fertility in the period immediately following the fall of the Berlin Wall in November 1989: the number of live births in East Germany fell dramatically around the time of reunification in response to economic uncertainty, migration of young couples from East to West, and the disappearance of pro-natalist policies enacted by the former German Democratic Republic (Witte and Wagner, 1995).

To account for these sharp population changes, most of my specifications add a series of state-level population controls \( \sum_{a=15}^{22} \delta^a \log P_{st}^a \), where \( P_{st}^a \) is a measure of each state’s potential age \( a \) population in year \( t \). Because changes in state population may be endogenous to the change in gymnasium duration—since students may migrate to another state upon graduating from high school—I set \( P_{st}^a \) equal to the state’s observed age-14 population in year \( t – (a – 14) \). The resulting age proxies are exogenous to the gymnasium reforms, provided that those reforms did not lead to population changes among students younger than 15. Using the German Socio-Economic Panel (GSOEP), Dahmann and Anger (2014) report that 92.3 percent of the gymnasium students in
their sample have not changed their place of residence since childhood, suggesting that cross-state mobility is unlikely to be a serious concern in this context.

3.2 Data

This paper draws on public-use data published by German statistical agencies and research institutes. My data come primarily from Germany’s Federal Statistical Office, which publishes state-year aggregate data on population, school enrollments, the number of students graduating with each type of secondary school degree from each type of secondary school, the number of new university enrollments, and GDP. The Statistical Office also reports each state’s number of newly signed apprenticeship contracts, overall as well as subdivided either by age or by type of secondary school degree.9 I also use a longer time series for the total number of new apprenticeship contracts published by Germany’s Federal Institute for Vocational Education and Training (BIBB).

4 Results

4.1 Impact on number of school-leavers

Table 1 reports regression results showing the impact of the high school reforms on the number of students exiting secondary schools during 2002–2015 (either through graduation or through dropout). In columns 1–3, the outcome variable is $100 \times \log$ number of gymnasium graduates in a given state-year. Column 1 reports results from estimating (1) with no controls other than state and year fixed effects. The coefficient $\hat{\beta}_0 = 53.08$ indicates that the number of gymnasium graduates rises by 53.1 log points (about 70.1 percent) in the year of the double cohort. However, inspection of the other event-time coefficients reveals a strong negative pretrend for event times $k \leq -3$, driven by the dramatic East German baby bust discussed in Section 3.1. Column 2 replicates this specification using West German states only; notwithstanding a modest but statistically significant negative coefficient for $k = -2$, the pretrend largely vanishes when I restrict to the West. Column 3 again pools West and East but—to account for the baby bust—adds controls for detailed population counts (ages 15–22), obtained by extrapolating forward from each state’s observed age 14

9 Data from the Federal Statistical Office may be downloaded from https://www-genesis.destatis.de/genesis/online. Additional data series are available through the office’s Statistical Library at https://www.destatis.de/GPStatistik/content/below/index.xml.
population in each year. Adding these population controls essentially eliminates the pretrend while only modestly impacting the "spike" coefficient $\hat{d}_0$, which falls to 49.8 log points (64.5 percent).\(^{10}\) All subsequent specifications include these population controls.

Why did the "double cohorts" only increase the number of new gymnasium graduates by about two-thirds? Several factors are likely at work. Huebener and Marcus (2017) show that grade repetition rates rose by about 3.6 percentage points under the compressed curriculum, as some students struggled to keep up with the faster instructional tempo. Grade repetition may have been especially common for the first cohort subject to the new curriculum, as these students would have had an incentive to repeat a grade to avoid graduating alongside the (extra-competitive) double cohort. Some students may have skipped a grade for the same reason, and others may have switched to the middle track to avoid confronting the more intensive curriculum.

The remaining columns replicate column 3's specification using additional outcomes. Column 4 shows that the double cohort had no apparent impact on the number of students exiting from other kinds of secondary schools, such as lower-track schools (Hauptschulen) and middle-track schools (Realschulen). Column 5 shows that the double cohort increased the total number of school leavers by 20.1 log points (22.3 percent), consistent with the cross-sectional fact that about one-third of students graduate from gymnasiums in a normal year. The last two columns look at the degree (if any) that students graduate with. Column 6 shows that the high school reforms induce a 50.0 log point increase in the number of students receiving the Abitur (allgemeine Hochschulreife) a university entrance exam, which is consistent with the fact that the vast majority of gymnasium students graduate with the Abitur (and few students from other schools obtain the Abitur). The last column shows no impact on the number of students leaving school without the Abitur. Taken together, these results confirm what we would expect given the nature of the institutional reforms: a marked increase in gymnasium/Abitur graduates with no apparent change in non-gymnasium/non-Abitur graduates. The lack of an impact on these other school/degree types will justify looking for evidence of spillovers on non-Abitur students' access to apprenticeship positions.

\(^{10}\) The marginally significant negative effect for $k \leq -3$ may indicate that states differ slightly in the growth rate of gymnasium as a share of all secondary school enrollments, but the effect is small and, if anything, suggests that the double cohort effect is slightly larger than my estimates imply.
4.2 Impact on new apprenticeships

Overall effects

German firms choose whether to participate in the apprenticeship system and, if so, how many apprentices to hire each year. Although apprenticeships are partly subsidized, prior research suggests that firms bear much of the financial costs of training a new worker (Acemoglu and Pischke, 1998). Furthermore, apprentice wages are determined institutionally so that firms are at least partly constrained in their ability to lower wages in exchange for absorbing an influx of additional trainees. Given these facts, it is not clear ex ante whether firms would have been willing to open additional apprenticeship positions to accommodate increased labor supply associated with the double cohorts. In addition, if school-leavers are willing to migrate across state boundaries in pursuit of an apprenticeship, supply shocks may be diffused nationally, with no disproportionate increase in hiring in double-cohort state-years. I now examine whether local supply shocks instead led to excess local hiring, above and beyond any proportional changes at the national level.

Table 2 columns 1–3 replicate the first three columns from Table 1, using the log number of new apprenticeship contracts signed in a given year as the outcome measure, observed in the BIBB data over 2002–2016. In the first specification, I find a positive point estimate of 2.7 log points in the year of the double cohort, but the effect is noisy and insignificant, and there is again a strong pretrend. The next columns address the pretrend, first (in column 2) by restricting attention to West Germany where population is more stable, and then (in column 3) by controlling for predicted population counts (ages 15–19). The shock coefficient is modestly attenuated but the standard errors shrink substantially, with a marginally significant positive effect observed in column 4. To further improve precision, column 4 and all subsequent specifications in this table add controls for the observed number of students graduating from non-gymnasium schools. Both the institutional details and Table 1 suggest that these non-gymnasium counts were not impacted by the high school reforms, which justifies treating them as exogenous in this specification. Adding these controls boosts the point estimate slightly and further increases precision, yielding a precisely estimated increase of about 1.9 percent in the year of the double cohort, and no significant effects before or after the double cohort year. Columns 5 and 6 show that this result is qualitatively and quantitatively robust to controlling for log state GDP (to proxy for shifts in labor demand) and to
allowing for state-specific trends in the number of new apprenticeships.

The next two tables will draw on an alternative data series from Germany's Federal Statistical Office (FSO). Since these FSO data are available only during 2004–2015, columns 7 and 8 replicate my preferred specification, column 4, using the shorter time period 2004–2015 in both the BIBB and FSO data. The point estimates are comparable to the effect in column 4.

A possible concern is that I have only 15 clusters, reflecting my inclusion of 15 of Germany’s 16 federal states. Although the wild-bootstrap method used here is believed to work well even for a small number of clusters (Cameron et al., 2008), I can also assess the statistical significance of my core apprenticeship result using a randomization inference approach. Concretely, I replicate my preferred specification from column 4 on 1,000 pseudo-samples obtained by randomly permuting the identities of the 15 states used here when assigning double-cohort years. These pseudo-samples preserve the distribution of reform years across states while randomizing the timing of treatment across states. Figure 4 shows the empirical pdf of the estimated “spike” coefficients ($\hat{\delta}_0$) across these pseudo-samples, together with the estimate obtained from the true sample (shown with a red line). The true effect is a clear outlier ($p = .013$), confirming that the precise positive effect is not an artifact of poor econometric performance of the wild bootstrap in this few-cluster setting.

On balance, the evidence from Table 2 suggests that the high school reforms increased the equilibrium number of new apprenticeships by about 2 percent in each reforming state. What do we learn from this result? First, we can clearly reject the null hypothesis that the number of apprenticeships is fixed in the short run, with firms offering a set number of slots regardless of how many school-leavers are seeking apprenticeship slots. Instead, it appears that labor demand is at least somewhat elastic, with firms hiring more in response to the shock. If collectively bargained apprentice wages were not previously binding (e.g., because employers routinely offered bonus payments), employers may have had room to lower wages in the face of increased supply. If wages were downward rigid, an alternative explanation is that firms faced better applicants on average (since the marginal graduates come from prestigious, upper-track high schools), so that they were willing to hire more apprentices at the same wage. A third possibility is that firms may have faced political pressure to expand hiring to accommodate the influx of new apprentice applicants. I cannot distinguish among these explanations with available data.11

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11 An additional question, which I cannot answer using my present data sources, is whether local growth in
A second conclusion we can draw is that these local supply shocks had differential equilibrium consequences for local labor markets: had the excess school-leavers from a reforming state simply diffused equiproportionally nationwide in pursuit of job opportunities, we would not observe excess hiring in treated state-years. This result suggests that a local labor markets design, applied to richer microdata, can hope to detect richer consequences of these supply shocks on local labor market outcomes. This accords with a growing body of empirical research (e.g., Autor et al., 2013) finding that local shocks have discernible effects on local wages and unemployment, even though workers can migrate freely across localities to escape localized adverse conditions.

Crowd-out?

An open question at this point is whether the double gymnasium cohorts crowded any non-gymnasium students out of the apprenticeship market. If the demand for apprentices is downward sloping—or, given possible wage rigidities, if apprenticeship positions are rationed on the margin—then the glut of new jobseekers graduating from gymnasiums may have displaced jobseekers coming from other types of high schools unaffected by the curriculum reforms. Conversely, the possibility of finding better matches could in principle have encouraged firms to create enough new positions to more than accommodate the supply glut (Hagedorn et al., 2016). Finally, if labor demand is perfectly elastic, or if gymnasium and non-gymnasium students apply to fully segmented apprenticeship markets, there might be no effect on non-gymnasium school-leavers.

To answer this question, Table 3 estimates the effect of the double-cohort shock on the number of new apprenticeship contracts broken out by high school degree type. Consistent data by degree type are available only during 2007-2015. Column 1 replicates my earlier specification on this shorter time period and finds a positive but imprecise effect in the year of the double cohort. The next three columns examine changes in the number of apprentices without a high school degree, with a lower-track degree (Hauptschulabschluss), or with a middle-track degree (Realabschluss or Fachhochschulreife).12 I find negative point estimates for all three groups in the year of the double

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12 The Fachhochschulreife is an uncommon degree that entitles students to enroll at a technical university but not a university proper. Some students first obtain the Realabschluss and subsequently obtain the Fachhochschulreife.
cohort, but the effects are imprecise (in part reflecting the short panel available for this analysis). The last column shows a roughly 6.9 percent increase in the number of new apprenticeship contracts among Abitur recipients, as would be expected given that the vast majority of gymnasium students graduate with the Abitur.

To obtain a longer time series, Table 4 instead breaks down the number of new apprentices by age, for which apprenticeship counts are available over 2004–2015. Because lower- and middle-track curriculums end after grade 9 or 10, whereas upper-track curriculums end after grade 12 or 13, new apprentices younger than age 18 represent some mix of lower- and middle-track graduates (as well as high school dropouts). By contrast, apprentices ages 18–19 mostly represent gymnasium graduates, though they may include some lower- and middle-track students who deferred the start of vocational training. Apprentices ages 20 and older are generally students who took one or more gap years before embarking on training. I find small but imprecise effects on the number of young apprentices, again providing no evidence for either crowd-out or crowd-in effects. I find large increases in the number of apprentices ages 18 and 19, consistent with the rise in gymnasium graduates taking apprentice jobs. After a one-year delay, I find a decrease in the number of new apprentices ages 20+, as would be expected given that gymnasium graduates are beginning their post-secondary-school lives one year sooner.13

This analysis reveals that state-level variation, paired with public-use aggregate data, is insufficiently rich to detect or rule out economically meaningful displacement of non-gymnasium students. In future work, I plan to exploit richer within-state variation, coupled with administrative microdata on the apprenticeship market, to provide more precise evidence on this question.

5 Conclusion

Labor markets must continually absorb new entering cohorts, the size of which depends on fertility rates, schooling durations, migration decisions, and other factors. Economists have long been interested in the effects of cohort-size fluctuations on labor market outcomes, but these effects are difficult to identify because natural variation in cohort size is slow-moving and hence commingled

13 For example, a student who might previously have graduated at age 19, taken a gap year, and then started working at age 21 may now graduate at 18, take a gap year, and start working at age 20.
with secular changes in technology, tastes, and other determinants of employment and wages.

To circumvent this problem, this paper exploits German state-level reforms that compressed the curriculum at upper-track high schools from 13 grades to 12. The double graduation cohorts produced by these reforms provide an exceptional opportunity for analyzing how local labor markets adjust to sharp fluctuations in the size of new labor market cohorts. Using an event-study design, I find a modest but precisely estimated increase in the equilibrium number of newly signed apprenticeship contracts in the year a state experiences a double cohort. This result rules out the null of perfectly inelastic demand for new apprentices, and it demonstrates that local supply gluts have a disproportionate impact on the states in which they originate. The increased number of apprentices is driven by upper-track graduates, with imprecise impacts on the number of lower- and middle-track graduates transitioning to apprenticeships.

My analysis has relied solely on public-use data, which provide only a limited view into the market for entry-level workers. In future work, I plan to conduct a more comprehensive analysis using linked worker-firm administrative microdata maintained by Germany’s Federal Employment Agency (IAB). These data will permit me to analyze a richer set of outcomes—including wages, turnover, and skill intensity—and to exploit richer variation in the intensity of treatment across occupations, industries, localities, and individual firms. Paired with high-quality microdata, Germany’s double graduation cohorts are well-suited to answering a host of questions about how supply gluts impact workers’ careers, firms’ personnel decisions, and local labor market equilibria.
Figures and Tables

Figure 1: Timing of the double gymnasium cohorts

Note: Each blue state experienced a double gymnasium cohort in the indicated year. The state of Hesse (light gray) implemented its reforms gradually, so that its extra cohort is spread out between 2012 and 2014. Rhineland-Palatinate (medium gray) adopted the compressed curriculum only at a small share of its gymnasiums, leading to a modest increase in gymnasium graduates in 2016. In Saxony and Thuringia, gymnasium education already concluded with the 12th grade—a legacy of the East German school system.

Figure 2: Case study: Bavaria’s 2011 double cohort

Note: The blue series plots the number of students graduating from gymnasiums, regardless of whether they obtained the upper-track degree (Abitur) or some other degree. The red series plots the number of students exiting all secondary institutions, including recipients of the lower-track degree (Hauptschulabschluss) and middle-track degree (Mittlere Reife or Realschulabschluss) as well as high school dropouts.
Figure 3: Double cohorts due to gymnasium reforms

i. Gymnasium graduates

Note: Each series plots log school-leavers per capita. Let $P_{st}^a$ denote the potential age- $a$ population residing in state $s$ in year $t$, which I set equal to the observed population of 14-year-olds in year $t - (a - 14)$. For the top panel, I denominate the number of school leavers by $P_{st}^{19}$ in years prior to the double cohort—reflected at the fact that most gymnasium students graduate at age 19 before the reform—by $P_{st}^{18}$ in years following the double cohort. For the double cohort year itself, I denominate by $\frac{1}{2}(P_{st}^{18} + P_{st}^{19})$. For the bottom panel, I denominate the number of school leavers by $P_{st}^{16}$ (since students typically leave lower- and middle-track high schools between the ages of 15 and 17).
Note: Permutation test of the effect of state-level high school reforms on the log number of newly signed apprenticeship contracts. I construct 1000 pseudo-samples by randomly permuting the identities of the 15 federal states used in my analysis before assigning each state its associated double-cohort year. I then estimate (1) on each pseudo-sample, using the specification from Table 2 column 4, and report the empirical distribution of the measured effect in the year of the double cohort ($\delta^s$). The red line denotes the effect estimated using the true data, which is an outlier relative to the distribution of placebo effects ($p = .013$).
Table 1: Impact of reforms on number of secondary school-leavers, 2002–2015

**Dep. var.: 100 x log secondary school-leavers**

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Note: Results from OLS estimation of (1). In each column, the dependent variable is 100 x the log number of secondary school-leavers: columns 1–3 look at changes in the number of gymnasium graduates, column 4 at the number of non-gymnasium school-leavers (including high school dropouts), column 5 at total school-leavers, and columns 6–7 at the number of students graduating with or without the Abitur qualification awarded to most gymnasium graduates. Columns 3–7 add controls for the state’s predicted (log) population ages 15, 16, …, 22 to flexibly allow for changes in state young-adult population. Standard errors are clustered at the state level and obtained via wild bootstrap. *: p < .10, **: p < .05, ***: p < .01. See Section 4.1 for details.
Table 2: Impact of reforms on number of new apprenticeship contracts

<table>
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<th>Dep. var.: 100 x log new apprenticeship contracts</th>
<th>BIBB data</th>
<th>(2002-2016)</th>
<th>(\text{FSO data})</th>
<th>(2004-2015)</th>
<th>(2004-2015)</th>
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<td>(1.18)</td>
<td>(1.15)</td>
<td>(1.13)</td>
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Note: Regression results patterned after Table 1. Columns 1–7 take apprenticeship counts from Germany’s Federal Institute for Vocational Education and Training (BIBB). Column 8 instead takes apprenticeship counts from the Federal Statistical Office. Columns 4–9 add controls for the (log) number of students graduating from lower- and middle-track secondary schools unaffected by the gymnasium reforms. *: \(p < .10\), **: \(p < .05\), ***: \(p < .01\). See Section 4.2 for details.
Table 3: Impact of reforms on number of new apprenticeship contracts by degree type

<table>
<thead>
<tr>
<th>Dep. var.: 100 x log new apprenticeship contracts</th>
<th>Total</th>
<th>No degree</th>
<th>Lower-track</th>
<th>Middle-track</th>
<th>All non-Abitur</th>
<th>Abitur</th>
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<td>(5)</td>
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<td>18.84**</td>
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<td>2.64</td>
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<td>(2.80)</td>
<td>(2.00)</td>
<td>(3.02)</td>
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<td>-1.91</td>
<td>1.24</td>
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<tr>
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<td>(7.62)</td>
<td>(2.79)</td>
<td>(1.97)</td>
<td>(1.39)</td>
<td>(2.60)</td>
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<tr>
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<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
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<tr>
<td>(.)</td>
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<td>(.)</td>
<td>(.)</td>
<td>(.)</td>
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<tr>
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<td>6.89**</td>
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<tr>
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<td>Reform x 1(t = 1)</td>
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<td>-5.41</td>
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<tr>
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<td>(2.59)</td>
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Note: Regression results patterned after Table 1. Apprenticeship counts are taken from the Federal Statistical Office and span 2007–2015. Lower track refers to the Hauptschulabschluss degree. Middle track refers to the Realschulabschluss and Fachhochschulreife degrees. *: p < .10, **: p < .05, ***: p < .01. See Section 4.2 for details.
Table 4: Impact of reforms on number of new apprenticeship contracts by age

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<th></th>
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<th>Age 19</th>
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<td>Reform x 1(t &lt;= -3)</td>
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<td>2.69*</td>
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<td>3.45</td>
<td>4.63**</td>
<td>6.28**</td>
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<tr>
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<td>(2.04)</td>
<td>(2.53)</td>
<td>(3.03)</td>
<td>(2.35)</td>
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<td>0.81</td>
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<td>(1.86)</td>
<td>(1.81)</td>
<td>(1.90)</td>
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<tr>
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<td>8.41***</td>
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<td>(1.76)</td>
<td>(1.55)</td>
<td>(1.64)</td>
<td>(1.86)</td>
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<td>9.35***</td>
<td>4.15**</td>
<td>-4.14**</td>
<td>-5.03*</td>
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<td>(2.72)</td>
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<tr>
<td>Reform x 1(t = 2)</td>
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<td>11.04***</td>
<td>4.72**</td>
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<td>-7.43*</td>
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Note: Regression results patterned after Table 1. Apprenticeship counts are taken from the Federal Statistical Office and span 2004–2015. *: p < .10, **: p < .05, ***: p < .01. See Section 4.2 for details.
Supplementary Figures and Tables

Appendix Figure 1: Impact of reforms on nationwide gymnasium cohorts

Note: The blue series plots the observed number of gymnasium graduates nationwide (including both reforming and non-reforming states). The red series plots counterfactual graduates in the absence of high school reforms. For each state experiencing a double cohort in year \( t \), I replace the observed graduate count with the simple mean of the counts in \( t - 1 \) and \( t + 1 \). For all other state-year combinations, I retain the observed graduate count.

Appendix Figure 2: Shrinking cohorts due to the East German baby bust

Note: Data from the Federal Statistical Office. The rapid decline in East German cohort size between 2005 and 2006 reflects the belated impact of East Germany’s post-reunification baby bust, when fertility rates plunged due to economic uncertainty, outmigration of young couples, and the disappearance of pro-natalist government policies (Witte and Wagner, 1995).
References for Chapter IV


