Essays on the Economics of Labor
Demand and Policy Incidence

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Essays on the Economics of Labor Demand and Policy Incidence

A dissertation presented

by

Andrew Garin

to

The Department of Political Economy and Government

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Essays on the Economics of Labor Demand and Policy Incidence

Abstract

This dissertation studies social incentives in pro-social behavior and its various implications, including but not limited to disclosure policies, fundraising strategies and geographic polarization. In Chapter 1, I (with Filipe Silverio) test how sensitive wages are to firm-level labor demand by estimating the incidence of idiosyncratic export demand shocks on the wages of incumbent workers in Portugal during the Great Recession (2008-2010). Using detailed export records, we construct measures of firm exposure to unanticipated shocks to the demands of different countries for specific products. The shocks predict changes in output and payroll at affected firms, but not at other similar firms. We combine the export demand measures with firm balance sheet data and matched longitudinal administrative employer-employee records to estimate the impact of idiosyncratic firm-level demand shocks on employee outcomes. We find that idiosyncratic shocks that decreased sales or value added by 10 percent caused wages of incumbent workers who were employed by affected firms in 2007 to decrease by 1.5 percent relative to trend. These causal effects are large enough to explain nearly all of the variance in wages that can be attributed to observational firm pay differentials. Furthermore, we find that these pass-through effects are stronger in industries with higher durability of employment relationships and lower employee turnover rates. These results support a model in which barriers to replacing incumbent workers give rise to internal labor markets within the firm, exposing workers to their employers’ idiosyncratic conditions.

Chapter 2 studies whether local infrastructure construction an effective way to boost employment in distressed local labor markets. I use new geographically-detailed data on highway
construction funded by the American Recovery and Recovery Act to study the relationship between construction work and local employment growth. I show that the method for allocating funds across space facilitates a plausible selection-on-observables strategy. I find that highway impacted construction employment at the county level: a dollar of additional Recovery Act spending on local construction increased local construction payrolls by thirty cents over 2009-2013. The magnitude of this effect matches the national labor share of construction revenues, suggesting that targeted spending did not crowd out other local construction. These effects are most pronounced among counties with smaller populations and smaller fractions of residents that commute to outside counties for work. However, when I test for general equilibrium effects on local employment and payroll aggregates, I find effects close to zero with very wide confidence intervals across all specifications. Although the Recovery Act was a significant enough intervention to have a sizable impact on the construction sector in counties with low mobility, these findings suggest that the local variation in highway spending was too small relative to baseline regional volatility to detect a local employment “multiplier.”

Chapter 3 examines the long-run local labor market effects of the publicly-financed construction of large manufacturing facilities during World World War II. I focus on a subset of large, new plants that the military was not able to incentivize private firms to stake any capital on, and likely would not have been built if not for the war. I compare recipient counties to counties that were similar but for conditions engendered by the war. After establishing an absence of pre-trends across a number of outcomes, I show that recipient counties experienced a large post-reconversion boost in manufacturing employment and wages that persisted for several decades. I show how these effects impact broader labor market outcomes in the post-war period and discuss methods for distinguishing between causal mechanisms using plant-level data.
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1. Do Wage Increases Reflect Firm-Level Labor Demand or Market Competition? Evidence from Idiosyncratic Export Demand Shocks

1.1. Introduction

In the canonical competitive labor market model, wages reflect the supply and demand for skill, and price-taking firms can adjust employment but not wages. However, in practice, labor markets feature barriers that make it costly for firms to replace current employees with new hires and for workers to find comparable new jobs. When firms’ internal labor markets are segmented from the external labor market, the wages of incumbent workers may directly reflect labor demand within the firm apart from the demand level in the external labor market. Recent work documenting substantial wage differentials across firms conditional on fixed worker attributes (Abowd et al., 1999; Card et al., 2016b; Song et al., 2015; Barth et al., 2016) has raised the possibility that heterogeneity in conditions across firms directly contributes to observed wage dispersion—even among workers with the same skills and abilities. This has led some to postulate that noncompetitive rents may play an important role in rising wage inequality, which might be a direct consequence of rising dispersion of firm productivity reflected in unequal worker rents.

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1 This chapter is joint work with Filipe Silverio.
2 The term “internal labor markets” was introduced by Doeringer and Piore (1971), who highlighted three kinds of frictions in particular: 1) costly on the job training, 2) need for specific skills that are costly to find in thin labor markets, and 3) “customs,” i.e. norms, laws, and collective bargaining institutions that make it difficult for firms to replace incumbents with outsiders.
Yet, to date, little is known about how much firm-level labor demand can affect wages apart from demand levels in the broader labor market. We present a theoretical framework that illustrates that, in general, one cannot identify the incidence of firm-specific demand changes on wages based on comparisons of observed changes in firm performance and observed changes in wages, because changes in firm performance may reflect also market-level factors or changes in worker characteristics. Rather, to measure how sensitive wages are to firm-level—rather than market-level—conditions, it is necessary to study variation in firm conditions that are unrelated to the overall state of the labor market (“idiosyncratic”) and are unrelated to the characteristics of workers themselves (“exogenous”).

To estimate the degree to which wages adjust in response to firm conditions, we study the wage incidence of quasi-experimental, idiosyncratic shocks to product demand, which exogenously shift labor demand conditions across firms without impacting broader labor markets. To identify idiosyncratic product demand shocks, we study exposure of more than 4,000 Portuguese exporters to demand fluctuations in foreign markets. We focus on demand shocks during the Great Recession, a period when global trade flows were sharply disrupted in unforeseeable ways, with significant variation in import behavior countries and across product markets. Because of unexpected changes in conditions across countries, exporters selling the same product but with pre-existing relationships with customers in different countries were exposed to differential changes in demand for their products. Crucially, this variation in exposure to different destinations within product markets generated differential shocks to demand for firms’ output—and to their demand for labor—without affecting the labor market as a whole.

Using detailed records on exports linked to balance sheets and matched employee-level data, we find that, after the onset of the recession, firms with better idiosyncratic demand shocks produce

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3This paper joins a growing literature combining rich firm-level export and import records with data on global trade flows to predict export demand and import supply shocks, for example Berman et al. (2015), Mayer et al. (2016), and Hummels et al. (2014). Our work is most related to the latter paper, which tests whether offshoring (input supply) shocks complement or substitute for incumbent workers. Their primary focus is substitutability/complementarity of low- and high-skill labor with off-shored inputs, though they simultaneously estimate import supply and export demand shocks as part of their analyses. They only report direct effects of (instrumented) log exports changes on wages, finding an elasticity of about 5% (if exports are a small fraction of sale, this presumably implies a larger sales-pass-through elasticity). This paper differs in its emphasis on identification shocks that are idiosyncratic to firms, and its focus on identifying the margins on which firms adjust their payroll, the magnitude of pass-through of shocks to total firm-level demand to wages, and the implications for wage determination.
more output, expand their total payrolls, and pay their incumbent workers more. In contrast, we find no evidence that these shocks predict changes in production or payroll at other similar firms.

To quantify the magnitude of these effects, we estimate a pass-through elasticity of wages with respect to sales that measures the causal change in wages per one-percent change in sales due solely to the demand shock. We show that, under weak assumptions, this elasticity is a lower-bound for the true elasticity between wages and the underlying change in firm-specific revenue productivity driving the changes in both sales and labor demand. We find that a shock that causes a ten percent increase (or decrease) in sales or value added results in a 1.5 percent increase (or decrease) in wages. Although sales at most firms declined during the recession, find that baseline nominal wage growth was nonetheless sufficiently inflationary that downward nominal rigidities did not appear to bind. We find that the pass-through effects are similar across all kinds of incumbent workers, though no effect is found for wages of new hires.

In our theoretical framework, we show that these results are difficult to reconcile with traditional models of competitive labor markets but are more consistent with a model featuring barriers that make it costly to fire incumbents and replace them with new hires. In line with such a model, we find that all employment adjustments occur on the hiring margin; there is no effect on differential retention. To further probe the plausibility of this explanation, we study heterogeneity in these pass-through effects across sectors where the extent of such barriers are likely to differ. In a wide class of models that microfound the barriers that give rise to internal labor markets, sectors with higher barriers to replacing incumbents will be marked by less labor market dynamism, lower turnover, and longer tenures. Accordingly, wage incidence should be most apparent in sectors with higher tenures and lower separation rates (higher “relationship-durability”). In sectors with lower separation rates, we find shocks that lead to a ten percent change in sales lead to an effect on wages exceeding three percent; by contrast, we find pass-through effects close to zero in low-

---

4 By “revenue productivity” we refer to the combination of firm-specific demand and technical productivity (TFP) shifters, as in Foster et al. (2008).

5 We classify firms according to the separation rate and typical tenure of permanent contract workers in the 5-digit industry. Examples of economic environments in which frictions generate higher relationship durability include models of firm-specific human capital following Becker (1962), such as Jovanovic (1979a) and Lazear (2009), models of heterogeneous match quality that require costly search, such as Jovanovic (1979b) and Mortensen and Pissarides (1994), and settings with institutional firing costs as in Lazear (1990) or convex costs to hiringAcemoglu and Hawkins (2014).
durability sectors. These results follow the pattern of treatment effect heterogeneity one would expect to see in the model we outline, supporting our explanation that wage incidence of firm shock reflects the extent of the barriers between internal and external labor markets.

These results provide novel evidence on the degree to which exogenous firm conditions can have substantial causal effects on wages. Although previous studies provide compelling evidence that product demand shocks to entire industries or regions have clear incidence on broad labor market segments (Abowd and Lemuix, 1993; Autor et al., 2014; Yagan, 2016b), little is known about the causal incidence of firm-specific shocks onto the wages of individual employees. To identify the wage impact of firm-specific product-demand conditions it is necessary to isolate variation in firm performance arising from shocks that are exogenous to labor inputs provided by individual workers (uncorrelated with unobserved labor supply factors) and idiosyncratic relative to the external labor market (uncorrelated with unobserved market-level performance). Without a clear source of variation in firm performance, observed changes in output over time may arise in practice for many reasons and may have little relationship to changes in underlying product demand that in turn shifts internal labor demand. Our research design overcomes these obstacles. We find that the sensitivity of wage growth to firm conditions is large. Our elasticity estimates imply that moving an individual from a firm at the 25th percentile of three-year output growth to a firm that at the 75th percentile would have caused their wage growth to increase by 6.5 percent, roughly half of the interquartile range of the full wage growth distribution in our worker sample.

Our findings shed new light on the prior literature that associates observed differences in firm

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6Some evidence does exist about the pass-through of firm-specific shocks to workers. One type of evidence comes from evidence on wage changes that occur after new innovations or patent awards. Van Reenen (1996) studies the effect of innovations on firm profits, and in turn the average wage bill of contemporaneous workers (one cannot observe whether or not the composition of workers changes in this case). Similarly, ongoing work by Kline et al. (2017) studies how patents and incumbent-worker earnings co-evolve after the award of a patent, exploring quasi-random variation in the timing of a patent. Both find significant effects of pass-through to workers’ wages, though the later study finds the largest effects for those actually named on the patent. While the presence of wage increases tied to success in innovation and patenting contradicts fully competitive price-taking behavior by firms period-by-period, these successes are attributable to previous efforts of individual workers in prior periods (though whether or when the work will pay off is unknown in advance). By contrast, it is not clear from these studies whether a fully exogenous demand shock would have any incidence on workers. Additional evidence comes from the “pay-for-luck” studies by Bertrand and Mullainathan who found that executives without strong principals both receive higher pay when firms profit for reasons out of their own control, and also appear to pay workers more regardless of the work of the employees (Bertrand and Mullainathan, 2001, 1999). As before this work suggests that wages co-move with profits in the presence of contracting frictions, but does not directly study effects of internal demand for labor.
performance and observed changes in wages without a quasi-experimental shock to firm performance and help reconcile seemingly inconsistent findings. Previous work studying changes of firm performance over time to changes in employee wages have found a small but very robust relationship between changes in sales or production and changes in wages or earnings in longitudinal employer-employee matched datasets.\(^7\) However, Card et al. (2016b) note that while observational studies of longitudinal correlations between firm performance and employees’ wages yield small elasticities (0.06 or less), the cross-sectional relationship between labor productivity and firm wages yields a relationship that is much larger. In particular, they find that firms with ten percent higher labor productivity have 1.2 percent higher firm pay premiums, reflected in firm fixed effects from a wage equation as in Abowd et al. (1999). Our results can partially reconcile these different findings. We find that OLS estimates of wage pass-through on the same sample yield estimates that are an order of magnitude smaller than the pass-through identified off exogenous demand shocks. We argue that this finding arises because observed changes in output are poor indicators of underlying product market conditions that determine labor demand, leading to attenuation of estimated pass-through of demand shocks.\(^8\) Thus, although market-level shocks and shocks to unobservable worker characteristics may bias observational estimates of rent sharing, in practice the first order concern for studies of rent-sharing that are not based on a well-defined firm shock may be that results are attenuated due to mismeasurement of labor demand.

The results relate to recent work that ties rising wage inequality to growing disparities across firms (Furman and Orszag, 2015; Card et al., 2013b; Song et al., 2015; Barth et al., 2016; Alvarez et al., 2018). In particular, our findings about the incidence of firm-specific trade shocks are consistent with recent work by Helpman et al. (2017), who show in an estimated structural model that increased dispersion in firm performance due to trade liberalization may directly result in increased wage dispersion within labor market segments. Moreover, our findings suggest that firms face

---

\(^7\)See surveys by Manning (2011) and Card et al. (2016b). These include both studies that study changes in contemporaneous firm-level average wages inclusive of changing composition of workers, and within-spell wage changes for fixed sets of individual workers who stay at firms over time. For example, in related work Budd et al. (2005) study how average wages at firms change in response to both own-firm profits and the profits of foreign parents.

\(^8\)This is similar the result found Abowd and Lemiux (1993) that observational changes in sales do not predict large wage changes, but sales predicted industry-level export shocks do predict large wage effects; Card et al. (2016b) note that simply instrumenting for output changes during a given interval with observed output changes over a slightly longer interval increases the estimated “rent-sharing” elasticity of wages to output from 3% to 6%.
substantial barriers to labor adjustment in response to shocks. Such labor-adjustment frictions may have important consequences for the aggregate performance of labor markets—for example, total job creation or the frequency and duration of unemployment spells—particularly during recessions or periods of high volatility.

The paper is organized as follows: Section 2 presents the conceptual framework that illustrates how labor market imperfections give rise to wage incidence of firm-specific shocks and how the objects we estimate relate to underlying frictions in the labor market. Section 3 provides background information about the economic and institutional context, and it describes the data sources we use. Section 4 presents the empirical strategy of the paper and provides preliminary evidence for its validity. Section 5 presents reduced-form estimates of effects on firms—both the impact on production and revenues as well as the firm-level effects on labor demand and payroll adjustment. Section 6 presents the main results about pass-through effects onto incumbent workers’ wages. In Section 7, we test for heterogeneous pass-through across sectors with different degrees of relationship durability. In Section 8, we discuss how our findings relate to estimates from alternative sources of variation and to previous estimates of rent-sharing. Section 9 concludes.

1.2. Conceptual Framework

This section presents a theoretical framework relating the objects we estimate to underlying labor market features. In our framework, the incidence of firm-specific demand shocks on wages reflects the product of two factors: First, the share of wages due to noncompetitive rents (reflecting the costs of replacing incumbents with external hires); second, the extent to which shocks change the relative productivity advantage of incumbents over outside hires. To illustrate these two factors,
we build a simple model of internal labor markets in which incumbent labor has a productive advantage over new hires from a perfectly competitive external market. In this specific setting, we show what conditions are necessary for firm-specific demand shocks to have incidence on incumbent wages, and we formally derive the pass-through elasticity we estimate. We then discuss how this interpretation extends to a broad class of models of wage determination in imperfectly competitive markets under more general assumptions.

1.2.1. A Stylized Model of Wage Incidence in Internal Labor Markets

1.2.1.1. Set Up

We begin with a stylized setup in which firms employ workers that are costly to replace after their hiring. We consider a set of firms requiring the same skill type, such that all employers and workers considered participate in the same labor market segment and all have access to a common competitive market.\(^{11}\)

**Timing.** The model takes place during a single period. The sequence of events during the period as follows: First, firms inherit demand and productivity attributes, as well as a stock of incumbent employees, and then realize a shock to their demand level. Second, firms negotiate wages with incumbent workers. Third, firms decide on the number of external hires they will employ. Finally, production occurs and wages are paid at the end of the period.

**Firms.** Firms \( j \) are small relative to the labor market and begin the period with a mass \( L^{inc} \) of incumbent workers. They produce revenue \( R_j \) using using \( L^{inc} \) incumbent workers and a mass \( L^{out} \) of hires from the outside market:

\[
R_j = P_j \times \bar{P} \times A_j \times f(L^{inc}, L^{out})
\]  

(1.1)

In this set-up, revenue is the product of \( f(L^{inc}, L^{out}) \) (assumed to be increasing and concave in its arguments) the total factor productivity (TFP) \( A_j \) of the firm, the market-level price index \( \bar{P} \), and a firm-specific price shifter \( P_j \). In this set-up, firm-specific demand shocks and TFP shocks enter the

\(^{11}\)For example, all firms may be in a single industry that uses an industry-specific skill. The important simplification is that all firms utilize the same skill type; it may be the case that subsets of firms operate in different product markets.
firms’ problem symmetrically through a common revenue productivity term \( P_j \times A_j \), as in Foster et al. (2008), so proportional shocks to either have identical effects. For the following exposition, we consider behavior when \( A_j \) and \( \bar{P} \) are fixed and suppress these variables.

A key feature of (1.1) is that incumbent labor and outside hires are not assumed to be perfect substitutes. As in Doeringer and Piore (1971), firms may face barriers to replacing internal incumbent workers with equivalent external hires, and workers may likewise face barriers finding other equally-suitable jobs. In particular, firms may have to forgo valuable production time in order to substitute new hires for incumbents if doing so requires time recruiting and training replacement workers with specialized skills, negotiating with unions with strike power, or engaged in legal proceedings triggered by labor laws. As a result, external hires are not perfect substitutes for incumbent workers, who have a strictly higher net marginal revenue product (inclusive of replacement and retraining costs): \( f_1(L^{inc}, L^{out}) > f_2(L^{inc}, L^{out}) \forall L^{inc}, L^{out} \). In Appendix A, I illustrate how this production function can arise from a standard production function using homogenous labor with firing and hiring costs.

**Labor Markets and Wage Determination.** Firms hire \( L^{out} \) homogenous new workers from a competitive external labor market, with going wage \( \bar{w} \). The competitive going wage \( \bar{w} \) is determined in equilibrium by the total labor supply (assumed to be fixed) and the total derived demand for hires, which reflects the prevailing market-level product demand level \( \bar{P} \) (thus \( \bar{w} \equiv \bar{w} (\bar{P}) \)). Incumbent workers at firms always have the option of entering the external labor market and earning \( \bar{w} \); the outside wage \( \bar{w} \) is taken as parametric by both individual firms and workers, who are small relative to the market.

By contrast, the stock of incumbents \( L^{inc} \) is exogenously determined. We make two important

---

12Following Doeringer and Piore (1971), theoretical work has highlighted three primary types of worker replacement costs that create barriers between internal and external labor markets: 1) costs of rapidly replacing firm-specific know-how acquired through experience Becker (1962); 2) costs of having to find replacements for good incumbent matches Jovanovic (1979b); or 3) direct institutional barriers to firing due to laws or unions (Lazear, 1990; Bertola and Cabellero, 1994).

13Studying employer responses to unexpected deaths of incumbent employees, Jaeger (2015) provides evidence that internal and external workers are not perfect substitutes.

14In this one-period model, we assume hires can be costlessly replaced during the recruiting process, so there is no wage incidence on hires. However, this is not an important assumption for the results that follow. In more general models, hires may become become costly to replace immediately upon contact, as in models with costly search.
assumptions about incumbent workers: First, $L^{inc}$ is sufficiently low relative to $P_j \times \bar{P} \times A_j$ that firms always want to make positive hires $L^{out} > 0$ even when they retain all incumbents $L^{inc}$.  
A direct result of this assumption is that for some firm-specific wage $w_j > \bar{w}$, the firm is strictly better off by retaining all employees than by replacing incumbent workers with external hires. Second, we assume that the set of non-retiring incumbents bargain over their wages (but not the level of firm hiring) collectively as a unit; thus, all incumbents are either retained at a settlement wage $w_j$ or leave as a bloc if the bargaining process falls apart.

We assume wages are determined as the generalized Nash bargaining solution, where the incumbents and the firm have have bargaining weights $\gamma$ and $(1-\gamma)$ respectively. Specifically, the settlement wage for incumbents at firm $j$, $w_j$ solves

$$w_j = \arg\max_{\omega} (\text{Firm Surplus}(\omega))^{(1-\gamma)} \times (\text{Combined Incumbent Surplus}(\omega))^{\gamma}$$

where the respective surpluses given wage $w_j$ relative to the outside options will be derived below.

1.2.1.2. Firm Behavior and Wage Determination

If negotiations with incumbents result in agreement on a settlement wage $w_j$, firm profits (given optimal hiring) are:

$$\Pi_j^1 = \max_{L^{out}} \{ P_j \times f(L^{inc}, L^{out}) - \bar{w}L^{out} \} - w_j L^{inc}$$

However, if negotiations fall apart, firm profits are:

$$\Pi_j^0 = \max_{L^{out}} \{ P_j \times f(0, L^{out}) - \bar{w}L^{out} \}$$

15 In a fully dynamic model, this might reflect an exogenous retirement rate that leads firms to always make positive hires in steady state.

16 This is similar to wage determination in the “Insider-Outsider” model of Lindbeck and Snower (2001). In this simplified variant, we assume workers negotiate only over their own wages, and not the hiring level itself (as modeled by Lindbeck and Snower). Hiring decisions are made unilaterally anticipating the resulting wage bargain with incumbents, similar to “right-to-manage” union models following Dunlop (1950).

17 We have suppressed $\bar{P} \times A_j$, which are assumed for now to be fixed multipliers of $P_j$. 

9
While the choice of $L^{out}$ does not depend on the settlement wage $w_j$, it does depend on whether or not the firm has retained the incumbent workers.\footnote{The choice of $L^{out}$ is the same for any level of $w_j$, since hires never earn incumbent wages in the one-period model, and since incumbents do not directly negotiate hiring levels jointly with the wage.} Hence, one can write firm profits in terms of the net revenues before incumbent wages are paid $V^1(L^{inc}, P_j)$ or $V^0(L^{inc}, P_j)$, which depend on the demand level $P_j$ but not the settlement wage $w_j$, and the payment to incumbents $w_jL^{inc}$ that occurs if an agreement is reached.

The firms’ surplus from retaining the incumbents can be written in terms of the two indirect profit levels combined with the negotiated incumbent wage bill:

$$\text{Firm Surplus}(w_j) = V(L^{inc}, P_j) - V(0, P_j) - w_jL^{inc} \quad (1.5)$$

The incumbents’ surplus is the excess of the wage bill over what they could earn in the outside market

$$\text{Combined Incumbent Surplus}(w_j) = w_jL^{inc} - \bar{w}L^{inc} \quad (1.6)$$

Given the bargaining weights $\gamma$ and $(1 - \gamma)$ for workers and the firm, respectively, the settlement wage that solves (1.2) is thus:

$$w_j = \bar{w}(\bar{P}) + \gamma \times \frac{1}{L^{inc}} \times [V(L^{inc}, P_j) - V(0, P_j) - \bar{w}L^{inc}]$$

$$\begin{align*}
&= \bar{w}(\bar{P}) + \rho(P_j) \\
&\text{Outside Option} + \text{Non-Competitive Quasi-Rent}
\end{align*} \quad (1.7)$$

where $\rho(P_j) = \gamma \times \frac{1}{L^{inc}} \times (V(L^{inc}, P_j) - V(0, P_j) - \bar{w}L^{inc})$ is the part of the wage comprised of a noncompetitive quasi-rent. This latter component is the share each worker captures of the total surplus from maintaining the employment relationship, relative to each parties outside options. Intuitively, the worker earns the competitive wage achievable on the market, plus a premium—a “rent-sharing” term—reflecting excess internal demand for incumbent labor, due to frictions in the labor market.
Proposition 1 summarizes how idiosyncratic shocks impact the quasi-rent sharing term:

**Proposition 1.** Under the assumptions of the model, when \( f_1(L^{inc}, L^{out}) > f_2(L^{inc}, L^{out}) \), the quasi-rent-sharing term \( \rho(P_i) \) always satisfies \( \rho(P_i) > 0 \) and \( \rho'(P_i) > 0 \).

**Proof.** See Appendix.

The first result (positive rents) follows directly from the productive advantage of incumbents over external hires. The second result (rents increase in firm demand) occurs because the revenue productivity advantage of incumbents is augmented by demand \( P_j \) in our model. If incumbents had a revenue productivity advantage that was positive but invariant to demand conditions—for example, if replacing workers required firms to incur a constant currency cost—this second result would not hold. By contrast, our formulation of (1.1) formalizes the intuition that the time spent replacing an incumbent with an outside hire is costlier when demand conditions are better.

As long as the conditions in Proposition 1 hold, firm-specific shocks affect incumbent wages through the rent-sharing term \( \rho(P_i) \).\(^{19} \) By contrast, first term \( \bar{w}(\bar{P}) \) depends only on market-level demand—it can therefore change in response to common demand shocks to \( \bar{P} \) but not idiosyncratic demand shocks to \( P_j \).

For comparison, the perfect competition benchmark is reflected in the case where incumbents and external labor are perfect substitutes, so that \( f(L^{inc}, L^{out}) = f(L^{inc} + L^{out}, 0) = f(0, L^{inc} + L^{out}) \) and therefore \( f_1(L^{inc}, L^{out}) = f_2(L^{inc}, L^{out}) \). When incumbents can perfectly substitute for external hires, the total surplus shrinks to zero, and, thus, the settlement wage per incumbent is \( \bar{w} \). Intuitively, in a perfectly competitive market, firms can always find replacement work at wage \( \bar{w} \)—thus, they never pay more. In this case, when firms experience demand shocks of any kind, payroll adjustment only occurs on the hiring margin; wages only reflect market-level demand (and hence common shocks to \( \bar{P} \)) but are invariant firm-specific labor demand.

**1.2.2. The Pass-Through Elasticity**

In the model above, the effects of idiosyncratic demand shocks on wages, holding \( \bar{P} \) and \( \bar{w} \) are

\(^{19}\)Under more general assumptions, it is not necessary that \( \rho'(P_i) > 0 \) is the case whenever \( \rho > 0 \). If \( \rho'(P_i) = 0 \), then wage will be zero despite the presence of positive rent-sharing.
characterized by the following pass-through elasticity:

\[
e^{\omega_j P_j} \equiv \frac{\partial \ln w_j}{\partial \ln P_j} = \frac{\rho(P_j)}{w_j} \times \frac{d \ln \rho(P_j)}{d \ln P_j}
\]

(1.8)

This elasticity is the product of two terms. The first term is the share of the wage that is comprised of the rents arising due to costs of replacing incumbent workers. Pass-through effects will therefore be larger when noncompetitive rents comprise a larger portion of wages, reflecting larger deviations from the benchmark of perfect competition. The second term is the elasticity with which the per-worker surplus (i.e., the relative demand for incumbents relative to outsiders) changes with respect to a productivity shock. Because of the presence of this term, no pass-through effect exists, even in cases where wages reflect positive rents, if the quantity of rents is invariant to demand shocks. Intuitively, this second term reflects how shocks change the relative productivity advantage of incumbents over outside hires due to imperfect substitutability.

This pass-through elasticity is not specific to the particular model derived above, in which incumbent labor is supplied inelastically and external hires are paid a competitive wage. Rather, a wide class of models of imperfect labor market competition yield wage equations of the form \( w_j = \bar{w} + \rho(P_j) \). First of all, this type of wage equation exists in most bargaining models where workers cannot be costlessly replaced in the short run, regardless of the specific bargaining protocol. These include the single-worker firms in the Diamond-Mortensen-Pissarides model (Pissarides, 2000), sophisticated models of multilateral non-cooperative bargaining between individuals and employers with hiring costs (Stole and Zwiebel, 1996; Acemoglu and Hawkins, 2014), and models of union bargaining (e.g. Brown and Ashenfelter (1986)). In the textbook monopsony model, firms also post wages and face a firm-specific upwards-sloping supply curve due to heterogeneous worker tastes, as in Manning (2011). Similar to our model, the unobserved heterogeneity in match quality that generates the upwards-sloping supply curve implies that incumbents are not perfectly replaceable by outside hires.\(^{20}\) A similar wage equation occurs in efficiency wage

\(^{20}\)Unlike our model, the baseline wage-posting model (where all employees receive the same posted wage) has the feature that increases in wages apply to all workers, including hires, and are directly related to increases in employment levels. However, one can write a similar version of our model where new hires are supplied with infinite elasticity,
models in which incumbent effort has higher returns than external hiring, and firms face incumbent effort curves that are upwards-sloping in wages (Katz, 1986).\footnote{21} In this latter case, increases in unobserved labor inputs would be empirically indistinguishable from increases in the wage for fixed quantities of efforts and would therefore be interpreted as rent-sharing in observed wage data.

In each of these models, the pass-through elasticity is a reflection of the same two properties of the labor market: the level of frictions reflected in the share of wage that comes from rents $\frac{r}{w}$ and the imperfect substitutability of incumbents and external hires reflected in the degree to which the surplus per incumbent changes with the demand level $\frac{d\ln \rho}{d\ln P_j}$. In all cases, the pass-through elasticity tends towards zero as the underlying friction diminishes and the labor market tends towards perfect competition. In addition, the elasticity tends to zero when incumbent labor and marginal external hires are perfect substitutes, even when there is positive rent-sharing ($\rho > 0$).\footnote{22}

The elasticity in (1.8) is highly similar to the “rent-sharing elasticity” defined by Card et al. (2016b), which is simply the second term $\frac{\rho}{w_j}$. The difference is that the latter quantity is the elasticity with respect to the surplus $\rho$, assuming it is known; by contrast, we do not assume the structural surplus is observable, so the expression in (1.8) incorporates the “first-stage” change in surplus. Importantly, positive pass-through therefore requires that the per-worker rent $\rho$ changes due to the shock. Even if $\rho > 0$, if $\rho$ is invariant to shocks, then pass-through elasticities cannot be

\footnote{21}As noted below, $\rho'(P_j) > 0$ only occurs if the marginal product of incumbent effort is higher than effort of new hires. Relatedly, models of non-contractable effort following Holmstrom (1979) can give rise to similar wage equations when it is difficult for firms to determine whether changes in firm performance are due to product demand shocks or employee performance. In the Holmstrom (1979) formulation of the incentive contract, firms will pay workers a base wage (reflecting market competition) plus some amount that varies with firm performance. In the Holmstrom (1979) formulation of the incentive contract, firms will pay workers a base wage (reflecting market competition) plus some amount that varies with firm performance.

\footnote{22}The cases where rents are positive ($\rho(P_j) > 0$) but invariant to shocks ($\frac{d\ln \rho}{d\ln P_j} = 0$) are those where all employees earn rents, but new hires and incumbents are perfectly substitutable. For example, Abowd and Lemieux (1993) show that this can occur in the strongly-efficient union bargains model of Brown and Ashenfelter (1986) where unions maximize total member payroll including workers in the union but outside the firm. Since the union views payroll of incumbents and payroll of union members who are firm outsiders as perfect substitutes, union bargains may accommodate number of workers while keeping the per- worker rents exactly constant (this occurs when the production function is iso-elastic / Cobb-Douglas in labor). A similar result occurs in the classic efficiency wage model of Solow (1979), in which firms maximize total effort $eL$, where $e$ is effort per worker and is increasing in the firm’s wage level. In the model where firms maximize $eL$—so that incumbent effort and effort of hires are perfect substitutes—wages are a constant markup over outside wages that is invariant to productivity shocks. However, if incumbent effort could not immediately be replaced by new hires, this invariance to shocks would no longer hold.
informative about $\rho$.

Importantly, this framework highlights that $e^{\omega, P_j\mid \bar{P}}$ can only be identified from shocks to firms that are exogenous to labor supply factors and idiosyncratic to the firm. While (1.8) yields the interpretation of pass-through of idiosyncratic demand shocks, it does not characterize wage changes due to common shocks to $\bar{P}$. Common shocks have an additional effect on the wage through the outside option wage $\bar{w}$, which workers earn regardless of firm conditions. Elasticities identified off of market-level variation may reflect some arbitrary combination of firm-level and market-level pass-through; one could only disentangle the two effects under strong additional assumptions. In addition, $e^{\omega, P_j\mid P}$ is not identified in the data if increases in output and wages occur due to labor-supply-side shocks (e.g., effort per worker), rather than firm-side factors ($P_j$). The elasticity in (1.8), only includes wage changes due to of firm-side shocks.  

While (1.8) defines the elasticity of wages with respect to a firm-specific shock to the demand level $P_j$ (or, equivalently, TFP $A_j$), $P_j$ is not directly observed in practice. In our analysis, we study changes in observed output, which can be used as a proxy for revenue productivity. We quantify how much firm-specific demand shock yielding a 1 percent change in observed output passes through to wages. Using output as a proxy for $P_j$ will generally produce a biased estimate of the true $e^{\omega, P_j\mid P}$; however, the model above allows us to sign this bias using weak assumptions. Whenever firms weakly increase (or decrease) output in response to a positive (or negative) shock to $P_j$ in excess of what would occur if they kept inputs constant, $\frac{d\ln R_j}{d\ln P_j} \geq 1$, so that percent changes in total output weakly overstate changes in demand.  

For example, in the setup above, firms always weakly increase (or decrease) the target employment level $L^*$ in response to positive (or negative) shocks. Thus, $\frac{d\ln R_j}{d\ln P_j} = 1 + f'(L) \times \frac{dL}{dP_j} \geq 1$. Although we believe benchmarking wage

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23 Increased labor supply by incumbents need not change the firms’ output, however, as firms may adjust its labor stock on other margins to keep the total number of efficiency units constant. This would occur in the perfectly competitive benchmark, for example.

24 Changes in per–worker output may have no similarly well-defined relationship to the underlying change in $P_j$. In many cases, changes in $\ln \frac{R_j}{L_j}$ may significantly overstate changes in underlying revenue productivity—indeed, in standard models $\ln \frac{R_j}{L_j}$ remains constant in response to any change in revenue productivity. For example, this occurs when production is isoelastic (i.e. Cobb-Douglas), so that the marginal product of labor is always in a constant ratio to average output per worker, and firms efficiently adjust employment quantities in response to idiosyncratic shocks to keep the marginal product of labor equal to the (constant) outside wage. For any change in underlying productivity, average product (output per worker) remains constant.
changes to the output change caused by the same shock is useful in its own right, our estimates also can be interpreted as a lower bound for $e^{u_{j|p}}$.  

1.3. Setting and Data

1.3.1. Portuguese Exporters in the Great Recession

Our analysis exploits variation in export demand experienced by Portuguese exporters during the Great Recession of 2008 and 2009. From 2007 to 2009, an unforeseen housing bust in the United States and the resulting financial crisis triggered large recessions around the world, precipitating a sharp import demand drop in many countries in 2009 and 2010 (Eaton et al., 2016). In Portugal, however, there was no major domestic housing or financial crisis that directly affected firms; nonetheless, they were highly exposed to global fluctuations in trade demand. Portugal is a small, open economy: while Portugal’s exports comprised only 11 percent of its GDP in 2007, exports amounted to over 31 percent of output (World Bank, 2016). Figure 1.1 highlights how the recession that occurred in Portugal during this episode was marked by a dramatic decline in total exports that mirrored global trends; indeed, exports did not return to pre-recession levels until 2011. During this initial recession, unemployment grew moderately, at a pace also comparable to other advanced European economies. By contrast, after the economy had entered recovery at the end of 2010, Portugal experienced a sovereign debt crisis during 2011 that resulted in a second, distinct recession episode, which then culminated in 2012 with dramatic increases in unemployment. To avoid concerns about confounds arising from this latter, domestic crisis, our study focuses on demand variation that occurred during the first recession episode through 2010.
Figure 1.1.: Growth and Exports in Portugal Around the Great Recession

Notes: Source: World Bank. Figure plots annual GDP and total exports for Portugal in real Euros, with each variable indexed to its 2000 level. Vertical lines indicate the start of the Great Recession in the US at the end of 2007 and the beginning of the Portuguese sovereign debt crisis in the spring of 2011.

While Portugal provides an ideal setting to identify idiosyncratic demand shocks, key features of labor market institutions should be taken into account when considering the external validity of our results. Portuguese labor markets are substantially more rigid than those in the United States; prior to the Great Recession, Blanchard and Portugal (2001) observed that while steady-state unemployment rates were actually quite similar in the United States and Portugal (in contrast to other Southern European nations), this similarity masked stark differences in dynamism. Entry into unemployment was much lower in Portugal, but durations were much higher. One potential explanation for the low degree of turnover in Portugal is the presence of very strong job protections. Similar to systems in several other European countries (e.g., France), most employees work under permanent contracts that can only be terminated if the firm has legally defensible cause. Popular accounts suggest layoffs are thus extremely rare, and the lack of an option to downsize results in higher hesitation of employers to create new jobs, particularly in the presence of uncertainty about future economic conditions.

In recent decades, firms have been granted limited scope to hire workers under fixed-term contracts, wherein after a set duration employees must either be released or promoted to a permanent
Prior to the Great Recession, fixed-term contracts accounted for about 15 percent of total private-sector employment. While these contracts offer firms some degree of flexibility, they are nonetheless still quite rigid compared to typical work arrangements in the United States because firms may not dismiss workers without cause until the full duration has elapsed. Instead, these contracts offer firms an opportunity to learn about the quality of a match before committing to a permanent contract; accordingly, they fully account for 50 percent of all hires in Portugal. Collective bargaining over wages is widespread in Portugal, with over 90 percent of private sector employees covered by bargained contracts between 2010 and 2012. Collective bargaining institutions are distinctive: although only 11 percent of private sectors are unionized, under Portuguese law, any wage floor negotiated for a specific job title automatically extends to all workers with the same job title. Firms can freely raise wages above these floors but have very limited scope to reduce nominal wages.

1.3.2. Data Sources and Sample Selection

To study how firm-level product market conditions pass through to wages, we use anonymized firm identifiers to link detailed administrative records on exports, balance sheets, and profit-loss statements to matched employer-employee personnel records. The export data is derived from administrative customs records for exports outside the European Union and from mandatory reporting on all intra-EU shipments in excess of a certain threshold. These data, which cover the period from 2005 to 2013, report export and import volumes at the firm by destination country by six digit product (HS-6) level. To relate firm behavior to demand condition in foreign markets, we use annual data on trade flows by six-digit product and country pair publicly available in the BACI database.

We link the export records to profit and loss statements and to balance sheets for the universe of

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25 Fixed term contracts may be renewed up to two times under certain circumstances, but were not indefinitely renewable during the study period.
26 Unionization and contract coverage figures are from Addison et al. (2017). Martins (2014) provides a detailed discussion the institutions that result in extension of bargained contracts—resulting in over 30,000 occupation-specific wage floors—and studies the impacts on the wage distribution.
27 110,000 Euros in 2007.
28 Gauiler and Zignago (2010) overview the underlying source data and discuss how the harmonized trade flow database was constructed.
firms operating in Portugal contained in the IES database, with coverage for the years 2005–2013. This database includes information on the sales, input costs, factor payments, profits, assets, and liabilities of all Portuguese firms on an annual basis. We use items in this dataset to calculate the value added of the firm, defined as the output of the firm net of purchases of intermediate goods in services. While gross sales are a useful measure of output as they are directly and reliably observed, value added is a more direct measure of the level of production in the firm.

We then use the same anonymized identifiers to link firms to the Quadros de Pessoal (QP), a matched employer-employee dataset produced by the Ministry of Employment based on a census of all private-sector employers during October of each year. Employers with at least one paid employee must report each employee’s baseline monthly contract wages, fringe payments, hours worked (regular and overtime), gender, detailed occupation, tenure, age, contract type, and education level of every worker actively employed during the reference month. We define workers as incumbents at firm if they worked full-time at firm in 2007 and if this was their primary source of reported earnings. Using longitudinal employee data enables the study of effects on the incumbent cohort over time, even as workers may move to other firms in subsequent years. In the primary analysis, we study effects on “attached” incumbents, where workers are deemed to be attached to the firm if they are observed working full-time at firm in 2005, 2006, and 2007; however, results for less-attached incumbents are presented as well. Because reported quantities pertain to a single reference month during the year, the primary compensation outcome is the base monthly contract wage, excluding fringe payments and overtime which may be inconsistent over the year, as this is most likely to stay constant throughout. We also study effects on hourly wages, calculated as the base wage divided by the regular hours specified in the employment contract.

Our analysis sample consists of all pre-period exporters subject to two restrictions. First, we re-
strict to firms that both export and employ more than one worker in 2005, 2006, and 2007—these firms are employers likely to export in future years. Second, we limit our primary focus to small- and medium-sized firms—those with average 2005–2007 employment of at least one and no greater than 100 employees—as these firms are most plausibly exposed to idiosyncratic demand shocks based on skewed exposure to specific destinations. Larger firms are observably more diversified across customer markets and are less exposed to idiosyncratic demand risk. Although the 1,000 largest firms excluded from the analysis account for a majority of sales and employment among exporters, the 4,178 firms in the analysis firms are most representative of typical private sector employers in Portugal.31 Appendix Figure A.1 shows that, in tabulations of the 2007 data on all firms, the firms in the sample are actually much larger than most firms in Portugal; however, when tabulating the distribution of workers, the firms in the sample are central in the distribution of firm sizes. The sample includes all sectors that export goods; while the majority of firms are manufacturers, the sample also includes firms in resource-extraction industries, wholesale and retail, and select service industries that produce intellectual property (such as book or software publishing).

Table 1.1.: Summary Statistics: Pre-period Exports of Sample Firms

<table>
<thead>
<tr>
<th></th>
<th>Analysis Sample</th>
<th>Large Exporters</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>P25</td>
</tr>
<tr>
<td>2007 Exports, Euros</td>
<td>1,132,340</td>
<td>41,718</td>
</tr>
<tr>
<td>Pre-Period Export Exposure</td>
<td>0.342</td>
<td>0.027</td>
</tr>
<tr>
<td># Destination Countries</td>
<td>5.21</td>
<td>2</td>
</tr>
<tr>
<td># Major (2D) Products</td>
<td>3.6</td>
<td>1</td>
</tr>
<tr>
<td># Detailed (6D) Products</td>
<td>10.3</td>
<td>2</td>
</tr>
<tr>
<td>Number of Firms</td>
<td>4,173</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Table displays export statistics for firms appearing in the export data in each of 2005, 2006, and 2007, tabulated from the firm-product-destination-year level data. Analysis sample contains all such firms with 100 or fewer employees during pre-period (2005-2007 average). Large exporters are remainder of firms with over 100 employees. Exports are measured in constant 2007 Euros. Exports/Sales is the ratio of total exports to total sales from the balance sheet data (a distinct source) averaged across years 2005-2007. Counts of destination countries and products (HS2 and HS6) pool 2005, 2006, and 2007 exports of each firms, to reflect construction of the exposure weights in (1.9).

31Exporting firms as a group tend to over-represent larger firms.
Table I presents summary statistics of the pre-period export behavior of sample firms. On average, exports reported in the shipment-level data (subject to reporting thresholds) account for 34 percent of sales reported on the firm’s profit/loss statement, though the median is smaller and approximately 20 percent. Firms in this sample are not highly diversified across destinations—the median firm only exports to three countries. Similarly, firms are not highly diversified across products; the median firm only exports two major products and four detailed varieties, whereas, by contrast, the larger export firms are notably more diversified and sell more products to more destinations.

Table 1.2: Comparison of Firms and Workers in Sample and Population

<table>
<thead>
<tr>
<th></th>
<th>Analysis Sample</th>
<th>Full Count Data</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>P10</td>
</tr>
<tr>
<td><strong>Firms</strong></td>
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<td></td>
</tr>
<tr>
<td>Employees</td>
<td>27.9</td>
<td>5</td>
</tr>
<tr>
<td>Sales/Worker if Emp&gt;0, Euros</td>
<td>173,226</td>
<td>33,803</td>
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<tr>
<td>Value Added / Worker if Emp&gt;0, Euros</td>
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<td>11,935</td>
</tr>
<tr>
<td>N Firms, Emp&gt;0</td>
<td>4,173</td>
<td></td>
</tr>
<tr>
<td>N Firms</td>
<td>4,173</td>
<td></td>
</tr>
<tr>
<td><strong>Workers:</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Monthly Wage, Euros</td>
<td>760.80</td>
<td>403</td>
</tr>
<tr>
<td>Hourly Wage, Euros</td>
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</tr>
<tr>
<td>Log Monthly Wage</td>
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<td>6.00</td>
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<tr>
<td>Log Hourly Wage</td>
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<td>Fixed Term, Percent of Sample</td>
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</tr>
<tr>
<td>Tenure, Months (All Workers)</td>
<td>121</td>
<td>9</td>
</tr>
<tr>
<td>Female, Percent of Sample</td>
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<tr>
<td>Regular Hours Per Month</td>
<td>171</td>
<td>162</td>
</tr>
<tr>
<td>N Workers</td>
<td>115,526</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Table compares firms and workers in the analysis sample to the population data for year 2007. All figures in currency units are in 2007 Euros. Data on firm sales and output is from the balance sheet data (IES) with N=278,226. Employment counts are tabulated from the matched employer-employee data (QP) for firms that appear in both the QP and IES, N=278,226. Worker statistics are tabulations of employee records in the cleaned QP, restricting to the highest paying full-time job (more than 120 hours per month) per worker, N=2,490,452. “Monthly Wage” and “Hourly Wage” are the base contract pay earned during regular hours during the reference month in the QP, excluding overtime, fringe payments, and bonuses. “Percent FTC” is the percent of workers at the firm with fixed-term contracts.

Table II presents summary statistics of the workers in the sample in 2007, as compared to the full population of full-time workers in the QP. The distribution of earnings, hours, and worker
demographics in the sample closely matches the distribution workers in the broader private sector. Workers at exporting firms have somewhat longer tenures—the median worker in the sample has been employed at the same firm for approximately three years more than the typical worker in Portugal. In both the sample and the broader population, the standard deviation of log wages is 0.5.\footnote{The statutory minimum monthly full-time wage of 403 Euros (2007) is not binding for most workers in the sample.} By contrast, there is almost no variation in the normal working hours among employees in the data.

1.4. Empirical Strategy for Identifying Idiosyncratic Shocks to Demand

To estimate the pass-through of idiosyncratic demand shocks to the wages of the incumbent cohort of workers present at the firm before the recession, we implement a dynamic difference-in-differences design, in which we compare differential changes in incumbent wages across exporters that experienced different idiosyncratic demand shocks during the Great Recession.\footnote{We study effects on the wages of a fixed cohort to ensure wage effects at the firm are not simply a product of changes in composition of the types of workers employed. We consider effects both unconditional and conditional on workers’ remaining at the initial firm, to account for the possibility of rent losses due to switching jobs.} In this section, we begin by describing how we use detailed export records to construct a firm-level predictor of export demand shocks during the recession. Second, we discuss how we decompose this initial shock into a component that is common to many producers and a component that we argue is idiosyncratic to each firm. Third, we present the estimating equations we use to study reduced-form effects and pass-through to workers. Finally, we summarize the identification assumptions that must hold in order to identify the objects of interest and offer evidence in support of these identifying assumptions.

1.4.1. Predicting Export Demand

We isolate exogenous, idiosyncratic changes in demand for firms’ products by studying quasi-experimental shocks to export demand during the Great Recession. To construct firm-level shocks to demand that do not mechanically reflect changes in firm productivity, we construct a firm-level “shift-share” predictor based on exposure to foreign markets.\footnote{Our shock is may be considered a firm-level Bartik shock, using firm-level exposure weights to country-by-product markets.} We measure the exposure
of exporting firms to different destination markets prior to the Great Recession during the years 2005–2007, then, based on this fixed exposure, we apportion to firms the changes in import demand in each country-by-product market from before the recession (2006–2007) to the trough of the decline in trade (2009–2010) Because we measure for destination-market demand using observed changes in imports from everywhere in the world excluding Portugal, differences in demand are not mechanically reflective of changes in Portuguese export productivity to those markets.\footnote{There may be a second-order effect of productivity shocks to Portuguese exports arising from crowd-out of competitors. In this case, an observed decline in non-Portuguese imports could reflect growing market share of Portuguese firms. To the extent that growth in observed non-Portuguese imports in a market actually predict growth—not declines—in Portuguese exports to that market, this channel is small relative to the first-order demand prediction.}

Formally, we measure the exposure weight of a firm $j$ to the market for six-digit (HS6) product $p$ in country $c$ as their share $s_{j,pc}$ of exports of product $p$ to country $c$ in the total 2005–2007 exports by the firm across all products (in set $P$) and countries (in set $C$):

$$s_{j,pc} = \frac{\text{Exports}_{j,pc}^{2005–2007}}{\sum_{p \in P, c \in C} \text{Exports}_{j,pc}^{2005–2007}}$$ \hfill (1.9)

The denominator is simply the total pre-period exports of firm $j$.

To construct an index of import demand changes in each destination market, we calculate the proportional change in imports of product $p$ by country $c$ from all countries, excluding Portugal between the two years before the global recession (2006 and 2007) and the two trough years of the global decline in trade (2009 and 2010). Since it is possible that some countries stopped importing some products altogether during this period, we approximate the percentage change using the symmetric growth rate (or “arc-elasticity”) concept commonly used in literature on firm dynamics Davis et al. (1996). Denoting the average 2006 and 2007 level of non-Portuguese imports (NPI) of product $p$ to country $c$ as $\text{NPI}^{pre}_{pc}$ and the corresponding average level during 2009 and 2010 as $\text{NPI}^{post}_{pc}$ we calculate the change in imports $\Delta_{pc}$ as

$$\Delta_{pc} = \frac{\text{NPI}^{post}_{pc} - \text{NPI}^{pre}_{pc}}{\frac{1}{2} (\text{NPI}^{post}_{pc} + \text{NPI}^{pre}_{pc})}$$ \hfill (1.10)
The baseline predicted change in export demand for firm $j$, $\Delta_j$, is calculated as the average change in each destination (country by product) market, weighted by the pre-period exposure of firm $j$ to that market:

$$\Delta_j = \sum_{p \in P, c \in C} s_{j,pc} \Delta_{pc}$$

(1.11)

If valid, the shock $\Delta_j$ will predict changes in firm $j$’s exports due strictly to changes in product demand.

1.4.2. Isolating Idiosyncratic Variation in Demand

The baseline shock $\Delta_j$ reflects two types of variation; one kind that is likely to impact entire markets, and another that is plausible idiosyncratic to individual firms. First, $\Delta_j$ reflects changes in demand for product $p$ that occur in all countries and therefore may affect all producers of $p$ in Portugal. Second, $\Delta_j$ also reflects differential demand adjustments across countries $c$ for the same product $p$. Formally, let $s_{p,j} = \sum_{c \in C} s_{pc,j}$ be the total share of pre-period exports by firm $j$ that are of product $p$ (regardless of the destination), and let $\Delta_p$ denote the common decline in demand for product $p$ across all global markets. Then $\Delta_j$ can be decomposed as:

$$\Delta_j = \sum_{p} s_{j,p} \Delta_p + \sum_{p,c} s_{j,pc} (\Delta_{pc} - \Delta_p)$$

(1.12)

So long as firms use the same workers to produce $p$ regardless of the destination, the latter component reflects idiosyncratic demand variation.\(^{36}\)

Accordingly, our approach is to decompose the predictor $\Delta_j$, tabulated in (1.11), into two components: the part reflecting product-level demand changes common to many firms and the residual variation in $\Delta_j$ orthogonal to that common component. To do this, we attempt to directly measure

\(^{36}\)It does not need to be the case that no two firms in the labor market have the same shock, but rather that shocks to firm $j$ do not predict product demand changes at enough other firms in the same labor market to change the wage. This might be violated if firms producing the same product $p$ sort into distinct labor markets corresponding to the location of their customers—if firms exporting product $p$ to different countries $c$ draw from different labor markets corresponding to their destination and product, then there is no idiosyncratic variation in $\Delta_j$. 

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changes in product-level import demand $\Delta_p$ common to all countries based on the bilateral trade flow data.\(^{37}\) We proxy for the common demand shock to product $p$ using the observed change in imports of product $p$ averaged across all countries. In the baseline analysis, we use a simple unweighted average taken across all countries: $\hat{\Delta}_p = \frac{1}{n_c} \sum_c \Delta_{pc}$.\(^{38}\) Given proxies for the product-level demand change common to all countries, we can then construct the predicted change in export demand based solely on the products exported by the firm $(s_{j,p})$ and the common change in demand for those products ($\hat{\Delta}_p$). We denote this initial “product-only” demand predictor for firm $j$ as $C^0_j = \sum_p s_{j,p} \hat{\Delta}_p$.

Next, we isolate the variation in $\Delta_j$ that is orthogonal to the product-only demand predictor $C^0_j$ and higher order terms. If one were to simply subtract $C^0_j$ from $\Delta_j$, as in (1.12), the resulting residual may still be correlated with the product-only predictor in samples where firms do not export to all destinations.\(^{39}\) Hence, to obtain the idiosyncratic component we residualize $\Delta_j$ on a quartic polynomial in $C^0_j$. In doing this, we attempt to remove all variation in $\Delta_j$ that can be predicted based solely on what products $j$ makes and the common demand change for those products.

The result is an empirical decomposition of $\Delta_j$ into two orthogonal components: first, a common component $C_j$ predicted by $C^0_j$ and higher-order terms and, second, the residual idiosyncratic component, which we use as our main shock and label $S_j$. The idiosyncratic $S_j$ only reflects differences in demand across countries for the same product that are uncorrelated with product-level demand changes.

\(^{37}\) An alternative approach would be to match firms with identical product compositions and study changes in demand predicted by cross-country differences alone. Matching firms with identical product portfolios is simply a method for identifying the common variation $\sum_p s_{j,p} \Delta_p$ without measuring each $\Delta_p$ directly. Since matched firms have identical $s_{j,p}$, then $\sum_p s_{j,p} \Delta_p$ would be absorbed by a match-level fixed effect—each firm in the pair acts as a counterfactual for the other. In practice, given the rich number of distinct products and the commonality of multi-product firms in the data, such a matching is not practical. Moreover, in small samples, these within-pair comparisons may noisy, making it difficult to obtain precise causal estimates.

\(^{38}\) We have also conducted the primary analyses under alternative weighting assumptions—for example, letting $\hat{\Delta}_p$ be an average where countries are weighted by their total global share of imports of $p$. The results are similar under alternative specifications.

\(^{39}\) If firms in the sample that export better-shocked products also happen to export to the subset of destinations with better shocks for their respective products, then the two components may be correlated.
Figure 1.2: Distribution of Demand Predictors and Idiosyncratic Component

Notes: Figure displays kernel density plots, based on an Epanechnikov kernel, of baseline demand predictor $\Delta_j$ and the idiosyncratic demand shock $S_j$. Also displayed is the “pre-period” predictor for firm $j$, holding its exposure weights fixed as in the calculation of $\Delta_j$, but using the symmetric growth rate from 2003-2004 to 2006-2007 of imports of product $p$ to country $c$.

Figure 1.2 plots the distribution of both the baseline demand predictor $\Delta_j$ and the idiosyncratic shock component $S_j$. In practice, the distributions of the two variables are very similar, with $S_j$ accounting for 87 percent of the variation in $\Delta_j$ and the common component $C_j$ accounting for only 13 percent. While the mean of the idiosyncratic shock is zero by construction, the zero mean of the baseline demand predictor is not mechanical but rather reflects the true distribution of export demand fluctuations faced by firms. Although the typical predicted demand change is only slightly negative, it is useful to compare these changes to typical pre-period demand changes over the same horizon. Figure 1.2 therefore also plots the distribution of the demand predictors calculated for each firm $j$ holding the exposure weights fixed, but using the import demand change from 2003 and 2004 to 2006 and 2007 at firm $j$’s destinations, reflecting the same time duration as the baseline shock. In comparison to the average 28 percent pre-period growth of demand in firms’ overseas markets, the recession shocks look markedly more grim. Accordingly, it is most reasonable to think of these shocks as being generally negative relative to firms’ expectations, with few truly advantageous shocks.
1.4.3. Estimating Equations

1.4.3.1. Firm-Level Analysis

Our analysis proceeds in two parts: First, we study reduced form effects on firm-level outcomes; second, we investigate pass-through to worker-level outcomes. To begin, we implement an event-study design to consider the dynamic, reduced-form effects of the demand shock on firms. Graphical analysis of the dynamic effects facilitates a direct assessment of the timing of the effects on different outcomes, and, by including pre-period observations, this analysis allows for straightforward inspection of different pre-period trends across different levels of the shock variable. For each outcome $Y_{jt}$ we estimate the following dynamic difference-in-differences equation:

$$Y_{jt} = \alpha_t + \delta_j + \sum_{k \neq 2007} \beta_k S_j \times 1\{t = k\} + \sum_{k \neq 2007} \gamma_k X_{j,pre} \times 1\{t = k\} + \nu_{jt}$$

(1.13)

The event-study design is formalized in the inclusion of the firm fixed effect $\delta_j$, which absorbs any differences in outcome levels in the year 2007. The year-specific coefficients $\beta_t$ identifies the effects of a one-unit higher shock on the difference in the outcomes between 2007 and year $k$; the 2007 effect is excluded, as it is absorbed in the fixed effect. We cluster standard errors at the firm level to account for any potential serial correlation in the outcome variables. We then plot the year-specific $\beta_t$ to implement the aforementioned graphical analysis.

The model includes year fixed effects $\alpha_t$ and a firm fixed effect $\delta_j$. For all outcome years, the shock $S_j$ takes the same fixed value, but as $1\{t = k\}$ is only nonzero during year $k$, $\beta_k$ is only identified off outcome variation in year $k$. We also allow for inclusion of a vector of fixed pre-period controls $X_{j,pre}$, which are allowed to have year-specific effects $\gamma_k$. In the baseline specification, we only control for pre-period export levels to ensure results are not driven by the baseline correlation between the demand shock and the export intensity of the firm, but we consider robustness to additional pre-period controls as well.

To summarize these effects into a single point estimate, we then estimate standard differences-in-differences regressions comparing differential changes of outcome levels from the “pre-period”
(2006-2007) to the years after the incidence of the trade decline (2009–2011).

\[ Y_{jt} = \alpha_t + \delta_j + \beta S_j \times Post_t + \sum_{k \neq 2007} \gamma_k X_j^{pre} \times 1\{t = k\} + \nu_{jt}, \quad t \in \{Pre, Post\} \] (1.14)

In the benchmark specification \( Pre \equiv \{2006, 2007\} \) and \( Post \equiv \{2009, 2010, 2011\} \), so as to capture effects that appear either during the 2009–2010 episode or with a one-year delay. While the treatment effect is pooled across years, we continue to control for year-specific effects of pre-period covariates and yearly fixed effects. In this specification, average pre-period differences (in both pre-period years) across differently shocked firms are absorbed by the firm fixed effect \( d_j \), and \( \beta \) identifies a pooled effect of a one-unit increase of the shock on the difference in the outcome between the pre-period and post-period.

1.4.3.2. Worker-Level Analysis

After characterizing firm-level effects, we then turn to analysis of worker-level effects on the incumbent cohort of employees at affected firms in 2007.\(^{40}\) Before estimating pass-through elasticities, we consider the reduced-form effects on wages of individuals in this cohort. We implement versions of the dynamic difference-in-differences regression in (1.13) and the pooled difference-in-differences regression in (1.14), where the outcome is the average logged wage taken across all individual full-time incumbent workers at affected firm \( j \) in 2007. By studying effects on a fixed cohort rather than contemporaneous employees, we rule out the possibility that wage changes are driven by changes in the composition of workers employed by the affected firm. Moreover, we study outcomes for workers regardless of whether they remain at the incumbent firm, which permits identification of changes in rents that are realized by moving out of the treated firm.\(^{41}\) Though we implement these regressions at the cohort level, we weight observations by the number of cohort members so that the analysis is approximately the same as a worker-level regression with a cohort-level treatment.\(^{42}\) In our primary specification, we restrict our focus to the cohort of

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\(^{40}\)Our worker-level analyses only include worker years where individuals are employed full time (≥ 30 hours per week), both in determination of the incumbent cohort and for measurement of outcomes.

\(^{41}\)We consider both specifications that include worker outcomes at any firm in the post period and specifications where only workers remaining at the 2007 are considered.

\(^{42}\)When workers do not appear in the data in post-period years, they are not included in the cohort-average outcome
attached incumbents, which we define as those individuals who were observed working full-time at the affected firm \( j \) at each of 2005, 2006, and 2007, as these workers are more likely to remain at the firm in the future in the absence of any intervention.

To quantify the magnitude of these effects, we estimate a pass-through elasticity of wages with respect to sales, which measures the causal change in wages per one-percent change in sales that is due solely to the demand shock. Formally, we estimate the elasticity \( e^{w,R} \) of incumbent wages with respect to observed output \( R_j \) by estimating the following difference-in-differences specification, analogous to (1.14), using the demand shock \( S_j \) as an instrument for output \( R_j \), which is the independent variable:

\[
\bar{w}_{\text{incumb}}^{jt} = a_t + d_j + e^{w,R} R_j \times \text{Post}_t + \sum_{k \neq 2007} \gamma_k X_j^{pre} \times 1\{t = k\} + v_{jt}, t \in \{\text{Pre, Post}\} \tag{1.15}
\]

where here \( j \) denotes the cohort employed at the affected firm in 2007. Formally, we instrument for \( R_j \times 1\{t \in \text{Post}\} \) with \( S_j \times 1\{t \in \text{Post}\} \) using a two-stage least squares estimator.\(^{43}\) When the exclusion restriction holds, \( e^{w,R} \) identifies the elasticity by which exogenous, idiosyncratic changes to firm output or productivity pass through to incumbent wages. We use both sales and value added as alternative measures of output \( R_j \); in practice, the results are not sensitive to the choice of output measure. For comparison, we also estimate (1.15) by “naive” OLS on the same data, without using \( S_j \) as a shock.

We also test for differential effects among specific subgroups of employees by implementing (1.15) above, taking the average employee outcome only among workers in the specified subgroup. Whenever we study effects on that subgroup, we weight cohort observations by the number of 2007 incumbent employees in the specified subgroup.

### 1.4.3.3. Heterogenous Pass-Through

In order to test whether pass-through effects are present in all types of firms or only in select
subsamples of theoretical interest, we also estimate variants of (1.13), (1.14), and (1.15) that allow treatment effects to differ for high relationship-durability and low relationship-durability industries. Letting $H_j$ denote an indicator for above-median (“high”) industrial relationship durability and $L_j = 1 - H_j$ denote the corresponding “low” indicator, we interact the treatment with both $H_j$ and $L_j$ and omit the main effect of the treatment, always controlling for the main effect of $H_j$ in interacted specifications. The interacted event-study specification is

$$\bar{w}_{incumb}^{jt} = \alpha_t + \delta_j + \sum_{k\neq 2007} \beta_H^k S_j \times H_j \times 1\{t = k\} + \sum_{k\neq 2007} \beta_L^k S_j \times L_j \times 1\{t = k\} + \sum_{k\neq 2007} \gamma_H^k H_j \times 1\{t = k\} + \sum_{k\neq 2007} \gamma_L^k H_j \times 1\{t = k\} + \nu_{jt} \quad (1.16)$$

The pooled reduced-form difference-in-differences equation is analogous.

When estimating heterogeneity pass-through elasticities, it is important to account for the possibility that the first stage effects of the shock differs across subsamples. Thus it is necessary to separately instrument for $R_j$ in each subsample. When estimating heterogeneity in the pass-through elasticity, we estimate the interacted equation

$$\bar{w}_{incumb}^{jt} = \alpha_t + \delta_j + \epsilon_W^{R,H} R_j \times Post_t \times H_j + \epsilon_W^{R,L} R_j \times Post_t \times H_j + \gamma^H \times Post_j \times H_j + \sum_{k\neq 2007} \gamma_{X_j}^pre \times 1\{t = k\} + \nu_{jt} \quad t \in \{Pre, Post\} \quad (1.17)$$

With two endogenous regressors, it is necessary to separately instrument for the shock effect among high- and low-durability firms. Thus, we instrument for $R_j \times Post_t \times H_j$ using $S_j \times Post_t \times H_j$ and likewise for the interaction with $L_j$.

### 1.4.4. Identification Assumptions

The key identification assumption is that differential changes in the outcome are only due to differential levels of the shock variable, which requires that all unobserved determinants of the outcome (reflected in $\nu_{jt}$) evolve in parallel for any level of the shock $S_j$, conditional on observables. This is the standard difference-in-differences “common trends” assumption. Formally, the shock must be uncorrelated with all unobserved determinants of changes in firm performance or wages since the pre-period: for the dynamic specifications, it must be that $E[S_j \times \Delta \nu_{jt}] = 0 \forall t$, where $\Delta$ denotes
the difference in year $t$ relative to 2007; for the pooled specification, the equivalent condition is that 
$E[S_j \times Post_t \times \epsilon_{jt} | \delta_j] = 0 \ \forall \ t$. To identify valid pass-through elasticities based on an endogenous output measure $R_j$ it must be the case that all variation in $R_j$ correlated with the shock $S_j$ is due only to the same underlying idiosyncratic, exogenous variation in demand that causes the change in wages, conditional on fixed effects and controls. Put differently, the orthogonality assumptions must jointly hold both for output and wage outcomes.

The conceptual framework highlighted two types of violations are of particular concern: endogeneity of shocks to labor supply factors (failures of the exogeneity assumption) and correlation of shocks with market-level demand factors (failures of the idiosyncrasy assumption). The estimates will be biased if the shock is not exogenous to worker inputs— in particular, if firms with better latent trends in firm performance and worker wages systematically sort to destinations with better demand shocks. The conjecture that estimates simply reflect differential trends in latent drivers of wage growth or output growth, which would occur regardless of the demand shock, is partially testable. If results are truly driven by the shock, rather than unobserved trend heterogeneity, then one should not observe differential evolutions of outcomes associated with the demand shock prior to the onset of the recession. We provide graphical evidence that the demand shocks only predict changes in firm behavior after the onset of the recession by plotting the year-specific effects of the demand shock on the primary outcomes obtained from the event-study specification in (1.13).

Even in the absence of differential pre-period trends, exogeneity could still be violated if firms and workers with a higher propensity to fare better during the recession (higher resilience) systematically sorted to destinations markets that were less affected by the recession. One concern is that a large share of Portuguese exports go to neighboring Spain, which experienced a particularly adverse recession in the same years. Since the decision to export to Spain is surely different than decisions to export to other destinations given its immediate proximity to Portugal, worse shocks due to exposure to Spain may be reflective of latent characteristics of firms and workers. Similarly, Angola, Portugal’s largest former colony, is a top trading partner that experienced particular strong growth during 2009— thus, good shocks reflecting exposure to Angola may also be
reflective of different latent characteristics of firms and workers. Thus, in all the primary analyses, we control for year-specific effects of the total pre-period share of exports that go to Spain and to Angola respectively. We also consider robustness of results to controls for year-specific effects of exposure of each of the other of Portugal’s top ten destination countries.

Several additional falsification tests provide additional evidence that firms and workers do not sort across destinations based on latent “resilience” (i.e., a tendency to perform better specifically after the onset of the Great Recession). First, one can test whether it is plausible that better-improving firms strategically sorted across destination markets by testing whether changes in demand during the recession were highly forecastable. Appendix Figure A.2 shows that there is almost zero correlation between change in imports in country-by-product markets during the recession and during the change that occurred in the three years prior; a similar finding occurs if one solely considers changes in demand across country, conditional on product fixed effects.

Second, one can use the detailed relationship-level data on firms with multiple destinations to directly test whether firms that had better across-the-board export performance selected into relationships with customers in foreign markets that had better demand shocks. Following Khwaja and Mian (2008), we estimate relationship-level regressions of firm exports to a foreign markets on the import demand shock in the destination. If firms with multiple export destinations do not sort on latent firm-level drivers of export performance, the estimates from this regression should be invariant to controls for firm fixed effects, which absorb firm-level drivers of export performance. In Appendix B.1, we discuss this test in greater detail and present results consistent with no sorting on unobservables.

Third, although differences in baseline levels of the outcomes and covariates across differently shocked firms do not in themselves violate the identification assumptions, it would be suspect if demand shocks were systematically correlated with many important firm productivity and worker characteristics. Figure 1.3 shows the baseline correlation of the idiosyncratic demand shock $S_j$ with 2007 attributes of the firm. The shock is uncorrelated with all measures of firm size, firm productivity, and employee characteristics; however, there is a significant correlation

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44The results are not sensitive to the inclusion or exclusion of these controls, in practice.
with the firm’s pre-period export level. Exogeneity would be violated if different export levels are correlated with latent drivers of firm performance and wage outcomes, even while total sales, firm productivity, and employee characteristics are not; to ensure such a correlation does not drive the results, we control for year-specific effects of exports (in logs, levels, and as a share of sales) in the primary analysis. Additionally, we show that results are robust to controls for a wide range of pre-period covariates and industry-level fixed effects.

**Figure 1.3.: Correlation of Shock $S_j$ with 2007 Covariates**

![Graph showing correlations]

Notes: Figure displays standardized correlation coefficients between the 2007 level of the y-axis variable and the idiosyncratic shock $S_j$. Coefficients and confidence intervals are obtained from a standardized regression with controls for baseline exposure of firms to each of Angola and Spain. Firms are weighted by pre-period average full-time employees, corresponding to the primary analysis.

The other primary threat to identification of true wage pass-through effects is the possibility that our demand shocks are still correlated with market-level shocks, which shift incumbent workers’ outside options. In this case, the estimated coefficient would reflect incidence of a *market-level* demand shock, without necessarily indicating that firms-specific shocks have incidence on wages. We directly test for market-level effects by testing whether the shock to firm $j$ has effects on the performance or labor demand of other similar firms, which are most likely to both factor into the outside option of $j$’s employees and be affected by firm $j$’s product demand shock. We find that the shocks do not predict outcomes at other similar firms using multiple alternative definitions of...
labor markets based on industry and geography; we discuss these tests in more detail in section 5.3. Another way in which our export shocks could correlated with market-level conditions is if they predicted domestic demand for sales of the firms' products, which would likely affect all other Portuguese producers of the product. We show that the effects of shocks on sales are mostly accounted for by changes in exports, as opposed to domestic sales.45

1.5. Effects of Demand Shocks on Firm Performance and Labor Demand

This section presents the effects of demand shocks on firms' revenues, output, and labor demand. First, we discuss the demand shifter's effects on actual exports, sales, and production at firms and establish an effect on sales that is largely accounted for by changes in exports. Next, we show that there is a reduced-form effect on total labor demand at shocked firms. We find that employment changes only occur on the hiring margin; there is no baseline effect on retention of incumbents. Finally, we test for a relationship between a firm's shock and the output or labor demand of other firms in the same narrow industry and region. We find no such relationship exists, supporting the validity of our idiosyncratic demand shock.

1.5.1. Effects on Output

Table 1.3 presents the baseline effects of the idiosyncratic shock on firms' revenue and production. Firms do not systematically exit in response to the shock; nonetheless 1,278 firms exit the data in at least one of the post-period years. Table 1.3 present results both for the full sample, treated as an imbalanced panel except in specifications studying growth rates that treat missing observations as zeros, and for the balanced panel of firms that never exit the data. Appendix Table A.1 shows that the composition of firms and workers in both samples are highly similar. While effects are very similar in both the balanced and imbalanced panels, the results are more precise in the balanced sample.

45Berman et al. (2015) argue that some spillover to domestic sales should be expected if shocks affect firms' ability to invest in future growth. Since the export data comes from administrative sources that are subject to reporting thresholds, exports effects may be understated relative to sales effects.
Table 1.3.: Effects on Firm Sales and Output

Panel A: Effect of Shock to Export Demand $S_j$

<table>
<thead>
<tr>
<th></th>
<th>Log Exports (1)</th>
<th>Any Exports (2)</th>
<th>Log Total Sales (3)</th>
<th>Log Value Added (4)</th>
<th>Any Sales (5)</th>
<th>Any Employees (6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Shock x Post</td>
<td>0.475</td>
<td>0.555</td>
<td>0.015</td>
<td>0.161</td>
<td>0.234</td>
<td>0.186</td>
</tr>
<tr>
<td></td>
<td>(0.15)**</td>
<td>(0.16)**</td>
<td>(0.036)**</td>
<td>(0.025)**</td>
<td>(0.064)**</td>
<td>(0.054)**</td>
</tr>
<tr>
<td>Mean Change</td>
<td>-0.093</td>
<td>0.002</td>
<td>-0.214</td>
<td>-0.08</td>
<td>-0.175</td>
<td>-0.041</td>
</tr>
<tr>
<td>Full Sample</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Never-Exiters</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
</tbody>
</table>

Panel B: Effect of Shock to Sales Demand $S_j \times \frac{Exports_{pre}}{Sales_{pre}}$

<table>
<thead>
<tr>
<th></th>
<th>Log Total Sales (1)</th>
<th>Log Value Added (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Shock x ExpShare</td>
<td>.491 (0.308)**</td>
<td>0.944 (0.229)**</td>
</tr>
<tr>
<td>Post</td>
<td></td>
<td>0.674 (0.282)**</td>
</tr>
</tbody>
</table>

Notes: Sample is either full analysis sample ($N = 4,173$) or sample of firms that always report positive employment (“never-exiters” $N = 2,926$), as specified. Table displays regression coefficients on the interaction between the idiosyncratic shock $S_j$ and $Post_t$, corresponding to $\beta$ in equation (1.14) in the text. “Pre” years are 2006, 2007 (pre-period) and “Post” years 2009, 2010, 2011 (post-period). Panel A displays effects of the idiosyncratic shock $S_j$, which corresponds to the predicted percentage change in exports. Panel A also presents the average change in the dependent variable from pre-to-post. Panel B displays effects of $S_j$ interacted with the pre-period share of sales in exports, corresponding to the predicted percentage change in sales due to a shock. Specifications in Panel B include controls for the post period direct effect of the export exposure $\frac{Exports_{pre}}{Sales_{pre}}$, as well as higher order terms. Firm-year observations with zeros are treated as missing when the outcome is in logs—therefore, the baseline sample is not a balanced panel, but the never-exiters sample is. Regressions are weighted by the average number of full-time employees in 2005, 2006, 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

The export demand shock $S_j$ is a powerful predictor of firm export performance. A predicted ten percent change in exports is associated with a five percent actual change from the pre-period to the post period.\(^\text{46}\) This change is directly reflected in the total sales of the firm, which change by about

\(^\text{46}\)Many firms remain in the data but cease exporting, thus the “intensive-margin” coefficient of .5 is attenuated by poten-
two percent in response to a ten percent shock. While our shock is a shifter to export demand, rather than total demand for firm sales, the magnitude of the effect on total sales is approximately what one would expect given the sample average pre-period share of sales in exports of 34 percent. One could redefine our independent variable as a direct shock to total sales demand by interacting $S_j$ with each firm’s pre-period share of sales in exports. Although we do not adopt this approach in our baseline analysis because firms’ export intensities are not randomly assigned, it is nonetheless useful to verify that a shock defined as a one percent change in sales actually shifts sales by one percent. Reassuringly, when the shock is directly interacted with the share of pre-period sales in exports the coefficient is approximately one. There is a small but statistically insignificant effect on domestic sales.\footnote{Berman et al \cite{berman2021} argue that due to internal economies of scale, domestic sales should be impacted to some degree by export shocks.} We find that value added changes in approximately equal proportion to sales.
Figure 1.4.: Year-Specific Effects on Exports, Sales, Value Added, and Payroll Scaled by Pre-Period Firm Assets

Notes: Figure shows year-specific effects of the idiosyncratic shock $S_j$ on balance sheet items in common units (Euros scaled by the affected-firm pre-period asset stock in 2007). Sample is balanced panel of firms that are employ at least one full-time worker in all years, N = 2,923. Figure displays year-specific coefficients from regressions of the specified outcome on the interaction between the idiosyncratic demand shock $S_j$ and an indicator for each year, with all interactions estimated jointly as in equation (1.13). Estimates for each outcome are from separate regressions. Figure plots confidence intervals based on standard errors clustered at the firm level to account for potential serial correlation of errors. Outcomes are all in Euro units (including zeros), and all are scaled by the average 2005-2007 level of assets at the firm. “Exports” are tabulated from the export transaction database, all other outcomes are from the IES database. Regressions are weighted by the average number of full-time employees in 2005, 2006, 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level.

Figure 1.4 plots dynamic effects ($\beta_k$) of the shock on exports, sales, and value added for the balanced panel of firms always present in the data. To facilitate direct comparisons of the magnitudes of these effects, we measure in common units (Euros relative to the pre-period asset stock). For all studied quantities, there is no differential trend across shock levels until the onset of the Recession in 2008. After 2008, recorded exports adjust in response to the shock, and sales adjust roughly Euro-for-Euro.$^{48}$ The proportional change in value added constitutes a smaller change in terms of Euros, as intermediates account for a substantial proportion of the value of sales.

$Larger effects occur in later years, though the discrepancy with exports may arise in part due to per-shipment reporting thresholds in the export data that attenuate measured effects as the impacts set in.
1.5.2. Effects on Labor Demand and Employment Adjustment

Next, we study how changes in demand for firms’ output affects firms’ demand for labor—both for incumbent workers and for external hires. Table 1.4 presents estimates of the effects on the total payroll and the quantity of labor employed by firms in response to shocks. We find that payroll has a clear response to the change in sales, though the effect on payroll is only about two-thirds the magnitude of the effect on total value added. In response to the change in effective revenue productivity that occurs due to the demand shock, firms adjust the quantity of labor employed, though the change in log employment is smaller than the change in log payroll. There is no major change in the average hours worked per employee, inclusive of overtime, however.
Table 1.4: Effects on Labor Adjustment

<table>
<thead>
<tr>
<th></th>
<th>Log Payroll</th>
<th>Log Employees</th>
<th>Log Hours Per Worker</th>
<th>Total</th>
<th>Post-2007 Hires</th>
<th>Incumbent Retention</th>
<th>All Workers</th>
<th>Post-2007 Hires</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>All Firms:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Shock x Post Effect:</td>
<td>0.111</td>
<td>0.076</td>
<td>0.011</td>
<td>0.057</td>
<td>0.041</td>
<td>0.016</td>
<td>0.027</td>
<td>-0.036</td>
</tr>
<tr>
<td></td>
<td>(0.044)**</td>
<td>(0.041)*</td>
<td>(0.007)*</td>
<td>(0.041)</td>
<td>(0.025)*</td>
<td>(0.027)</td>
<td>(0.011)**</td>
<td>(0.048)</td>
</tr>
<tr>
<td>Mean Change</td>
<td>0.020</td>
<td>-0.077</td>
<td>0.000</td>
<td>-0.139</td>
<td>0.138</td>
<td>-0.276</td>
<td>0.105</td>
<td>0.455</td>
</tr>
<tr>
<td><strong>Never-Exiters:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Shock x Post Effect:</td>
<td>0.126</td>
<td>0.095</td>
<td>0.005</td>
<td>0.084</td>
<td>0.069</td>
<td>0.014</td>
<td>0.024</td>
<td>-0.069</td>
</tr>
<tr>
<td></td>
<td>(0.043)**</td>
<td>(0.038)**</td>
<td>(0.006)</td>
<td>(0.032)**</td>
<td>(0.030)**</td>
<td>(0.017)</td>
<td>(0.013)**</td>
<td>(0.053)</td>
</tr>
<tr>
<td>Mean Change</td>
<td>0.070</td>
<td>-0.029</td>
<td>0.000</td>
<td>0.010</td>
<td>0.177</td>
<td>-0.170</td>
<td>0.103</td>
<td>0.453</td>
</tr>
</tbody>
</table>

Notes: Sample is either full analysis sample (N = 4,173) or sample of firms that always report positive employment (“never exiters” N = 2,926), as specified. Table displays regression coefficients on the interaction between the idiosyncratic shock $S_j$ and $Post_t$, corresponding to $b$ in equation (1.14) in the text. “Pre” years are 2006, 2007 (pre-period) and “Post” years 2009, 2010, 2011 (post-period). Table displays effects of the idiosyncratic shock $S_j$, which corresponds to the predicted percentage change in exports. Firm-year observations with zeros are treated as missing when the outcome is in logs—therefore, the baseline sample is not a balanced panel, but the never-exiter sample is. Log Hours Per Worker is average log hours (including overtime) for all workers present at the firm in the observation year. Percent changes in employment hold the denominator fixed as the 2007 employment at the firm; the hires and retention results are scaled by the same denominator. "Post-2007 Hires” in column 5 is the change in employment over 2007 accounted for by hires since 2007 that remain at the firm at the outcome year. ”Incumbent Retentions” is the percentage of 2007 incumbents present at the firm in the observation year. Average Log Monthly Wage, Current Workers is the average log wage of full-time employees (regardless of contract or tenure) of workers currently at the firm; “Post-2007 Hires” in column 9 excludes workers who were present at the firm in 2007. Table also presents the average change in the dependent variable from pre-to-post. Regressions are weighted by the average number of full-time employees in 2005, 2006, 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

To shed more light on the margins of labor adjustment, we exploit the longitudinal employer-employee data to decompose the percent change in full-time employment (change relative to pre-period full-time employment, inclusive of zeros) into the part of employment growth (or decline) due to increased retention (or separations) of incumbent employees and the part due to increases (or decreases) in the number of additional hires made and retained that contribute to the current employment stock. Formally, we note that changes in employment from 2007 until year $k$ can be mechanically decomposed into the number of hires made since 2007 (who have not already left
the firm\textsuperscript{49} and the number of retentions of 2007 incumbents by year \( t \) (reflecting separation effects including layoffs)”

\[
\Delta \text{Emp}_{j,t,2007} = \sum_{\tau=2007}^{t} \frac{\text{Accumulated Retained Hires}_{j,\tau}}{\text{Hires}_{j,\tau}} - \sum_{\tau=2007}^{t} \frac{\text{Incumbent Retentions}_{j,\tau}}{\text{Seps}_{j,\tau}}
\]

Using this identity, we can directly decompose the reduced form effect of the shock on the percent change in employees into a hiring effect and a layoffs/retention effect:

\[
\frac{\Delta \text{Emp}_{j,t}}{\text{Emp}_{j,t}} = \frac{\text{Hire}_{j,t}}{\text{Emp}_{j,t}} + \frac{\text{Seps}_{j,t}}{\text{Emp}_{j,t}}
\]

\textsuperscript{49}Since new employees typically depart firm with positive probability in their first few years, an increase in accumulated hires will mechanically generate an increase in separations due to any fixed attrition rate. To distinguish between separations mechanically tied to hires, we account for departed hires in the calculation of net accumulated retained hires \( \text{Hires}_{j,\tau} \).
Figure 1.5.: Year-Specific Effects on Employment Adjustment: Hiring Versus Incumbent Retention

![Image of Figure 1.5](image)

Notes: Figure shows year-specific effects of the idiosyncratic shock $S_j$ on employment, and fully decomposes the main effect into two margins of adjustment. Sample is balanced panel of firms that are employ at least one full-time worker in all years, N = 2,923. Figure displays year-specific coefficients from regressions of the specified outcome on the interaction between the idiosyncratic demand shock $S_j$ and an indicator for each year, with all interactions estimated jointly as in equation (1.13). Estimates for each outcome are from separate regressions. Figure plots confidence intervals based on standard errors clustered at the firm level to account for potential serial correlation of errors. Outcomes are all tabulated from the employer-employee matched dataset, units are counts (based on full-time workers, including zeros) scaled by 2005-2007 average full-time employment at the firm. “Retention of 2007 Incumbents” is the percentage of 2007 incumbents present at the firm in the baseline year. “Accumulated hires” is the total number of hires made since 2007, less the number of new hires that have left the firm by the observation years, and is algebraically equal to the effect on employment less the effect on retention (prior to 2007 counts of hires are subtracted rather than added so this identity holds in all years). Regressions are weighted by the average number of full-time employees in 2005, 2006, 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level.

Figure 1.5 plots the dynamic effects of the shock on employment. As with sales, there are no different pre-period trends across firms corresponding to different shock levels; consistent with exogenous assignment, firms adjust employment only after the onset of the recession. Due to the nature of the decomposition, the employment effect in each year is equal to the sum of the separation and hiring effects. We find that the observed adjustment in employment is wholly attributable...
to differences in hiring behavior—there is a tight zero effect on the departure rate of incumbent employees. Even though most shocks are adverse during this period, there is no evidence that the employment adjustment among continuing firms is due to differential incidence of layoffs; this is consistent with anecdotal evidence that firing costs are prohibitively large in Portugal. It is not clear, however, whether the absence of any effect on incumbent labor quantities is in fact due to firing constraints; if such constraints exist, they may not binding, so long as the baseline departure rate of incumbents over the study period of 17 is sufficiently high to enable firms to implement a desired downward adjustment. More generally, the finding that all employment effects occur on the hiring margin is consistent with models where incumbent labor can not be perfectly substituted with external hires.

1.5.3. Effects on Output and Labor Demand at Other Firms

The effects of the demand shifter $S_j$ are consistent with identification of an exogenous to shock to demand. Yet, prior to studying wage pass through it is important to first assess whether it appears to be idiosyncratic. In particular, identification of pass-through of firm shocks separately from labor market shocks requires that the shocks to the firm are not correlated with changes in performance at other firms in the labor market $\bar{\theta}$ that could in turn change market-level labor demand reflected in $\bar{\omega}$.

If the effects of shock $S_j$ are firm-specific—that is, idiosyncratic relative to the labor market—one should not observe changes in performance or labor demand at the firms that are most likely both to be affected by the shock and to hire employees with the same skill set as those at the shocked firm. Accordingly, we test for effects on sets of other firms in the Portuguese labor market, excluding the treated firm, that are likely to satisfy both these conditions. A natural group to look at are other firms in $j$'s narrow industry—if the shock is idiosyncratic to $j$, one should not observe any systematic change in output or payroll at those other firms. For each firm $j$, we calculate the leave-one-out mean$^{50}$, sales, value added, payroll, and employment among other firms in the same industry and study whether the shocks predict similar effects on these other firms.

$^{50}$Outside firms are weighted by pre-period employment to account for their importance in the labor market.
Table 1.5: Test of Effects of Idiosyncratic and Common Components on Other Firms

<table>
<thead>
<tr>
<th>Effect on Mean Outcome Level for Other Firms in Same:</th>
<th>5-Digit Industry and Municipality</th>
<th>5-Digit Industry</th>
<th>Municipality</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Log Sales</td>
<td>0.0393</td>
<td>1.067</td>
<td>-0.079</td>
</tr>
<tr>
<td></td>
<td>(0.0498)</td>
<td>(0.223)**</td>
<td>(0.101)</td>
</tr>
<tr>
<td>Log Value Added</td>
<td>-0.022</td>
<td>1.798</td>
<td>-0.112</td>
</tr>
<tr>
<td></td>
<td>(0.072)</td>
<td>(0.502)**</td>
<td>(0.098)</td>
</tr>
<tr>
<td>Log Payroll</td>
<td>-0.009</td>
<td>0.452</td>
<td>-0.107</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.080)**</td>
<td>(0.081)</td>
</tr>
<tr>
<td>Log Employees</td>
<td>-0.008</td>
<td>0.300</td>
<td>-0.079</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.064)**</td>
<td>(0.032)</td>
</tr>
</tbody>
</table>

Shock Component

<table>
<thead>
<tr>
<th>N Treated Firms</th>
<th>Idiosyncratic</th>
<th>Common</th>
<th>Idiosyncratic</th>
<th>Common</th>
<th>Idiosyncratic</th>
<th>Common</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>3885</td>
<td>3885</td>
<td>4166</td>
<td>4166</td>
<td>4166</td>
<td>4166</td>
</tr>
</tbody>
</table>

Notes: Sample includes all firms in analysis sample with at least one other firm in the same group; the number of sample firms included in each specification is reported in the table. Table displays results from regressions where the treatment is the either the idiosyncratic shock to each sample firm $j, S_j$, or the common component of demand, $C_j$, and the outcome is the leave-one-out (employment-weighted) average outcome level of all other firms (excluding $j$) in the same specified group as $j$. Entries are regression coefficients on the interaction between the idiosyncratic shock $S_j$ and $Post_t$, corresponding to $b$ in equation (1.14) in the text; each estimate is from a separate regression. Regressions are run at treated firm level; regressions are weighted by firm-$j$ employment to match specification in Table 1.3. Specification includes year and treated-firm-group fixed effects; treated-firm controls are omitted, though results are similar if included. Municipalities are 24 subregions in Portugal, 5-digit industry code is the most detailed NACE classification available. Standard errors are clustered at the category-by-year level. ** indicates $p < .05$, * indicates $p < .10$.

Results are presented in Table 1.5. While the idiosyncratic shock $S_j$ has a clear effect on output and labor demand at the shocked firm, there is no evidence that the shock affects output or labor demand at other firms in the same industry or the same municipality. While there is some evidence of an effect of on sales for other firms in the same five-digit industry and the same municipality, there is no significant prediction for value added and no evidence that those firms’ labor demand responds comparably to the the treated firm—that is, there is no effect on payroll or employment. Figure 1.6 plots the dynamic effects on labor demand (shifts in total payroll) at both shocked firms and other firms in the same industry-municipality pair. Although the idiosyncratic shock is not informative about pre-period for shocked firms or other firms, only shocked firms display a payroll response in the main post-period years (2009–2011). In each of these years, the difference
between the effect on shocked firms and other similar firms is statistically significant.

**Figure 1.6.: Payroll Effects at Affected Firms and Other Similar Firms**

**Year-Specific Effect of Idiosyncratic Component \( S_j \) on Log Payroll**

**Year-Specific Effect of Common Component \( C_j \) on Log Payroll**

Notes: Figure shows year-specific effects of the idiosyncratic demand component \( S_j \) and the common demand component \( C_j \) on both the payroll of directly affected firms and on the average payroll of other firms in the same detailed industry and municipality. During the main post-period years (2009-2011), the idiosyncratic component affects labor demand at directly-affected firms, but not other similar firms—however, the common component has similar effects on both the directly-affected firms and on other similar employers. The sample of affected firms is the balanced panel of firms that are employ at least one full-time worker in all years, \( N = 2,923 \). For this same sample, we calculate the employment-weighted average log payroll of all other firms in the same 5-digit industry and the same municipality (one of 24 subregions in Portugal). Figure displays year-specific coefficients from regressions of the specified outcome on the interaction between the idiosyncratic demand shock \( S_j \) and an indicator for each year, with all interactions estimated jointly as in equation (1.13). Estimates for each type of shock and each set of firms are from separate regressions. For comparability, we employ the same specification as in (1.4) when studying both own-firm and other-firm effects. Regressions are weighted by the average number of affected-firm full-time employees in 2005, 2006, 2007. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 affected-firm exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level for own-firm effects, and the 5-digit-industry-by-municipality level for the other-firm effects.
By contrast, the common component $C_i$ of the baseline demand predictor $\Delta_j$ appears to have effects common to many firms in the market, as apparent in Table 1.5. Likewise, Figure 1.6 shows that common shock has similar dynamic effects on the payrolls of both shocked firms and of other similar firms. It should be noted that common effects across similar firms are not mechanically implied by the construction of the shock—“commonality” was inferred based solely on foreign import flows, not based on firm export behavior in Portugal. However, the common component of predicted demand strongly predicts increases in sales, output, payroll, and employment at other firms in the same five-digit industry, whether or not these firms were located in the same region. This finding underscores the importance of isolating the idiosyncratic component of demand variation, as the baseline shock contained variation that was not plausibly specific to single firms, but was instead related to changes in payrolls at firms throughout the labor market.

1.6. Pass-Through Effects to Workers

In this section, we study the incidence of the firm-specific demand shocks on the wages of incumbent workers who worked present full-time at a given firm. First, we present reduced-form evidence on the dynamic effects of the shock on wages. Next, we estimate pass-through elasticities of output changes to wage changes, which measure the percent changes in incumbent worker wages that result from idiosyncratic shocks that change output by a given percentage. We discuss how these can be estimated and identified using an instrumental variables approach, and show that our findings are robust to alternative specifications. The section concludes by studying heterogeneity in pass-through rates across different subgroups of workers.

1.6.1. Reduced-Form Effects on Workers Wages

To provide initial evidence on wage incidence, Figure 1.7 plots the dynamic reduced-form effects of the shocks on the wages of the workers who were incumbent to the firm in 2007. We focus on the subset of attached workers who were employed full-time at the treatment firm in each of 2005, 2006, and 2007; we begin by studying the log monthly base wage of these workers, regardless

51 The large magnitude of the effects is due to the common component of demand also strongly predicting domestic sales of firms, as global changes in product demand can be reflected in Portuguese demand as well.
of where they were employed. As evidenced in the figure, wages respond following demand shocks, with no differential trend apparent prior to the shock. While these effects on the monthly wage could be driven in theory by changes in hours, the effects are in practice similar for monthly and hourly wages, as effects on hours per worker are small (see Table 1.4).

Figure 1.7: Dynamic Effects on Log Contract Wage of Attached Incumbents

Notes: Figure shows year-specific reduced-form effects of the idiosyncratic shock $S_j$ on the wages of individuals who worked full-time at shocked firms in 2005-2007. Sample is all firms in analysis sample with at least one attached incumbent, defined as workers who were present at the treated firm full-time in 2005, 2006, and 2007, N = 4,100 firms with 90,298 attached incumbents total. Figure displays year-specific coefficients from regressions of the specified outcome on the interaction between the idiosyncratic demand shock $S_j$ and an indicator for each year, with all interactions estimated jointly as in equation (1.13). Estimates for each outcome are from separate regressions. Figure plots confidence intervals based on standard errors clustered at the firm level to account for potential serial correlation of errors. Outcome is the average log monthly base wage of full-time workers, taken across all workers in the cohort of individuals who were incumbent to the affected firm in 2007. Cohort averages in “Any Firm” specification include observations of workers at different firms, if they have moved firms since 2007. In “Same Firm” specification, averages are taken over workers remaining at the firm in the observation year. When workers do not appear in the data, they are not included in the observation-year average, therefore, the underlying sample of workers is not balanced. Firms are weighted by the total number of attached incumbents present in 2007, weights are fixed across years. All regressions include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level.

52Since wages are only observed in a single reference month, we exclude overtime and fringe payments that may vary throughout the year. We begin with the monthly salary as this is the object most likely to remain constant across months, though we show robustness to studying hourly wages and total salaries in the following subsection. Workers who exit the data in a given year are treated as missing in that year, though workers whose initial firm exits the data, but who themselves find employment at other firms in subsequent years are included.
To test whether this effect is being driven by workers who switch firms and receive differential rents elsewhere, Figure 1.7 also plots the effect on the monthly contract wage limiting the sample to observations where the worker remains at the incumbent firm. The effects are nearly identical, regardless of this restriction, suggesting that the primary effects occur within-spells and not due to “wage scars” of job switchers. Given the lack of a layoff effect, this is to be expected. In years during which workers are not present in the QP, they are omitted from the analysis rather than treated as zeros—an omission unlikely to bias the results, as there is no observed effect on non-employment and the findings are robust compared to other scalings of the outcome that incorporate zeros.  

These findings suggest that over the two-to-four year horizon, wages are not so inflexible to prohibit any wage adjustment. While downward nominal wage adjustments are rare in Portugal and generally prohibited (Martins, 2014), it does not appear that downward nominal rigidities were binding during this period, as average wage growth was approximately nine percent on average from 2006–2007 to 2009–2011. To test whether particularly negative shocks had muted wage effects due to binding rigidities, Figure A.3 presents a residualized binned scatter plot of wage \( \times \) post again the shock, where the residuals are obtained from the specification (1.14) so that the average slope exactly corresponds to the estimated coefficient. As we residualize both the shock and the outcome on observable controls, this exercise tests for asymmetries in shock responses relative to forecastable differences in shock levels. We include a plot of the quadratic fit to provide graphical evidence of any possible nonlinearities in the causal relationship. The relationship is approximately linear. We have also examined spline regressions where slopes are allowed to differ for firms with positive and negative absolute \( S_j \) values—if anything, we find that wages are more responsive to negative \( S_j \) shocks than to positive shocks. These findings suggest that downward rigidities do not bind for negative values of the shock.

The effect of idiosyncratic demand shocks on incumbent wages that we find cannot be reconciled with fully competitive wage determination. In the framework presented in Section 2, this effect

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53 Even in the absence of an average worker-exit effect, it may still be the case that the change in incumbent wage in part reflects changes in the composition of separations across the wage distribution within the firm. However, we find no evidence of differential non-employment effects across wage ranks.
implies the presence of a market friction or a worker-replacement cost that generates a quasi-rent within the employment relationship. However, the reduced-form findings alone do not provide a clear sense of the magnitude of the effects. The next section presents results using an instrumental variables estimator to gauge how large these effects are relative to overall change in performance of the firm.

1.6.2. Estimates of the Pass-Through Elasticity

In this section, we estimate the magnitude of the incidence of firm-specific demand shocks on workers. Ideally, one would observe the instantaneous demand \( P_j \); in this case, one could estimate the first-stage effect of the demand predictor \( S_j \) on \( P_j \) and then estimate the causal relationship between \( P_j \) and incumbent wages, using \( S_j \) as an instrumental variable for \( P_j \). However, in practice, one can only measure observable behaviors of the firm, such as output. Therefore, we estimate the log change in wages that corresponds to a one-percent change in output or revenues that result from a given firm-specific demand shock. Under the assumption that the elasticity of output with respect to \( P_j \) is greater than one, this pass-through elasticity is a lower bound of the true elasticity of wages with respect to \( P_j \) itself.
## Table 1.6: Pass-Through Elasticity: Effect in Wages of Attached Incumbents for Given Change in Output

<table>
<thead>
<tr>
<th>Pass-Through of Output Measure to:</th>
<th>Log Monthly Wage, Any Job</th>
<th>Log Hourly Wage, Any Job</th>
<th>Log Monthly Wage, if Same Firm</th>
<th>Log Hourly Wage, if Same Firm</th>
<th>Log (1 + Hourly Wage)</th>
</tr>
</thead>
<tbody>
<tr>
<td>IV Elasticity</td>
<td>0.137</td>
<td>0.158</td>
<td>0.128</td>
<td>0.131</td>
<td>0.123</td>
</tr>
<tr>
<td></td>
<td>(0.053)**</td>
<td>(0.068)**</td>
<td>(0.052)**</td>
<td>(0.066)**</td>
<td>(0.055)**</td>
</tr>
<tr>
<td>First stage F</td>
<td>73.79</td>
<td>33.56</td>
<td>73.79</td>
<td>33.56</td>
<td>73.55</td>
</tr>
<tr>
<td></td>
<td>(0.004)**</td>
<td>(0.002)**</td>
<td>(0.003)**</td>
<td>(0.002)**</td>
<td>(0.004)**</td>
</tr>
<tr>
<td>OLS Elasticity</td>
<td>0.027</td>
<td>0.02</td>
<td>0.022</td>
<td>0.021</td>
<td>0.017</td>
</tr>
<tr>
<td></td>
<td>(0.002)**</td>
<td>(0.002)**</td>
<td>(0.003)**</td>
<td>(0.002)**</td>
<td>(0.003)**</td>
</tr>
<tr>
<td>Output Measure:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log(Sales)</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Log(VA)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean Pre-Post Change</td>
<td>0.096</td>
<td>0.110</td>
<td>0.103</td>
<td>0.114</td>
<td>0.086</td>
</tr>
</tbody>
</table>

Notes: Sample is all firms in analysis sample with at least one attached incumbent, defined as workers who were present at the treated firm full-time in 2005, 2006, and 2007, N = 4,100 firms with 90,298 attached incumbents total. Table displays estimates of pass-through elasticities, obtained from difference-in-difference regressions of average incumbent log monthly wages on the specified outcome variable; elasticity is coefficient $e^{\alpha_R}$ on the interaction of the output measure $R_j$ with the post period indicator $Post_t$ in (1.15). Table displays elasticities obtained both from OLS estimation (1.15) and instrumental variables (two stage least squares) estimation using the idiosyncratic shock $S_j$ as an instrument for the output measure $R_j$. Output measure is either log total sales or log value added, as specified; value added is calculated as total factor payments (labor costs plus firm earnings before interest, depreciation, amortizations, and taxes). Outcomes are the average of the specified wage metric taken across all workers in the incumbent cohort; outcome averages in Columns 1-4 and 9-10 include observation-year wages at different firms, if employers have moved; outcomes in columns 5-8 are averages taken over workers remaining at the firm in the observation year. When workers do not appear in the data, they are not included in the observation-year average, therefore, the underlying sample of workers is not balanced in Columns 1-8; Columns 9 and 10 treat missing values as zeros for calculation of log(1+wage). Firms are weighted by the total number of attached incumbents present in pre-period, weights are fixed across years. Baseline pre-to-post changes are displayed, all figures are nominal. All specifications include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

The benchmark estimates of the wage elasticity of attached incumbent workers with respect to output are presented in Table 1.6. We find an elasticity of approximately 0.15—with estimates generally ranging from 0.13 to 0.16 across specifications—which holds regardless of whether the outcome is the monthly base wage, the hourly wage, or total monthly compensation including overtime and fringe payments; it also holds regardless whether we use sales or value added as the measure of output. The elasticity is also similar whether or not one considers all observed

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54 While value added is perhaps the more relevant output measure, the first stage is weaker because value added is a calculated outcome rather than a directly observed behavior like sales.
job years, including at other subsequent jobs, or whether one limits analysis to within-job-spell changes. Since the baseline effects on separations are negligible, alternative measures of wages that include zeros—for example, the logarithm of one plus the wage—yield similar results.

These estimates are an order of magnitude larger than the coefficients estimated by simple OLS incorporating all observed changes in output, rather than those identified by the demand shock. Across all specifications, we find an small but very precise observed elasticity of about 0.02. Despite concerns that OLS estimates would be upward-biased due to simultaneous effects of labor supply shocks on wages and output or due to common shocks, the OLS appears to be significantly downward biased. In Section 8, we relate this finding to a broader literature that has found similar effects, suggesting that the first-order problem with studying observational pass-through elasticities is mismeasurement of demand shocks (or revenue productivity shocks, more generally).
Table 1.7.: Robustness of Pass-Through Estimates

<table>
<thead>
<tr>
<th>IV Elasticity with Respect to:</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
<th>(9)</th>
<th>(10)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Sales</td>
<td>0.137**</td>
<td>0.096**</td>
<td>0.106**</td>
<td>0.143**</td>
<td>0.132</td>
<td>0.146</td>
<td>0.117**</td>
<td>0.041</td>
<td>0.149*</td>
<td>0.141</td>
</tr>
<tr>
<td></td>
<td>(0.053)</td>
<td>(0.044)</td>
<td>(0.046)</td>
<td>(0.062)</td>
<td>(0.081)</td>
<td>(0.09)</td>
<td>(0.049)</td>
<td>(0.158)</td>
<td>(0.082)</td>
<td>(0.088)</td>
</tr>
<tr>
<td>Log Value Added</td>
<td>0.158**</td>
<td>0.117**</td>
<td>0.151**</td>
<td>0.167**</td>
<td>0.159</td>
<td>0.143</td>
<td>0.140**</td>
<td>0.007</td>
<td>0.150*</td>
<td>0.148*</td>
</tr>
<tr>
<td></td>
<td>(0.068)</td>
<td>(0.057)</td>
<td>(0.072)</td>
<td>(0.078)</td>
<td>(0.105)</td>
<td>(0.091)</td>
<td>(0.066)</td>
<td>(0.06)</td>
<td>(0.079)</td>
<td>(0.086)</td>
</tr>
</tbody>
</table>

N firms                      | 4,100 | 4,100 | 4,100 | 4,100 | 3,993 | 2,578 | -     | 5,008 | 4,100 | 5,008 |
N workers                     | 90,298 | 90,298 | 90,298 | 90,298 | 87,846 | 67,504 | -     | 303,095 | 90,298 | 303,095 |

Baseline Controls             | x     | x     | x     | x     | x     | x     | x     | x     | x     | x     |
Pre-Period Attribute Controls | x     | x     | x     | x     | x     | x     | x     | x     | x     | x     |
Destination Controls          | x     | x     | x     | x     | x     | x     | x     | x     | x     | x     |
5 Digit Industry FE           | x     | x     | x     | x     | x     | x     | x     | x     | x     | x     |
Manufacturing Only            | x     | x     | x     | x     | x     | x     | x     | x     | x     | x     |
Always-Employers Only         | x     | x     | x     | x     | x     | x     | x     | x     | x     | x     |
Including Large Firms         | x     | x     | x     | x     | x     | x     | x     | x     | x     | x     |
Firm-Weighted                 | x     | x     | x     | x     | x     | x     | x     | x     | x     | x     |

Notes: Table displays robustness of instrumental variables estimates in Table 1.6 to alternative specifications. Column 1 displays IV estimates from Columns 1 and 2 of Table 1.6, see table notes for details. Baseline controls are year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. “Pre-period attribute controls” include controls for year-specific effects of 2005-2007 average employment, sales, assets, hiring, labor productivity, wage levels, and fixed term contract employment. Destination controls include the share of pre-period exports going to each of 10 top destination countries, as well as predicted demand using 2003-2007 changes in imports at baseline destinations. 5-digit industry FE includes industry-by-year fixed effects for 5-digit industry codes, the most detailed NACE classification available. “Manufacturing only” includes all NACE codes below 40, inclusive of agriculture and mining industries. “Firm-weighted” indicates estimation of unweighted regressions. Standard errors are clustered at the firm level. ** indicates \( p < .05 \), * indicates \( p < .10 \).

Table 1.7 presents results from alternative specifications in order to probe the robustness of results. Column 1 displays our benchmark estimates, and estimates with no controls are in Column 2. Column 3 includes controls for year-specific effects of a rich set of pre-period covariates, and Column 4 includes controls for year-specific effects of exposure to each of the top ten destination countries and for pre-period demand growth at each firm’s destination countries. The sign, magnitude, and statistical significance of the estimates are largely robust to the choice of controls. Column 5 controls for five-digit-industry-by-year fixed effects, so that all effects are identified only off of differential shocks within narrow industries. While this reduces the precision of the estimates, the point estimates remained unchanged. Column 6 limits the sample to manufacturing firms; again, while the estimates identified off the smaller sample are less precise, the point estimates
remain similar. To relate the results to the firm-level effects in Tables 4 and 5, Column 7 restricts
the sample to the balanced panel of firms that never exit. This restriction has very little effect on
the point estimates or their statistical significance. The remaining columns explore robustness to
inclusion of the largest firms under alternative weightings. When the baseline specification is es-
timated using firm-level weights, the magnitudes are similar but somewhat less precise.\footnote{While the first-stage power actually improves under this alternate weighting, the reduced form effects on wages are noisier when smaller firms are given higher weight.} When
firms are employment weighted, no effect is present when large firms are included, as the results
are dominated by noisy zero effects for large firms. However, when these firms are included and
firms are weighted equally, the results remain similar regardless of whether the largest firms are
included. Thus, while the precision of the estimates varies across specifications, the finding of a
pass-through elasticity of 0.15 among the primary sample of small and medium firms exposed to
idiosyncratic trade risk is not sensitive to specification.

1.6.3. Differential Pass-Through to Subgroups of Workers

Previous work has suggested rent-sharing may differ across gender groups Card et al. (2016a)
or across income subgroups Kline et al. (2017). Accordingly, we estimate separate pass-through
estimates on a number of subgroups of incumbent employees. In each case, we estimate equation
(1.15) where the outcome is the average cohort wage for members in the subgroup, and regressions
are weighted by the number of members of the subgroup at the firm in the incumbent cohort. As
all subgroups are not equally represented at all firms, any differences across groups may be due
to differences in firm characteristics, and, if workers sort across heterogeneous firms, then any
worker-type effect heterogeneity may be incidental to the worker’s type. Nonetheless, it is of
interest whether any such heterogeneity is apparent.
### Table 1.8: Pass-Through Elasticity: Subgroups of Workers

<table>
<thead>
<tr>
<th>Subgroup:</th>
<th>Log Monthly Wage, Any Job (1)</th>
<th>Log Monthly Wage, Stayers (2)</th>
<th>N workers (5)</th>
<th>N firms (6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Attached</td>
<td>0.137 (0.053)** 0.158 (0.068)**</td>
<td>0.131 (0.055)** 0.152 (0.070)**</td>
<td>90,417</td>
<td>4,114</td>
</tr>
<tr>
<td>(At firm 2005-2007)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Permanent Contract</td>
<td>0.128** 0.145**</td>
<td>0.140** 0.159**</td>
<td>90,629</td>
<td>4,094</td>
</tr>
<tr>
<td>Fixed Term Contract</td>
<td>0.173 (0.111) 0.166 (0.116)</td>
<td>0.148 (0.118) 0.146 (0.121)</td>
<td>23,300</td>
<td>2,926</td>
</tr>
<tr>
<td>All Workers</td>
<td>0.228** 0.203**</td>
<td>0.138** 0.148*</td>
<td>116,368</td>
<td>4,173</td>
</tr>
<tr>
<td>Very Attached (Tenure &gt; 10 Yrs)</td>
<td>0.146*** 0.204**</td>
<td>0.163*** 0.228**</td>
<td>44,548</td>
<td>3,262</td>
</tr>
<tr>
<td>Permanent Contract and:</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>0.173* 0.174*</td>
<td>0.185* 0.191</td>
<td>98,881</td>
<td>3,787</td>
</tr>
<tr>
<td>Male</td>
<td>0.082 (0.055) 0.095 (0.067)</td>
<td>0.082 (0.058) 0.096 (0.071)</td>
<td>50,748</td>
<td>3,915</td>
</tr>
<tr>
<td>Wage ≤ Firm Median</td>
<td>0.135** 0.140*</td>
<td>0.141** 0.142*</td>
<td>46,688</td>
<td>3,954</td>
</tr>
<tr>
<td>Wage &gt; Firm Median</td>
<td>0.153** 0.188**</td>
<td>0.164** 0.204**</td>
<td>43,734</td>
<td>4,114</td>
</tr>
</tbody>
</table>

Notes: Table display pass-through elasticities corresponding to the instrumental variables estimates in Columns 1 and 2 of Table 1.6 pertaining to alternative types of workers besides “attached incumbents”. East estimate is obtained from a separate equation. Specification is identical to that in Columns 1 and 2 of Table 1.6, except outcome is average log monthly wage taken over all workers in the incumbent cohort who are members of the specified subgroup. Sample in each specification includes all firms in analysis sample with at least one 2007 incumbent full-time worker in the stated group. Outcomes are the average of the specified wage metric taken across all workers in the incumbent cohort who are in the stated group, firms are weighted by the number of 2007 incumbents in this group. Weights are fixed across years. Number of firms and incumbent workers included in each specification are displayed in Columns 5 and 6. All specifications include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level. ** indicates \( p < .05 \), * indicates \( p < .10 \).

In practice, we do not find significant heterogeneity in the magnitude of effects across subgroups of workers. Results are presented in Table 1.8. The pass-through affects onto less-attached workers with less than three years’ tenure and onto workers with fixed-term contracts—regardless of tenure length—are substantially less precise than the effects estimated for workers with longer tenures or permanent contracts. However, the magnitudes of pass-through for these less-attached workers, with their higher separation hazards, are not significantly smaller. Among attached
workers, there are no clear differences in effects across tenure lengths, gender, or pay levels.

1.7. Heterogeneity in Pass-Through: The Role of Relationship-Specific Surplus

The previous section established that idiosyncratic firm shocks have significant incidence on the wages of workers. In the conceptual framework of Section 2, such pass-through effects occur because firms cannot perfectly substitute for incumbent workers with external hires. If this account is accurate, then pass-through effects should only exist to the extents that labor markets are not fully fluid and that replacing workers is costly. This section studies whether pass-through effects differ across firms that are in more or less fluid labor markets.
Table 1.9.: Heterogeneity by Industry Relationship-Durability

<table>
<thead>
<tr>
<th>Pass-Through to Wage if:</th>
<th>By Separation Rate in Industry</th>
<th>By Typical Tenure in Industry</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Any Job</td>
<td>Same Job</td>
</tr>
<tr>
<td>(1) High Relationship Durability</td>
<td>0.323**</td>
<td>0.278**</td>
</tr>
<tr>
<td>(2) Low Relationship Durability</td>
<td>0.031 (0.143)</td>
<td>0.038 (0.128)</td>
</tr>
<tr>
<td>Coeffs Equal, p-value</td>
<td>0.0438**</td>
<td>0.102</td>
</tr>
<tr>
<td>Output Measure:</td>
<td>Log(Sales)</td>
<td>x</td>
</tr>
<tr>
<td>Log(VA)</td>
<td>x</td>
<td>x</td>
</tr>
</tbody>
</table>

Notes: Sample is all firms in analysis sample with at least one attached incumbent, defined as workers who were present at the treated firm full-time in 2005, 2006, and 2007, N = 4,100 firms with 90,298 attached incumbents total. Table displays estimates of subsample-specific pass-through elasticities, obtained from the interacted difference-in-difference regression specification in (1.17). The sample is split into high and low employment relationship durability groups based on whether a firm’s industry’s average of one of two measures is above or below the median for the sample. The two measures are either the out-of-sample sample five-digit industry average of median tenure of permanent contract workers in 2003-2007, or the out-of-sample average annual separation rate of permanent contract workers in 2003-2007, or the relevant measure is specified in the table. Interacted coefficients are estimated jointly, where interactions of the endogenous independent variable ($R_j$) with the heterogeneity indicator are instrumented by interactions of the same indicator with $S_j$. All interacted specifications include controls for the categorical indicators times $Post_t$. Output measure is either log total sales or log value added, as specified; value added is calculated as total factor payments (labor costs plus firm earnings before interest, depreciation, amortizations, and taxes). Firms are weighted by the total number of attached incumbents present in pre-period, weights are fixed across years. All specifications include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years. Standard errors are clustered at the firm level. ** indicates $p < .05$, * indicates $p < .10$.

Although the true replacement costs of incumbents within firms are not directly observable, theory points to useful proxies. In many models featuring relationship-specific surplus, labor markets with greater replacement frictions are market by more durable firm-employee relationships ("relationship durability"): once a worker is deemed a good fit, they are less likely to leave the firm than if labor markets were fully fluid. For example, when firms have sunk costly investment in firm-specific human capital that is less useful on the outside market (Becker, 1962; Jovanovic, 1979a; Lazear, 2009), workers and firms have an ex post incentive to maintain the employment relationship, as both firm and worker lose the benefit of that investment in the outside labor market. Similarly, when ex ante unobservable match quality varies substantially across firms and poten-
tial employees such that both parties undergo costly searches to find a good fit (Jovanovic, 1979b; Mortensen and Pissarides, 1994), both parties also have substantial option value to maintaining a relationship once a good match is made. And when firing is costly for institutional reasons, these constraints on downward adjustment are more likely to bind when the baseline attrition rate is lower. Accordingly, we study heterogeneous pass-through across observable measures of relationship durability.

We measure two proxies of relationship durability: typical tenure lengths and separation rates in the detailed industry level. In particular, we focus on separations and tenures of workers who have been offered permanent contracts, as the offer of a permanent contract indicates the firms’ belief that the match quality is high, and as the high barriers to layoffs suggests most separations are due to voluntary quits for these workers. To avoid incorporating mean-reverting behaviors of sample firms into the index, we calculate the typical tenure (weighted average across firms of median permanent employee tenure in 2003–2007) and the separation rate (average ratio of separations to stock of permanent workers across in 2003–2007) for permanent contract workers among all non-primary-sample firms in the five-digit sectors.\footnote{To calculate the degrees of fluidity most likely to characterize the sample firms, we calculate these averages for all firms with 100 employees or fewer within the industry. These industry-level indicators are highly predictive of the corresponding tenure lengths and separation rates at the sample firms in the same industry.} We then divide the analysis sample into two equally sized subsamples for each separate index that correspond to above-sample-median and below-sample-median levels of a specified industry relationship-durability index.
**Figure 1.8:** Heterogeneity in Reduced-Form Wage Effects  
**Relationship Durability Defined As:**

<table>
<thead>
<tr>
<th>Typical Permanent Worker Tenure in Industry</th>
<th>Permanent Worker Quit Rate in Industry</th>
</tr>
</thead>
<tbody>
<tr>
<td><img src="image" alt="Graph" /></td>
<td><img src="image" alt="Graph" /></td>
</tr>
</tbody>
</table>

Notes: Figure shows heterogeneity in reduced-form effects of the idiosyncratic shock $S_j$ on the wages of individuals who worked full-time at shocked firms in 2005-2007, based on two different measures of industry-level employment relationship durability. Sample is all firms in analysis sample with at least one attached incumbent, defined as workers who were present at the treated firm full-time in 2005, 2006, and 2007, $N = 4,100$ firms with 90,298 attached incumbents total. Outcome is the average log monthly base wage of full-time workers, taken across all workers in the cohort of individuals who were incumbent to the affected firm in 2007, averages include observations of workers at different firms, if they have moved firms since 2007. Figure displays year-specific coefficients from regressions of the cohort-level outcome on the interaction between the idiosyncratic demand shock $S_j$ and an indicator for each year, separately for firms high and low relationship durability industries. All interaction coefficients for both subgroups are estimated jointly as in equation (1.16). The sample is split into high and low employment relationship durability groups based on whether a firm’s industry’s average of one of two measures is above or below the median for the sample. The two measures are either the out-of-sample sample five-digit industry average of median tenure of permanent contract workers in 2003-2007, or the out-of-sample average annual separation rate of permanent contract workers (averaged across years)—the relevant measure is specified in the table heading. All regressions include year-specific controls for the relevant “high durability” indicator. Firms are weighted by the total number of attached incumbents present in pre-period, weights are fixed across years. Standard errors are clustered at the firm level. All specifications include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years.

To provide graphical evidence of the differential dynamic wage adjustments across the sub-sectors, Figure 1.8 plots the coefficients reflecting the year-specific effects of the shock for each subgroup, using both proxies for relationship-durability. The estimates are obtained from an interacted version of the specification plotted in Figure 1.7—for each year, we estimate and plot year-specific coefficients on the shock $S_j$, interacted with either an indicator for high durability or for low durability (no main effect is included). In all interacted specifications, we control for year-specific ef-
fects of the durability indicator itself. Regardless of the choice of durability index, similar results are found: There is no differential relationship between the shock and wage across durability levels prior to the Recession. However, after 2008, there are only significant effects in the sectors with high relationship durability.

While the graphical evidence is informative, a proper test of heterogeneity in pass-through requires one to adjust for the possibility that differential wage adjustments may in part reflect differential adjustments in revenues or output. Table 1.9 presents estimates of the subsample-specific pass-through elasticity using interacted IV specification in equation (1.17). To identify separate elasticities for high-durability and low-durability sectors, we estimate the interacted differences-in-differences specification using interactions of the shock $S_j$ with each subgroup as instruments for the interactions of output levels with each subgroup, again controlling for the year-specific effects of the subgroup indicators themselves. Figure 1.9 plots the main estimates for easier comparison.
Figure 1.9.: Heterogeneity in IV Pass-Through Rates

Notes: Figure shows heterogeneity in IV pass-through elasticities, which measure the change in log wages of individuals who worked full-time at shocked firms in 2005-2007 that corresponds to a one-unit change in log output caused by the idiosyncratic shock $S_j$. Sample is all firms in analysis sample with at least one attached incumbent, defined as workers who were present at the treated firm full-time in 2005, 2006, and 2007, $N = 4,100$ firms with 90,298 attached incumbents total. Figure displays estimates of subsample-specific pass-through elasticities, obtained from the interacted difference-in-difference regression specification in (1.17); estimates are the same as those in Table 1.9. The sample is split into high and low employment relationship durability groups based on whether a firm’s industry’s average of one of two measures is above or below the median for the sample. The two measures are either the out-of-sample sample five-digit industry average of median tenure of permanent contract workers in 2003-2007, or the out-of-sample average annual separation rate of permanent contract workers (averaged across years)—the relevant measure is specified in the table. Interacted coefficients are estimated jointly, where interactions of the endogenous independent variable ($R_j$) with the heterogeneity indicator are instrumented by interactions of the same indicator with $S_j$. All interacted specifications include controls for the categorical indicators times $Post_t$. Output measure is either log total sales or log value added, as specified; value added is calculated as total factor payments (labor costs plus firm earnings before interest, depreciation, amortizations, and taxes). Firms are weighted by the total number of attached incumbents present in pre-period, weights are fixed across years. Standard errors are clustered at the firm level. All specifications include year fixed effects, as well as controls for year-specific effects of 2005-2007 exports, log exports, the export share of sales, and the share of exports going to Spain or Angola in those years.

The results are nonetheless similar, even after accounting for potential difference in the first stage. Pass through in low-durability sectors is never statistically different from zero, and point estimates are generally close to zero. By contrast, the large, precise pass-through effects found in high-durability sectors appear to have been driving the baseline findings in Table 1.6—and, in some specifications, the point estimates are substantially larger than the pooled estimates, exceeding 0.3. While this contrast is present across specifications, only in the first specification in Table 1.9 are the estimates precise enough to rule out equality of the elasticities at a five-percent level.

To the extent that these differences in relationship durability reflect differences in frictions and
fluidity across labor markets, these findings support our framework, in which positive wage incidence of firm-specific shocks arises due to barriers to replacing workers and jobs. Because these differences across industries are observational, rather than due to some exogenous source of variation in fluidity across sectors, one must be cautious when interpreting the treatment effect of heterogeneity as causal. Nonetheless, there is reason to believe these differences in relationship durability have a direct relationship to frictions that give rise to pay differentials. Appendix Figure A.4 relates our measures of relationship durability to firm wage premiums, estimated as AKM firm fixed effects.\footnote{We estimate wage equations of the form $w_{ijt} = a_i + f_j + \beta X_{it} + \delta_t + \epsilon_{ijt}$ as in Card et al. (2016a) and Card et al. (2016b), where the firm fixed effects of interest ($f_j$) are estimated off of workers who move jobs.} We find our measures have a strong cross-sectional correlation with firm pay premiums both at the industry level and at the firm level within industries. Put differently, firms in sectors that appear to have more job or worker replacement frictions pay wages that are higher than can be explained by workers’ attributes alone—this supports the claim that these industries have replacement costs that generate larger surpluses in the employment relationship. Taken together, these pieces of evidence suggest that firm pay differentials do in fact arise due to pass-through of firm shocks to incumbent employees, particularly in labor markets with higher degrees of imperfections.

1.8. Discussion

1.8.1. Interpretation of Effect Magnitudes

We found that idiosyncratic demand shocks that caused a 10 percent change in sales resulted in a roughly 1.5 percent change in the wages of attached incumbent workers—specifically, our main estimates of the pass-through elasticity in Tables 1.6–1.7 ranged between 0.13 and 0.16. Although we estimated this elasticity in a single IV regression, this magnitude roughly corresponds to the reduced-form effect of the demand shock on wages—between 0.03 and 0.04, as in Figure 1.7—divided by the reduced-form effect on output—approximately 0.20, as in Table 1.3. In sectors with high relationship durability, measured by the out-of-sample separation rate of workers with permanent contracts, this elasticity was significantly larger and exceeded 0.30 in some specifications.
To assess whether these estimates are economically large, we benchmark them against baseline levels of wage variability in the data. In the analysis sample of incumbent workers, a worker at the 25th percentile experienced a log nominal hourly wage change of 0.033 between 2007 and 2010, while a worker at the 75th percentile experienced a change of 0.163 (the IQR is thus 0.13). In comparison, the variation in the idiosyncratic demand shock accounts for very little of cross-individual differences in wage growth. For example, the reduced-form estimate of 0.03 implies that moving an individual from a firm with a 25th percentile idiosyncratic export demand shock $S_j$ to a firm with a 75th percentile shock would only increase wages by one percent. Of course, as the variation idiosyncratic shock can only account for a very small portion of overall dispersion in output growth, this small figure is a more direct reflection of the size of the shock than of the size of the effect.

A more useful approach is to consider how much of the observed variation in wage growth can be explained by the total variation in output growth, given our pass-through elasticity. This exercise more directly addresses how much of all changes in wages could plausibly be due to firm-level factors. The answer is striking: the pass-through elasticity for value added (0.158) implies that moving an individual from a firm at the 25th percentile of log value added growth to a firm at the 75th percentile would increase wages by 6.8 percent—over half of the total wage-growth IQR. In fact, limiting the exercise to high-durability sectors, moving an individual from the 25th to the 75th percentile of firm value added growth would increase wages by amount that equal to the full wage growth IQR.

This latter figure implies that, for most workers in the high-attachment sample, dispersion in firm performance on its own could generate as much dispersion in wage growth as is actually observed in practice. Of course, this calculation rests on extreme assumptions that are not realistic: It presumes that all variation in value added growth is due to exogenous shocks to firm revenue productivity. Moreover, it assumes that all shocks pass through with the same elasticity we estimated above. Thus, while we find this exercise useful to interpret the magnitude of our results, we

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58 The median was 0.08 and the mean was .10. Due to extreme values in the tails, the standard deviation in the raw data (0.179) significantly larger than one half of the IQR. The distribution of wage changes is very similar in both the high-durability and low-durability subsamples.
are hesitant to make claims about overall wage dispersion based on extrapolations of our results.

1.8.2. Comparison of Elasticities Using Different Sources of Output Variation

We found that instrumented estimates of the pass-through elasticity are significantly larger than coefficients from OLS regressions of wages on output. A standard concern with observational estimates is the potential that positive relationships between output growth and wage growth reflect changes in worker inputs or changes in market-level demand that shift workers outside options, as opposed to idiosyncratic firm shocks. In that case, the observed coefficients would overstate the causal pass-through of firm-level shocks to workers. Given this concern, our findings are somewhat surprising.

We argue this is because observed changes in output are poor indicators of underlying product market conditions that determine labor demand, leading to attenuation of estimates of demand shocks pass-through. This story is supported by prior observational studies that find the relationship between output and wages nearly doubles in size when changes in sales over a given horizon are instrumented for with changes in sales over a longer horizon to account for pure measurement error. Such findings are consistent with attenuation bias in the OLS, suggests that the independent variable measures the underlying labor demand conditions with substantial error (Card et al., 2016b). Moreover, the problem attenuation due to mismeasurement of revenue productivity shocks is not unique to studies of firm-level pass-through; this problem would similarly attenuate estimates of market-level wage incidence. Indeed, Abowd and Lemieux (1993) found evidence of similar attenuation in their study of industry-level rent-sharing. Similar to our results for firms, they found a trivially small observational relationship between wages and industry value added, but they saw significantly larger wage incidence when industry labor demand was instrumented using the product demand shocks. Although the setting is different, the reason for attenuation may be similar; specifically, observational changes in sales are likely very poor measures of the surplus value in the relationship. This is not to say that changes in sales do not reflect substantive changes in firm conditions; rather, firms may experience many kinds of shocks that do not directly shock demand for incumbent labor. For example, planned firm expansions,

Card et al. (2016b) show that their estimates imply a productivity pass-through elasticity of 22 percent.
increased performance of other factors of production, and foreseen variation in revenues for long-run projects could all create large variations in firm performance that do not discretely increase the demand for incumbent labor at the same time.

We note that, even given significant attenuation bias, OLS estimates may be still further biased due to other confounds. In fact, it may be the case that observational changes in sales similarly attenuate these biases, as observed changes in output measure all components of incumbent revenue productivity with substantial error. Even if OLS estimates are attenuated due to measurement error, pass-through estimates instrumented with market-level demand shocks can still cause upwards bias in estimates of rent-sharing within the firm. Market-level shocks not only affect the internal labor demand for workers; they may also cause wages to rise due to increases of the outside option. One would therefore expect the pass-through elasticities estimated in the common product-level demand to be larger than those estimated off of idiosyncratic shocks.

In practice, we find that firm responses to common shocks $C_j$ and idiosyncratic shocks $S_j$ during the Great Recession were qualitatively different, rather than simply quantitatively different. Appendix Table A.3 shows that for firms that do not exit, the pass-through effects of sales to wages estimated from common shocks are similar to the baseline effects in Table 1.6. However, in contrast to our findings that idiosyncratic shocks have zero effect on firm exit and incumbent job loss, Appendix Figure A.5 shows that common shocks have large effects both on firm exit and on the probability that 2007 incumbent employees exit the data completely.

It is therefore difficult to directly compare the magnitude of the impacts of common shocks to the idiosyncratic shocks, as one cannot observe the underlying demand (or “potential sales”) level for adversely shocked firms that exit the data. Because the common shock, unlike the idiosyncratic, leads firms to exit—perhaps selectively—results vary dramatically depending on how one conditions on firm survival. Moreover, Appendix Figure A.5 shows that, in contrast to our findings based on idiosyncratic shocks, continuing firms are more likely to adjust quantities of incumbents in response to product-level shocks in the long run. When firms adjust quantities of incumbents by making layoffs, wage effects for remaining incumbents may be muted, as per-remaining-incumbent surplus changes less. Further, if firms selectively lay off workers based on unobserved
attributes, observed wage effects on remaining incumbents may be further muted by selection bias. Finally, it is important to note that product-level demand shocks result in persistently larger changes in sales—in contrast the findings of Section 6, downward nominal wage rigidities more plausibly begin to bind, further muting the response. Thus, while layoffs and nominal rigidities were not first-order issues when studying the idiosyncratic shocks in our particular settings, they may be important in other situations and should be accounted for when estimating rent-sharing effects.

1.8.3. Comparison to Previous Literature

Similar to our OLS results, prior studies have typically found small but very robust relationships between observed changes in sales or production and observed changes in wages or earnings in longitudinal employer-employee matched datasets; Manning (2011) and Card et al. (2016b) provide detailed surveys of this work. In Italian data similar to ours, Card et al. (2013a) find a .04 longitudinal elasticity of wages to output after adjusting for changes in outside options of workers. Guiso et al. (2005) argue that the small magnitude of observed relationship between short-run changes in sales and wages may be partially explained if risk-neutral firms insure risk-averse workers against transitory shocks, as in Baily (1974), Azariadis (1975) and Akerlof and Hajime Miyazaki (1980). In the same data, they find that wages appear to be invariant to transitory shocks—as inferred based on the fit of a structural time series process—but sensitive to permanent shocks to firm income, though only with an elasticity of 0.06.60 Similarly, Card et al. (2016b) find that in the population QP data, longitudinal correlations between firm performance and employees’ wages yield small elasticities of (0.06 or less) even over five year horizons.

However, the larger magnitudes of our baseline pass-through estimates—which are a lower bound for the elasticity of incumbent wages with respect to firm productivity—are highly similar to the associations found in cross-sectional comparisons of wage premiums and revenue productivity. In particular, studying the same employer-employee data in Portugal during the period 2005–2009, Card et al. (2016b) find that the coefficient found when AKM firm fixed effects are regressed on value added per worker across firms is approximately 0.13—much larger than their observed

60 A similar result was subsequently found in Portugal by Cardoso and Portela (2009)
longitudinal correlation between firm output changes and wages but remarkably similar to our baseline estimates of approximately 0.15.\textsuperscript{61} The magnitude of our point estimates is large enough to account for a causal relationship between firm pay differentials firm output levels of the magnitude they observe.

It should be noted, however, that firm pay premiums identified through firm movers and those identified through pass-through of shocks to incumbents may reflect distinct types of quasi-rent-sharing. Estimates of firm pay premiums obtained from studies of job-movers reflect pay changes that are realized quickly upon joining a firm—and that may or may not be invariant to firm productivity. By contrast, our framework emphasizes rent-sharing in internal labor markets, where incumbent wages may change relative to outside options due to shocks but no premiums accrue to workers who join the firm after a shock is realized. Relatedly, we note that even if our results can account for a large disparities in wage growth over a medium-run horizon, it is uncertain whether accumulation of such medium-run changes could generate cross-sectional wage dispersion similar to that observed in practice.\textsuperscript{62}

These results also relate to findings of changes in wages and firm profitability in response to firm innovations (Van Reenen, 1996) or to unexpected approval of patent applications (Kline et al., 2017). The magnitude of the pass-through effects found here are comparable on magnitude to the effects found in those studies. While demand shocks and grants of intellectual property both boost profitability of firms, however, they also differ somewhat in nature. The finding by Kline et al. (2017) that grants of valuable patents affect earnings shortly after their award, with the largest effects for employees listed on the patent, leads the authors to suggest that these pass-through effects reflect incentive pay arrangements. For example, research staff may be rewarded for past work—which had been of unclear value \textit{ex ante}—after the fruits of the labor were re-

\textsuperscript{61}We do not use per-worker measures of output to proxy for revenue productivity \textit{changes} in response to well-defined shocks, as the dynamic response of per-worker measures additionally reflect the behavioral response of firms that adjust labor quantities in response to the shock. Importantly, while one can bound the degree of revenue productivity pass-through using changes in total output, there is \textit{no} clear relationship between changes in per-worker output and underlying revenue productivity due to these quantity responses. Indeed, when firms are Cobb-Douglas producers and perfectly flexible price-takers, $Y/L$ remains constant in response to all changes in revenue productivity. Nonetheless, measures are arguably better proxies for productivity differentials in the observational \textit{cross-section} of firms during a given time period, as these measures reflect static differences in outputs given a fixed number of inputs.

\textsuperscript{62}The answer depends both on the persistence and mean-reversion of shocks and on the persistence of wage effects.
revealed to be valuable. Importantly, although the timing of the award may be quasi-random, the work that went into the patent can be directly attributed to individual workers. Even though the non-contractibility of research quality does, in a sense, constitute a market imperfection that generates wage pass-through, it remains unclear whether these effects imply workers would share incidence of firm demand shocks, especially when demand conditions are perceived to be exogenous to effort. Similarly, it is not clear whether their results imply that one should expect a firm-specific subsidy to have incidence on the wages of incumbent workers. The frictions generating wage pass-through in our setting—which we have argued reflect costs to replacing jobs and workers—appear to be different than the contracting frictions that generates rent-sharing in patenting firms.

1.8.4. External Validity

While we find significant incidence of idiosyncratic firm demand shocks in Portugal during the Great Recession, the important questions of how behavior would differ in different institutional settings or in response to different kinds of shocks remain. Our finding that pass-through effects only occur in industries with higher levels of relationship durability raises the possibility that rent-sharing behavior may differ substantially in alternative contexts. Our setting, Portugal, is characterized by very strong labor market protections—while many economies feature similar institutional protections, in other advanced economies such as the United States, employment protections are significantly weaker and employment is “at-will.” It therefore may be the case that even the “high-durability” industries would feature less rent-sharing in alternative regulatory contexts.

In addition, our finding that firms engage in qualitatively different behavior in response to large changes in global demand for entire product groups raises the possibility that real-world firms adjust differently to different types of shocks. In particular, firms may respond to differently during expansionary periods to the same idiosyncratic shocks as those studied here, compared to our results taken from the recession. We focus on the Great Recession for purposes of identification, as we believe import changes were harder to foresee during this period; we nonetheless believe that
it would be useful to understand how unforeseeable demand shocks would affect wages during better times.

1.9. Conclusion

This paper presented evidence that exogenous, firm-specific shocks have significant incidence on employees’ wages. We found that an export demand shock that changes firm output, measured via sales or value added, by 10 percent leads to a 1.5 percent change in the wages of attached incumbent employees. These findings cannot be reconciled with fully competitive wage determination in markets where firms are price-takers. Accordingly, these effects were concentrated in labor markets characterized by more durable employment relationships and lower fluidity, which can indicate larger labor market imperfections, where pass-through effects exceed three percent per 10 percent output change. The pass-through elasticities we estimate with respect to output provide a lower bound for the magnitude of the incidence of the underlying demand shocks on worker’s wages.

An important implication of these findings is that where one works can make a significant difference in how much a worker is paid, regardless of the skills, abilities, and efforts one brings to a job. Thus, this work adds to a growing body of research that has found that a significant amount of wage dispersion can be attributed to cross-firm pay differentials, even conditional on worker attributes or fixed effects. We provide novel evidence that this observed relationship may indeed be causal in nature in economies with frictional labor markets similar to those in Portugal. A related implication is that, in such labor markets, policies that are designed to enhance the performance of individual firms, such as targeted production or investment subsidies, may have incidence the specific workers employed at targeted firms. The potential for incidence on workers should be taken into account in distributional analyses of supply-side policies that may not be redistributional in intent.

This paper sheds new light on how wages are determined in imperfect markets, which is of direct importance for evaluating the distributional incidence of policies and economic shocks, as well as channels by which shocks propagate throughout economies. In addition to the distri-
butional implications, however, the presence of imperfect competition likely has important con-
sequences for the dynamic efficiency of labor markets since anticipated rent-sharing effects may
lead to distortions in hiring and training decisions. While a rich body of theoretical work has char-
acterized how such distortions may occur in principle, an important area for future work will be
to build upon empirical studies of wage-pass through and employment adjustment in frictional
economies—such as this paper—to empirically quantify how large the inefficiencies are in gen-
eral equilibrium. In particular, it would be useful for future work to consider what types of social
welfare functions could justify introduction of labor market protections that achieve distributional
goals at the cost of aggregate efficiency.
2. Putting America to Work, Where? Evidence on the Effectiveness of Infrastructure Construction as a Locally-Targeted Employment Policy

2.1. Introduction

Between 2007 and 2009, the United States economy shed over eight million jobs including nearly two million in construction. Yet there were major differences in how different communities felt this downturn—construction and total private employment grew by 2.3 and 20.4 jobs per thousand residents in Harris County, TX (home of Houston), while they respectively fell by 18.1 and 44.4 jobs per thousand residents in Clark County, NV (home of Las Vegas). Given such substantial variation in the severity of the Great Recession across regions, many argued that government stimulus should be directed to hard-hit areas to facilitate recovery in those areas most in need. However, debates about the merits of spatially-targeted policy raise a more fundamental question: in an open economy, how much scope is there for policy to affect local outcomes using localized spending?

This paper studies a frequently proposed policy intended to boost local construction employment and overall economic health: spending on “shovel-ready” public infrastructure construction projects. Such projects are intuitively appealing as way to boost employment in distressed areas because they can be geographically targeted in a highly visible way. These projects create a need
for construction laborers to go to work in precise locations. Not only might this localized increase labor demand increase local construction employment, broader employment may expand further as those workers spend their earnings in turn. Despite the intuitive appeal of these arguments, however, it is important to evaluate whether the data supports them. To test these hypotheses, I study the the 2009 American Recovery and Reinvestment Act (the “Recovery Act” or ARRA for short), which authorized $27 billion for supplemental “shovel-ready” road-construction projects that could begin construction promptly, with priority given to economically distressed areas. In contrast to standard Federal road expenditure, the Recovery Act required detailed reporting about all stimulus road construction projects nationwide. This provides a unique opportunity to study the local employment effects of infrastructure spending unique dataset.

Using this spatially-detailed infrastructure spending data, I test whether places that received relatively more stimulus infrastructure spending experienced more favorable employment outcomes than those that received relatively less using a variable treatment intensity difference-in-differences design. To the extent that construction employees might have been doing construction work elsewhere in the absence of stimulus spending, it does not suffice to count bodies at project sites–one must determine causal effects of spending relative to the no-spending counterfactual. To this end, I use a rich local-level dataset to consider the plausibility of a selection-on-observables methodology. While one might be concerned that funds were systematically targeted to places with unobservably worse downturns, I surprisingly find little evidence of any targeting based on observable employment trends.

One advantage of studying government highway spending is that there is a clear transmission mechanism by which expenditures should affect local employment. The first-order “direct effect” should be on employment of construction workers involved in the projects themselves, and only to the extent that those workers’ expenditures in turn support additional jobs should there be a “local multiplier effect” on non-construction employment in the same region.\(^1\) For any local multiplier effect to be plausible, it is necessary to first establish a credible direct effect on the construction sector. However, even if many workers are engaged on stimulus projects, there many be no causal

\(^1\)There may also be direct effects on construction firm profits, but these are unlikely to have localized multiplier effects, and, because of data limitations, cannot be directly measured at the local level.
direct effect on local construction employment. This could occur due to crowd-out: Recovery Act spending on highway projects in a locality may result in the redirection of state and local funds to other localities—or even other uses—resulting in a small or even zero net increase in total local construction spending. Second, if firms have mobile employees, then the direct effect could be dispersed across workers based in diffuse localities, attenuating any localized direct effects on construction employment. If a direct effect exists, there may in turn be a “local-multiplier” effect if the increment to local construction employment and income helps supports further jobs in the same locality (Moretti). But if individuals spend their income on goods and services produced with value added in dispersed locations, there may be an “diffuse multiplier” effect that does not register in the same municipality.

I find that highway construction did indeed have a direct effect on construction employment at the county level. In particular, a dollar of additional Recovery Act spending on local construction increased local construction payrolls by thirty cents over 2009-2013, nearly exactly labor’s share of construction revenues nationwide. I find no evidence of differential pre-period trends across differently-treated counties, that labor-market effects were largest in 2010, and that the effects dissipate gradually afterwards. These findings support the identification assumption that all effects are casual results of additional stimulus spending. Moreover, the finding that the magnitude of the direct effect is roughly what one would expect with zero crowd-out suggests that targeted Recovery Act spending during the Recession did not crowd out other local construction. However, I find that commuting matters—local spending only impacts local employment in more isolated locales with smaller populations and smaller fractions of residents that commute to outside counties for work. When I test spillovers to nearby locales that are common commuting origins or destinations, I find some evidence of spillovers, but the estimates are highly imprecise.

When I test for general equilibrium effects on local employment and payroll aggregates, I find effects close to zero with very wide confidence intervals across all specifications. I find suggestive evidence that places with less commuting penetration have larger total employment effects, but the results are too imprecise to conclude that there is a large local multiplier anywhere. In sum, although I do not find evidence of large local multipliers, neither can I rule out large local multi-
pliers. A localized multiplier of zero could simply indicate that the multiplier effects of localized spending are highly spatially diffuse. Yet, a noisy zero may also result from a lack of statistical power: although the Recovery Act was a significant enough intervention to have a sizable impact on the construction sector in counties with low mobility, the local variation in highway spending may have been too small relative to baseline regional volatility to detect a local multiplier.

While a long line of research has attempted to assess the aggregate employment effects of government purchases, this paper stands apart from most earlier work in this vein in two respects: first, it in its direct focus on local employment effects, and second, in its tests for distinct direct and multiplier effects of government spending. A growing strand of research, summarized by (Chodorow-Reich, n.d., (forthcoming)) aims to estimate the macroeconomic “multiplier” effects of fiscal expenditures using cross-regional variation in government expenditures and economic outcomes across regional macroeconomies. Most of these studies exploit some exogenous source of variation in state–level government spending to directly estimate the effect of spending on regional aggregate employment and income, though typically without providing evidence on the transmission mechanism. Like this paper, several of the papers surveyed by (Chodorow-Reich, n.d., (forthcoming)) study cross-state variation in Recovery Act spending, instrumented by pre-Recession policy obligations Chodorow-Reich et al. (2012), highway spending and tax obligations Conley and Dupor (2013); Wilson (2012), and seniority of legislators Feyrer and Sacerdote (2011) among other approaches, finding the cost per job-year landing anywhere between $25,000 and $150,000.2 Most relevant to this paper is work by Leduc and Wilson (2017) that directly evaluate the effects Federal highway spending both during and prior to the Recovery Act, which does test transmission mechanisms of highway spending by exploiting institutional rules governing the allocation of funds. Their primary focus is on potential crowd-out of state government expenditures by federal expenditures—in fact, they find evidence of crowd-in of state spending—however, in

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2 Nakamura and Steinsson (2014) demonstrate that the relationship between regional multipliers and aggregate multipliers is ambitious in a models with immobile agents, price rigidities, and non-tradable goods. In their model, local spending can drive up short-run prices for local non-tradables; depending on the monetary policy regime, aggregate multipliers may either be larger or smaller than local multipliers, in contrast to simple intuition suggesting that aggregate multipliers must be larger than regional multipliers.
the Appendix they test for direct effects on highway construction sector employment at the state-level and find that $1 million creates only two jobs (in 2010 only).

Recently, several papers have studied the employment effects of government spending, including the Recovery Act, at finer levels of geographic detail. Suarez Serrato and Wingender (2014) studies county-level spending multipliers using variation induced by Census revisions that result in unexpected adjustments in Federal spending levels, finding a cost per job of $30,000 per year in the affected county, but no effect in adjacent counties. More closely related to this paper is recent work by Dube et al. (2014) and and Dupor and McRory (2017) that draw from the same underlying data on Recovery Act awards to study county-level macroeconomic multipliers. These papers examine all types of awards by location of recipient (not just highway construction grants) and study effects on total employment both on recipient counties and nearby counties. These studies find an own-county effect of 5-10 job-years per $1 Million dollars, but an additional 20-30 job-years per $1 Million in nearby counties, measured either by commuting flows or geographic proximity, corresponding to a total costs per job-year of $30,0004.

Studying the local employment effects of a national school infrastructure investment program in Germany, Buchheim and Watzinger (2017) find a comparable local employment effect. While these papers find large employment effects, these papers do not distinguish between a “direct” effect of spending on recipients and an implied “multiplier effect.” However, other studies in urban economics literature have attempted to directly estimate the “local employment multiplier” effects of additional jobs (usually motivated by models in which individuals are partially geographically mobile). Both Moretti (2010) and van Dijk (2016) find that an exogenously-added job in a county

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3For many types of Federal awards that support State programs, the recipient location is the headquarters office of the local agency.

4This paper differs from their work in important ways. In general, the site of an “award” is not informative about the location of the economic activity—for example, purchases of new buses will be listed as an award taking place at the a state’s Department of Transportation. A large share of stimulus spending is nominally designated as being “spent” in state government offices, even when these funds are not actual paid to workers are firms nearby. I focus on highway construction because award affected-population ZIP codes have clear economic meaning—they are the physical construction site. Likewise, the time variation in the data based on “award date” does not necessarily correspond to the timing of any induced demand increase; rather, it is distinct from the date of any purchases or performed services. Therefore, I do not rely on this information. Finally, while spending could plausibly have larger impacts in slack markets, they test this hypothesis by stratifying the sample based on post-2009 slack, which may be an outcome of the Recovery Act itself. Because stratification on outcomes may confound causal interpretations, I have only examined interactions of highway spending with slackness in the first year of the recession (that is, the change in the private-sector employment rate from 2006–2008), yet I find no evidence of a differential effect in slack areas to report in this paper.
or metropolitan area results in one additional service sector job in the same locale, though the
effects are somewhat larger when the first job is in a high-skill sector. To be consistent with these
latter studies, the studies of expenditure would need to have very substantial “direct” effects to
be plausible.

What warrants such explicit focus on local labor market effects? First, there are substantive rea-
sons: place-based policies are commonly employed as strategies to benefit local residents and
workers⁵; hence, is important to know whether or not the impacts of location-based policies ef-
fectively target local individuals. To that end, this paper joins a growing literature evaluating the
local incidence of place-based policies in open economies, notably the study of the Empowerment
Zone program by Busso, Gregory, and Kline Busso et al. (2013), the classic study of local taxa-
tion by Feldstein and Wrobel Feldstein and Wrobel (1998), and other research surveyed by Kline
and Moretti Kline and Moretti (2014b). During the Great Recession, there was particular desire
to target policy to specific regions due to the spatially heterogenous nature of the recession—for
instance, Yagan Yagan (2016a) found that where one lived and worked during the onset Great Re-
cession had major implications for one’s longer-run income and employment prospects. Yagan’s
finding reinforces earlier work by Blanchard and Katz Blanchard and Katz (1992) demonstrat-
ing that labor markets are spatially segmented, giving rise to cross-regional variation in under-
employment. In theory, this could create an opportunity for a spatially targeted counter-cyclical
employment policy to improve on UI extensions or Keynesian policies with no spatial aspect. In
models with price rigidities and labor market frictions, spending and UI create larger aggregate
demand externalities when labor markets are slack Farhi and Werning (2012); Michaillat and Saez
(2015); Kekre (2016). This creates a rationale to target stimulus at hard-hit regions—so long as
the policy actually boosts local employment. Recent work by Monte et al. (2016) notes that the
local employment elasticity in response to local expenditures may vary significantly in different
settings, depending crucially on commuting behavior. This paper assess the plausibility of using
targeted infrastructure construction to boost local employment given the mobility of the agents
involved. Such approaches come at a cost, however—while more disaggregated datasets offer

⁵In this regard, all policies enacted by local governments are “place-based”
richer variation, the relationship between regional and aggregate outcomes becomes less clear at finer levels of aggregation. In county-level analyses, one must also consider the implications of labor mobility: if local job gains are due to in-migration, then one region’s gain is another’s loss. Thus, while the local detail of the data employed here facilitates transparent tests of local employment effects, I do not directly estimate an aggregate multiplier. Nonetheless, I can speak directly to a crucial step in the transmission mechanism of government spending onto aggregate outcomes.

The rest of the paper proceeds as follows: Section 2 provides background information about the institutional features of public highway construction funded by ARRA and about the overall performance of the highway-construction sector in the context of the broader construction boom, bust, and recovery. Section 3 describes the data sources and sample construction. Section 4 examines the effects of road construction work on local labor markets and argues for the plausibility of the selection-on-observables assumption. Section 5 considers the zero result and provides evidence that Recovery Act contractors were generally not located in the same local labor market as their projects. Section 6 proposes tests for effects in the locale of the Recovery Act contractors but ultimately argues that the variation in the data does not identify local labor demand shocks. Section 7 concludes.

2.2. The Local Distribution of Recovery Act Highway Spending: Background and Data

2.2.1. Background

After the onset of the Great Recession in the United States in 2008, the United States Congress passed the American Recovery and Reinvestment Act (henceforth the Recovery Act or the “stimulus bill”) in January 2009. The bill authorized $821 billion of emergency supplemental expenditure by the United States federal government as an attempt to stimulate macroeconomic growth and reduce unemployment; by 2011, nearly $500 billion had been spent directly and $184 billion had been expended through tax reductions CBO (2012). Proponents of the law argued that funds

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6This point is made formally in Kline and Moretti Kline and Moretti (2014a)
could boost employment expansion in the construction sector by funding “shovel-ready” infrastructure projects—projects that could begin immediately if public funds were made available. The construction sector had been particularly impacted by the Great Recession and the associated housing bust. Between 2006 and 2009 the annual rate of expenditure in residential construction fell by over $300 billion—roughly one-half of the 2006 level—during which time construction employment had fallen by over a million on net, contributing significantly to the aggregate decline in employment by five million over the same period. Thus, many hoped that a rapid expansion of government infrastructure spending might stem these losses.

In practice, only a small fraction of the Recovery Act authorizations (roughly $100 billion in total) were designated for public infrastructure construction. Of this portion, $43 billion was apportioned for some type of transportation spending and $27 billion was designated for highway construction in particular. While this $27 billion in total highway spending was small relative both to the overall stimulus both and to the aggregate decline in annual construction revenues, this amount was large relative to typical pre-Recession annual highway appropriations, which averaged around $40 billion. It was also large relative to the overall size of the highway construction sector, the total revenues of which were $102 billion in 2007.

As with standard Federal highway expenditures, the federal government did not directly execute or even select the projects that were funded by Recovery Act. The distribution of Recovery Act highway funds across states was determined by the preexisting apportionment formulas. While it was left to states to decide which projects to fund, the stimulus bill provided several directions as to how projects should be selected. Specifically, the bill stated that “priority shall be given to projects that are projected for completion within a 3-year time frame, and are located in economically distressed areas” US GPO (2009). The first directive established an enforceable “shovel-readiness” requirement—all projects had to be obligated (that is, to have had construction contracts signed) within a year, or else the funds would be retracted. This generally ruled out the use of funds for projects that required extensive new planning or design. In practice, stimulus

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7 The term “shovel-ready” was first used to describe the proposed stimulus measure by President-Elect Barack Obama during a televised interview on December 8, 2008.
9 See for detailed discussion of these formulas.
funds were spent fairly quickly: by May of 2011, about 60 percent of Recovery Act transportation infrastructure funds had been spent, and 95 percent had been obligated for specific projects GAO (2011). As a result, the vast majority of Recovery Act highway spending went to pavement improvement, road-widening, and resurfacing projects that could be completed in a short time frame. Appendix Figure 1 shows that over two-thirds of spending fell into this category. The second directive was to prioritize spending in “economically distressed areas”. However, the law provided no criteria by which one might determine whether economically distressed areas had been sufficiently prioritized—no quotas were set, and there was no prohibition of projects in non-distressed areas. More importantly, states had relatively free reign in designating cities, towns, and counties as “economically distressed,” and thus they applied the term liberally. Although states had to report all selected projects to the Federal Highway Administration (FHWA) for approval, no projects were rejected in practice.

While publicly-funded roadbuilding may evoke memories of millions of Works Progress Administration (WPA) workers building roads in the 1930s, procurement for Recovery Act projects followed a strikingly different model than that of the WPA. As with all modern federal highway spending, governments do not directly engage in construction, nor do they directly employ construction workers. Rather, after states authorities chose project sites, they are required to select vendors through standard competitive bidding processes. Firms were chosen strictly on competitive cost-bid bases, not by discretion. The Recovery Act did not give federal, state, or locals authorities any special authority to interfere with the procurement process.

10 State agencies were generous in designating regions as “distressed,” and therefore particularly worthy of funds. For example, on a website providing states with implementation guidance, the FHWA posted a map of West Virginia illustrating the designation of “economically distressed areas” (EDAs) (see Appendix Figure 2) in the state. Likewise, the Commonwealth of Massachusetts lists towns designated as EDAs on its website—a list which comprises a majority of towns in the state, including Boston and Cambridge. In this light, the language urging prioritization of projects in EDAs has little bite.

11 Title 23 U.S.C. 112 requires that all Federal-aid highway funds be awarded to firms based on competitive bidding, and the Recovery Act maintained this requirement. Politicians and bureaucrats may not legally interfere with procurement processes to favor certain firms, and there is no particular reason to suspect systematic corruption at this point in history.
Modern private road construction firms are relatively high-tech, relying heavily on expensive, specialized machinery. In 2012, compensation of employees represented only 28 percent of the cost of construction\textsuperscript{12}. Modern Highway construction is a relatively skill-intensive, high-wage sector; the average annual earnings for all employees in 2012 was $56,276. Given these figures, with no crowd-out and inelastic demand, the mechanical employment effect a “marginal” million dollars of construction spending should be 5.5 job-years\textsuperscript{13}. This is in sharp contrast to the WPA, which was an explicit workforce program that directly hired unskilled workers during the Great Depression. While states and local governments were responsible for selecting and financing construction projects, the WPA provided them with free labor from workers on its payroll. WPA projects typically centered around labor-intensive work like clearing simple dirt roads and covering them with gravel using shovels. As a result compensation of workers accounted for a full 69 percent of spending on WPA projects US GPO (1946).

\textsuperscript{12} This figure is calculated as the total cost of payroll for all employees (including white collar workers) plus the value of fringe benefits, divided by the total value of receipts net of subcontracts let out. Importantly, netting for sub-contracts amounts to considering the labor share inclusive of all subcontracts, under the assumption that subcontractors are also in the same sub-sector. Figures are taken from the 2012 Census of Construction.

\textsuperscript{13} An effect of 5.5 job-years per $1 million spent implies the $27 billion spent during ARRA would have supported 150,000 construction job-years. If 5.5 is the correct figure, one should note that even in this no-crowdout case, each additional construction would need to in turn to support at least five additional jobs to yield an aggregate regional multiplier as large as the ones estimated in Suarez-Serrato and Wingender Suarez Serrato and Wingender (2014).
Figures 2.1 and 2.2 display how total highway construction spending and employment evolved in the US in the context of the broader construction boom and bust. Figure 2.1 shows that highway spending rose dramatically during the housing boom, and that it actually continued to grow through 2008—before the Recovery Act was enacted—despite the dramatic decline in residential construction. However, the years following Recovery Act did witness any net increase in the total level of highway construction expenditure. In the 2007 Census of Construction, 64 percent of all highway construction was on infrastructure owned by governments (federal, state, or local); by 2012, that number had risen to 78 percent. Figure 2.2 plots the evolution of employment in the same three categories during the boom, bust, and recovery. The boom-and-bust cycle is particularly apparent in the construction sector, where employment grew by 15 percent (about one million jobs) through 2006 and then declined dramatically—by two million jobs—come 2011, at which point the sector began to recover. Highway construction employment also grew during the boom period, albeit less rapidly. Interestingly, although highway spending did not decline in 2009 or 2010, highway employment declined significantly on net despite the Recovery Act, shedding 80,000 jobs through 2013. It should be observed that the decline in highway employment was both less rapid and more persistent than the decline in the broader construction sector, potentially due to cushioning effects of the Recovery Act.
2.2.2. Data Sources

To study the local distribution of stimulus funds, I use project-level data obtained from the website of the Recovery Act Accountability and Transparency Board\textsuperscript{14}. Although the federal government does not generally collect or report data about the projects state and local governments finance with federal funds—however, to ensure transparency, the Recovery Act explicitly required public disclosure of all awards made under the law. Awardees were required to disclose the ZIP codes where and the purposes for which the funds were used, so that local representatives could account for the amounts and uses of funds spent in each district and jurisdiction. From this repository, I have assembled a comprehensive dataset of each highway construction project funded by the Recovery Act, including information on the amount spent, the precise geographic location of the project, the data of completion and the nature of the project\textsuperscript{15}.

In addition, local governments were requested to report information about the vendors contracted to work on each project. For most states, the reported data on payments to vendors accounts for the majority of funds awarded. I am therefore able to determine which firms won bids to be the general contractor in most instances in the data\textsuperscript{16}. In these cases, I can determine how much was paid to each contracting establishment, as well as the name, industry, and address of the establishment\textsuperscript{17}. An important limitation is the lack of information on subcontractors, who may perform a significant part of the work. This unique dataset allows not only for analysis of project impacts at fine geographic levels, it also permits one to observe where contracting establishments are located relative to the projects at a national level. To my knowledge, this is the first such dataset

\textsuperscript{14}While the website, www.recovery.gov, is no longer active, the full award-level dataset is available from the author upon request.
\textsuperscript{15}A “project” is a grant sub-award. In the case of highway construction, a sub-award is functionally equivalent to a “project”—that is, improvements to a specified piece of infrastructure at a specified site. The data lists two ZIP codes for each sub-award, one corresponding to the location of the “recipient” authority overseeing the project and one corresponding to the “population” affected by the project. While for many types of purchases these ZIPs are the same and correspond to the local agency overseeing the use of the grant, in the case of highway construction, the “population” ZIP is intended to reflect the location of the construction work. I therefore use the “population” ZIP to designate project locations.
\textsuperscript{16}The locations correspond to the regional office of the firm in charge of supervising the project. According to conversations with industry professionals, these offices are the same locations where the construction workers involved in individual projects would have been reported as employed.
\textsuperscript{17}The data list recipient ZIPs for each vendor that are distinct from the award recipient and correspond to the location of the office of the vendor supervising construction.
with national coverage.

**Table 2.1.: Summary Statistics**

<table>
<thead>
<tr>
<th>Counties (N= 2,922)</th>
<th>Sample Total Sum</th>
<th>Mean</th>
<th>Median</th>
<th>Mean if &gt; 0</th>
<th>Median if &gt; 0</th>
<th>Standard Dev.</th>
<th>95th Percentile</th>
<th>Nonzero Obs</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
<td>(8)</td>
</tr>
<tr>
<td>All Projects</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Project Expenditure ($Millions)</td>
<td>23,210</td>
<td>7.94</td>
<td>1.96</td>
<td>9.52</td>
<td>2.99</td>
<td>23.30</td>
<td>32.90</td>
<td>2,437</td>
</tr>
<tr>
<td>Project $ Per Capita</td>
<td>83.82</td>
<td>180.97</td>
<td>53.95</td>
<td>216.99</td>
<td>73.43</td>
<td>602.52</td>
<td>603.42</td>
<td>2,437</td>
</tr>
<tr>
<td>Projects With Vendor Information</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Project Expenditure ($Millions)</td>
<td>16,780</td>
<td>5.70</td>
<td>1.17</td>
<td>7.72</td>
<td>2.53</td>
<td>14.90</td>
<td>32.60</td>
<td>2,174</td>
</tr>
<tr>
<td>Project $ Per Capita</td>
<td>60.60</td>
<td>147.26</td>
<td>35.17</td>
<td>197.92</td>
<td>65.42</td>
<td>560.03</td>
<td>528.55</td>
<td>2,174</td>
</tr>
</tbody>
</table>

Notes: Table summarizes county-level Recovery Act highway spending exposure for 2,922 counties in primary analysis sample.

Table 2.1 describes the data on stimulus road construction projects and county-level aggregates. Comprehensive data on projects are available for 46 of the continental states\(^{18}\), accounting for $23 billion in expenditures. However, only $15.4 billion of these funds can be attributed to vendors. I drop all projects with no information on vendors from my primary analysis—the locations in these observations appear to be problematic, and are frequently in state capitol complexes. However, I examine robustness of my results to the inclusion of these projects in the Appendix. This limits my sample to $16.8 billion of spending of projects. In order to study the impacts of these projects on local employment, I aggregate spending measures at the county level, which is the finest geographic unit with detailed industry-level data. However, because individuals frequently commute across county lines, I also examine effects using alternative definitions of local labor markets, including Metropolitan Statistical Areas (MSAs) and Commuting Zones (CZs), which are aggregated groups counties that represent distinct and self-contained labor markets, based on commuting patterns (Tolbert and Sizer 1996).\(^{19}\)

The data report addresses of the primary vendors for each project, which allows for direct inspection of where these firms’ offices were reported to have been located. In the analysis sample fully 78 percent of vendors report their offices offices in different counties than where the project

\(^{18}\)Michigan is excluded from these data, as most projects were erroneously reported as located in the State Capitol complex, and Illinois lacks any data on vendors.

\(^{19}\)These are similar to Metropolitan Statistical Areas, but unlike MSAs CZs cover all counties.
sites are\textsuperscript{20}. The county-level correlation coefficient of total per-capita spending on Recovery Act projects and total per-capita vendor receipts for Recovery Act projects is only 0.039. Even larger CZs may not be sufficiently self-contained—in fact, 55 percent of vendors have offices in different CZ than the project sites in my data. Even at this broader level, the coefficient of correlation between per capita spending on projects in a county and per capita payments to firms for Recovery Act roadbuilding work to firms sited in the same county is only 0.079. The mean distance between project sites and vendor offices in the analysis sample is 79 miles—roughly the same distance as Philadelphia is from New York City. While these distances seem large, they are actually consistent with standard bidding behavior for large projects. Construction offices routinely bid for projects at considerable difference, particularly when the demand for construction work is lower\textsuperscript{21}. The state may be the lowest level of geography at which vendor offices and projects are co-located—90 percent of vendors are in the same state as the project sites in my data. However, it is important to note that the vendor’s administrative offices may not be the establishments at which the on-site construction workers are reported as employed; this consideration is even more import to the extent that prime vendors subcontract out tasks to third-party employers.

The primary outcomes are private-sector employment levels within a locality, broken out by industry. I obtain annual county-by-sector employment totals from the Quarterly Census of Employment and Wages (QCEW), released by the Bureau of Labor Statistics. The QCEW data is compiled from administrative establishment-level records collected by state unemployment insurance systems. The resulting dataset includes the annual average employment levels and average salary

\textsuperscript{20}This figure does not change significantly if it is calculated on a percent-of-spending instead of a percent-of-projects basis. The reported vendor addresses in these data reflect the location of the office supervising the project—the same office that should be reporting the employees in the administrative datasets that underly this data. While one might suspect that firms are giving addresses for a national headquarters office, this does not appear to be the case in the data—vendors who appear more than once in the data often report from different ZIP codes. Conversations with industry professionals indicate that it is standard for large multi-establishment firms to have a headquarters and “regional” offices that supervise bids, work, and employment on projects in their area. These regional offices appear to be what appear in my data.

\textsuperscript{21}It is not uncommon for construction workers to drive 90 minutes to a project site on a daily basis. An illustrative example can be found at a road construction site near Harvard Square. A worker at the site in Cambridge informed the author that his employer (the vendor on the project) was about fifty miles north in New Hampshire—and therefore in a separate Commuting Zone. Interestingly, this worker lived fifty miles to the south in the Providence Commuting Zone. In this case, the worker would appear as employed in New Hampshire in employer reported data, though the worker actually lived in a CZ different than both the employer and the project. This case illustrates the difficulty in attempting to define a “self-contained” local labor market where workers, employers, and worksites would be co-located.
levels, broken out by county and industry. While data is available for detailed industries (NAICS codes) within counties, data at fine levels of disaggregation with small numbers of establishments are frequently suppressed for confidentiality reasons. Thus, I focus on total construction sector figures, which are nearly always available at the county level, in my primary analysis. I also study effects on finer sub-industries, though results are likely attenuated due to censoring.

Importantly, the employment concept I employ is based on place of work, not place of residence. To the extent that individuals commute beyond counties, the level of employment at local establishments may differ substantially from the level of employment along local residents. Both concepts are meaningful measures of local economic health. However, there is only scant data available on employment by place-of-residence, primarily collected through household surveys that are too small to use for local-level analyses.\(^\text{22}\)

Additionally, I supplement these data with additional control variables and outcome variables from different sources. County-level unemployment and labor-force participation levels and rates are available in the BLS Local Area Unemployment Series; however, these data are largely imputed from sparse Current Population Survey micro-data and should be interpreted with caution. Additional demographic variables are taken from the 2000 Decennial Census long form surveys and the combined 2005–2009 American Community Surveys (Minnesota Population Center Center (2011)). Data on new permits issued for the construction of housing units are obtained from the Department of Housing and Urban Development. County-level income data are obtained from IRS Statistics of Income Data. Data on road mileage and quality are assembled from road-segment data in the Highway Performance Management System compiled by the DOT. Throughout, I scale variables by 2010 population from the full-county Decennial Census, which I refer to as “per capita” units.

\(^{22}\)The Local Area Unemployment Series is an exception, where household survey data are used to estimate local unemployment and labor force participation levels by county of residence. My use of these data is discussed below.
2.3. Methodology

Research Design

To evaluate the local labor market impact of government construction spending, I implement a difference-in-differences design allowing for heterogeneity in treatment intensity. The basic principle of this methodology is to test how employment outcomes in places with higher levels of per-capita stimulus construction spending evolved differently than in places with lower levels of per-capita spending. This approach is valid so long as unobserved determinants of employment evolutions are uncorrelated with spending levels—that is, so long as differently-“treated” places would have changed identically in the absence of stimulus spending.

I begin with a partial equilibrium approach, testing first for effects in the construction sector. Any plausible effect on the broader labor market should stem from the project’s direct effect on construction work, so it is important to verify that local spending does indeed have an effect on local construction payroll. As noted above, if the construction sector were large, competitive, and Cobb-Douglas with labor share of 0.28 (the observed labor share in the highway construction sector), then in a closed economy, an marginal dollar of construction should boost local construction payroll by 28 cents; likewise, a “marginal” million dollars in construction should increase employment by 5.5 jobs. That is, \( Y_{ct} \) denote the county-level value of road construction at time \( t \), \( W_{ct} = w_{ct} \times L_{ct} \) be the county-level construction wage bill (the product of the local wage and employment level), and \( a \) being the labor share of construction employment, it should be be case that \( W_{ct} = aY_{ct} \) where \( a = .28 \). The first task is to verify whether such an effect is obtained in practice. I then test for local general equilibrium effects on aggregate employment and income.

To motivate the difference-in-differences specification for the partial-equilibrium analysis, consider a simple two-period scenario in which total non-stimulus road construction spending in county \( c \), \( G_{ct} \), is supplemented with a additional stimulus amount \( S_{c,post} \) in the second (or “post”) period. While the total spending in country \( c \) in the post period is \( S_{c,post} + G_{c,post} \), employers in outside jurisdictions may bid to work on these projects, and firms in \( c \) may be eligible to bid on outside projects. Suppose that \( v_{ct} \) represents other latent drives of local construction payroll, such
as unobserved productivity attributes. Then, letting $\rho^{\text{out}}$ be the average share of local construction work done by employees of firms in other jurisdictions, and $\rho^{\text{in}}$ be the share of all spending outside of $c$ that is done by construction employees at firms based in $c$, we can obtain a regression equation as follows:

$$\Delta W_c = (1 - \rho^{\text{out}})\alpha (S_{c,\text{post}} + \Delta G_c) + \rho^{\text{in}} (S_{-c,\text{post}} + \Delta G_{-c}) + \Delta \nu_c$$

(2.1)

Or, in terms of observable variables:

$$\Delta W_c = \beta S_{c,\text{post}} + \Delta \epsilon_c$$

(2.2)

where the $\Delta$ operator indicates post-pre differences. Here, $\beta \equiv (1 - \rho^{\text{out}})\alpha$ is the effect on local construction payroll accounting for outside competition. The error term $\Delta \epsilon_{c,t} \equiv (1 - \rho^{\text{out}})\alpha (\Delta G_{c,t}) + \rho^{\text{in}} (S_{-c,\text{post}} + \Delta G_{-c,t}) + \Delta \nu_{c,t}$ reflects the unobserved determinants of local construction payroll. If supplemental stimulus spending was randomly assigned across space, independently of baseline spending and other components of the error term so that $E[S_{c,\text{post}}\Delta \epsilon_{c,\text{post}}] = 0$, then this $\beta$ would be identified by the difference-in-differences regression in (2).

**Empirical Model**

I estimate the following dynamic version of the simple equation in 2.2, normalizing the spending and outcome variables by the 2008 regional population level:

$$\frac{\text{Outcome}_{c,t}}{\text{Pop}_{c}^{2008}} = \delta_c + \gamma_{\text{state},t} + \sum_{\tau \neq 2008} \beta^\tau \frac{\text{Spend}_c}{\text{Pop}_c^{2008}} \times \mathbb{1}\{t = \tau\} + \sum_{\tau \neq 2008} \eta^\tau X_{c}^{\text{pre}} \times \mathbb{1}\{t = \tau\} + \Delta \epsilon_c$$

(2.3)

With the inclusion of the county fixed effect $\delta_c$, each year-specific $\beta$ can be interpreted as the effect of $\$1$ of additional stimulus spending on the level of the outcome in year $\tau$, relative to 2008. While the normalization by 2008 population allows for a better fit to the data, the interpretation of $\beta$ is unchanged from equation 2.2: if $\text{Outcome}_{c,t}$ is average annual employment, then $\beta$ is the number of job-years added in county $c$ during year $t$ as per Recovery Act dollar spent on a project in that county.
I restrict my focus to within-state variation in some specifications by including a state-by-year fixed effect $\gamma_{state,t}$. When including covariates, I control for differences in trends that vary with the included observable variables. Because per-capita spending measures are mechanically correlated with the baseline population level that is in the denominator, I include a control for the log 2008 region population in all specifications. Standard errors are clustered at the county level. While some information is available about the timing of spending authorizations, the data does not provide detailed information about when work took place. Accordingly, I suppress all time variation in the treatment variable, and instead study the dynamic effects of the total Recovery Act highway construction spending level, which is defined as a time invariant level.

When examining alternative outcomes and specifications, I focus on a simple two-period differences-in-differences equation using 2008 as the pre period year and 2010 as the post-period year. Since 2010 is both the year when the highest level of Recovery Act construction was reported to have occurred, and the year in which I find the largest construction-sector effects, it is natural to examine the effects in 2010 in more detail. I estimate the following equation:

$$\frac{Outcome_{ct}}{Pop_{c2008}} = \delta_c + \gamma_{state,t} + \beta_{post} \frac{Spend_{c}}{Pop_{c2008}} \times 1\{t = 2010\} + \sum_{t \neq 2008} \eta T X_{c}^{pre} \times 1\{t = 2010\} + \Delta \varepsilon_c \quad (2.4)$$

The baseline specification includes state-by-year fixed effects, time-invariant county fixed effects, and a control for year-specific effects of 2008 population. In practice, estimating $\beta_{post}$ from the simple difference-in-differences specification in 2.4 will yield nearly identical point estimates to $\beta_{2010}$ estimated from the dynamic specification in 2.3.

**Threats to Identification**

Equation (1) points to several limitations and concerns that arise when estimating (2) in actual data, where $S_{c,post}$ is not fully randomized. First, even if $S_{c,post}$ were randomized the local em-
ployment effect $\beta$ will be less than even the full partial equilibrium employment effect $\alpha$. This is because of a “firm-commuting effect”: if some contractors working on projects in care be based in other locales, and $S_{c,\text{post}}$ is uncorrelated with the amount of stimulus work in other locales that firms based in $c$ are able to win, then the local employment effect will only be a fraction $(1 - \rho^{\text{out}})$ of the total partial equilibrium effect\(^{23}\). This firm-commuting effect is not an econometric bias, but rather an economic effect—as the spatial mobility of firms increases, the ability to target demand to firms in $c$ using local construction in $c$ will diminish. However, if the level of geographic aggregation is too fine relative to meaningful concepts of local markets, one may obtain an arbitrarily small employment effect. Thus, I consider robustness to different definitions of local labor markets in the analysis below.

A second concern is that the level of local stimulus spending $S_{c,\text{post}}$ may not be orthogonal to other road spending $G_{c,t}$ if the Recovery crowds out other public or private infrastructure spending. One well-documented form of crowd out are “flypaper effects” as in Knight (2002) and Leduc and Wilson (2017): since budgetary funds are fungible, state and local governments may use federal construction grants to offset their own planned spending and in turn use the additional budgetary resources for different purposes. At the state level, Leduc and Wilson (2017) find that federal highway awards actually lead to an increase in state-level spending on highway construction. Nonetheless, crowd-out may exist at the local level within states if states systematically direct their own funds away from regions with stimulus-funded projects towards projects in other locales. Unfortunately, there is no nationwide data source on non-stimulus federal, state, and local government road construction spending that is disaggregated below the state level; thus it is not possible to directly test for local flypaper effects. Yet, if such behaviors were first-order, then in light of the findings of Leduc and Wilson, one should expect crowd-out to be more severe when focusing on within-state variation rather than cross-state variation through inclusion of state-by-year fixed effects. I show below that this is not the case.

Another major concern is that authorities might have systematically targeted funds towards regions experiencing different labor market or industry trends in a manner that violates the parallel

\(^{23}\text{Separately, any partial equilibrium effect will also omit general equilibrium effects, such as macroeconomic multipliers, not captured in (1).}\)
trends assumption (i.e. \( E[S_{c, post} \times \Delta v_c] \neq 0 \)). This assumption warrants particular scrutiny in the context of counter-cyclical spending. In particular, if funds are directed towards regions experiencing larger adverse shocks that are transient and mean-reverting, then the more-treated locales would have grown faster in the absence of any intervention. In the case of the Great Recession, one might separately worry that hard-hit counties experienced persistent adverse trend-breaks in 2009 that continued throughout the observation window. If there was systematic selection into treatment in the case of Recovery Act spending, it is likely that locales that received higher levels of spending were observably different.

I address this identification concern in several ways. First, I implement a dynamic event study specification that facilitates a direct test of whether differently-treated locales experienced different trends prior to 2009. If the parallel trends assumption is valid, then Recovery Act spending should be uncorrelated with pre-2009 trends in the outcome variables. However, even in the absence of differential pre-period trends, the parallel trends assumption may still be violated if state governments targeted funds on the basis of changes in \( \Delta v_c \) that occurred after 2008. While it is impossible to fully rule out any sorting of this sort, I probing the validity of the no-selection assumption by testing for robustness to varied control sets. If estimates are robust as the choice of \( X_i \) varies, then any unobservable source of bias would have to be systematically correlated with the error term \( e_{int} \), but systematically uncorrelated with all of the observable dimensions of heterogeneity. This insight guides my inference below.

**Targeting of Stimulus Spending and Covariate Balance**

The Recovery Act encouraged state and local governments to create jobs in distressed areas, but where did the funds flow in practice? Panel A of Figure 2.3 plots per-capita spending against the 2007-2009 change in quarter 1 unemployment, netting out state averages and adjusting for population size. Interestingly, spending was no higher in regions with higher unemployment levels. While these figures from the LAUS are measured with considerable noise, they represent the best information policymakers could have used to base any intentional targeting of spending to slack markets. Another indicator of local labor market distress prior to the Recovery Act is the
decline in the construction employment rate from 2006–2008; however, there is once again no clear relationship between a larger decline (more negative growth) and local spending.

**Figure 2.3.: Relationship between Covariates and ARRA Treatment Variables**

**Panel A: 2009 First Quarter Unemployment Rate (LAUS)**

**Panel B: Change in First Quarter Unemployment Rate, 2007-2009 (LAUS)**

**Panel C: 2008 Per Capita Lane Miles of Primary Roads (HPMS)**

Notes: Each diagram is a binned scatter plot: the sample is divided into twenty equally-sized bins corresponding to quintiles of the x-axis variable, and the average per capita ARRA road spending level within each bin is calculated and plotted against the y-axis. N = 2,922 counties included in primary analysis sample.

Figure 2.4 plots the pairwise standardized correlation coefficients between Recovery Act construc-
tion spending and a broader range of covariates, residualized on state-level covariates in order to isolate within-state correlations. There is a clear relationship between spending per-capita and 2008 log population—while this relationship is mechanical in part, it also reflects the substantive fact that rural regions with small populations have disproportionately more roads relative to population. As I control for 2008 log population in all specifications, all other correlations in Figure 2.4 are adjusted accordingly. Conditional on population size, there is little relationship between spending and observable indicators of local labor market distress. Rather, the clearest determinant of the spatial allocation of spending appears to be the how many frequently used roads were in each county. Looking within states, primary road lane-miles per capita, average daily vehicle-miles traveled (VMT) per capita, and land area, are the strongest correlates of per-capita Recovery Act road construction spending.24 This is unsurprising: since the majority of Recovery Act road-construction funds went to pavement improvement and road-widening projects, work naturally occurred in places where more roads already existed.

\footnote{24“Major roads” includes all roads designated by the Federal Highway Administration as “principal arterial” or that are part of the Interstate Highway or National Highway Systems in the Highway Performance Management System data. Lane-miles are a distinct concept from road-miles; one mile of a road with two lanes running in each direction would constitute four lane-miles of road.}
Figure 2.4.: Pairwise Correlations of Spending Variables with Covariates

Panel A: Local Project Construction, $ Per Capita

Baseline Correlations

Within-State Correlations

Panel B: Local Vendor Receipts, $ Per Capita

Baseline Correlations

Within-State Correlations

Notes: Each point estimate is the standardized coefficient of correlation between the Recovery Act exposure measure and the stated covariate. 95% confidence intervals implied by the robust standard errors for each point estimate are plotted around each correlation coefficient. Except for the first correlation in each diagram (with 2008 log population), all correlation coefficients are calculated conditional on 2008 log population by including a control in a standardized regression. “Within-state correlations” isolate within-state variation by including state-level fixed effects in these regressions. N = 2,922 counties included in primary analysis sample.

The stock of roads is by no means randomly assigned. However, it is hard to imagine what latent drivers of the boom, bust, and recovery cycle would be correlated with the base-line stock of roads but not observable indicators of economic distress. Leduc and Wilson argue that measures of local road milage and usage (VMT and lane-miles, in particular), are completely orthogonal to latent drivers of short-run growth, and they employ these variables to construct instruments for highway spending based on formulary apportionment rule. I do not take a strong stance on this assumption here, but I will show that results estimated below are robust to either conditioning on these variables or using them as instruments for local project spending.
2.4. County-Level Spending Effect

I first examine estimates of effects of project spending on local construction employment, as construction work is a key first step in any plausible local stimulus transmission mechanism. In order to determine when any effect occur and whether pre-period trends differ with the treatment variable, I estimate the dynamic specification in equation 2.3; results are displayed in Table 2.2. To facilitate inspection of these results, Figure 2.5 plots the year-specific effects of local per-2008-capita project spending (in millions of dollars) on per-2008-capita construction sector employment in the recipient county presented in Column 1 of Table 2.2. This baseline specification includes state-by-year fixed effects and allows year-specific trends to vary with the 2008 population level; the coefficients can be interpreted as the additional number of construction jobs per million dollars spent. Consistent with the parallel trends assumption, construction employment trends evolve similarly on average across all spending levels. While there is little to no effect in 2009 (consistent with an implementation lag), there is a significant effect in 2010 of two additional jobs per million dollars, relative to the counterfactual. This within-state, cross-county effect is nearly identical to cross-state employment effect on construction employment found in Leduc and Wilson (2016). In 2011, there is a borderline significant effect of 1.58 jobs per million, which continues to dissipate in subsequent years. The magnitude and timing of this effect are highly plausible—strikingly, the combined 2009-2013 point estimates suggest a combined local effect of 5.9 job-years per million dollars, close to the back-of-the-envelope closed economy calculation above had predicted.

---

25 This suggests that $27 billion in total spending could have supported over 50,000 jobs in 2010. Direct surveys of contractors involved in Recovery Act construction found that number of full-time-equivalent employees at work on Recovery Act highway projects peaked at 40,000 in September 2010 CBO (2012).
Figure 2.5: Event Study: Dynamic Effects of Local Construction Spending on Construction Employment

Notes: Figure plots year-specific $\beta$ coefficients estimated jointly from the dynamic difference-in-differences specification in equation 2.3, also presented in Column 1 of Table 2.2. The outcome is the county-level annual average construction employment level from the QCEW, both the treatment and the outcome variables are scaled by 2008 population. Each point estimate is the the county-level effect of $1$ Million per capita of Recovery Act road construction spending on construction employment per-capita in the specified year, relative to the 2008 level of the outcome variable. Regression includes state-by-year fixed effects and year-specific controls for 2008 log population. The treatment variable is not time-varying, rather the same treatment variable is interacted with dummies for each outcome year. Year dummies and interactions for 2008 are omitted due to inclusion of identify county-level fixed effects. Standard errors are clustered at the county level, and the implied 95% confidence intervals are plotted around each point estimate. N = 35,052, reflecting 2,922 counties included in primary analysis sample.

Table 2.2 also reports year-by-year point estimates from the dynamic specification in 2.3 for other partial-equilibrium outcomes. Column 2 tests for employments in the “heavy and civil construction” sub-industry (NAICS 237), in which nearly all general contractors for highway projects would be classified. The time pattern of results is similar to Column 1, though point estimates are less than one-half of the size. This attenuation is primarily due to censoring in the QCEW; tests for effects in other sub-sectors yield point estimates close to zero in the post-period.\(^{26}\) If construction labor supply is imperfectly elastic, increased construction demand may be reflected?

\(^{26}\)One may be concerned that the construction effect reflects a boost in residential or nonresidential building construction that is not plausibly due to Recovery Act road spending. However, in Appendix Table B.3 I find this is not the case, and that the combined 2009-2013 effect on employment in these subsectors is approximately zero. Nor is the effect driven by the remaining construction subsectors, where I find a combined 2009-2013 effect of 0.197 (1.796). These results are all consistent with the censoring explanation.
in higher wages, in addition to higher employment levels. The results in Column 3 indicate that annual construction salaries did, in fact, rise in 2010 and 2011 by three dollars in response to each dollar per resident spent on local products. Column 4 reports the combined effect on construction payrolls, reflecting both wage and employment increases. The estimates imply that for each additional dollar of spending, local construction payroll rose by 10 additional cents in 2010. Over the post-period combined, the estimates in Column 4 imply that thirty percent of road expenditures wound up in the pockets of construction workers at local establishments.
### Table 2.2: Dynamic County-Level Effects of Spending on Construction Employment and Pay

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All Construction (1)</td>
<td>Heavy and Civil Constr. (2)</td>
<td></td>
</tr>
<tr>
<td>Sum 2009-2013:</td>
<td>5.924* (3.462)</td>
<td>2.265** (1.078)</td>
<td>0.301** (0.151)</td>
</tr>
<tr>
<td>N Counties</td>
<td>2921</td>
<td>2921</td>
<td>2921</td>
</tr>
<tr>
<td>Observations</td>
<td>35,052</td>
<td>35,052</td>
<td>35,052</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.864</td>
<td>0.780</td>
<td>0.864</td>
</tr>
</tbody>
</table>

Notes: Table displays year-specific $\beta$ coefficients estimated jointly from the dynamic difference-in-differences specification in equation (1), each column reports the output from a separate regression. In columns 1 and 2, the outcomes are the per-2008-capita county-level annual average construction (NAICS 23) employment level and “Heavy and Civil Construction” (NAICS 237) employment level, respectively. The outcome in Column 3 is the per-2008-capita construction total payroll level. The outcome in Column 4 is the average annual salary in the construction sector, this variable is the only outcome not in per capita units. Dollar figures are in constant 2009 dollars. Each point estimate is the the county-level effect of $1 Million per capita of Recovery Act road construction spending on the outcome variable in the specified year, relative to the 2008 level of the outcome variable. Regressions include state-by-year fixed effects and year-specific controls for 2008 log population. The treatment variable is not time-varying, rather the same treatment variable is interacted with dummies for each outcome year. Year dummies and interactions for 2008 are omitted due to inclusion of identify county-level fixed effects. The sum of the coefficients for years 2009-2013 are reported with standard errors in the “Sum 2009-2013” row. Standard errors are clustered at the county level. * indicates two-sided p-value less than 10% , * indicates two-sided p-value less than 5%.
To probe the robustness of this result, Table 2.3 presents the 2010 point estimate under a range of alternative specifications. Columns 1, 2, and 3 report results with no region-year fixed effects, state-year fixed effects, and commuting zone-year fixed effects. The magnitude of the effect is invariant to the inclusion of state fixed effects. This suggests that within-state crowd-out factors do not have a first-order effect on the results. More surprisingly, the effect is only slightly reduced by the inclusion of commuting zone fixed effects. This latter finding implies that the local employment effects identified in the county-level analysis are quite local indeed, and imply that demand spillovers to neighboring counties are minimal.

Table 2.3: Robustness of 2010 Construction Employment Effects

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Effect of $1 Million per Capita on Construction Jobs per Capita</td>
<td>2.054**</td>
<td>2.141**</td>
<td>1.874**</td>
<td>2.441**</td>
<td>1.852**</td>
<td>1.854**</td>
<td>1.608**</td>
</tr>
<tr>
<td></td>
<td>(0.752)</td>
<td>(0.798)</td>
<td>(0.896)</td>
<td>(0.865)</td>
<td>(0.672)</td>
<td>(0.669)</td>
<td>(0.673)</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.935</td>
<td>0.939</td>
<td>0.952</td>
<td>0.945</td>
<td>0.954</td>
<td>0.954</td>
<td>0.954</td>
</tr>
<tr>
<td>Effect of $1 per Capita on Construction Payroll per Capita</td>
<td>0.103**</td>
<td>0.094**</td>
<td>0.085**</td>
<td>0.103**</td>
<td>0.072**</td>
<td>0.070**</td>
<td>0.057*</td>
</tr>
<tr>
<td></td>
<td>(0.034)</td>
<td>(0.035)</td>
<td>(0.039)</td>
<td>(0.039)</td>
<td>(0.033)</td>
<td>(0.033)</td>
<td>(0.034)</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.935</td>
<td>0.938</td>
<td>0.952</td>
<td>0.947</td>
<td>0.952</td>
<td>0.952</td>
<td>0.952</td>
</tr>
</tbody>
</table>

2008 Population Control: x
State x Year FE: x
CZ x Year FE: x
Intensive Margin Only: x
Pre-Period Industry and Demographic Controls: x
Pre-Period Unemployment Controls: x
Pre-Period Road Controls: x
N obs: 5844 5842 5736 4344 5056 5056 4796
N counties: 2922 2921 2868 2172 2528 2528 2398

Notes: Table displays 2008-2010 difference-in-differences coefficients estimated from the two-period specification in equation 2.4. Each point estimate is obtained from a separate regression. In all columns the outcome is the per-2008-capita county-level annual average construction employment level. Each point estimate is the the county-level effect of $1 Million per capita of Recovery Act road construction spending on the outcome variable in 2010, relative to the 2008 level of the outcome variable. “Intensive Margin Only” restricts the sample to counties with positive spending levels. “Pre-period Industry and Demographic Controls” include 2006 and 2008 log employment and log payroll, both total and in the construction sector, as well as a control for population density and 2000-2008 log changed in population. Dollar figures are in constant 2009 dollars. “Unemployment Controls” include 2007 and 2009 Q1 average unemployment from the LAUS. “Road Controls” include 2008 primary road lane miles per capita and 2008 average daily vehicle miles travelled per capita from the HPMS. Year dummies and interactions for 2008 are omitted due to inclusion of identify county-level fixed effects. The size varies across specifications due to missing covariates. Standard errors are clustered at the county level. * indicates two-sided p-value less than 10%, * indicates two-sided p-value less than 5%.
In Columns 4 and 5, I test for confounding selection on observables by including state-year fixed effects, and adds a wide array of covariates including county size, density, the 2007 and 2009 Q1 unemployment rates, the number of housing permits issued in 2003, 2006, and 2008, the housing density change between 2000 and 2010, and the 2008 size and 2006-2008 growth of the local construction sector. The point estimates are largely robust to these inclusions, consistent with a causal effect. Additionally, Column 6 tests whether the effects are driven by the extensive margin (the comparison between places with some spending and those with none) or the intensive margin by restricting the baseline specification of Column 2 to the subsample of places that received positive spending. The point estimates are essentially the same, suggesting that the main effects occur primarily on the intensive margin.

**Figure 2.6.:** Event Study: Dynamic Effects of Local Construction Spending on Total Employment

Notes: Figure plots year-specific $\beta_j$ coefficients estimated jointly from the dynamic difference-in-differences specification in equation 2.3. The sum of the 2009-2013 effects is displayed in Table 2.4. The outcome is the county-level annual average total employment level from the QCEW, both the treatment and the outcome variables are scaled by 2008 population. Each point estimate is the the county-level effect of $1 Million per capita of Recovery Act road construction spending on construction employment per-capita in the specified year, relative to the 2008 level of the outcome variable. Regression includes state-by-year fixed effects and year-specific controls for 2008 log population. The treatment variable is not time-varying, rather the same treatment variable is interacted with dummies for each outcome year. Year dummies and interactions for 2008 are omitted due to inclusion of identify county-level fixed effects. Standard errors are clustered at the county level, and the implied 95% confidence intervals are plotted around each point estimate. N = 35,052, reflecting 2,922 counties included in primary analysis sample.
Having established a clear partial equilibrium effect, I next test whether the gains (or averted losses) in the construction sector are reflected in county-wide industry aggregates. The results are ambiguous. Table 2.4 summarizes the results, and Figure 2.6 plots the year-specific effects of local per-2008-capita project spending (in millions of dollars) on total per-2008-capita private sector employment in the recipient county analogous to Figure 2.5. In both the short and long run, there is no clear effect on aggregate employment. Yet, although the combined 2009-2013 effect is less than two job-years per million dollars, the confidence interval implied by the standard error is large. These findings are consistent with employment effects of anywhere between a loss of twenty-five jobs to a gain of over thirty jobs per million dollars. Table 2.4 presents results from tests under a number of alternative specifications. The 2010 point estimate is not stable to specification, nor is the estimate economically large in any instance.

Table 2.4.: County-Levels Effects of Spending on Total Employment and Pay

<table>
<thead>
<tr>
<th></th>
<th>2010 Effect</th>
<th>Total 2009-2013 Effect</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Effect of $1 Million per Capita</td>
<td>-2.257</td>
<td>-0.579</td>
</tr>
<tr>
<td>on Total Jobs per Capita</td>
<td>(2.708)</td>
<td>(2.787)</td>
</tr>
<tr>
<td>Effect of $1 per Capita</td>
<td>-0.019</td>
<td>-0.097</td>
</tr>
<tr>
<td>on Total Payroll per Capita</td>
<td>(0.123)</td>
<td>(0.130)</td>
</tr>
<tr>
<td>Controls:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2008 Population</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Full Controls</td>
<td>x</td>
<td></td>
</tr>
<tr>
<td>State x Year FE</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>N obs</td>
<td>5844</td>
<td>5842</td>
</tr>
<tr>
<td>N counties</td>
<td>2922</td>
<td>2921</td>
</tr>
</tbody>
</table>

Notes: Each point estimate is obtained from a separate regression. In all columns the outcome is the per-2008-capita county-level annual average total employment level across all sectors. “2010 Effect” indicates estimate is 2008-2010 difference-in-differences coefficient estimated from the two-period specification in equation 2.4. “Total 2009-2013 Effect” is the sum of the 2009, 2010, 2011, 2012, and 2013 coefficients from the dynamic specification in equation 2.3, corresponding to the “Sum 2009-2013” estimates in Table 2.2. “Full Controls” includes “Pre-period Industry and Demographic Controls,” “Unemployment Controls,” and “Road Controls” as defined in Table 2.3. The size varies across specifications due to missing covariates. Standard errors are clustered at the county level. * indicates two-sided p-value less than 10%, ** indicates two-sided p-value less than 5%.

The simplest explanation for this small, imprecise finding is a lack of statistical power. Stimulus highway spending may have had a first-order impact on the construction sector, yet the intervention was likely too small relative to the fluctuations in total employment during the Great
Recession to detect any plausibly-sized impact on local employment. Although the small effect is consistent with there being crowd-out of non-construction spending by stimulus projects, it is also the case that large effects on the order of those in Suarez Serrato and Wingender (2014) are also not ruled out by the obtained confidence intervals.

2.5. Regional Spillovers and Spatial Heterogeneity

This section tests examines how effects might propagate through space, and how the answer may differ across settings. To the extent that firms and workers are mobile across space, there may be regional labor market impacts of highway construction projects that extend beyond county borders. As a result, one may expect to find larger effects when conducting the analysis at levels of aggregation reflecting more meaningful labor markets, as there will be less “leakage” of labor demand into other jurisdictions (in the context of equation 2.1, this corresponds to a higher level of $\rho^{out}$). I first test for effects at more aggregated geographies, as well as for localized spillovers using observed commuting flows in the spirit of Dupor and McRory (2017). I find no evidence of significant regional demand spillovers. I then test for heterogeneity in the effects. Importantly, I find that significant effects on construction employment only primarily in places that are smaller and less open to commuting. This is consistent with effects only being detectable in setting where intervention was sufficiently large relative to the local economy.

Tests for Regional Effects

To study how the labor-market impacts of local spending vary at different geographic scales, Table 2.5 presents results at the county, commuting zone (CZ), metropolitan statistical area (MSA), and state levels. While urban agglomerations represented by MSAs offer the most standard notion of a labor market, an important part of the identifying variation comes from regions outside urban MSAs, as discussed below. Therefore, CZs offer a comparable alternative that includes all counties in the data. Since the largest effects occurred in 2010, I use a version of the baseline difference-in-differences specification in Equation 2.4 in order to yield a clear basis for comparison with the baseline results\textsuperscript{27}.

\textsuperscript{27}The specifications includes a control for 2008 population, but exclude state fixed effects. At broader levels of aggrega-
Table 2.5: Results at Varying Levels of Spatial Aggregation

<table>
<thead>
<tr>
<th>Aggregation Level</th>
<th>Effect of $1 Million Per Capita on Jobs Per Capita:</th>
<th>Effect of $1 Per Capita on Total Payroll $ Per Capita:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Construction (1)</td>
<td>All Employment (2)</td>
</tr>
<tr>
<td>County</td>
<td>2.054**</td>
<td>2.185**</td>
</tr>
<tr>
<td></td>
<td>(0.752)</td>
<td>(0.673)</td>
</tr>
<tr>
<td>N</td>
<td>2922</td>
<td>2522</td>
</tr>
<tr>
<td>Commuting Zone</td>
<td>3.336**</td>
<td>2.235</td>
</tr>
<tr>
<td></td>
<td>(1.511)</td>
<td>(1.529)</td>
</tr>
<tr>
<td>N</td>
<td>690</td>
<td>668</td>
</tr>
<tr>
<td>Metropolitan Area</td>
<td>0.344</td>
<td>-0.310</td>
</tr>
<tr>
<td></td>
<td>(2.680)</td>
<td>(2.152)</td>
</tr>
<tr>
<td>N</td>
<td>336</td>
<td>322</td>
</tr>
<tr>
<td>State</td>
<td>8.062</td>
<td>52.019</td>
</tr>
<tr>
<td></td>
<td>(10.614)</td>
<td>(39.712)</td>
</tr>
<tr>
<td>N</td>
<td>94</td>
<td>94</td>
</tr>
</tbody>
</table>

Controls

| 2008 Population | x | x | x | x | x | x | x |
| Additional covariates | x | x | x | x | x |

Notes: Table displays 2008-2010 difference-in-differences coefficients estimated from the two-period specification in equation 2.4 estimated at differing levels of aggregation. Each point estimate is obtained from a separate regression. Outcomes are as defined in Tables 2-4. Year dummies and interactions for 2008 are omitted due to inclusion of identify county-level fixed effects. “Additional Covariates” includes “Pre-period Industry and Demographic Controls” as defined in Table 2.3. Regressions do not include state-by-year fixed effects. Standard errors are clustered at the county level. * indicates two-sided p-value less than 10%, ** indicates two-sided p-value less than 5%.

At broader levels of aggregation, the results are less precise due to the small number of observations. Although the baseline point estimates of the per-dollar effects of spending on construction employment and payroll effects are indeed larger at the CZ, these results are less robust to specification. This supports the hypothesis that stimulus construction work increases demand at construction firms outside the immediate locale of the project, so that the total regional labor market effect may exceed those found above. At the MSA and state level, these results become increasingly imprecise, but remain mostly consistent with that hypothesis. The effects on total employment remain small and highly imprecise at all levels of aggregation. There is no increase in power, as any increases in potential effect size at higher levels of aggregation are offset by lost precision due to the smaller sample size.

Ation, the arguments that justified the simple differences-in-differences design for the county-level analysis above may be less valid. Accordingly, these estimates are presented for comparison.
A more direct test for geographically-diffuse demand spillovers would be to examine whether high levels of spending in proximate areas have effects on local employment. To develop a economically meaningful index of exposure, I use data on 2006-2010 county-to-county commuting flows from the American Commuting Survey (ACS) to measure how much stimulus road spending occurred in the places where people living in the observation counties go to work. Although the primary outcome data is based on place of work rather than place of residence, worker flows may still provide a useful proxy for firms’ ability to send their employees to work in projects in another given locale. I construct exposure using two alternative methods. First, I measure average per-capita spending in the places to which residents of the observation county commute (outside of the observation county itself). For an observation county $o$ and each destination county $d$, I calculate $\lambda_{od}^{out}$, the number of residents in $o$ who commute to county $d$ as a share of all residents in $o$ who commute somewhere outside of $o$ (so that $\sum_{d \neq o} \lambda_{od}^{out} = 1 \forall o$). I then use these shares to calculate the average treatment intensity that commuters from $o$ are exposed to, $Exp_o^{out} \equiv \sum_{d \neq o} \lambda_{od}^{out} \times \frac{Spend_d}{Pop_{2008_d}}$. Second, I measure how much spending in other counties $d$ would go to local firms in $o$ if the share of projects in $d$ that worked on by firms in $o$ were the same as the number of workers in $d$ who reside in in $o$ and commute. To do this, I denote the share of workers in $d$ who commute from $o$ as $\lambda_{do}^{in}$, and calculate the expected outside revenues (per capita) in $o$ as $Exp_o^{in} \equiv \frac{1}{Pop_{2008_o}} \sum_{d \neq o} \lambda_{do}^{in} \times Spend_d$. I then test whether these measures predict employment outcomes when included in regressions along with the main own-county spending measure.

---

28 While the data is less reliable at fine scales, I also examine employment outcomes for local residents based in treated locales obtained from the ACS and LAUS in the Appendix.
Table 2.6: Effects of Exposure to Spending in Commuting-Proximate Counties

<table>
<thead>
<tr>
<th></th>
<th>Effect of $1 Million Per Capita on Jobs Per Capita:</th>
<th>Effect of $1 Per Capita on Total Payroll $ Per Capita:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Construction (1)</td>
<td>All Employment (2)</td>
</tr>
<tr>
<td></td>
<td>Effect of $1 Million Per Capita on jobs per capita</td>
<td>Effect of $1 Per Capita on total payroll $ per capita</td>
</tr>
<tr>
<td></td>
<td>Job in construction</td>
<td>All employment</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td><strong>Exp</strong>&lt;sup&gt;out&lt;/sup&gt;</td>
<td>1.307 (1.416)</td>
<td>0.077 (0.063)</td>
</tr>
<tr>
<td></td>
<td>1.452 (1.359)</td>
<td>0.098 (0.064)</td>
</tr>
<tr>
<td></td>
<td>1.987 (5.803)</td>
<td>0.241 (0.287)</td>
</tr>
<tr>
<td></td>
<td>-0.023 (5.950)</td>
<td>0.265 (0.299)</td>
</tr>
<tr>
<td><strong>Exp</strong>&lt;sup&gt;in&lt;/sup&gt;</td>
<td>5.610 (3.787)</td>
<td>0.353** (0.170)</td>
</tr>
<tr>
<td></td>
<td>4.253 (3.801)</td>
<td>0.233 (0.178)</td>
</tr>
<tr>
<td></td>
<td>21.840 (15.449)</td>
<td>0.117 (0.755)</td>
</tr>
<tr>
<td></td>
<td>-12.713 (15.524)</td>
<td>-0.271 (0.760)</td>
</tr>
<tr>
<td>Controls</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2008 Population</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td></td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td></td>
<td>x</td>
<td>x</td>
</tr>
</tbody>
</table>

Notes: Table displays 2008-2010 difference-in-differences coefficients estimated from the two-period specification in equation 2.4. **Exp**<sup>out</sup> is the weighted average per-capita spending level in counties that residents of county c commute to work, weighted by how many residents commute to each outside county and excluding c’s own exposure. That is, **Exp**<sup>out</sup><sub>c</sub> = c<sub>out</sub> <sup>cd</sup> × Spend<sub>d</sub> × Pop<sub>2008</sub><sub>d</sub>. **Exp**<sup>in</sup><sub>c</sub> measures how much outside construction work would be done by local employees if the probability that a project in outside county d were constructed by a worker based in observation county c were the same as the probability (the observed share) of workers in d who commute from c; this amount is then scaled by local resident population in c. That is, **Exp**<sup>in</sup><sub>c</sub> = 1<sub>pop2008</sub> × c<sub>in</sub> <sup>cd</sup> × Spend<sub>d</sub>. The inbound and outbound commuting shares are obtained from the 2006-2010 American Community Surveys. Each point estimate is obtained from a separate regression. Specifications are as in the first row of Table 2.5, see notes for details. “Additional Covariates” includes “Pre-period Industry and Demographic Controls” as defined in Table 2.3. Regressions do not include state-by-year fixed effects. Standard errors are clustered at the county level. * indicates two-sided p-value less than 10%, ** indicates two-sided p-value less than 5%.

Results are presented in Table 2.6. For both measures, there is no evidence of a significant positive construction demand spillover across space. This is consistent with the aggregated results and the finding in Section 4 that county-level results are insensitive to the inclusion of even Commuting Zone fixed effects. In particular, any positive “partial equilibrium” effects on construction are either in the project in the locale or are so highly dispersed across space (relative to standard commuting patterns) that it is hard to detect under any standard definition of a broader labor market. However, theory is ambiguous as to whether regional spillovers should be positive. An alternative explanation may be that spending in a locale can have negative partial-equilibrium spillovers onto construction employment in other locales if construction workers leave establishments in neighboring regions in order to work on local projects. Thus, the lack of observed spillovers may because the spillover effects are actually zero on net, rather than because they are positive but
difficult to identify in the data. As with the main own-county spending variable, effects on total employment are too imprecise to make any definitive conclusion.

Given the high incidence of vendors being located outside of project counties, one can also test for effects in locales where vendors were located. Thus, rather than defining the county-level variable as total per-capita spending on projects within a county, I define the “vendor treatment” variable as the total per-capita receipts by vendor firms based in a county. While it is straightforward to use this variable as the treatment in the differences-in-differences, the no-selection-on-unobservables assumption is much less plausible, so one should be hesitant to interpret any results as having a causal interpretation. In particular, firms and regions with better latent productivity trends may be in a better position to win competitive bids, which could bias results upwards. At the same time, firms that work on stimulus projects may be systematically less likely to work on non-stimulus projects, leading to a possible downwards bias. Appendix Figure B.2 plots the annual coefficients estimated from the dynamic specification in 2.3, using per capita vendor payments as the treatment and per-capita construction employment as the outcome variable. While there is no clear post-2008 effect, there is a pronounced increase in construction employment in 2008 (before the Recovery Act was passed) associated with higher levels of receipts, indicating confounding selection. Thus, only specifications controls for pre-period evolutions in sectoral employment can plausibly be interpreted as causal; however, even with controls, any causal interpretation is suspect.

Regardless, these results do not constitute evidence of net negative spillovers.

There are nonetheless sources of variation in vendors’ receipts of stimulus funds that would satisfy a parallel trends assumption. For example, if new road construction opportunities were scarce, so that firms that lost stimulus bids had little to do otherwise, and if the factors that determined which firms won bids were mostly arbitrary, then
Table 2.7: Vendor Location versus Project Location: Difference in Difference Estimates

<table>
<thead>
<tr>
<th>Panel A: 2010 Effect on Construction Jobs Per Capita</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
</tr>
<tr>
<td>Spending on Projects in Region ($Millions per Capita)</td>
</tr>
<tr>
<td>Payments to Vendors in Region ($Millions per Capita)</td>
</tr>
<tr>
<td>Region Level</td>
</tr>
<tr>
<td>Controls</td>
</tr>
<tr>
<td>Additional covariates</td>
</tr>
<tr>
<td>N obs</td>
</tr>
<tr>
<td>N counties</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: 2010 Effect on Construction Jobs Per Capita</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
</tr>
<tr>
<td>Spending on Projects in Region ($Millions per Capita)</td>
</tr>
<tr>
<td>Payments to Vendors in Region ($Millions per Capita)</td>
</tr>
<tr>
<td>Region Level</td>
</tr>
<tr>
<td>Controls</td>
</tr>
<tr>
<td>Additional covariates</td>
</tr>
<tr>
<td>N obs</td>
</tr>
<tr>
<td>N counties</td>
</tr>
</tbody>
</table>

Notes: Table displays 2008-2010 difference-in-differences coefficients estimated from the two-period specification in equation 2.4 estimated at differing levels of aggregation, including both treatment variables in each equation. Coefficients in each column of each panel are estimated jointly. Year dummies and interactions for 2008 are omitted due to inclusion of identify county-level fixed effects. “Additional Covariates” includes “Pre-period Industry and Demographic Controls” as defined in Table 2.3. Regressions do not include state-by-year fixed effects. Standard errors are clustered at the county level. * indicates two-sided p-value less than 10%, ** indicates two-sided p-value less than 5%.

Table 2.7 displays differences-in-differences estimates of the 2010 effects of both local project spending per capita and local vendor receipts per capita, when both included jointly in regressions. Interestingly, inclusion of the vendor variable has little to no impact on the project spending variable, relative to Table 2.3. This may be unsurprising given the lower correlation of the two variables, yet this implies that the county-level construction employment effect does not operate through the primary vendor, suggesting that many construction workers on-site employed at establishments other than the prime vendors (either at regional branch offices of the vendor firms, or at separate
subcontracting firms\(^{31}\)). While the size of the estimated effect of local project spending is mostly robust to controls, the effect of vendor receipts is more sensitive. With the inclusion of controls, the vendor receipts effect becomes positive and similar in size to the local spending effect. Taking the local results at face value, these effects imply that there is a 2010 effect of roughly two jobs per million dollars in each of the project and vendor county, suggesting that there may in fact be an aggregate effect that is larger than the initial effects implied. The state is the only level where vendors and projects are sufficiently co-located that the effect only loads onto the spending variable. Vendor spending is also associated with larger total employment effects conditional on controls; however, while suggestive, these results are tenuous at best given the sensitivity to specification.

**Treatment Effect Heterogeneity and Nonlinear Effects**

Since the effects of spending may not be constant in all places, I next examine heterogeneity in the treatment effect. In particular, Equation 2.1 highlights the possibility that effects will be larger in places where local projects are more likely to be worked on by employees of local establishments. Accordingly, I test whether the effect is different in counties that are more or less open to commuting (measured by the share in the ACS who commute from out of state)\(^ {32}\). In addition, it is possible that the effects may differ across counties with larger and smaller populations, as a given change in spending per capita could represent a bigger proportional shock to demand aggregates in smaller locales.\(^ {33}\) Thus, I test for heterogeneity by 2008 population as well.

\(^{31}\)Since data on subcontractors is not available, it is not possible to determine for certain whether this is the case.

\(^{32}\)To test for heterogeneity in effects, I estimated interacted difference-in-differences specifications of the form:

\[
\text{Outcome}_{c,t} = \delta_c + \gamma_{\text{state}} + \beta_{\text{open, post Spend, Pop}} \times 1\{t = 2010\} \times 1\{\text{HiCommute}\} + \beta_{\text{closed, post Spend, Pop}} \times 1\{t = 2010\} \times 1\{\text{LoCommute}\} + \gamma_{\text{open}} 1\{t = 2010\} \times 1\{\text{HiCommute}\} + \sum_{T \neq 2008} \eta^T X^c_{t-1} \times 1\{t = 2010\} + \Delta c
\]

\(^{33}\)Put differently, the variation generated by Recovery Act road spending may be too small to significantly impact larger economies.
Table 2.8: Heterogeneity and Nonlinearities in Effects on Construction Employments

<table>
<thead>
<tr>
<th>Term</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Per Capita Spending</td>
<td>1.852**</td>
<td>-2.811</td>
<td>12.274**</td>
<td>5.103**</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.672)</td>
<td>(2.030)</td>
<td>(5.577)</td>
<td>(1.595)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Per Capita Spending)^2</td>
<td>10.066**</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(4.324)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per Capita Spending x Log (2008 Pop)</td>
<td></td>
<td>-1.069*</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.550)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per Capita Spending x (Pop &gt; Median)</td>
<td>-0.116</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.838)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per Capita Spending x (Pop ≤ Median)</td>
<td>2.803**</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.898)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per Capita Spending x % Commute</td>
<td>-9.632**</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(3.877)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per Capita Spending x (% Commute &gt; Median)</td>
<td>-0.104</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.807)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per Capita Spending x (% Commute ≤ Median)</td>
<td>3.423**</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.983)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

** Controls:**
- 2008 Population: x x x x x x
- Additional Covariates: x x x x x x
- Interaction Main Effect: x x x x x x
- State x Year FE: x x x x x x

N obs: 5056 5056 5056 5056 5056 5056
N counties: 2528 2528 2528 2528 2528 2528

Notes: Table displays 2008-2010 difference-in-differences coefficients estimated from the two-period specification in equation 2.4, including interactions with the treatment effect. All coefficients in a column are estimated jointly. Whenever a variable is interacted with the treatment variable (times the Post indicator), I also include a control for the interaction of that covariate and the Post indicator. (Pop > Median) is an indicator for whether the observation county had a 2008 resident population greater than median. Commuting shares are obtained from the 2006-2010 American Community Surveys. “Additional Covariates” includes “Pre-period Industry and Demographic Controls” as defined in Table 2.3. Standard errors are clustered at the county level. * indicates two-sided p-value less than 10%, ** indicates two-sided p-value less than 5%.

The results are presented in Table 2.8. Focusing on the core effect on construction employment in 2010, it appears that there is no effect in counties that are above the median population in 2008 or that have a share of workers who commute outside the country that is above median. By contrast, the baseline effects appear to be driven by a more pronounced effect in counties that are smaller and less open to commuting. While interactions with continuous measures of openness and population yield less precise results, the signs of the estimate are with the main effects beings...
driven by smaller, more isolated counties. These findings are consistent with claims that in larger local economies and in economies that are more interconnected with broader regions, it is more difficult to detect the impacts of an intervention due to low power.
Figure 2.7.: Heterogeneity by Commuting Openness

Outcome: Per Capita Construction Payroll

Notes: Figure plots separate year-specific $\beta^{\text{group}}$ coefficients estimated jointly from a variant of the dynamic difference-in-differences specification in equation (1) that includes two sets of coefficients, one set interacted with a indicator of whether the county has a higher-than-median share that commutes outside the county ("High Commuting Openness"), and the other set interacted with an indicator of the opposite ("Low Commuting Openness"). Each set of coefficients are plotted as a distinct series indicated in the legend, but are estimated jointly in a regression that includes controls for the indicator variable interacted with year dummies. The sums of the 2009-2013 effects are displayed in Table 2.9. The outcomes are the county-level annual average per-capita payroll level, both in the construction sector and overall, from the QCEW. Each point estimate is the the county-level effect of $1 Million per capita of Recovery Act road construction spending on payroll dollars per-capita in the specified year, relative to the 2008 level of the outcome variable. Dollar figures are in constant 2009 dollars. Regression includes state-by-year fixed effects and year-specific controls for 2008 log population. The treatment variable is not time-varying, rather the same treatment variable is interacted with dummies for each outcome year. Year dummies and interactions for 2008 are omitted due to inclusion of identify county-level fixed effects. Standard errors are clustered at the county level, and the implied 95% confidence intervals are plotted around each point estimate. N = 33,182, reflecting 2,922 counties included in primary analysis sample.
Table 2.9 and Figure 2.7 examine heterogeneity across commuting openness levels for a broader range of outcomes. I find the effects on the construction sector (and the heavy/civil construction sub-sector in particular) are much more pronounced in areas with less commuting both in the short run and in the long run. This is intuitive in the context of the framework laid out above, and summarized in Equation 2.1: when commuting mobility is lower (corresponding to a lower level of $r^{out}$) there will be less “leakage” of labor demand into other regions and a more concentrated effect in the target area. This commuting effect—which is really about the ability of firms to send workers to other locales—is somewhat different from the effects of commuting discussed in Monte et al. (2016). That study considers commuting flows of residents in one locale ($o$) to firms in different locales ($d$), presuming the demand level of firms in locale $d$ is known. As $d$ becomes more open to commuting, the employment impact of a given local labor demand boost in $d$ will be greater. Since the local labor supply elasticity is higher when it is more open to commuting, a given change in labor demand affects the quantity margin (employment) more and the price margin (wages) less. By contrast, in the scenario studied here, the first-order question is whether local spending has a detectable effect on local labor demand in the same region; this is the question formalized in the framework above.
### Table 2.9: Effect Heterogeneity across High and Low Commuting Share Counties

<table>
<thead>
<tr>
<th>Per Capita Spending x (% Commute &gt; Median)</th>
<th>Effect of $1 Million Per Capita on Jobs Per Capita:</th>
<th>Effect of $1 Per Capita on Total Payroll $ Per Capita:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Construction (1) (2)</td>
<td>All Employment (3) (4)</td>
</tr>
<tr>
<td>-0.104</td>
<td>(0.807)</td>
<td>-5.495 (3.666)</td>
</tr>
<tr>
<td>Per Capita Spending x (% Commute ≤ Median)</td>
<td>3.425** (0.983)</td>
<td>1.469 (4.258)</td>
</tr>
</tbody>
</table>

| P Value, Coefficients Equal | 0.004 | 0.009 | 0.189 | 0.021 | 0.004 | 0.008 | 0.157 | 0.027 |

| 2010 Effect | x | x | x | x | x | x | x |

| Sum 2009-2013 Effect | x | x | x | x | x | x | x |

Notes: Table displays effects of the primary spending exposure variable interacted with indicator for whether or not the observation county had an outside-commuting share that exceeds the sample median. The outcomes are payroll dollars per 2008 capita in constant 2009 dollars. All coefficients in a column are estimated jointly. “Total 2009-2013 Effect” is the sum of the 2009, 2010, 2011, 2012, and 2013 coefficients on the corresponding interaction term from an interacted version of the dynamic specification in equation 2.3, see Figure 2.7 for more details. “2010 Effect” indicates the estimates are 2008-2010 difference-in-differences coefficient estimated from the two-period specification in equation 2.4, corresponding to the specification in Column 6 of Table 2.8. Whenever a variable is interacted with the treatment variable and any year interactions, I also include a controls for that covariate interacted with the corresponding year indicators. Commuting shares are obtained from the 2006-2010 American Community Surveys. All specifications include control for 2008 log population and “Pre-period Industry and Demographic Controls” as defined in Table 2.3. All regressions include state-by-year fixed effects. Standard errors are clustered at the county level. * indicates two-sided p-value less than 10%, ** indicates two-sided p-value less than 5%.

If the absence of an effect in bigger regions is due to lack of power because the treatment variation is too small, then one should expect effects to be easier to detect in the upper regions of the treatment distribution. To test whether effects are more pronounced as the intervention grows, I also include a quadratic term into the baseline specification in Table 2.8. Indeed, it appears that the main effect loads onto the quadratic term, suggestion that even when restricting the focus to construction employment, the spending intervention was only large enough to have a noticeable impact at very high levels. Meanwhile, when looking at broader employment outcomes, even the upper ranges of per capita spending fail to identify a precise effect. Similarly, although 2.7 provides suggestive evidence that there may be larger payroll multipliers in more self-contained regions, the results are too imprecise to rule out a multiplier of zero. Given the massive background fluctuations in regional economies and the relatively small size of the Recovery Act highway construction program, it is likely that these tests lack the statistical power to detect any plausibly-sized...
multiplier effect with a high degree of confidence.

2.6. Conclusion

In the years following 2009, many highway, tunnel, and bridge projects across the United States were accompanied by signs bearing the slogan “Putting America to Work.” This paper examined whether this slogan held true at the local level—and whether construction projects are a good way to stimulate local labor markets during slack periods. Should local governments prioritize highway construction as a way to boost local employment? I used a unique dataset containing detailed information on all projects funded by the Recovery Act and their contractors to test whether places that had more construction work had better local labor market conditions during the recovery period. The siting of these projects appears to have been uncorrelated with any observable indicators of economic performance, but I examine robustness of results to a wide array of selection-on-observables of assumptions, as well as an instrumental variable approach.

I find that highway impacted construction employment at the county level: a dollar of additional Recovery Act spending on local construction increased local construction payrolls by thirty cents over 2009-2013. The magnitude of this effect matches the national labor share of construction revenues, suggesting that targeted spending did not crowd out other local construction. These effects are most pronounced among counties with smaller populations and smaller fractions of residents that commute to outside counties for work. However, when I test for general equilibrium effects on local employment and payroll aggregates, I find effects close to zero with very wide confidence intervals across all specifications. Although the Recovery Act was a significant enough intervention to have a sizable impact on the construction sector in counties with low mobility, these findings suggest that the local variation in highway spending was too small relative to baseline regional volatility to detect a local employment “multiplier.”

My findings suggest the credibility of larger estimates rests on the plausibility of the local transmission mechanism—if one finds large employment/income multipliers, one should also observe first-order effects on the direct recipients of government funds. I argue that while it is highly plausible that that construction spending has a detectable effect on construction employment but does
not have a detectable effect on aggregate employment, it is less plausible that there could be a true effect on aggregate employment without an effect on construction payrolls. If this link is missing, this casts doubt on the validity of the source of variation used to estimate the multiplier.

Importantly, though the magnitude of the effects on the construction sector is what one might expect in the absence of crowd-out, the implied cost-per-job I estimate is high relative to other estimates in the literature. My estimate of 5 construction job-years per $1 Million imply that one construction job-year requires $200,000 in highway spending to sustain. While I find a larger impact on construction employment compared to the state-level findings in Leduc and Wilson (2017), the cost per construction job-year is much more expensive than the roughly $30,000 cost per total job year in Suarez Serrato and Wingender (2014) and Buchheim and Watzinger (2017). In order to reconcile the construction employment effects I find with a $30,000 cost per total job, a construction job would have to result in the creation of five additional construction jobs. While not ruled out by my estimated effects on total employment, this is not plausible based on prior studies of local employment multipliers, which typically find that an exogenous one job-year increase in low skilled work only supports one additional job in the same metropolitan area Moretti (2010); van Dijk (2016).

Thus, the primary value of infrastructure improvement work in a community arises in the value of the infrastructure itself. To this end, the goals of policymakers should be to channel funds to projects with high infrastructure value. It is possible that improved highway infrastructure supports robust local employment growth in the much longer run, although I do not find clear evidence of any effects as of 2013. In a classic survey, Gramlich Gramlich (1994) notes that the returns on investment on road construction work are potentially quite high, but they vary significantly depending on the type of project selected—spending on building new rural roads has little value, while repair of heavily-used highways can have a rate of return as high as 35 percent. Thus, while it could be easy to justify repairing aging highway arteries with stimulus funds, there appears to be little justification for widening sparsely-used roads in a rural county hard-hit by the housing bust in hopes of boosting local employment. One should note that concentration of Recovery Act construction in paving activity rather than in new construction is not inherently problematic
in this view, as the social value of resurfacing crucial roads may exceed that of building a new “bridge to nowhere.”

While most analyses of stimulus spending have studied interventions like the Recovery Act from a macroeconomic angle, this paper has taken a more microeconomic approach in focusing on the spatial nature of the transmission mechanism. Further analysis of microeconomic effects of stimulus spending using disaggregated data is a promising direction for future work and will help supplement theoretical micro-foundations for macroeconomic models with better empirical content. Even if highway spending did not have effects on specific local labor markets, it may nonetheless have proved effective in providing the means for individual firms facing constraints to stay afloat during the construction bust and in keeping individual workers attached to the labor force during the downturn. If such effects exist, better evidence would point to more cost-effective counter cyclical policy measures. The data assembled for this paper should be of use to those pursuing work along these lines.
3. Public Investment and the Spread of "Good-Paying" Manufacturing Jobs: Evidence from World War II’s Big Plants

3.1. Introduction

Rising income inequality and disappearance of “middle-skill” jobs have been hallmarks of the United States economy for the past half-century. Yet, in the long arc of history, the postwar period appears to have been a period of exceptional opportunity for the middle-class. By the end of World War II the premium for skill had fallen notably, the pre-tax wage distribution had compressed, and the employment level in middle-class manufacturing occupations had reached unprecedented heights Goldin and Margo (1992); Piketty (2014). Many suspect there was a causal link between the events of the war and the prolonged period of middle-class prosperity. However, only scant evidence exists as to how and why various aspects of the military and industrial mobilization for war impacted postwar labor markets.

This paper conjectures that durable investments in productive capacity made as part of the war mobilization effort had labor market effects that long outlived the war. The industrial mobilization effort entailed a large-scale, rapid and publicly-financed push to construct new manufacturing plants for war-specific purposes (such as bomber assembly). Yet as the war ended in 1945, these plants, built to churn out unprecedented output quantities for a lengthy war, stood intact. While no company was willing to finance a dime of such a massive plant designed to produce war
materiel, most such plants were bought by private firms at large markdowns and repurposed for civilian production after the war. Thus, for reasons that were in large part idiosyncratic and due to the short-run strategic goals of the war, many communities in the United States found themselves with large, durable, state-of-the-art industrial facilities. I exploit idiosyncratic variation in wartime investment to test the extent to which public investment in industrial infrastructure can transform local labor markets so as to boost manufacturing employment and buoy middle-class opportunities in the long-run.

This research speaks directly to a core question for economic policy-makers: can a brief public intervention have beneficial impacts that persist well beyond the period of government intervention? Understanding the answer is vital to understanding whether industrial policy is justified in developing regions, whether place-based labor policies can help the middle class, and the extent to which infrastructure shapes how urban clusters form and persistent. However, opportunities to study this question directly are extremely rare—big “pushes” are few and far between, and when they do occur (in the form of plant openings or major infrastructure works) they are typically systematically targeted at places that are expected to grow or stagnate. Moreover, it is very difficult to determine what the reason for persistence is.

The industrial expansion for World War II provides a unique opportunity to study this question. Due to the short-run military emergency, political and military leaders demanded that the United States increase its domestic industrial output nearly threefold over the course of only three or four years. This increment to output primarily consisted of airplanes, ships, ordnance (guns and ammo of all varieties), explosives, and the metals and chemicals that were of particular use in the production of those various types of materiel. Although the military attempted to incentivize firms to put their own capital on the line and build plants as necessary, for some particularly large plants in secure locations, these were insufficient to attract any private investment. This was particularly likely when an expensive plant was built to churn out a type of product that was much less likely to be demanded during peacetime. In these cases, the plants were ordered and owned by the US government. Given the complete unwillingness of firms to invest a dime in these plants, it is unlikely that similar plants would have been sited in the same locations if not for the
war. These large, durable, public plants are more plausibly located for quasi-random short-run reasons than any comparable infrastructure investment in Western history.

To estimate the impacts of siting a large plant in a specific locale, I compare counties that received large and completely federally-funded plants to counties that were observably similar at the dawn of the great depression. My conjecture is that in the absence of a war, neither the control nor the treatment counties would have had such an additional plant open; the only reason potential outcomes differ across treatment and control counties were circumstances created by the war. I hone in on control groups using several methods, although the choice of method does not significantly impact the results. For a given control group definition, I examine event studies using a wide array of outcomes. I find that while there is no difference in trends in the run-up to the war, there is a pronounced difference in outcomes that emerges only at the end of the “reconversion” period that occurred in the immediate aftermath of the war, and then persists throughout the balance of the century. The across-the-board absence of pre-trends motivates a difference-in-differences estimator to estimate the short-run and long-run impacts of plant openings.

While the identification of persistent effects on local labor markets is of interest, the implications for policy depend on the reason for persistence. Persistent effects may result from dynamics that are entirely internal to the plant–this may simply reflect slow depreciation of durable capital, but it may also arise from dynamic complementarities that incentivize firms to continue to reinvest in the same location (which would give rise to path dependence). Alternatively, persistence may also arise due to external economies–the creation of manufacturing clusters due to productivity spillovers, the formation of a large labor pool, or the establishment of infrastructure with benefits beyond the original plant. The county-level analysis is not particularly conducive to tests of specific mechanisms. However, I discuss methods to distinguish between these mechanisms using restricted-access plant-level data that I am in the process of acquiring.

This paper contributes to several strands in the economics literature. First, it contributes to the literatures how place-based policies impact economic geography, regional development, and local-labor market. The focus on plant openings is similar to Greenstone, Hornbeck, Moretti Greenstone et al. (2010), while the focus on persistent effects of place-based policies follows work by Moretti
and Kline on the Tennessee Valley Authority Kline and Moretti (2014a). Other notable work on localization economies and “big push” policies are Ellison, Glaeser and Kerr Ellison et al. (2010), Lee Lee (2016), and Murphy, Shleifer and Vishny Murphy et al. (1989). Second, it contributes to a growing empirical literature in macroeconomics that exploits variation in military spending over time and place to estimate the short and long-run impacts of different types of government spending on regional economic performance; Barro Barro and Redlick (2011), Ramey Ramey (2011) Nakamura and Steinsson Nakamura and Steinsson (2014) are key papers in this vein. Finally, this paper joins a growing literature that explores the long-run effects of World War II on various aspects of the post-war economy. Goldin and Margo Goldin and Margo (1992) were the first to clearly document that a distinct “great compression” in the wage distribution occurred during the 1940s. Angrist and Krueger Angrist and Krueger (1994) looked at the impact of the military service GI bill on post-war education and earnings, finding that most of the effects appeared to be driven by selection. Fishback and Cullen Fishback and Cullen (2013) examine the relationship between aggregate local spending and post-war retail sales and population growth, and appear to find relatively small effects. Higgs Higgs (2004) and Mulligan Mulligan (1998) present calculations that suggest the effects of the war on the labor force and postwar growth was minimal. And several papers, notably Goldin Goldin (1991), Goldin and Olivetti Goldin and Olivetti (2013) and Acemoglu, Autor, Lyle Acemoglu et al. (2004) have attempted to measure to impacts of the war on female labor force participation in the post war period.

The remained of the paper proceeds as follows. Section 2 provides historical and institutional background on the economic mobilization for WWII places decisions to build plants with public funds in that context. In Section 3, I develop a research design that exploits the institutional context to obtain credibly causal estimates of the impact of wartime plant openings on post-war local economies. Section 4 presents the baseline results pertaining to post-war manufacturing output, employment, and wages. Section 5 expands the analysis to examine effects on broader labor market outcomes. Section 6 discusses what one can and cannot reasonably conclude from these findings, and suggests several tests to empirically distinguish between the potential mechanisms at play. Section 7 Concludes.
3.2. Institutional Background: New Plants for War

The industrial mobilization during WWII was by many counts the most dramatic economic expansion in United States history. When war broke out in Europe in 1939, the industrial sectors in the USA were ill-equipped to support a sustained war effort. American industry had been poorly organized during the First World War, and as a result never truly converted to coordinated production of war material. While manufacturing had boomed during the 1920s, industrial output lagged during Great Depression, during which period many firms had closed their doors for good. Moreover, the vast majority of American manufacturing experience in 1939 had been concentrated in sectors that could not easily pivot towards production of metal- and chemical-intensive war goods like airplanes, ordnance, and explosives—primarily agricultural processing, textiles and apparel manufacturing, and wood/paper processing. Yet, from 1940 to 1944, annual output of planes rose from approximately 6,000 military planes per year (out of 13,000 total civilian and military aircraft produced) to over 96,000 military planes per year—a sixteen-fold increase in output Craven and Cate (1955). During that time, industrial output had nearly tripled. Employment in the chemical- and metal-working sectors had nearly tripled from about three million to nearly eight million, while the other industrial sectors expanded only slightly from the 1939 base employment of five million. The once-tiny aircraft manufacturers increased their employment fourteen-fold. Annual government purchases of these goods amounted to nearly half of the size of the entire US economy in 1939.

While the government was the source of demand for materiel, production was rarely done by public employees; rather, the vast majority of production was done by the private sector under contract. However, few firms had the capacity to meet demanded output levels of any war good. Most contracts required expansion of productive capacities to some extent. In some cases, firms simply enhanced existing plants to increase output of pre-war goods (like canned food, uniforms, or iron). In other cases, producing war goods required complete conversion and retooling of factories to make an entirely new good—automobile plants converted to make airplane engines,

1While the automobile sector had grown since the dawn of the century, it was still dwarfed by these other, more traditional manufacturing sectors; and, while some now-famous aircraft companies had opened doors before the outbreak of the war, aircraft production comprised a trivial part of American manufacturing capacity in 1939.
Appliance companies started making guns, and a Quaker Oats plant even started making TNT. Yet, some orders called for such large-scale output of new types of products that a completely new plant was required.

Given the urgency of the mobilization and the need to make dramatic changes to product lines, competitive bidding was ruled out from the start. Instead, “cost-plus-a-fixed-fee” contracts were directly negotiated by a wide array of government military agencies with manufacturing firms. The War Production Board (along with its predecessors, the National Defense Advisory Committee and the Office of Production Management) was established to help these myriad agencies connect with firms that had the capabilities to take on major projects without creating bottlenecks or misallocations of resources. Generally, this involved directly approaching the large metal and chemical product manufacturing companies and negotiating what a reasonable payment would be to get the job done quickly. During the process, government agencies and firms negotiated what kind of plant expansion was necessary to fulfill a given order, who was responsible for financing the expansion, and where the expansion would take place White (1980).

By the end of the War, an incredible amount of new manufacturing capital had been put in place. The value of plant construction put in place to support various materiel orders amounted to over $20 billion in 1940 dollars (approximately $300 billion in 2015 dollars)—that amounts to over twenty percent of 1939 GNP, and approximately fifteen percent of all war outlays. This is a large increase compared to the outstanding manufacturing capital stock at the dawn of the war, which was valued at approximately $40 to $60 billion dollars ($1940). Of the over $20 billion spent on plant expansions, the majority was spent on brand new factories—especially very large new factories Board (1944, 1945a). Notably, just three hundred large plants account for ten billion dollars of

\[ \text{Note: } 2 \text{One should compare this to the recent American Recovery and Reinvestment act, which authorized $111 billion dollars for infrastructure construction. Even after adjusting prices, the real economy has expanded over sevenfold since WWII—thus, the $111 billion is a dramatically smaller intervention relative to the size of the economy than the WWII plant expansion. One might also compare this to about $15 billion (1940 dollars) spent on public works during the New Deal from 1933-1939 (under the PWA and WPA).} \]

\[ \text{Note: } 3 \text{Civilian Production Administration (successor to the WPB) estimates place total wartime investment in plant and equipment through the end of 1945 at $23 billion. They estimate of 1939 manufacturing plant excluding land value as $39.5 billion—amounting to an expansion worth 58.4% over the pre-war plant stock, and the federally financed portion worth 43.5% of the total prewar stock. The same study notes that capital expenditures in facilities in 1939 totaled $1.2 billion, while annual private investment in war manufacturing facilities was about $1.8 billion per year on average throughout the war; the publicly financed investment—which was approximately three times the amount of private investment—was entirely supplemental.} \]
the wartime plant expansion—approximately half of the value of plant put in place. One should note that these plants were valued for their utility during the war—the valuation of these facilities declined precipitously at the end of the war, since many of the machines needed to be replaced, the locations were not ideal, and repurposing would be costly. Yet they were not completely without value—most plants built during the war were either converted to civilian production after the war or put to continued use for defense production as the Cold War began. The willingness of firms to raise private capital for plant expansions depended on the amount of risk such a plant entailed. Firms were usually eager to have the government share the cost of smaller plants with clear long-run value—for example, machine tool shops in major industrial hubs or petroleum refineries. However, firms and their financiers were much less eager to risk capital on very large industrial plants with highly uncertain post-war value—for example, ordnance factories or bomber assemblies. If the war lasted for a long time, these plants might have been highly profitable; however, an unexpectedly quick end to the war would radically reduce that profitability.

When even generous tax amortization incentives and favorable costing arrangements could not attract private capital to a project deemed necessary to the fulfillment of a crucial order, the US government financed new plants itself. In these cases, the contracted firm would typically construct and operate the facility, but the facility itself would be fully owned by the US government, usually under the auspices of the Defense Plant Corporation. At the end of the war, the plants were to be auctioned off to private firms, with right of first refusal offered to the operator. Such arrangements allowed the government to assume the full financial risk of these crucial plants.

When negotiating what new plants would be built, the US government faced important trade-offs. In general, the lowest cost option was to build smaller plants in cities with well-developed industrial infrastructure where major firms already operated—not only could these be built at lower cost,

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4. If you find the potential incentives to prolong the war alarming now, you can bet it was a subject of major concern at the time. There was widespread concern about war profiteering throughout the war, and Congressional committees (most notably the Truman Committee) were continuously holding hearings to ensure that no one had a pecuniary interest in prolonging the war.

5. Business interests and anti-New Dealers were highly concerned about government ownership of productive facilities that might potentially compete with private interests in the post-war. The authorizing legislation therefore required that plants only be operated by private firms after the war, but liquidated in such a way that allowed incumbent firms to get a foothold in the postwar civilian economy first.
they were most likely to receive private investment. However, despite the low cost, such specifications were spurned by the government when it came to the largest plants. The goal was reach maximum capacity as quickly as possible, given available resources and labor, which required large plants. Production airplanes typically required that plants be built along large airfields, which required rapid assembly of the sorts of large parcels that were difficult to find in major cities. And high output alone was not enough. Interruptions to the production of crucial materiel posed a major threat to the war effort—therefore, security was a paramount concern. Concentration of industry in large hubs posed major risks, both because of the vulnerability of a single city (e.g. bombings or power outages) and because of service interruptions due to urban congestion. Location of plants along coasts and borders raised the specter of bombing raids; hence, although many key industries were concentrated in coastal cities (aircraft, in particular), the military urged that all new expansions take place two hundred miles or more inland, if possible Craven and Cate (1955). Larger plants outside major industrial centers, which satisfied these security and short-run efficiency concerns, were both more expensive to build and less obviously valuable for post-war use.

Thus, private firms often could not be convinced to put up a single dime to finance the largest plants that the US government desired built. After the war, Air Force historians noted that “The industrialists’ reluctance to invest in dispersed plant facilities was at odds with the government’s hope that private capital could finance new inland construction; Hence, the War department could carry out its policy only to the extent that the government was willing to put up the money” Craven and Cate (1955).

Nonetheless, in many cases the military decided that such plants were sufficiently necessary to justify full government financing, even at extraordinary cost. Large bomber assemblies, ordnance

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6A December 1941 letter by Major T.A. Sims, Assistant Technical Executive and later Deputy Chief of Staff in the Army Air Force Material Command (which oversaw aircraft procurement), suggested that aircraft producers that had factories along the coasts should construct new modification centers in the interior to ensure continuous operations: “It is obvious that our aircraft factories located along the coast lines are going to be working under unfavorable conditions, such as blackouts and wide dispersion of their products just as soon as it becomes flyable. ... It is therefore proposed that we face this situation on a semipermanent basis, and require that each airframe manufacturer within 200 miles of our oceanic coastline establish an inland modification and dispersal base to which flyable airplanes awaiting the completion of certain installations to make them completely acceptable articles can be flown and completed at the inland modification base.” Similar standards were later applied when entirely new aircraft assembly plants were necessitated by the expansion.
works, aluminum and steel plants, chemical processing facilities, and other large plants were built in small cities in small cities and large towns that had little history of large scale chemical processing or metal working. Given that these small cities had few major advantages (besides basic amenities like transport connectivity and water sources, which many small cities had), the actual choice of location was driven by fairly idiosyncratic concerns. Perhaps the foremost was to find a parcel spanning hundreds of acres that could be purchased or seized with minimal difficulty. Often times, entrepreneurial federal representatives and local officials in remote areas that could not attract investment in normal time would offer military officials full disposal of local public services and even free land if they located war plants in their jurisdictions. This willingness to accommodate helped attract plants given the needs to build dispersed plants in the war—however, such incentives had failed to attract private peacetime investment.

Beyond the need to produce ordnance and airplanes in secure locations, the wartime expansion also required major short-run increases in the supply of particular materials to specific regions. The long-run value of these resources were unclear, so the federal government frequently had to finance plants to rapidly expand production. For example, aluminum and magnesium were important components of aircraft and ordnance, but less commonly used in standard commercial products of the time. The largest manufacturing plant in the history of the State of Nevada, the Basic Magnesium Plant, was built in 1941 on the outskirts of Las Vegas in a location close to both the Magnesium mines at Gabbs and the water and power provided by Lake Mead and the Hoover Dam White (1980). The plant cost $126 million to build in contemporary dollars—nearly $2 billion in 2015 dollars. While magnesium was rarely used in manufactured products prior to the war, producers learned about various uses of magnesium through the experience of the war, and the Basic plant was able to covert to civilian production after the war. The plant is still in operation today; and the settlements surrounding the plant became the city Henderson, which is still the second largest municipality in the State of Nevada.

Another example of the government stepping in when private finance would not is the case of western production of steel. The steel industry in the Eastern and Midwestern heartland was relatively mature, and most industries that used steel products were close by and easily accessible
by rail. However, World War II brought about both a pressing need for ship construction on the
West coast to fight in the Pacific Theater and the closing of the Panama Canal due its vulnerabili-
ties. Shipping steel in bulk from the forges of Ohio and Pennsylvania to California, Oregon, and
Washington States was impractical (particularly given needs to supply the East coast shipbuilding
effort). Thus, the federal government supported a massive expansion of steel production facilities
in the heartland of the West at sites that were close to major ore deposits and easily accessible
from major Pacific shipbuilding centers. The Geneva steel mill built in Utah (near Provo), which
opened in 1942, was the largest steel plant ever built west of the Rockies. Whetten notes that while
private financiers had seen little prospect in such a large steel plant in Utah, the federal govern-
ment stepped in for reasons of short-run necessity: “The officials at the OPM did not aim to foster
regional industry or to bring the American West out of the third world and into the first; they
simply wanted to address national defense contingencies and the supply and demand issues that
loomed ahead of the attack on Pearl Harbor” Whetten (2011). These priorities created a unique
opportunity for political entrepreneurs to attract investment, even when efforts to attract private
capital had come up empty handed. Whetten notes that: “Local powers in Utah County attempted
to both facilitate and benefit from federal use of power. They were not a colony that accepted fed-
eral choice and watched powerlessly, and they were not capitalists who spent their own capital
to build the plant. ... Local businessmen and politicians tried to both support and steer federal
decisions by suggesting locations, adapting local infrastructure, and attempting to sway public
opinion” Whetten (2011). Similar stories of political entrepreneurs attracting federal investment
during this unique wave of public investment were common among the other federally-financed
construction projects. Yet, without a major government intervention in manufacturing plant in-
vestment, such efforts would have likely borne little fruit.

These large, new plants that no firm was willing to stake a dime on are the focus of this research.
While these plants were not located at random—plants so expensive have never been located at
random—the reasons for their construction and siting had very little to do with forecasts of long
run profitability in peacetime markets. If the government had not stepped in to finance their
construction, it is unlikely that similarly large plants would be have been built in those same
locations.

Large plants have never been randomly built. However, the construction of these plants the closest thing to a random assignment of major industrial infrastructure works that has ever occurred in the Western world at such a large scale.

3.3. Data and Research Design

3.3.1. Treatment Notion

To study large plants, I draw from a rich, newly-digitized War Production Board (WPB) database of all substantial capital expenditures made for the sake of war production. Although procurement contracts were negotiated by more than a dozen distinct government and military offices, all contracts for the production of war goods were reported the War Production Board and all requests for public funds, tax write-offs, and other public resources for plant expansion required WPB approval, so as to ensure efficient use of scarce materials and elimination of redundancies. Hence, the WPB was able to compile administrative databases detailing the placement of each supply contract and the universe of investments with a war rationale.\(^7\) This paper draws from a 1945 WPB data book, *War Manufacturing Facilities Authorized Through October 1944 by General Type of Product Operator Board* (1945b).\(^8\) The data book has plant-level detail on each plant’s operator, the 1939 industry of the operating firm, the city in which the plant is located, the plant’s war products and output volumes as specified in the operator’s contract, the date of completion, and the cost of facilities expansion. The cost data is subdivided into privately financed and publicly financed amounts, and those amounts are further subdivided into expenditures on structures and equipment.

\(^7\)All capital expansions with a war rationale were eligible for tax incentives or public funds, which required WPB approval. All plants involved in war production sought these incentives, and WPB approval was almost never denied, since the requests occurred after military procurement agencies had completed negotiations. One should note that use of scarce construction inputs (steel, iron, concrete, etc.) was highly restricted in the civilian sector during the war, so these plant investments account for the universe of industrial construction during 1941-1945.

\(^8\)Very few new projects were authorized in the final year of the war–these data account for approximately 90% of all authorizations.
While the data document plant expansions at tens of thousands of establishments, the greatest part of the plant expenditure was spent on a handful of major expansions. Approximately half of the expenditure on plant expansions was spent in only three hundred large plants. Figure 3.1, which plots the distribution of expenditure amounts across plants on a log scale, illustrates how thick the upper tail is. I use these data to identify large, publicly-financed, new plants. I define these as plants costing at least $1 million ($1940) built at new sites, for which one-hundred percent of investment in durable, immobile structures was publicly financed. There are 582 plants fitting...
this definition in the data. However, even within this group the bulk of expenditure occurred in a small subset of very large plants.

**Table 3.1:** New Public Plants in Total War Expansions

<table>
<thead>
<tr>
<th>Category</th>
<th>Cost ($Mil)</th>
<th># Establ</th>
</tr>
</thead>
<tbody>
<tr>
<td>All Cap Ex &gt; $1 Million</td>
<td>20,597</td>
<td>12,900</td>
</tr>
<tr>
<td>New Plants</td>
<td>12,262</td>
<td>6,278</td>
</tr>
<tr>
<td>No Private Capital</td>
<td>6,763</td>
<td>582</td>
</tr>
<tr>
<td>In Treatment County</td>
<td>4,277</td>
<td>130</td>
</tr>
<tr>
<td>(With Pre-war Mfg Data)</td>
<td>3,825</td>
<td>118</td>
</tr>
</tbody>
</table>

In the county-level analysis, the “treatment” notion is having received extremely large investments in new public plants. In practice, I define a county as “treated” if it had over $500 per 1939 employee in expenditure on new public plans costing over $1 million. Only 209 counties have any large new public plants. The core treatment notion narrows to the focus to the 99 counties in which there were dramatically large plant openings (relative to the baseline size of the labor market).11

**Figure 3.2:** Location of Large Publicly-Financed Plants

Notes: Map displays location of large, publicly-financed plants in main sample.

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11 The only difference between using total spending and per-worker spending to define the threshold in practice is that very large cities (such as Chicago and Detroit), which were orders of magnitude larger than other cities prior to the war, are not counted as “treated” simply by virtue of their size.
The cutoff is chosen as approximately where the “thick tail” begins, as shown in Appendix Figure C.2. In the 99 counties above this cutoff, a single enormous plant typically accounts for the overwhelming majority of expenditures on large new publicly-financed manufacturing facilities. Figure 3.2 maps these 99 counties and the industry of the largest public plant. These large plants produced aircraft and ordnance (both typically inland); dramatically expanded capacity to produce key metallic inputs to aircraft (aluminum and magnesium) and ships (steel)—particularly inland off the Pacific seaboard; and supplied chemicals that were used for explosives, fuel, and synthetic rubber. Table 3.1 reports that the 130 large publicly-financed new plants in the 99 treatment counties account for nearly one-fifth of all plant investment during the war.

3.3.2. Empirical Specification and Identification

As discussed above, all available evidence suggests that the 100% publicly-financed war plants were sited for highly idiosyncratic reasons. The historical evidence suggests that no firm would have constructed such a large plant in that location if not for circumstances created by the war. In what sense, then, might this provide as-good-as random variation across counties? The experimental framework I propose is to compare two observably similar counties, neither of which would have likely received a large plant opening in the counterfactual world where no war occurred, but only one of which received a plant due to circumstantial concerns raised by short-run strategic war needs. If the only reason one county got a plant and the other similar county did not is due to war-specific advantage—that is, features that were relevant for plant siting during the war, but would not have affected the path of industrial and labor market development otherwise—then the latter can be used to infer the counterfactual potential outcomes of the former if it had not been “treated.”

I identify the causal impacts of plant openings on several county-level output and labor-market using event-studies, which compare treated counties to comparable controls before and after the war. Because many observably underpopulated or agrarian counties could not have supported

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12 I initially employ a binary treatment rather than a continuous one for clarity in light of the large number of zeros and the highly skewed distribution of expenditures. I explore heterogeneity effects corresponding to differential treatment intensity later in the paper.
plants in any scenario, and other large cities would have doubtless received major manufacturing
expansions in any scenario, I allow for selection into treatment on certain 1930-1935 observables\textsuperscript{13}. The assumption is \textit{not} that plant assignment was random between the two observably similar counties. Rather, it is that conditional on a core set of pre-war observable characteristics, the remaining sources of variation in plant assignment are statistically independent of other latent determinants of post-war economic development.

I estimate the treatment effect in pre- and post-war years under this assumption using two approaches. My primary approach is to estimate linear regression equations for each outcome year $t$ of the form:

$$\ln Y_{it} = a_t + \beta_t \text{TREAT}_i + \gamma_t X_{i1930} + \delta_{st} + \epsilon_{it}$$

(3.1)

where $\delta_{st}$ is a state fixed effect for the specific outcome year, $X_{i1930}$ is the 1930s vector of observable covariates, and $\beta_t$ is the year specific treatment effect. Under the linear effects selection-on-observables assumption (conditional independence)

$$\epsilon_{it} \perp \text{TREAT}_i | X_{i1930}$$

(3.2)

each $\beta_t$ is identified. When the OLS is implemented, I exclude counties with missing pre-war data on the core outcomes (including those with no reported manufacturing) from the analysis, but include all other counties—yielding 1483 counties total. Rather than define an explicit control group, this approach relies on the linear structure of the model to partial out the effects of confounding covariates.

Second, I relax the linear and constant effects assumptions to estimate average treatment effect on the treated (ATOT) using propensity score weighting. This essentially amounts to creating a synthetic control group based on the selection variables, and non-parametrically estimating the

\textsuperscript{13}The control variables include the following 1930 variables, measured both in logs and per-capita levels, and the squares of each transformation: Population; Employment; Population Density; Manufacturing Value Added, Employment, Payroll, Average Earnings, Employment of Production Workers, and Average Production Wage; Retail Employment, Payroll, average Wages, and Sales; Value of All, Owner-Farmed, Manager-Farmed, and Tenant-Farmed land; Wholesale Wages, Employment, and Stocks; Black Population; Foreign Born Population; Urban Population; and Workers Laid Off. I also include certain important 1930s government spending variables from the New Deal, specifically: 1933-1935 and 1933-1939 Log Total Public Works Spending, 1933-1937 AAA Road Construction Grants, and 1937 Log Emergency Workers. Because no 1930 data is available, I also include one potentially important 1940 variable: Log Median Housing Value, as one might be concerned if housing prices were not accounted for in the selection-on-observables assumption.
treatment effects. To do this, I first estimate a probit regression of $TREAT_i$ on the same covariate set $X_i^{1930}$, that is:

$$\Pr(Treat_i = 1|X_i^{1930}) = \Phi(\beta X_i^{1930})$$

(3.3)

I estimate propensity scores $\hat{p}_i$ as the the predicted treatment probabilities from the fitted model $\hat{p}_i = \Phi(\hat{\beta} X_i^{1930})$. All observations with propensity scores outside the overlap region (almost the entirety of which are control observations with propensity scores below the range observed in the treatment group) are trimmed from the sample, as they have no comparable counterpart in the opposing sample. I define the propensity score weight $W_i$ as equal to one for all treatment observations, and equal to $W_i = \frac{\hat{p}_i}{1 - \hat{p}_i}$ for the remaining controls. Hirano and Imbens (2005) note that under the selection on observables assumption, the propensity score weighting estimator

$$ATOT = \frac{1}{N_{Treat}} \sum_{i:Treat_i=1} Y_{it} - \frac{\sum_{i:Treat_i=0} Y_{it}\hat{p}_i}{\sum_{i:Treat_i=0} 1 - \hat{p}_i}$$

(3.4)

is a consistent estimator of the average treatment effect on the treated. One reason for using this technique is that one can transparently assess which counties are contributing most the (synthetic) control group that is being used and also assess whether they are indeed a valid control group using tests of covariate balance. It is also possible to show the control counties on a map. The map in Panel (b) of Appendix Figure C.1 displays both the treatment and control counties that are not outside the overlap region. There are 83 treatment counties in the trimmed sample (each with weight 1) and 1,051 control counties in the trimmed sample, where the sum of weights in the control group is 80.7. The map divides counties into four differently-shaded bins such that each bin has equal amounts of weight (rather than split control counties into weight quartiles with equal numbers of counties). Thus, the sum of weights of the 32 counties in the highest weight bin is the same as the sum of the weights of the 70 counties in the next weight bin. This is meant to highlight the extent to which a relatively small number of counties disproportionately drive the control group average, as intended in the reweighting method.

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14In the canonical treatment effects literature, where county $i$’s potential outcome if treated is $Y_1$, and its potential outcome if untreated is $Y_0$, the conditional independence assumption can be formalized as $Y_0, Y_1 \perp TREAT_i | X_i^{1930}$. 
Table 3.2: Covariate Balance: Treatment Versus Control Mean Differences

<table>
<thead>
<tr>
<th>Variables in Propensity Score</th>
<th>Unweighted $\Delta \bar{Y}$</th>
<th>$t$-stat</th>
<th>Reweighted $\Delta \bar{Y}$</th>
<th>$t$-stat</th>
</tr>
</thead>
<tbody>
<tr>
<td>1930 Log Population</td>
<td>0.48</td>
<td>5.28**</td>
<td>0.01</td>
<td>0.15</td>
</tr>
<tr>
<td>1930 Log Employment</td>
<td>0.50</td>
<td>5.33**</td>
<td>0.01</td>
<td>0.19</td>
</tr>
<tr>
<td>1930 Log Manuf Value Added</td>
<td>1.04</td>
<td>5.38**</td>
<td>0.02</td>
<td>0.24</td>
</tr>
<tr>
<td>1930 Log Production Workers</td>
<td>0.92</td>
<td>4.99**</td>
<td>0.03</td>
<td>0.33</td>
</tr>
<tr>
<td>1930 Log Avg Production Wage</td>
<td>0.14</td>
<td>3.93**</td>
<td>0.00</td>
<td>-0.03</td>
</tr>
<tr>
<td>1930 Log Retail Per Capita</td>
<td>62.547</td>
<td>4.16**</td>
<td>-2.56</td>
<td>-0.31</td>
</tr>
<tr>
<td>1930 Log Population Density</td>
<td>0.50</td>
<td>4.52**</td>
<td>0.00</td>
<td>-0.01</td>
</tr>
<tr>
<td>1930 % Black</td>
<td>-1.63</td>
<td>-0.85</td>
<td>0.47</td>
<td>0.55</td>
</tr>
<tr>
<td>1930 % Foreign Born</td>
<td>0.85</td>
<td>1.24</td>
<td>0.32</td>
<td>-0.92</td>
</tr>
<tr>
<td>1930 % Urban</td>
<td>15.41</td>
<td>6.08**</td>
<td>0.56</td>
<td>-0.41</td>
</tr>
<tr>
<td>1930 % Laid off</td>
<td>0.12</td>
<td>1.98**</td>
<td>0.00</td>
<td>0.13</td>
</tr>
<tr>
<td>1933-1935 Log Fed Public Works Spnd</td>
<td>0.21</td>
<td>3.52**</td>
<td>0.02</td>
<td>0.78</td>
</tr>
<tr>
<td>1937 Log Emergency Workers</td>
<td>0.62</td>
<td>5.32**</td>
<td>0.00</td>
<td>-0.05</td>
</tr>
<tr>
<td>1940 Log Median Housing Val</td>
<td>1.04</td>
<td>5.38**</td>
<td>0.02</td>
<td>0.24</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Balancing Variables Excluded from Score</th>
<th>Unweighted $\Delta \bar{Y}$</th>
<th>$t$-stat</th>
<th>Reweighted $\Delta \bar{Y}$</th>
<th>$t$-stat</th>
</tr>
</thead>
<tbody>
<tr>
<td>1940 Log Population</td>
<td>0.49</td>
<td>5.39**</td>
<td>0.01</td>
<td>0.11</td>
</tr>
<tr>
<td>1940 Log Employment</td>
<td>0.50</td>
<td>5.13**</td>
<td>0.00</td>
<td>0.03</td>
</tr>
<tr>
<td>1940 % of Workers in Manuf</td>
<td>3.30</td>
<td>2.40**</td>
<td>-0.15</td>
<td>-0.22</td>
</tr>
<tr>
<td>1940 Log Manuf Value Added</td>
<td>1.02</td>
<td>5.12**</td>
<td>0.02</td>
<td>0.24</td>
</tr>
<tr>
<td>1940 Log Production Workers</td>
<td>0.90</td>
<td>4.78**</td>
<td>0.03</td>
<td>0.36</td>
</tr>
<tr>
<td>1940 Log Avg Production Wage</td>
<td>0.13</td>
<td>3.19**</td>
<td>-0.02</td>
<td>-0.91</td>
</tr>
<tr>
<td>1939 Per Capita Retail Sales</td>
<td>113.36</td>
<td>3.85**</td>
<td>-1.83</td>
<td>-0.12</td>
</tr>
<tr>
<td>1939 Retail Wage</td>
<td>0.07</td>
<td>2.91**</td>
<td>-0.01</td>
<td>-1.18</td>
</tr>
<tr>
<td>1939 Service Wage</td>
<td>0.13</td>
<td>3.92**</td>
<td>0.00</td>
<td>-0.12</td>
</tr>
<tr>
<td>1940 % Black</td>
<td>-0.01</td>
<td>-0.80</td>
<td>0.00</td>
<td>0.54</td>
</tr>
<tr>
<td>1940 Log Median Contract Rent</td>
<td>0.14</td>
<td>3.23**</td>
<td>0.00</td>
<td>-0.10</td>
</tr>
<tr>
<td>1940 Log Male Clerical Emp</td>
<td>0.80</td>
<td>6.12**</td>
<td>0.01</td>
<td>0.12</td>
</tr>
<tr>
<td>1940 Log Female Clerical Emp</td>
<td>0.81</td>
<td>5.95**</td>
<td>0.00</td>
<td>0.02</td>
</tr>
<tr>
<td>1940 Log Craftsmen</td>
<td>0.74</td>
<td>5.95**</td>
<td>0.00</td>
<td>-0.03</td>
</tr>
<tr>
<td>1940 Log Craftswomen</td>
<td>0.79</td>
<td>5.09**</td>
<td>0.01</td>
<td>0.08</td>
</tr>
<tr>
<td>1940 Log Male Operatives</td>
<td>0.74</td>
<td>5.73**</td>
<td>0.05</td>
<td>0.70</td>
</tr>
<tr>
<td>1940 Log Female Operatives</td>
<td>0.75</td>
<td>4.12**</td>
<td>0.03</td>
<td>0.34</td>
</tr>
</tbody>
</table>

Notes: Figure displays raw and propensity-score reweighted mean differences in pre-war characteristics between treatment (N=81) and control (N=1,039) counties.

While the assumption of selection on 1930s observables only is strong given the availability of a wealth of 1940 data, it allows for a crucial falsification test of the identifying assumption. If
treatment is truly randomly assigned conditional on 1930 observables, then there should be no systematic relationship between the treatment and outcomes in 1939 before war broke out. Thus, every event study for outcomes with available pre-war data tests for a 1940 pre-trends. in the analysis includes

Under the conditional independence assumption and correct specification of the propensity score, one should observe covariate balance between the treatment and re-weighted control group for both conditioning variables and other pre-treatment variables that are likely not to be independent of potential outcomes. Testing for covariate balance can partially validate the identifying assumptions. Table 3.2 presents tests for covariate balance for both select 1930 variables included in estimation of propensity score and for 1940 variables that are not used in determination of the weights. While the treatment was clearly not unconditionally randomly assigned, there is no evidence of imbalance between the reweighted control group and the treatment group. However, even with no observable pre-trends, the casual interpretation could be confounded if other economic changes that occurred 1940s (for example, technological innovations that benefit certain industries) would have disproportionately affected treatment counties, in which case a spurious result might be found.

The county-level outcome data are tabulations of data from the Censuses of Manufactures, the Censuses of Business, and the Decennial Population Censuses compiled in the County Data books and in work by Haines (2005). First, I examine the effects of war plants on post-war manufacturing value added (which measures the production level), employment of production-line workers and wages of those workers, since manufacturing is the sector directly impacted by plant investment. I then turn to broader labor-market outcomes both in other sectors and the aggregate. I control for a second order polynomial in the log levels and per-capita levels of most outcome variables in 1930, as well as sets of variables collected by Fishback et al. (2005) that account for various geographical features and that measure the severity of the Great Depression and local exposure to various New Deal interventions.
3.4. Long Run Impact on Manufacturing

The primary reason one should expect that the construction of durable war plants would have lasting impact on local labor markets is that the durable industrial infrastructure is likely to attract some degree of manufacturing activity in the post-war period. Thus, I first establish that manufacturing activity expands in treatment counties relative to similar untreated counties, and that this effect appears to be causal.

The event studies in Figure 3.3 plots the year-specific ATOT effects estimated from the specification in 3.4 above for four county-level manufacturing outcomes in each year that those variables are available (the regression estimates are similar, as visible in Table 3.3). The outcomes are log value added (which is measures the production level as value of outputs produced less the cost of purchased input parts), log average annual earnings of manufacturing workers involved in production (as opposed to clerical and managerial positions), the log employment level of manufacturing production workers, and the log manufacturing establishment count. Each point estimate is obtained using 1930 and Great Depression controls, state fixed effects, and standard errors clustered at the state level. Panel (a) in Figure 3.3 displays the basic effect on manufacturing production. If plants were assigned for reasons uncorrelated with potential outcomes, there should be no evidence of a treatment effect in the pre-period. Consistent with the conditional independence assumption, there is no apparent pre-trend in manufacturing value added in either 1940 or 1920. Yet a pronounced effect appears after 1947—throughout the 1950s and 1960s, total value added is approximately 30 percent higher in counties that received war plants relative to those that did not. While this difference fades somewhat over time, manufacturing activity remains at a higher level in treatment counties throughout the balance of the century.
### Table 3.3.: OLS Point Estimates, Conditioning on 1930 and 1940 Observables

<table>
<thead>
<tr>
<th>$t$</th>
<th>Log Value Added</th>
<th>Log Production Workers</th>
<th>Log Production Wage</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\beta_1^{\text{Treat}}$</td>
<td>0.416***</td>
<td>0.275***</td>
<td>0.189***</td>
</tr>
<tr>
<td>$se$</td>
<td>(0.079)</td>
<td>(0.063)</td>
<td>(0.067)</td>
</tr>
<tr>
<td>$N$</td>
<td>1,483</td>
<td>1,483</td>
<td>1,481</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.925</td>
<td>0.863</td>
<td>0.780</td>
</tr>
</tbody>
</table>

Notes: Sample includes all counties with manufacturing wage data in all pre-war years and all three outcome years listed. Each point estimate is the coefficient on the Treatment dummy in a separate OLS regression corresponding to the outcome in a given year. A county is “treated” if it had over $500 per 1939 employee in expenditure on new public plans costing over $1 million. Standard errors are clustered at the state level. Specifications control for 1930 logs and per-capita levels of population, employment, manufacturing wage/employment/value added/establishment variables, retail employment/wages, wholesale employment/wages, black population, foreign born population, value of farm land, and total area; as well as New Deal public works spending in the 1930s, maximum elevation and elevation range, number of extremely wet and dry months in 1930, dust bowl severity, and indicators for location on coasts and major rivers. These specifications also control for the 1939 or 1940 levels of all available outcome variables used in the paper.

The lack of a full treatment effect in 1947 is to be expected. Unlike private plants, which raced to reconvert to civilian production after V-J day, public plants could not immediately begin civilian operation. Because these plants were owned by the government under military sponsorship, the military had to first deem these plants surplus and no longer necessary for immediate defense purposes before they could be sold or leased. Following the surplus declaration, plants were auctioned off by government bureaucracies designed to assure the public that they were getting a good deal and to assure big business that they were not trying to flood any particular market with cheap capital. Once a deal had been agreed to, the plants needed to be re-equipped for production of civilian goods. As a result, few large public plants plants began civilian production before 1947—and many plants only began civilian operations even later. Thus, few treatment plants would have been operating at full capacity until the very end of the decade Administration (1947); White (1980).

One would expect a rise in manufacturing production to increase labor demand for production workers, which should be reflected in either wages or employment levels. Panels (b) and (c) show that while treatment counties pay similar wages to production works and hire similar quantities before the war, there is a pronounced boost to local production employment and wages in the
post-war period. Wages are approximately 5 to 10 percent higher in counties that had a publicly-financed plant built during the war—this effect appears to be permanent. This result is inconsistent with simple models with homogenous production labor, homogenous production tasks, and perfect mobility across locations and jobs—however, many types of frictions (geographic mobility costs, search frictions, efficiency wages, rent-sharing, etc.) and sources of heterogeneity (across latent skills or task qualities) could lead to such a result. Employment of production workers is over 20 percent higher in the 1950s and 1960s, but converges back slightly later in the century. The effects on establishment counts are less clear; but given the bias of war production towards large firms, it is not clear whether one should have predicted any particular effect.\textsuperscript{15}

The absence of pre-trends across all outcomes with notable treatment effects is crucial to the interpretation of these results as causal. One may wonder whether the absence of pre-trends only arises due to a specific choice of specification—however, the absence of pre-trends is a fairly generic result. In Appendix Figure C.3, I plot the raw difference in mean outcome levels between the treatment counties and the 211 “control” counties discussed in the previous section (I also plot the level trends for each group). Notably, the gap between treatment and control groups is essentially flat and very close to zero throughout the pre-war period, despite the fact that these plots do not condition on anything.\textsuperscript{16} Even more strikingly, when one plots the raw difference in mean outcome levels between the treatment counties and all other counties with some pre-war manufacturing, the gap is mostly flat throughout the pre-period, despite the fact that these plots do not condition on anything or limit to comparable counties. This is consistent with no differential trends in treatment counties, although both treatment and “comparable counties” are substantially larger than the average county (of course, the unweighted average of all counties is highly influenced by a large number of mostly rural counties).

While the absence of pre-trends does not prove causation—contemporaneous shocks and unobserved latent trends could still be at play—the finding of no pre-trends is also not trivial. Many as-

\textsuperscript{15}To interpret the magnitude of these effects, the median treatment county had 5566 production workers employees in 1954, which accounts for roughly one-fifth of employment in the typical treatment county. 20% fewer production employees in the typical treatment county would amount to over one thousand fewer jobs.

\textsuperscript{16}Because these specifications control for nothing, the point estimates are fairly noisy. With controls the point estimates remain similar, but the standard errors shrink notably.
pects of the economic mobilization (contract placement, privately-financed expansion, etc.) were associated with latent trends that were occurring regardless of the events of the war, and those trends are evident in the pre-war period. One focal example are the war plants that were constructed with some degree of private financing. *A priori*, one would think these plants were sited with long-run, post-war forecasts in mind. I conduct the following exercise: I change the treatment definition to include only the 100 counties with the most per-1939-employee spending on new war plants that were *privately financed at least in part*. I then replicated Figure 3.3 using this altered treatment definition. The results are plotted in Appendix Figure C.4. While these “private-finance treatment” counties did experience a persistent post-war increase in manufacturing, they also experienced significantly higher growth in the pre-war years. These findings suggest that the assignment of privately-financed plants reflected underlying trends, and did not necessarily cause the full post-war increment.

Select point estimates from OLS regressions are displayed in Table 3.3. Although one must exclude 1940 conditioning variables to examine a pre-trend, including those conditioning variables can increase power given that the identification is valid. Thus, Table 3.3 includes controls all specifications also control for the 1940 level of all available outcome variables. If the initial selection-on-1930’s-observables assumption is valid, inclusion of these controls should not significantly effect the point estimates—this is consistent with the results obtained.

One might wonder whether the treatment effects are more pronounced in treatment counties that received larger plant investments (relative to baseline size of the local economy). To investigate this, I divide the 192 counties in the analysis sample with at least one large publicly-financed plant into four equal-sized bins corresponding to treatment intensity quartiles (that is, quartiles of spending on large public-financed war plants per 1939 employee), and estimate the effect by quartile in the following regression specification

$$\ln Y_{it} = a_t + \sum_{q=1}^{4} \beta^q_t \text{TreatQuartile}_{iq} + \gamma_1 X_{i1930} + \delta_{st} + \epsilon_{it}$$

The point estimates\(^{17}\) for the three primary manufacturing outcomes are plotted in Figure 3.4.

---

\(^{17}\)Standard error bands are not displayed due to space constraints; however, the point estimates for the smaller, 46-county quartile bin dummies are noisier than the baseline results.
While the pattern of effects are similar across quartiles for all outcomes, the effect size is generally increasing monotonically in intensity level and the largest effects are concentrated in the top quartile. Thus, it does appear that larger investments are associated with larger long-run effects on local manufacturing production and employment.

Though the mere presence of sizable post-war manufacturing effect may not be surprising given the physical presence of a large plant, the persistence of these effects is of note. The increments to manufacturing employment and activity in treatment counties outlasts any realistic depreciation horizon for the initial war-time infrastructure. Such persistence implies that either the operating firms continued to reinvest and upgrade these former war plants throughout the post-war period, or other firms made additional investments in the locale of the former war plant that outpaced investment in untreated counties. While county-level data on pre-war manufacturing capital expenditures is not currently available, Figure 3(e) shows that post-war manufacturing capital expenditures was indeed substantially higher during the post-war period in counties that had received a publicly-financed war plant.

3.5. Long Run Impacts on Local Labor Markets

Does higher post-war manufacturing employment and wages spill over to broader employment outcomes? Figure 3.5 plots propensity-score event studies for five broader labor market outcomes for which pre- and post-war data are available. Panels (a) and (b) plot effects on (log) aggregate population and employment levels. Both display similar patterns—minimal differences between treated and untreated counties in 1940, but somewhat higher levels peaking in 1960 (though these estimates are imprecise). The employment point estimates never exceed 10 log points, and the population effect estimate barely surpasses 5% at its peak.
Panels (c) and (d) provide detail on retail and service employment—both of which reflect employment in relatively non-tradable sectors. The results are highly imprecise, but are consistent with sectoral employment growth that is proportional to the broader employment and populations effects (between 5-10% higher). It appears, then, that manufacturing employment growth outpaces growth in other sectors. Panel (e) confirms that manufacturing’s share of employment does in fact grow in treated counties—rising by about 3 percentage points relative to untreated counties in 1960. Table 3.4 shows that OLS point estimates are generally consistent with results from the propensity score reweighting estimator employed in Figure 3.5.

| Table 3.4.: OLS Point Estimates, Conditioning on 1930 and 1940 Observables |
|-----------------|-----------------|-----------------|
|                | Log Population  | Log Employment  | Log Median Family Income |
| \( \beta_{t}^{Treat} \) | 0.051*** | 0.069**  | 0.041  | 0.041  | 0.102*** | 0.059  | 0.030*  | 0.035*** | 0.005  |
| se              | (0.018) | (0.033) | (0.045) | (0.029) | (0.036)  | (0.051) | (0.016) | (0.012) | (0.015) |
| N               | 1,483  | 1,483  | 1,483  | 1,483  | 1,483    | 1,483  | 1,483  | 1,483  | 1,483  |
| R²              | 0.989  | 0.947  | 0.905  | 0.988  | 0.951    | 0.898  | 0.924  | 0.838  | 0.742  |

Notes: Sample includes all counties with manufacturing wage data in all pre-war years and all three outcome years listed. Each point estimate is the coefficient on the Treatment dummy in a separate regression corresponding to the outcome in a given year. A county is “treated” if it had over $500 per 1939 employee in expenditure on new public plans costing over $1 million. Standard errors are clustered at the state level. Specifications control for 1930 logs and per-capita levels of population, employment, manufacturing wage/employment/value added/establishment variables, retail employment/wages, wholesale employment/wages, black population, foreign born population, value of farm land, and total area; as well as New Deal public works spending in the 1930s, maximum elevation and elevation range, number of extremely wet and dry months in 1930, dust bowl severity, and indicators for location on coasts and major rivers. These specifications also control for the 1939 or 1940 levels of all available outcome variables used in the paper.

Figure 3.6 examines broader effects on wages, income and housing prices. Somewhat surprisingly, I find that while wages rose substantially in manufacturing jobs, there is essentially zero wage effect for retail and service employees, as shown in panels (a) and (b). This suggests that either production workers earn premiums for harder work in unfavorable conditions, or the markets for production workers and service/retail employees are stratified—which could stem both from the employment of differentiated skill/occupational groups or from firms that pay collective bargaining or efficiency wages and thus are not hiring on the margin.
Nonetheless, panel (c), shows that median family incomes in treatment counties appear to be nontrivially higher during the post-war decades. Yet housing prices were not notably different, as shown in panel (d). This suggests that either incomes are growing in other sectors not examined here, or the coupled increases in earnings and employment in the manufacturing sector are large enough to make a notable dent on the aggregate income distribution.

3.6. Discussion of Results and Possible Mechanisms

The results above provide strong evidence that construction of a fully-publicly-financed large war plant had local impacts that far outlived the wartime conditions that necessitated its construction. Treatment counties had notably larger manufacturing sectors for most of the balance of the 20th century. Moreover, as employment levels expanded, so did the average blue-collar wage. Yet while these wage gains were larger enough to impact the local median income level, there was no clear boost to wages in other sectors (although there is weak evidence of small employment expansions). In fact, it is unclear whether the increase in the average wage is due to increases in wages for incumbent local workers or new external hiring of higher-wage workers. While the effects found above are of economically significant magnitude, the implications of these findings depends crucially on the specific channels through which war-time investment boosted local income and employment in the post-war era. I will briefly discuss several of the economic mechanisms that could be at play and their implications; however, at this stage, I cannot do much to rule out any specific mechanisms given available data. After discussing several rival theories, I will posit several tests that might be implemented using data I am in the process of accessing. Perhaps it is best to start with the potential explanation that is least heartening—but is nonetheless entirely plausible. It may simply be the case that these effects reflect shifts in economic activity across space with no notable aggregate or distributional impacts. The following story presents itself: an industrialist, looking to build a new plant in the post-war era, initially considers construction from scratch in a new location. However, upon learning that a large facility in a less-
than-ideal location is being offered by the federal government at a low discount price, this
industrialist decides to re-tool and utilize this high-capacity facility since the low price justifies the
imperfect location. Suppose this high-tech private establishment employs a large number of rel-
atively skilled manufacturing workers (more skilled than the typical production employee in the
locale if the war plant), who are either retained from war work or recruited to move to the locale
of the plant. Suppose also that labor and goods are perfectly mobile, land is plentiful, and there
are no productivity externalities. In this case, one would observe production employment and the
average production wage grow simply by virtue of the single large plant, which employs skilled
workers who would earn more wherever they worked, with no effect on any other aspect of the
local economy. Moreover, although none of these workers would have located locally if this plant
had not been built during the war, they would have been employed wherever else the industrialist
chose to build a new plant from scratch–any local gains to employment and earnings in the former
locality are the other’s loss.

The pessimistic interpretation above highlights several important theoretical distinctions that af-
fect the interpretation of these results. First is the existence or non-existence of non-pecuniary pro-
duction externalities; that is, benefits that the plant provides that it does not internalize in its
own revenues. The standard example are those highlighted in Alfred Marshall’s seminal text-
book: productivity enhancements via local know-how and social interactions, easier searches for
specialized labor due to the initial attraction of a base pool of skilled laborers, access to dense
markets at low cost, or construction of high-fixed cost core infrastructure that benefits other busi-
ness. These non-pecuniary externalities should boost productivity of other local firms at no cost,
leading to increased entry and heightened productivity among incumbents; in closed economies,
it is Pareto-improving to subsidize plant construction if these externalities exist.

But heightened employment and wages need not occur due to non-pecuniary externalities–they
might be driven by labor demand within a single large establishment. There may be pecuniary
price externalities that operate through the market mechanism–in particular, through changes
in the local wage for a given occupation group–but these only have distributional, not Pareto-
efficiency, consequences. Competitive labor markets could require all firms to increase wages,
which could diminish hiring and output at other firms while still boosting manufacturing labor income on net. Yet it might also be the case that labor markets are not perfectly competitive, such that wages are not set at marginal product. For example, if the new firms have strong unions that achieve a stake in the surplus from production, or if large plants require on-site training on product-line specific knowledge that given incumbent employees a productivity advantage, then higher wages at the new plant may not translate into higher wages at other plants since the new high-wage plant will only be willing to hire a set number of workers. Another form of pecuniary externality would be an increased demand for local services resulting from the increase in local manufacturing incomes, which could boost employment and/or wages depending on local labor supply responses (I find evidence of the former, but not the latter). Again, such localized pecuniary externalities might be small if most spending is on nationally or globally tradable goods and services, and even then the existence of these market-mediated effects do not evidence any market failure.

Since I only observe aggregate earnings and employment data in the current outcome data, I cannot determine whether the employment and wages effects I find are evidence of true non-pecuniary agglomeration spillovers, market-level impacts, or simply a reflection of activity at one large plant. If there are agglomeration effects, then the plants were an unambiguous win for local counties—even beyond the profits they provided to their owners. If there are increases in market wages for given types of labor (or new high-paying job opportunities for the kinds of workers that would have worked at other places in the locale), then although the size of the local pie would not grow more than the surplus produced internal to the plant, there may have been shifts that disproportionately benefited local labor. However, if there were no externalities or local market-level effects, and all wage increases simply reflect workers with specific skills or tolerance for industrial work moving into the county to take jobs at the plant, then there are no direct gains to local residents—beyond the basic advantages of having a larger population and tax base, perhaps.

Yet, the example above illustrates a further, crucial distinction. Even if externalities and market-mechanism effects were present that rationalized policy intervention from the perspective of local authorities, these plant openings may not have been net gains from a national perspective.
if they simply shifted activity away from another locality. From the perspective of the federal-government, this would appear as near-perfect crowd-out. Any productivity spillovers and wage effects gained in the winning county are spillovers and wage increases lost in the county that was “crowded-out.” This point about the general equilibrium consequences of place-based policies was examined in depth in Kline and Moretti (2014a), who note that the gains in the “winning” county have to be disproportionately larger than the losses in the county that was “crowded-out” to justify public influence in where manufacturing investment takes place (for economic, not military-strategic reasons). Thus, beyond determining the form of local spillovers, it is important to infer the extent to which investments would have been made elsewhere in the counterfactual scenario before one makes conclusions about the aggregate economic benefit of local investments.

With plant-level data on productivity, output, payroll, employment, and investment, it is possible to empirically distinguish between pure productivity spillovers, price effects, and internal economies. Each of these contingencies yield different predictions about how other local firms should behave, compared to the former war plant. If productivity, scale, and wages grow at incumbent competitor firms or entry of competitors increase nearby, such a finding would constitute evidence of agglomeration externalities. If wages rise at other firms, but productivity and scale fall, then this suggests that the dominant force is heightened competition in competitive markets. If outside firms are entirely unaffected, then this suggests either that former war plants do not hire at the margin or that localized economies do not matter much. In order to implement such tests, I would need to construct longitudinal plant-level data on pre-war incumbent firms, the post-war operations of the war plants, and post-war entrants. Such a database does not exist, but I am working with the Census bureau to assess the feasibility of the construction of such a database based on newly recovered plant-level data from the 1930’s and 1950’s.

It is more difficult to distinguish between heightened opportunities for local workers and attraction of skilled workers from external regions in available data—but in theory, this can be tested. The simplest tests require panel data on pre-war residents of treated and untreated counties. If individuals who lived in the treatment county at the outset of the war (i.e. in the 1940 Census) were found to earn higher wages or to be more likely to work in manufacturing after the war regard-
less of where they lived afterwards, this would support causal beneficial impacts on local labor markets—if the test were correctly specified, such effects would not be present if the total post-war labor market effects were driven by external migrants. However, while the complete 1940 Census is publicly available, it is extremely difficult to obtain post-war individual-level datasets that can be linked to the 1940 Census at the moment.

By contrast, testing for general-equilibrium “shifting” or “crowd-out” is a fundamental challenge, both in theory and in practice. This is because in general equilibrium, an investment in one location could affect a whole host of other locations in a number of ways. From this perspective, everyone is “treated” in some sense by every plant opening. Without a clear control group, it is impossible to identify causal crowd-out effects. As a result, any inference on this subject must be guided by structural assumptions about the nature of competition across space and the types of heterogeneity and complementary that exist among different production factors and consumption goods.

3.7. Conclusion

This paper has examined the extent to which durable investments in productive capacity made as part of the World War II mobilization effort had local labor market effects that long outlived the war itself. I have argued that the highly idiosyncratic location and investment decisions concerning a specific subset of plants built during the war—large plants that were constructed in new locations with absolutely no private capital that were necessitated by short-run strategic concerns for the war—are the closest to a random “helicopter drop” of major industrial infrastructure improvements that has ever occurred in the Western world. This claim is backed up both by the qualitative history of the war and, more importantly, by robust absence of pre-trends across treatment and non-treatment counties. Using this “natural-experimental” setting, I test for the causal, long-run impacts of receiving a large industrial plant for idiosyncratic reasons. I find that post-war manufacturing output, employment, and payroll in the recipient county are markedly higher in recipient counties than in similar, untreated counties. While a short-run increment to manufac-
turing activity is not surprising, I also find that these effects are remarkably persistent over the course of five decades. These effects in the manufacturing sector carry over to aggregate labor-market outcomes, such as total employment and median wages. Yet, wages in other sectors in the recipient counties remain unaffected.

While these economically significant findings very plausibly can be interpreted as causal, it is nonetheless unclear whether there are impacts on economic actors beyond those who actually operate the former war plant, whether the war plant actually created new high-wage work opportunities for local residents, or whether such local gains came at the expense of other regions that would have received more post-war investment were it not for the construction of the war plants. I am engaged in ongoing work that will use new micro-level data to better answer these outstanding questions.

Nonetheless, these findings are important. To date, little evidence has been proffered that suggests a clear local link between wartime spending and post-war economic performance. I have shown that a clear statistical link—a link that is very likely causal—does indeed exist when one focuses on a specific type of spending: investment in durable productive infrastructure and capital. Having adopted this focus, I find that short-run strategic considerations during the war had important impacts on the economic geography of the United States in the postwar. Even though the sole imperative that drove public investment was to win the war, these durable plants nonetheless left a distinctive mark on the longer-run performance of regional economies. Thus, even though construction of these plants was planned with little regard to industrial policy, place-based labor policy, or macroeconomic policy, one might glean lessons about the costs and benefits of these policy levers. My hope is that future research on mechanisms can garner lessons for policy makers aiming to spur growth in underdeveloped regions, provide good-paying work opportunities for low-skilled workers in specific regions, and to boost aggregate productivity.
Figure 3.3.: Manufacturing Outcomes: Reweighting Estimates of TOT

Notes: A county is “treated” if it had over $500 per 1939 employee in expenditure on new public plans costing over $1 million. Sample includes 83 treated counties and 1,051 untreated counties that remain after fitting the propensity score probit model (3) and trimming the non-overlap region. Each point estimate is an implementation of the propensity-score reweighting estimator in (4) corresponding to the outcome in a given year.
Figure 3.4.: Treatment Intensity Heterogeneity: *Darker red* → Higher Intensity

Notes: Sample includes all counties with manufacturing wage data in all pre-war years and the given outcome year. There are 192 counties in the analysis sample with at least one large publicly-financed plant; these are divided into four equal-sized bins corresponding to treatment intensity quartiles (that is, quartiles of spending on large public-financed war plants per 1939 employee). Each point estimate is the coefficient on an intensity-quartile dummy, with all four quartile effects estimated simultaneously. Specifications control for 1930 logs and per-capita levels of population, employment, manufacturing wage/employment/value added/establishment variables, retail employment/wages, wholesale employment/wages, black population, foreign born population, value of farm land, and total area; as well as New Deal public works spending in the 1930s, maximum elevation and elevation range, number of extremely wet and dry months in 1930, dust bowl severity, and indicators for location on coasts and major rivers.
Figure 3.5: Broader Employment Outcomes: Reweighting Estimates of TOT

(a) Log Total Population

(b) Log Total Employment

(c) Log Retail Employment

(d) Log Service Employment

(e) Share of Employment in Manufacturing

Notes: A county is “treated” if it had over $500 per 1939 employee in expenditure on new public plans costing over $1 million. Sample includes 83 treated counties and 1,051 untreated counties that remain after fitting the propensity score probit model (3) and trimming the non-overlap region. Each point estimate is an implementation of the propensity-score reweighting estimator in (4) corresponding to the outcome in a given year.
Figure 3.6.: Broader Wage/Earnings Outcomes: Reweighting Estimates of TOT

(a) Log Retail (Annual) Wage
(b) Log Services (Annual) Wage
(c) Log Median Family Income
(d) Log Median Housing Value

Notes: A county is “treated” if it had over $500 per 1939 employee in expenditure on new public plans costing over $1 million. Sample includes 83 treated counties and 1,051 untreated counties that remain after fitting the propensity score probit model (3) and trimming the non-overlap region. Each point estimate is an implementation of the propensity-score reweighting estimator in (4) corresponding to the outcome in a given year.
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A. Appendix to Chapter 1

A.1. Theory Appendix to Chapter 1

A.1.1. Deriving Imperfect Substitutability from Incumbent Replacement Costs

This appendix illustrates how the production function in (1.1), in which incumbent labor and external labor do not enter symmetrically, can arise from a standard production function using homogenous labor with firing and hiring costs.

Consider the following version of the model above. A firm \( (j) \) that is small relative to the labor market produces revenue \( R_j \) using an mass \( L \) of workers—regardless of recency of status as a hire or incumbent—according to the revenue function \( R_j = P_j \times \bar{P} \times A_j \times \tilde{f}(L) \). Output is produced using an increaing, concave function of labor \( \tilde{f}_L > 0, \tilde{f}_{LL} < 0 \). As before, for the following exposition, we consider behavior when \( A_j \) and \( \bar{P} \) are fixed and suppress these variables. Firms inherit an stock of incumbents \( L^0 \), as well as an initial firm-specific demand level \( P^0_j \). Firms can adjust their new employment level to a choice level \( L^* \) by hiring an additional mass \( L^{out} \) of workers. We again assume firms always want to make positive hires (assuming, for example, that \( L^0 \) reflects some retirement/attrition of worker who firms intend to replace in the absence of a shock).\(^1\)

While addition hires can be costless obtained on the external market, replacing incumbents is more disruptive then other hiring. Let \( L^{inc} \) denote the number of retained incumbents. Noting that the firm wants to increase its employment level relative to \( L^0 \), if firms choose to only retain

\(^1\)While the model is not dynamic, a similar feature would occur when considering a shock to a dynamic steady state—given steady-state \( P_j \), firms always hire \( h = \delta L^0 \) to maintain steady-state \( L \). Under the assumption that shocks are never so negative so that the constraint that \( h \geq 0 \) binds, firms would always adjust employment by adjusting hiring levels.
$L^{inc}$ workers (or $L^0 - L^{inc}$ workers quit unexpectedly) firms must spend a share $\tau (L^0 - L^{inc})$ of their production time recruiting and training replacements. Let $R^*_j = P_j \times \tilde{f}(L^*)$ denote the production level given chosen employment $L^*$, the total cost is $\tau (L^0 - L^{inc}) R^*_j$;\footnote{This specific functional form of the cost is not important, but the implied assumption that the costs of adjustment are higher when $P_j$ is higher is critical, as will become apparent below.} This cost is strictly increasing everywhere in its argument $\tau' > 0$, and $\tau(0) = 0$.

These replacement frictions reflect a combination of the three kinds of barriers Doeringer and Piore (1971) cite as separating internal and external labor markets: costs of rapidly replacing firm-specific know-how acquired through experience Becker (1962); costs of having to find replacements for good incumbent matches Jovanovic (1979b); or direct institutional barriers to firing due to laws or unions Lazear (1990); Bertola and Cabellero (1994).\footnote{For example, if employees can bring suit after a dismissal, or if unionized incumbents can directly slow production at a firm via disruptive striking, then firms may lose a fraction of their output due to resulting shutdowns.} Each of these reflects an opportunity cost to replacing incumbents—forgoing production in order to retrain and recruit is costlier when product market conditions are better.

Firm profits are as follows

$$\Pi_j = (1 - \tau (L^0 - L^{inc})) \times P_j \tilde{f}(L^{inc} + L^{out}) - \bar{w}L^{out} - w_j \times L^{inc}$$

Rewriting $(1 - \tau (L^0 - L^{inc})) \tilde{f}(L^{inc} + L^{out}) = f(L^{inc}, L^{out})$, we can rewrite profits as

$$\Pi_j = P_j \times f(L^{inc}, L^{out}) - \bar{w}L^{out} - w_j \times L^{inc}$$

Thus, the model is exactly as in Section 2.\footnote{Technically, $f_1(L^{inc}, L^{out})$ is not defined for $L^{inc} \geq L^0$. However, since upwards adjustment of $L^{inc}$ is not possible in the model in the main text, this restriction does not impact the analysis or the proof of Proposition 1.}

\textbf{A.1.2. Proof of Proposition 1}

Proposition one stated that under the assumptions of the model, when $f_1(L^{inc}, L^{out}) > f_2(L^{inc}, L^{out})$,
the quasi-rent-sharing term \( \rho(P_i) \) always satisfies \( \rho(P_i) > 0 \) and \( \rho'(P_i) > 0 \). We prove the result here.

The goal is to show that \( V(L^{inc}, P_j) - V(0, P_j) - \varpi L^{inc} \) is always positive and increasing in \( P_j \).

First, consider the derivative of the optimized quantity \( V(L^{inc}, P_j) = \max_{L^{out}} \{ P_j \times f(L^{inc}, L^{out}) - \varpi L^{out} \} \) with respect to the number of incumbents \( L^{inc} \) around some initial level of \( L^{inc} \). Invoking the envelope theorem, \( \frac{dV^1}{dL^{inc}} = P_j \times f_1(L^{inc}, L^{out}) \), where \( L^{out} \) is the optimal choice of \( L^{out} \). Under the assumption that \( f_1(L^{inc}, L^{out}) > f_2(L^{inc}, L^{out}) \) for all \( L^{inc}, L^{out} \), this derivative satisfies \( \frac{dV^1}{dL^{inc}} = P_j \times f_1(L^{inc}, L^{out}) > P_j \times f_2(L^{inc}, L^{out}) = \varpi \), where the last equality results from the firm’s first order condition for \( L^{out} \).

Since \( \frac{dV}{dL^{inc}} > \varpi \) holds for all \( L^{inc} \), reducing \( L^{inc} \) to zero from any initial level would lower \( V^1 \) by strictly more than \( \varpi \times L^{inc} \). Thus \( V(L^{inc}, P_j) - V(0, P_j) = \int_0^{L^{inc}} \frac{dV}{dL^{inc}} d\lambda > \int_0^{L^{inc}} \varpi d\lambda = \varpi L^{inc} \), directly implying that \( \rho(P_j) > 0 \) for all \( P_j \).

Next, consider how the derivative \( \frac{dV^1}{dL^{inc}} \) varies with \( P_j \). In particular, \( \frac{dV^1}{dL^{inc}dP_j} = f_1(L^{inc}, L^{out}) > 0 \) for all \( P_j \) and \( L^{out} \). Thus, \( \frac{d[V(L^{inc}, P_j) - V(0, P_j)]}{dP_j} = \frac{d}{dP_j} \int_0^{L^{inc}} \frac{dV}{dL^{inc}} d\lambda = \int_0^{L^{inc}} \frac{dV^1}{dL^{inc}dP_j} d\lambda > 0 \). Since \( \varpi L^{inc} \) is invariant to \( P_j \), it directly follows that \( \rho'(P_j) > 0 \) for all \( P_j \).

### A.2. Test of Selection of Firms into Export Markets

To motivate a test of exogenous assignment of firms to differently-shocked markets, consider a simple model of selection into export markets with better outcomes. Let \( \Delta_{pc} \) be the change in total non-Portuguese imports in the market for product \( p \) in country \( c \) during the recession as defined in (1.10), and let the corresponding change in observed exports by the individual firm \( j \) to that same \( p, c \) market during the same period as \( \tilde{E}_{j,pc} \). Suppose export growth \( \tilde{E}_{j,pc} \) is determined by market level demand \( \Delta_{pc} \), unobservable firm productivity \( \phi_j \) (denoted as such as it may reflect changes in worker productivity \( \phi_i \) for workers \( i \) at firm \( j \)) that increases export performance of firms across all destinations in constant proportions, and an idiosyncratic residual \( v_{j,pc} \) according
to:

\[ \tilde{E}_{j,pc} = \phi_j + \beta \Delta_{j,pc} + \nu_{j,pc} \]  

(A.2)

In this setting, selection occurs when firms with higher levels of \( \phi_j \) tend to have relationships in markets with higher level of \( \Delta_{j,pc} \)—that is, firms experiencing larger labor productivity shocks select into relationships with customers in markets with better import demand. When this is the case, changes in the average level \( \Delta_{j,pc} \) at firm \( j \) will be correlated with unobserved heterogeneity in \( \phi_j \), confounding identification of idiosyncratic shocks.

Although this type of selection can not be directly tested in firm-level data, it can be partially tested by studying firms with multiple export destinations in the relationship-level data in a test similar to that used by Khwaja and Mian (2008) to test for sorting of borrower firms to lending banks that experienced differential credit supply shocks. Under the assumption of constant effects of supply productivity across destinations, no selection implies \( \Delta_{j,pc} \perp \phi_j \). If this is the case, then regression estimates of \( \beta \) in equation (A.2) for firms with multiple pre-period destinations should be both positive (necessary condition for a causal effect of demand shocks) and invariant to inclusion of a firm fixed effect \( \phi_j \). If inclusion of a firm fixed effect significantly reduces the estimated magnitude of \( \beta \), this would imply a positive correlation between \( \Delta_{j,pc} \) and \( \phi_j \), contradicting the no-selection condition.

Appendix Table A.2 presents estimates of equation (A.2) with and without fixed effects. The firm-by-market sample includes all firms with at least two export destinations (across product-country cells), and only includes relationships that had positive exports in the pre-period. For the benchmark estimates, the outcome is the symmetric growth rate of exports by the firm to the specified product-by-country market from 2006-2007 to 2009-2010, which incorporates export volumes of zero in the post period (which occurs in the majority of individual relationships). Import behavior at the destination is a strong predictor of exports by the firm—though the magnitude of the estimate is small, reflecting both imprecision in the relationship-level prediction and the prepon-
derance of zero outcomes. However, the magnitude is very similar with or without inclusion of the firm fixed effect, consistent with no sorting on latent productivity. The following columns separately display effects on the intensive margin (adjustment conditional on exports) and the extensive margin (probability of zero). The magnitude of the intensive margin (measured as the effect on the log change in exports conditional on positive flows) is substantially larger and approximately 40%, and the probability of terminating the relationship is also responsive to import demand conditions. The intensive margin effect is somewhat smaller when firm fixed effects are included, but the extensive margin response is offsettingly larger when firm fixed effects are included. These findings support the claim that demand conditions have a causal effect on firms’ sales to its trading partners, and that firms do not systematically sort to destinations with better demand growth based on latent productivity trends—and, therefore, also support the use of destination-level shocks to construct an exogenous firm-level demand predictor.
A.3. Appendix Figures to Chapter 1

Figure A.1.: Comparison of Sample and Population of Firms
Panel A: Firm Size Distribution

Notes: Figures plot distributions of firms in 2007 in the IES, “All firms” include all firms in the IES with positive employment, even if not reported in the matched employer-employee data. Employment bins are not equally sized. “Worker Distribution” plots present counts where firms are frequency weighted by the number of employees. “All” and “Sample” counts are plotted on separate axes.
Figure A.2.: Recession Import Growth vs. Pre-Period Growth in Foreign Markets

Notes: Figure plots the bin-average of the y-axis variable within 20 equally-sized quantile bins of the x axis variable. Observations are product-by-country markets. Y axis variable are the arc changes in non-Portuguese imports from 2006 and 2007 to 2009 and 2010 ($\Delta_{pc}$ in Equation 1.10). X axis variable is 2003-2006 arc change in non-Portuguese imports in the same markets, years are chosen so the two periods do not overlap.
Figure A.3.: Linearity/Symmetry of Wage Effects

Residualized Bin Scatter + Quadratic Fit

Idiosyncratic Shock

Average nominal $\bar{w}$ change in sample is 9.2%

Notes: Sample includes all firms with attached incumbent workers in 2007, N = 4,100. Figure plots mean residual values of the outcome within 20 equally-sized quantile bins of the residualized shock levels, where residuals are constructed to reflect all specification details of the reduced-form difference-in-difference specification in equation (1.14). The average slope line represents the differences-in-differences coefficient that would be obtained from the regression, with firms weighted by the number of attached incumbents in 2007. Also displayed is the best fit quadratic polynomial to the residuals given the specification details. Specification details are as in Table 7 (estimating the reduced-form effect of the shock on the outcome, rather than IV estimation using output as the explanatory variable).
Figure A.4.: Firm Pay Premiums and Job Durability

Notes: Figure plots the bin-average of the y-axis variable within 20 equally-sized quantile bins of the x-axis variable, including the best fit line. Sample is largest connected set of firms and full-time male workers over 18 years old who change jobs, based on years 2003-2007 in the QP. “AKM” firm effects are firm fixed effects from a wage regression with firm fixed effects, person fixed effects, year effects, and age × education group dummies. X-axis variable in “Industry Average Rate” pane is the 5-digit industry index used in Table 1.9 calculated for all industries in the QP, common to all firms in same 5-digit industry. X-axis variable in “Firm Rate” pane is the ratio of current-year employees not present at the firm in the following year divided by all current-year employees, averaged across 2003-2007; both X and Y variables are residuals conditional on 5-digit industry fixed effects.
Figure A.5: Effects of Idiosyncratic and Common Shocks on Exit and Job Loss

**Effect on Probability that Firm Has Any Employees**

- **Idiosyncratic**
  - Event Study Coefficient
  - Year: 2005 to 2013
  - Influence on probability that a firm remains operational.

- **Common**
  - Event Study Coefficient
  - Year: 2005 to 2013
  - Influence on probability that a firm remains operational.

**Effect on Probability that Attached Incumbents Have Jobs**

- **Idiosyncratic, Full Sample**
  - Event Study Coefficient
  - Year: 2004 to 2013
  - Percentage of attached incumbents employed at the same firm.

- **Common, Full Sample**
  - Event Study Coefficient
  - Year: 2004 to 2013
  - Percentage of attached incumbents employed at any job.

- **Idiosyncratic, Never-Exiter Firms**
  - Event Study Coefficient
  - Year: 2004 to 2013
  - Percentage of attached incumbents employed at the same firm.

- **Common, Never Exiter Firms**
  - Event Study Coefficient
  - Year: 2004 to 2013
  - Percentage of attached incumbents employed at any job.

Notes: Figure shows effects of idiosyncratic shock component $S_j$ and common shock component $C_j$ both on firm survival and on incumbent employment and retention, whether or not firm survives. Figure displays year-specific coefficients from regressions of the specified outcome on the interaction between the idiosyncratic demand shock $S_j$ and an indicator for each year, with all interactions estimated jointly as in equation (1.13). Estimates for each outcome are from separate regressions. Sample and specification for firm-level survival analysis (“any employees”) is the same as in Figure 1.4, except includes full sample of firms including exiters ($N = 4,173$). Sample and specifications for analysis of effects on attached incumbents are exactly as in Figure 1.7. “Any Employees” denotes presence of at least one full-time employee in the QP. “Any job” is a worker-level indicator denoting presence of any full-time job in the QP. “Same firm” is an indicator denoting whether employee is employed full-time at 2007 firm.
A.4. Appendix Tables to Chapter 1

Table A.1: Comparison of Firms and Workers in Sample and Population

<table>
<thead>
<tr>
<th></th>
<th>Industry Heterogeneity Subsamples</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>High Separation Rate</td>
<td>Low Separation Rate</td>
<td>Never-Exiter Firms</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Mean</td>
<td>P50</td>
<td>SD</td>
<td>Mean</td>
</tr>
<tr>
<td><strong>Firms</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Employees</td>
<td>27</td>
<td>19</td>
<td>23.34</td>
<td>29</td>
</tr>
<tr>
<td>Sales/Worker if Emp&gt;0, Euros</td>
<td>180,041</td>
<td>104,549</td>
<td>265,262</td>
<td>164,039</td>
</tr>
<tr>
<td>Value Added / Worker if Emp&gt;0, Euros</td>
<td>35,250</td>
<td>25,224</td>
<td>49,015</td>
<td>34,030</td>
</tr>
<tr>
<td>N Firms, Emp&gt;0</td>
<td>2,223</td>
<td></td>
<td></td>
<td>1,944</td>
</tr>
<tr>
<td>N Firms</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Workers:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Monthly Wage, Euros</td>
<td>747.48</td>
<td>545.00</td>
<td>540.85</td>
<td>774.81</td>
</tr>
<tr>
<td>Hourly Wage, Euros</td>
<td>4.37</td>
<td>3.17</td>
<td>3.20</td>
<td>4.53</td>
</tr>
<tr>
<td>Log Monthly Wage</td>
<td>6.46</td>
<td>6.30</td>
<td>0.50</td>
<td>6.51</td>
</tr>
<tr>
<td>Log Hourly Wage</td>
<td>1.32</td>
<td>1.15</td>
<td>0.51</td>
<td>1.37</td>
</tr>
<tr>
<td>Fixed Term, Percent of Sample</td>
<td>0.20</td>
<td></td>
<td></td>
<td>0.20</td>
</tr>
<tr>
<td>Tenure, Months (All Workers)</td>
<td>115</td>
<td>90</td>
<td>99.75</td>
<td>128</td>
</tr>
<tr>
<td>Female, Percent of Sample,</td>
<td>0.51</td>
<td></td>
<td></td>
<td>0.36</td>
</tr>
<tr>
<td>Regular Hours Per Month</td>
<td>171</td>
<td>173</td>
<td>7.45</td>
<td>171</td>
</tr>
<tr>
<td>N Workers</td>
<td>59,446</td>
<td>55,910</td>
<td>84,520</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Table compares firms and workers in full sample to firm subsamples examined in the main analysis. All items are exactly as in Table 1.2.
Table A.2.: Test of Sorting for Firms with Multiple Destinations

Outcome is Change in Exports by Firm \( j \) of Product \( p \) to Country \( c \), Measured by:

<table>
<thead>
<tr>
<th>Symmetric Growth Rate</th>
<th>Log Change (if &gt;0)</th>
<th>Any Exports</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Coefficient: Change in Destination ( c ) Imports of Product ( p )</td>
<td>0.0674***</td>
<td>0.0682***</td>
</tr>
<tr>
<td></td>
<td>(0.0150)</td>
<td>(0.0114)</td>
</tr>
</tbody>
</table>

Baseline Controls

Firm Fixed Effects

N

101,344
101,344
35,193
34,804
101,344
101,343

Notes: Tables reports results from regressions of changes in exports by firm \( j \) of product \( p \) to destination \( c \) on the change (symmetric growth rate) of imports of \( p \) to country \( c \) from all other countries during the same period, as in equation (A.2). Observations are firm-markets pairs (markets are country x 6-digit product). Sample includes all firms in primary analysis sample with exports to at least two distinct markets in the pre-period. Changes are taken from 2006-2007 to 2009-2010. When no exports occur in the post period, log values are treated as missing. Regressions are unweighted. Standard errors are two-way clustered at the firm and market level.

Table A.3.: IV Pass-Through of Common Demand Shocks to Wages: Conditioning on Survival Matters

Pass-Through to Wages of Sales Changes Instrumented by:

<table>
<thead>
<tr>
<th>Idiosyncratic Component</th>
<th>Common Component</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>All Firms</td>
<td>0.131</td>
</tr>
<tr>
<td></td>
<td>(0.055)**</td>
</tr>
<tr>
<td>Never-Exiters Only</td>
<td>0.112</td>
</tr>
<tr>
<td></td>
<td>(0.050)**</td>
</tr>
</tbody>
</table>

Sample:

Monthly Wage

x

Hourly Wage

x

Notes: Table compares pass-through elasticities estimated off of common component of demand \( C_j \) versus the idiosyncratic component of demand \( S_j \), highlighting the the sensitivity to conditioning on exit when studying common shocks. Table displays estimates of pass-through elasticities, obtained from difference-in-difference regressions of average incumbent log monthly wages on the specified outcome variable; elasticity is coefficient \( e^{\omega|R} \) on the interaction of the output measure \( R_j \) with the post period indicator \( Post_t \) in (1.15). Specifications correspond to columns (5) and (7) in Table 1.6, using either monthly or hourly base wages as noted; effects of idiosyncratic shock in all-firm estimates in column (1) and (2) in this table are identical to corresponding entries of Table 1.6. See notes to Table 1.6 for specification details.
B. Appendix to Chapter 2

B.1. Supplemental Figures to Chapter 2

Figure B.1.: Federal Highway Administration Example of “Economically Distressed Areas”

Example Map with Projects Added

Notes: Figure displays counties in West Virginia designated as “economically distressed” (in red), and “not economically distressed” (in blue), and was posted on the website of the Federal Highway Administration 2010 as an example of how States might make this designation. Eighty-five percent of counties were designated as “economically distressed”.
Figure B.2.: Event Study: Dynamic Effects of Local Vendor Receipts on Construction Employment

Notes: Figure plots year-specific \( \beta \) coefficients estimated jointly from the dynamic difference-in-differences specification in equation 2.3, where the treatment is defined as the per-capita amount of Recovery Act highway construction dollars (in $ Millions per capita) that went to firms located in the observation county according to the vendor records. The outcome is the county-level annual average construction employment level from the QCEW, both the treatment and the outcome variables are scaled by 2008 population. Each point estimate is the the county-level effect of $1 Million per capita of Recovery Act road construction receipts on construction employment per-capita in the specified year, relative to the 2008 level of the outcome variable. Regression includes state-by-year fixed effects and year-specific controls for 2008 log population. The treatment variable is not time-varying, rather the same treatment variable is interacted with dummies for each outcome year. Year dummies and interactions for 2008 are omitted due to inclusion of identify county-level fixed effects. Standard errors are clustered at the county level, and the implied 95% confidence intervals are plotted around each point estimate. N = 35,052, reflecting 2,922 counties included in primary analysis sample.
B.2. Supplemental Tables to Chapter 2

Table B.1.: Summary Statistics of Recovery Act Road Construction Projects

<table>
<thead>
<tr>
<th></th>
<th>All</th>
<th>Pavement/Resurfacing</th>
<th>Bridge and Road Construction/Reconstruction</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Number of Projects</strong></td>
<td>6,993</td>
<td>2,452</td>
<td>903</td>
</tr>
<tr>
<td><strong>Project Expenditure ($)</strong>:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean</td>
<td>1,666,175</td>
<td>1,734,447</td>
<td>1,880,179</td>
</tr>
<tr>
<td>Median</td>
<td>569,490</td>
<td>679,317</td>
<td>666,600</td>
</tr>
<tr>
<td>SD</td>
<td>4,013,480</td>
<td>3,302,186</td>
<td>3,690,526</td>
</tr>
<tr>
<td>Max</td>
<td>105,000,000</td>
<td>51,200,000</td>
<td>46,900,000</td>
</tr>
<tr>
<td><strong>Number of Contracts Awarded</strong></td>
<td>13,581</td>
<td>3,936</td>
<td>2,386</td>
</tr>
<tr>
<td><strong>Contract Value ($)</strong>:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean</td>
<td>805,596</td>
<td>1,043,307</td>
<td>655,777</td>
</tr>
<tr>
<td>Median</td>
<td>165,285</td>
<td>275,162</td>
<td>157,154</td>
</tr>
<tr>
<td>SD</td>
<td>2,578,052</td>
<td>2,519,135</td>
<td>1,979,237</td>
</tr>
<tr>
<td>Max</td>
<td>86,800,000</td>
<td>51,200,000</td>
<td>42,900,000</td>
</tr>
<tr>
<td><strong>Distance of Vendor from Project (Mi):</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean</td>
<td>77</td>
<td>70</td>
<td>85</td>
</tr>
<tr>
<td>Median</td>
<td>42</td>
<td>43</td>
<td>46</td>
</tr>
<tr>
<td>SD</td>
<td>133</td>
<td>112</td>
<td>148</td>
</tr>
<tr>
<td>Max</td>
<td>2,295</td>
<td>1,827</td>
<td>2,295</td>
</tr>
<tr>
<td><strong>Projects by Number of Vendors</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1</td>
<td>4,963</td>
<td>1,840</td>
<td>436</td>
</tr>
<tr>
<td>2</td>
<td>1,099</td>
<td>417</td>
<td>148</td>
</tr>
<tr>
<td>3 or More</td>
<td>931</td>
<td>195</td>
<td>319</td>
</tr>
</tbody>
</table>

Notes: Table summarizes all individual Recovery Act infrastructure projects (sub-awards) administered by the Federal Highway Administration (FHWA) in the 48 contiguous states which include information on at least one vendor. These figures are aggregated to the county level to generate the primary treatment variable summarized in Table 2.1. “Pavement and Resurfacing Projects” restrict to the sample of projects with terms in the text field containing the project description that corresponding to resurfacing, paving, widening, lane addition, or curve improvement. “Bridge and Road Construction” restricts to projects with descriptions that include words related to building, constructing, bridge work, and tunnel work.
Table B.2.: Robustness to Other Definitions of Recovery Act Road Construction Spending

<table>
<thead>
<tr>
<th>Spending Variable</th>
<th>Effect of $1 Million Per Capita on Jobs Per Capita:</th>
<th>Effect of $1 Per Capita on Total Payroll $ Per Capita:</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Construction (1)</td>
<td>All Employment (2)</td>
</tr>
<tr>
<td></td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Primary Spending Definition</td>
<td>2.141***</td>
<td>5.924*</td>
</tr>
<tr>
<td></td>
<td>(0.798)</td>
<td>(2.708)</td>
</tr>
<tr>
<td>All Projects inc. Missing Vendors</td>
<td>1.894***</td>
<td>5.715*</td>
</tr>
<tr>
<td></td>
<td>(0.669)</td>
<td>(2.640)</td>
</tr>
<tr>
<td>Paving / Resurfacing Only</td>
<td>2.401*</td>
<td>8.682</td>
</tr>
<tr>
<td></td>
<td>(1.258)</td>
<td>(4.967)</td>
</tr>
<tr>
<td>Bridge and Other Major Construction Only</td>
<td>3.054</td>
<td>4.547</td>
</tr>
<tr>
<td></td>
<td>(2.488)</td>
<td>(7.489)</td>
</tr>
</tbody>
</table>

Notes: Table shows robustness of main estimates to other definitions of Recovery Act road construction expenditure. Sample is N = 2921 counties in primary sample. All spending variables are in per-2008-capita units. “All projects including missing vendors” corresponds to the variable summarized in the top rows of Table 2.1. “Paving/resurfacing only” and “Bridge and other major construction only” are variables aggregated from the corresponding projects in Table B.1, and restrict to projects with vendor information as in the primary treatment variable definition. Each point estimate is obtained from a separate regression. “2010 Effect” indicates estimate is 2008-2010 difference-in-differences coefficient estimated from the two-period specification in equation 2.4. “Total 2009-2013 Effect” is the sum of the 2009, 2010, 2011, 2012, and 2013 coefficients from the dynamic specification in equation 2.3, see notes to Table 2.4 for more details. All specifications include state-by-year fixed effects and year-specific controls for 2008 log population. Standard errors are clustered at the county level. * indicates two-sided p-value less than 10%, ** indicates two-sided p-value less than 5%.
Table B.3: Effects on Other Outcomes

<table>
<thead>
<tr>
<th></th>
<th>Effect of $1 Million Per Capita on Building Construction Jobs Per Capita:</th>
<th>Effect of $1,000 Per Capita on Unemployment Rate (pp)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Spending on Projects in County ($Millions per Capita)</td>
<td>0.140</td>
<td>0.160</td>
</tr>
<tr>
<td></td>
<td>(0.284)</td>
<td>(0.332)</td>
</tr>
<tr>
<td>Controls</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2008 Population</td>
<td>x</td>
<td>x</td>
</tr>
<tr>
<td>Additional covariates</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N obs</td>
<td>5,842</td>
<td>5,056</td>
</tr>
<tr>
<td>N counties</td>
<td>2921</td>
<td>2528</td>
</tr>
</tbody>
</table>

Notes: Table displays 2008-2010 difference-in-differences coefficients estimated from the two-period specification in equation 2.4. “Additional Covariates” includes “Pre-period Industry and Demographic Controls” as defined in Table 2.3. All regressions include state-by-year fixed effects. Sample size varies across columns due to missing values of covariate and outcome variables. Standard errors are clustered at the county level. * indicates two-sided p-value less than 10%, * indicates two-sided p-value less than 5%.
C. Appendix to Chapter 3

C.1. Additional Figures

Figure C.1.: County Treatment Description

(a) Treatment Counties

(b) Matched Control Counties

(c) Pre-War Log Manufacturing Value Added

(d) Wartime vs. Pre-War Manufacturing
Figure C.2.: County-Level Distribution of Spending on Large Publicly-Financed Plants

(a) All Counties  
(b) Counties with One or More Publicly-Financed Plants

Figure C.3.: Raw Difference in Means of Treatment Group and All Other Counties

(a) Log Manufacturing Value, Difference  
(b) Log Average Wage of Production Workers, Difference  
(c) Log Employment of Production Workers, Difference

Notes: Sample includes all counties with manufacturing wage data in all pre-war years and the given outcome year. A county is "treated" if it had over $500 per 1939 employee in expenditure on new public plans costing over $1 million. Each point estimate is the raw difference and means of the outcome variable between treatment groups and all other counties in a given year.
Figure C.4.: Manufacturing Event Studies, Privately-Financed Plant “Treatment”

Notes: Sample includes all counties with manufacturing wage data in all pre-war years and the given outcome year. Each point estimate is the coefficient on the Treatment dummy in a separate regression corresponding to the outcome in a given year. A county is “treated” if it was one of the 100 counties with the most per-1939-employee spending on new war plants that were privately financed at least in part. Standard errors are clustered at the state level. Specifications control for 1930 logs and per-capita levels of population, employment, manufacturing wage/employment/value added/establishment variables, retail employment/wages, wholesale employment/wages, black population, foreign born population, value of farm land, and total area; as well as New Deal public works spending in the 1930s, maximum elevation and elevation range, number of extremely wet and dry months in 1930, dust bowl severity, and indicators for location on coasts and major rivers.