Essays on Firm Behavior

by

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Dissertation submitted in partial fulfillment of the
requirements for the degree of Doctor of Philosophy
in the Department of Economics
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ABSTRACT

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Abstract

This dissertation studies three questions in which economic behavior at the firm level plays an important role. Chapter 2 studies how individual owner-managers shape firm conduct in the labor market. I use survey and administrative microdata from the U.S. Census to link firm and worker outcomes to the past local unemployment rate exposure (URE) of owner-managers. Using a difference in differences approach centered on changes in firm ownership, I find that firms acquired by high URE owner-managers increase worker earnings on average while displaying no differential trends in firm employment. These results also hold in worker-level analysis, which reveals that firm-level differences are driven in part by immediate pay increases for older and more educated workers, resulting in greater retention among these cohorts. These results are further validated among firms that do not experience ownership changes. Using an instrumental variables design, I find that URE is associated with greater rent-sharing at the firm level. Together, these results demonstrate that owner-managers have substantial scope to determine pay and hiring policy at their firms.

Chapter 3, coauthored with Mark Curtis, Daniel Garrett, Eric Ohrn, and Juan Carlos Suárez Serrato, studies plant-level responses to a large federal tax incentive, known as bonus depreciation, that lowered the cost of capital investment. Difference-in-differences estimates using confidential Census Data on manufacturing establishments show that tax policies increased both investment and employment, but did not

stimulate wage or productivity growth. Using a structural model, we find that the primary effect of the policy was to increase the use of all inputs by lowering costs of production and that capital and production workers are complementary inputs in modern manufacturing. Our results show that tax policies that incentivize capital investment do not lead manufacturing plants to replace workers with machines.

The fourth chapter assesses how state and local taxes influence firm entry decisions in the video gambling industry in Illinois, which comprises almost 7,000 establishments each operating up to five slot machine-like gambling terminals. Using variation in local gambling ordinances and an event-study framework, I estimate that gambling legalization leads on average to a 3.0% increase in local tax revenue and a noisy 1.6% increase in local spending. I then develop and estimate an equilibrium model of entry and exit to explore the effects of counterfactual tax increases. Simulations reveal that uniform increases in the marginal tax rate increase tax revenue while reducing the extent to which gambling establishments select into low-income neighborhoods. Taken together, these results suggest that taxation can effectively offset the regressivity of gambling activity if revenue is effectively targeted to local governments.

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Chapter 2 of this research uses data from the Census Bureau's Longitudinal Employer Household Dynamics Program, which was partially supported by the following National Science Foundation Grants: SES-9978093, SES-0339191 and ITR-0427889; National Institute on Aging Grant AG018854; and grants from the Alfred P. Sloan Foundation.

I would like to dedicate this dissertation to the memory of Donald A. Wilson, my first intellectual role model.

Chapter 1

Introduction

Following the 2008 Financial Crisis, popular economic debate has increasingly come to focus on rising wealth and income inequality in the economically developed world [Milanovic, 2016]. This evolution has coincided with an increase in academic inquiry that studies how the distribution of economic activity across firms, governments, and workers relates to wider trends in economic efficiency and equity. In the case of labor economics, a broad literature has documented how the existence of wage-setting power affects different kinds of workers [Card, 2022]. Labor economists have also studied how technological development, such new equipment that can replace labor in production, shapes inequality [Acemoglu and Restrepo, 2020]. Analysis of public policy has documented instances where ostensibly broad-based policy changes, such as consumption tax hikes, can still have distributional consequences across workers and consumers [Allcott et al., 2019].

In market economies, *firm behavior* lies at the heart of many of these questions. Firms set wages that determine the distribution of income across workers, they decide whether to adopt emerging technologies, and they respond to policy changes by shifting prices, wages, and production. This dissertation explores the role of firm behavior in three distinct settings.

Chapter 2 sheds new light on differences in firm-level pay by focusing on the discretion of owner-managers to set pay for workers. The chapter does so by leveraging a novel data linkage that relates the personal labor market experiences of owner-managers to firm and worker outcomes. Specifically, I find that owners with greater exposure to elevated local unemployment rates offer higher pay on average. These results are validated using several complementary designs, and reveal that these effects are consistent with several potential motives for increasing pay. These effects seem likely to reflect efforts to retain key workers when a new owner takes control of a firm, while also suggesting better pay for new hires. Taken together, these results provide new insights on the nature of firm pay policy and how they relate to the characteristics of owner-managers.

Chapter 3 studies the effects of tax subsidies that lower the cost of capital at the plant level. The chapter combines tax policy variation from bonus depreciation with confidential data to gain empirical leverage on popular debates on whether policies that subsidies capital investment help or hurt workers. Using a difference in differences design that compares plants that benefited most from the policy to those that benefited least, we find that both capital and labor increased in response to the policy. Our results document several previously unexplored responses to capital investment incentives. First, we find that production labor increases more than non-production labor, and that both increase in statistically and economically important ways. We also show that the average earnings for workers at affected plants actually decrease, despite increases in labor inputs. This decrease is explained by increases in the shares of workers that are less-educated, younger, more racially diverse, and more likely to be women. While bonus depreciation did not affect plant productivity, it did lead manufacturing plants to increase their scale.

We also find that bonus depreciation was less effective at stimulating manufacturing activity for industries that were more exposed to import competition from China. Bonus was also more effective at plants with high degrees of capital and skill intensity. Finally, we reject the hypothesis that bonus decreased employment in industries that were highly exposed to robotization; in fact, bonus had larger effects on employment in these plants. Overall, bonus does not seem to encourage plants to double-down on 20th century modes of production or to grow in industries that are at a comparative disadvantage.

Using a structural model, we separate the scale and substitution effects induced by the policy. Because bonus lowered costs of production, the policy led to a large and statistically significant scale effect. While the majority of the effect on employment is driven by this scale effect, we also consistently find that capital and labor are complements in production, and we are able to rule out relatively small elasticities of substitution. We verify the complementarity between capital and labor by showing empirically that plants invest more when labor costs are low, including at non-unionized plants, RTW states, and concentrated labor markets.

Chapter 4 focuses on tax policy in legal gambling markets and how policy interacts with firm entry. Gambling legalization remains a controversial subject in the United States despite apparent consumer demand and tax revenue potential. Detractors often point to adverse social effects and the possibility that low income individuals are most likely to bear the costs associated with legalization. This chapter gains empirical purchase on this topic using detailed data on video gambling industry in Illinois, which comprises almost 7,000 establishments each operating up to five slot machine-like gambling terminals.

This chapter does so by combining several sources of empirical variation. First, municipality-level legalization decisions are used to assess how tax revenue is raised and spent as a result of taxes on gambling activity. Next, a structural model of establishment entry is developed to understand the distribution of gambling activity across Illinois. Model simulations show that uniform increases in the marginal tax rate increase tax revenue while reducing the extent to which gambling establishments select into low-income neighborhoods. Taken together, these results suggest that taxation can effectively offset the regressivity of gambling activity if revenue is effectively targeted to local governments.

Chapter 2

Firm Pay Policy and the Personal Labor Market Experiences of Business Owners

Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this information product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 1667 (CBDRB-FY23-P1667-R10157).

2.1 Introduction

How do business owners decide how they pay their employees? Average worker earnings varies substantially across firms, with a large literature attributing these differences to factors such as productivity [Abowd et al., 1999, Barth et al., 2016], rent-sharing [Card et al., 2018, Kline et al., 2019], and firm wage-setting power [Berger et al., 2019, Lamadon et al., 2022, Jarosch et al., 2019]. While these factors have proven effective at explaining observed labor market dynamics, there is relatively little evidence on how these differences may be explained by manager-level discretion over pay-setting.

In this paper, I present evidence on a novel driver of firm pay policy: the past labor market experiences of business owners. I find that U.S. firms operated by owner-managers who were previously more exposed to local unemployment rate fluctuations offer higher pay to workers. These differences cannot be explained by differential firm employment or revenue, or by direct effects on the financial resources of potential owners. The existence of "experience effects" has been documented in a variety of social and economic contexts [Malmendier, 2021]. This literature has shown that past economic experiences have a persistent effect on economic behavior and preferences, even after controlling for income and wealth differences across individuals.

Establishing a causal link between owners' past labor market experiences and the pay policy of firms is challenging for a number of reasons. First, data linking the work histories of business owners to their firms is not readily available in existing data sources. While worker-entrepreneur transitions can be inferred from matched employer-employee data, prior approaches do not identify which owners are actively involved in management and firm financial decision-making. These approaches also rely on firm tax identifiers to infer entrepreneurial activity, which can be an unreliable measure of new firm entry for smaller firms [Chow et al., 2021].

Second, it is unclear how negative economic shocks shape the behavior of owner-managers based on existing evidence on experience effects. Some studies have found evidence that past exposure to economic shocks induces lower consumption and increased pessimism regarding economic conditions [Malmendier and Shen, 2019], while others have found somewhat mixed evidence that recession exposure increases preferences for redistribution [Cotofan et al., 2021, Hansen and Stutzer, 2021]. Beyond these potentially competing effects on the preferences of owner-managers, exposure to economic shocks may also generate selection on who becomes an entrepreneur. For these reasons, an empirical strategy that isolates behavioral responses due to preferences from those due to selection is essential to understanding how owner-manager experiences influence pay policy.

Finally, differences in firm pay policy may reflected a variety of strategies that firms employ to maintain a productive workforce. For example, while firm rent-sharing may reflect normative beliefs about what constitutes "fair" compensation, it could also reflect efforts to retain workers that are particularly essential to the operation of the firm. Similarly, worker pay across certain firms may be similar on average but vary dynamically depending on the implicit contracts under which worker compensation is determined [Beaudry and DiNardo, 1991]. This feature of pay policy necessitates credible empirical designs paired with both firm and worker-level data to identify the mechanisms underlying firm pay-setting heterogeneity.

I address these challenges by creating a novel data linkage that uses survey data to identify owner-managers in matched employer-employee data from the US Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. This linkage identifies the work histories of individuals who report involvement in the day-to-day management of their firms. In my baseline sample, these firms are relatively small, averaging thirty-four employees, and disproportionately comprise "closely-held" S-corporations where owners are likely to be particularly influential in the operation of their firms [Smith et al., 2019]. This feature is particularly appropriate for studying how managerial discretion affects pay policy, since larger firms may be more likely to institute standardized pay structures [Hazell et al., 2022]. I use this data to generated measures of owners' cumulative, lifetime unemployment rate exposure (URE) up to the year in which they acquire their firm [Malmendier and Shen, 2019]. I classify owners by whether their URE measure is above the median value in my sample, which yields a "treatment" group comprising owners whose average URE was 6.89% when they acquired their firm relative to a "control" group whose average URE was 5.27%. I then relate these measures to both firm- and worker-level outcomes using multiple research designs that clarify the economic forces that mediate the relationship between owner experiences and labor market outcomes.

My baseline empirical design estimates firm-level worker earnings, employment, and revenue responses to changes in ownership. To abstract from mechanical effects associated with ownership transfers, my difference-in-differences design compares firms acquired by high URE owners to firms acquired by low URE owners. On average, firms acquired by high URE owner-managers increase mean earnings by 4.0% five years after the ownership change, with an average earnings effect of 2.5% over the same period. These results are not accompanied by increases in employment, while revenue responses are positive but imprecise. These results hold for an alternative calibration of my experience measures, and for specifications that control for contemporaneous unemployment rates or owner income and wealth proxies prior to ownership.

I then show that this firm-level response is driven in part by an immediate, 4.0% increase in earnings for incumbent workers that come under the ownership of high URE owner-managers. These results are strongest for older and college-educated workers. These subgroups are also more likely to remain at the firm, consistent with high URE owner-managers raising earnings to retain key personnel. At the same time, workers hired within the three year period after ownership changes also receive large, persistent earnings increases relative to their prior job. Together, these results suggest that the firm-level responses estimated in the previous analysis reflect changes in pay policy rather than a compositional shift toward different kinds of workers, and that these gains accrue to both incumbent and new hires alike.

One potential challenge to a causal interpretation of these results is that the noisy firm-level revenue effects mask heterogeneity under which high URE firms are more productive and pass on these gains to workers. For example, prior LAU exposure could induce selection on who decides to become an entrepreneur. Additionally, because my experience measures are likely to be correlated at the local level, they could reflect local labor market norms that evolve out of historical conditions and not the experiences of owners. In all of these cases, the causal claim that experiences induce changes in managerial discretion over pay policy would be misleading.

To address these concerns, I use a separate sample of firms with owner labor market history linkages to estimate rent-sharing elasticities while controlling for time-varying commuting zone level shocks [Card et al., 2018]. These regressions estimate worker-level earnings responses to exogenous shocks to firm revenue-per-worker using an instrumental variable (IV) design. Additionally, because the empirical link between earnings and firm-level revenue shocks could also reflect firms insuring workers against shocks, I separately estimate these parameters before and after the 2008 financial crisis. I estimate firm rent-sharing elasticities of 0.064 and 0.219 for workers employed at firms operated by low and high URE owners, respectively, over the 1997-2007 period. Interestingly, this pattern reverses following the 2008 recession, when firms were likely to face greater financial stress. I estimate statistically different elasticities of 0.581 and 0.263 across low and high URE owners, respectively. While my IV design cannot directly distinguish between positive and negative shocks, these patterns are consistent with high URE owners insuring worker earnings against firm-level shocks [Guiso et al., 2005]. Supporting this interpretation are OLS regressions showing that the labor share of revenue at high URE firms is higher during this period. These rent-sharing and insurance patterns also translate to higher average earnings for workers employed by high URE owner-managers, as demonstrated in event study regressions centered on job-to-job transitions.

My results contribute to a burgeoning literature on the sources and nature of firm wage-setting power in the labor market [Card, 2022]. My estimates on the earnings and retention elasticities of incumbent workers following acquisitions can be used to calculate the labor supply elasticity to firms, a key parameter measuring the degree of monopsony power possessed by firms [Manning, 2011]. These estimates imply firm-level labor supply elasticities of 1.1-1.8, in line with prior estimates in the literature and consistent with acquired firms possessing monopsony power over incumbent workers [Bassier et al., 2021]. I also find effects on incumbent earnings are largest in more concentrated local labor market concentration, defined with a measure that incorporates cross-industry labor flows to define labor markets. To the extent that this measure denotes local monopsony power, this result is consistent with the idea that managerial discretion influences pay policy most strongly in the absence of labor market competition.

More generally, this paper provides new evidence on the drivers of firm-level earnings heterogeneity, showing that the personal labor market experiences of owners influences firm pay policy. My work is most closely related to that of Acemoglu et al. [2022], which finds that publicly-traded companies under the leadership of CEOs with MBAs offer lower earnings. Though similar in its focus on worker outcomes, my paper differs to theirs in that I study smaller, closely held firms that have received relatively less attention in the literature on firm pay-setting. As in their paper, I find that differences in rent-sharing can partly explain how managers influence firm pay premia.

This paper also relates to a recent literature exploring why workers transition into entrepreneurship [Babina, 2020, Wallskog, 2022]. My primary contribution to this literature is methodological, as I demonstrate how survey data on owners can be used to identify entrepreneurs and the role they perform at their new firm. I also show how survey data can be used to identify firm ownership changes, a non-negligible channel for entrepreneurial entry that previous methods could conflate with true new firm entry.

Finally, by focusing on relatively smaller firms, my paper links the literature on firm pay policy to a distinct literature on the role of small firms in labor markets. New firms play an outsized role in creating new jobs [Haltiwanger et al., 2018]. Likely due to limit data availability, there is relatively little evidence on the pay-setting behavior of small firms [Babina et al., 2019]. I provide new estimates of firm labor supply and rent-sharing elasticities that can be used to inform fiscal and labor market policies that are likely to affect worker outcomes at small firms.

Section 2.2 describes the construction of the owner-firm panel used in my analysis. I outline my research design in Section 2.3, while Sections 3.4 and 2.5 present the resulting empirical results.

2.2 Data

I link detailed survey responses from the US Census Bureau's Survey of Business Owners (SBO) to administrative employer-employee records from the US Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program. This section describes these source data in detail and outlines the matching procedure implemented to construct the samples used in the empirical analyses.

The Survey of Business Owners (SBO) is a large randomly sampled survey of US business owners that was conducted every five years over the 1972 to 2012 period.[1]

[1]Due to declining response rates, the U.S. Census discontinued the SBO following the 2012

The survey selects businesses from a list of all firms reporting receipts of at least $1,000 during the survey year, excluding select industries.[2] These data include economic and demographic characteristics of up to four business owners linked to a firm Employment Identification Number (EIN). Importantly for my analysis, the survey reports the role that each owner performs at their firm and the timing and means by which they acquired ownership of the firm. The survey also reports a number of other outcomes, such as information on the sources of startup and expansion capital used for the firm and whether the firm provides certain employee benefits.

The Longitudinal Employer-Household Dynamics (LEHD) administrative files are a matched employer-employee dataset that provides quarterly earnings and employment information at the worker-job level. These data are derived from state unemployment insurance (UI) system wage records and the ES-202 program, jointly administered with the Bureau of Labor Statistics. Earnings records encompass various forms of compensation, such as bonuses, stock options, profit distributions, and, in some states, gratuities [Acemoglu et al., 2022]. This project uses data from twenty-two states and the District of Columbia.[3] For these states, my data covers over 90% of private sector workers over the 1990-2015 period, though time coverage varies across states. The LEHD also provides the age, sex, race, ethnicity, education level, and country of birth of workers. Firms in the the LEHD are defined at the state level using the UI-based State Employer Identification Number (SEIN). This defini-

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survey. In its place, the Census conducted the Annual Survey of Entrepreneurs (ASE) from 2014 to 2016. This survey in turn evolved into the Annual Business Survey (ABS), first conducted in 2018.

[2]The excluded industries are: Crop and Animal Production (NAICS 111, 112), Scheduled Passenger Air Transportation (NAICS 481111), Rail Transportation (NAICS 482), Postal Service (NAICS 491), Funds, Trusts, and Other Financial Vehicles (NAICS 525), Religious, Grantmaking, Civic, Professional, and Similar Organizations (NAICS 813), Private Households (NAICS 814), and Public Administration (NAICS 92).

[3]The states used in this analysis are: Arizona, Arkansas, California, Colorado, Delaware, Florida, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Montana, New Mexico, Oklahoma, Oregon, Pennsylvania, South Carolina, Tennessee, Washington, West Virginia, and the District of Columbia.

tion typically comprises all establishments operating under a single firm in a given state. The LEHD also provided a federal Employer Identification Number (EIN) which permits linkages across states and to other Census data products.

I supplement these core data sources with firm-level revenue data from the US Census Bureau's Longitudinal Business Database (LBD), which is available over the 1997 to 2015 period. The US Census Bureau's Business Register (BR) maintains the EIN identifiers that link firms across these three data sources.

2.2.1 Identifying Firm Acquisitions

I link survey responses from the 2002-2012 waves of the SBO to administrative records using EIN firm identifiers derived from tax records and available across Census datasets. Throughout the paper, I refer to "firm acquisition" as the first year in which a firm EIN appears in my data. In practice, new EINs can appear for a variety of reasons beyond the birth of a new firm, most notably when a firm undergoes an ownership change. As a result, EIN changes alone may be misleading when attempting to identify entrepreneurial activity. To address this shortcoming, I verify the timing and nature of acquisition using the SBO, which reports the year in which each owner acquired the firm and whether the specified owner founded, purchased, inherited, or acquired the firm as a gift. For simplicity, I limit my analyses to firms that were either founded or purchased, which comprise over 90% of firms in the public-use 2007 SBO microdata.

While the EIN is a reliable identifier for firms that continue to be owned and operated by their original founder, changing EINs poses a significant challenge to tracking firm outcomes across ownership changes.[4] I manually create consistent firm

[4]Constructing ownership-invariant establishment identifiers is a primary goal of the US Census

identifiers for SBO firms that undergo ownership changes using a combination of Business Register data on EIN births and deaths and LEHD data on worker flows across firms. I first limit attention to purchased SBO firms where the reported acquisition year matches the birth of the firm EIN. Next, I match these EINs to all EINs that disappeared in the same or previous year and operated in the same state. I then examine the SEIN to which these EINs are attached in the LEHD. Because the LEHD reports employment and payroll at the quarterly frequency, I further refine these matches by limiting attention to SEIN-to-SEIN matches where apparent the apparent birth-death occurs in the same or consecutive quarters. Finally, I examine whether workers attached to the candidate successor SEIN in the LEHD eventually appear at the SBO firm SEIN. Specifically, I define a valid EIN-to-EIN match as those where more than 50% of the workers attached to the successor SEIN at its death appear at the SBO firm within one quarter.[5]

I make additional sampling restrictions for both purchased and founded SBO firms to ensure that my analyses focus on owners for which I can accurately measure prior labor market experience and that are likely to have a significant role in the management of firms that maintain a reasonable pool of employees. First, I discard all firms that never employee more than five workers (including owners receiving labor compensation) in a year. Second, I drop firms for which I do not observe at least five years of owner employment history prior to firm acquisition. Third, I only consider firms where the primary owner reports their role at the firm as either "managing day-to-day operations" or "financial control with the authority to sign loans, leases, and

Bureau's LBD program. The LBD creates annual, ownership-invariant establishment identifiers through a combination of the comprehensive Economic Census (EC), conducted every five years, and the annual Report of Organization. However, because the latter source only surveys large firms, the LBD is unable to provide consistent identifiers across time for many smaller firms like those reported in SBO [Chow et al., 2021].

[5]This Census project did not gain access to the LEHD Successor-Predecessor File (SPF), which conducts a similar procedure to link SEIN births and deaths.

contracts." This ensures that the owners I identify can be reasonably characterized as firm managers. This approach naturally fits with my sampling procedure, which implicitly selects owners that report nonzero labor earnings at their firm and thus are likely to be involved in its day-to-day functioning. Fourth, while I impose no explicit maximum firm size, I do not consider EINs that operate in more than one state, as identified using the comprehensive firm and establishment records in the LBD. This restriction limits potential noise introduced by the inclusion of firms with more complex management structures in which the owners are less likely to exercise direct control over pay policy. This restriction also reduces the possibility of spurious demographic matches in the LEHD if an owner receives labor compensation in a state outside my LEHD sample. Finally, I do not consider firms that undergo ownership changes driven by M&A activity by existing firms, as defined by enterprise-level firm identifiers available in the LBD, over the sample period.

2.2.2 Linking Owner-Manager Labor Market Histories

Using the firm matches identified using the criteria above, I link owners from the SBO to their LEHD worker histories using the demographic information available in both data sources. Because assigning owners to spurious LEHD worker identifiers could significantly bias my estimates, I adopt a fairly conservative approach to flagging valid owner-worker matches. Following recent literature tracking entrepreneurs in matched employer-employee data, I first restriction attention to workers at match firms that are among the top three earners at the time of acquisition (either via purchase or founding).[6] I then flag all owner-worker pairs with the same age, sex, race, ethnicity, and place of birth. I discard all other owner-worker pairs, as well as

[6]See Babina [2020] and Wallskog [2022] for recent examples.

those where the LEHD reports an unimputed education level that does not match the education level reported in the SBO. I further eliminate owner-worker pairs where the worker does reported nonzero earnings at the SBO firm for each year between the acquisition year and the survey year in which the SBO owner match was flagged. For firms acquired via a purchase, I also exclude owner-worker matches where the worker appears at the firm predecessor EIN identified in the previous section. While it is likely that some firms are acquired by individuals that were previously employed by the firm, such matches are otherwise difficult to parse from spurious matches. In certain cases, these refinements could yield firms for which two or more workers may be a demographic match to two or more owners reported in the SBO. I keep all such matches as long as the number of workers is equal to or less than the number owners in the match pool. I drop all other firms where I'm unable to distinguish owners from potentially spurious matches.

2.3 Empirical Strategy

My empirical research strategy uses these linkages to relate owner URE to firm outcomes. I first describe how I construct measures of LAU exposure for new entrepreneurs. I then separately describe the difference-in-differences and IV empirical designs I employ to relate owner URE to firm outcomes.

2.3.1 Measuring Unemployment Experience

I follow Malmendier and Shen [2019] to compute measures of local unemployment exposure of owner-managers when they gain ownership of their firm. They model the effects of past unemployment rate experience as a weighted average of past realized unemployment rate exposure parameterized to capture the degree of relative recency bias. While their experience-based learning model implies life-cycle dynamics as new experiences are gained, I fix an individual owner-manager's experiences at the time of firm acquisition to ensure that I do not conflate post-acquisition firm-level shocks with experience-based learning.

Specifically I define cumulative, lifetime unemployment rate exposure at time t_{Entry} as a function of past local realizations UR_{t-k} in periods $\{0, t-1\}$

$$E_{t_{Entry}} = \sum_{k=0}^{t-1} w(\lambda, t, k) W_{t-k}. \tag{2.1}$$

where the weighting function $w(\lambda, t, k)$ is given by

$$w(\lambda, t, k) = \frac{(t-k)^{\lambda}}{\sum_{t=0}^{t-1} (t-k)^{\lambda}}.$$

The parameter $\lambda > 0$ thus captures the degree of recency bias under which an individual internalizes new experiences. Following Malmendier and Shen [2019], I use linear weights given by $\lambda = 1$ in my baseline analysis while also showing robustness to larger values of λ.

To operationalize these measures, I define an owner-manager's location as their county of residence as identified using the LEHD-SBO merge described in Section 3.2. I obtain data on county- and state-level unemployment rates from the U.S. Bureau of Labor Statistics' (BLS) Local Area Unemployment Statistics program. A limitation of this data is that county-level unemployment rate data is only available after 1989, while state-level data is only available after 1976. I construct experience measures using the smallest geographic data available in a given year. For years prior to 1976, I use national unemployment rate data obtained from BLS.

While the LEHD provides data on individuals date of birth, I only observe county of residence in a limited window of years preceding firm acquisition. I thus impute residency in missing years using the most recently available location observed in the LEHD. This naturally introduces measurement error for individuals that move locations. As long as this error is not systematically correlated with both experience and unobserved owner characteristics, it will only produce attenuation bias in my difference-in-differences results. Additionally, given the available data coverage in both the BLS and LEHD data, an advantage of my approach is that I use progressively more conservative measures of unemployment exposure as measurement error increases.

Throughout my analysis, I categorize owner-managers as either "high URE" or "low URE" based on whether their calculate unemployment exposure is above or below the median value of $E_{t_{Entry}}$ in my sample of owner-firm matches. This binary treatment variables reduces the sensitivity of my estimates to outliers in $E_{t_{Entry}}$ while also avoiding challenges associated with interpreting coefficients in difference-in-differences settings with continuous treatment [Callaway et al., 2021].

2.3.2 Firm Acquisitions Design

My baseline empirical analysis estimates the differential effects of firm ownership transfers on firm and worker outcomes based on the URE of the acquiring owner-managers. I implement a "stacked" difference-in-differences that compares firms in event time relative to the time at which an ownership change occurs [Cengiz et al., 2019]. For firm outcomes Y_{jt}, I estimate regressions of the form:

$$Y_{jt} = \sum_{k=-3, k\neq -1}^{5} \beta_k [E_{it} \times 1(e = k)] + \gamma \mathbf{X_{jt}} + \mu_j + \varepsilon_{it}, \tag{2.2}$$

where subscripts i, j, and t denote owner-managers, firms, and years relative to an ownership transfer, respectively. The treatment variable E_{it} is equal to one if a firm is acquired by a high URE owner-manager. The coefficients of interest, β_k, thus describe the average difference in outcomes across owner-manager URE status in event time k relative to the difference in outcomes in the year preceding the acquisition event. All regressions include firm fixed effects μ_j and event-time fixed effects interacted with various controls, given by $\mathbf{X_{jt}}$. In my baseline specifications, these controls are (1) firm size quartiles, defined by total firm employment at event time $t = -1$, (2) 3-digit NAICS industry indicators, and (3) an indicator for whether the average local unemployment rate in the five years preceding the ownership transfer was above or below the median value in the sample. I also fully interact these controls with an indicator for whether the firm acquisition took place after 2002. This coarse time control allows me to account for common time-varying shocks, in particular exposure to the post-2008 recession, while also leaving sufficient variation to estimate parameters of interest. As a result, my identification strategy restricts attention to comparisons between similarly-sized firms in the same industry that where acquired in the same five-year period, and who experienced similar unemployment rates in the years following the ownership transfer. Because variation in URE primarily varies according to age and location, I cluster standard errors at the birth year-county level [Cameron and Miller, 2015].

This empirical strategy enables me to isolate changes in outcomes due to the arrival of new ownership rather than due to unobserved differences in firm- and market-level characteristics. By restricting my difference-in-differences comparisons to acquired firms only, I also ensure my results are not driven by any mechanical effects of ownership transfers relative to firms whose ownership remains unchanged.

The identifying assumption underlying my empirical strategy is that outcomes at firms acquired by high URE owners would have evolved in a similar fashion to those acquired by low URE owners if the acquiring individuals had the same prior local unemployment rate exposure. There are two primary threats to the causal interpretation of model estimates as the effect of experiences on managerial pay policy. The first potential threat to identification is that, because treatment is defined by geographic exposure, owner URE could be correlated with contemporaneous unemployment rates and other market-level shocks that are likely to affect firm and work outcomes. While estimating effects on within-firm outcomes ensures that time-invariant differences in labor market characteristics do not confound my results, time-varying shocks could interact with the timing of acquisition. My empirical design allows me to evaluate this concern by testing pretrends on firm outcomes prior to acquisition. I also consider specifications that further interact $\mathbf{X_{jt}}$ with indicators for whether local unemployment rates were above or below the sample median in the four years preceding firm acquisition.

The second threat to identification worthy of consideration is that URE drives selection into entrepreneurship in a way that correlates with managerial ability. If this were the case, my results could be driven by an increase in firm productivity rather than a behavioral difference in managerial policy. I conduct several tests that help rule out selection on unobservables as a driver of my results. I consider specifications that control for owners' financial resources prior to acquisition, arguably the most direct way that URE may induce selection into entrepreneurship. Specifically, I interact $\mathbf{X_{jt}}$ with controls for owners' average labor income in the five years prior to firm acquisition, and for indicators denoting whether the firm acquisition was funded via debt financing.

Beyond the direct effects on entrepreneurs' financial resources, the direction of further selection bias in my empirical design is ex ante ambiguous. If elevated past exposure to high LAU periods shocks entrepreneurs' preferences toward greater risk-aversion, then it is possible that relatively lower-ability workers select out of entrepreneurship due to LAU exposure, biasing my treated sample toward higher-productivity owner-managers. Conversely, Babina [2020] finds that financial distress at the firm level induces entry into entrepreneurship for affected workers. If this effect dominates, greater LAU exposure could induce less-capable individuals to enter into ownership, biasing firm outcomes downward. I discuss the potential direction of bias in Section 3.4, where I present estimates on firm and worker outcomes that directly speak to potential correlation between owner-manager productivity and worker compensation.

2.3.3 Firm Acquisitions Sample

I estimate these regressions on a balanced, 10-year panel firms. To ensure that my firms are informative of managerial policy toward worker pay, I only consider firms that maintain at least 5 employees throughout the four year period preceding the ownership change. Table 2.10 presents select owner and firm characteristics across my treatment categories for the 2,450 firms for which I identify ownership changes. On average, owner-managers classified as "low URE" have a URE value of 5.27 percent unemployment, while high URE owner-managers have URE of 6.89 percent unemployment. Owners and firms are otherwise similar across treatment groups. High URE owner-managers are slightly younger on average, but with identical prior experience in self-employment and business ownership. Acquired firms were of similar size prior to acquisition, averaging about 34 employees, and are equally likely to provide nonwage amenities such as health insurance and profit sharing arrangements under the acquiring ownership.

2.3.4 Pass-through Estimation

I conduct a separate empirical analysis that estimates the pass through of firm-level shocks onto worker earnings as a function of owner-manager URE status. Firm rent-sharing has been studied extensively as a driver of firm-level pay discrepancies (e.g. Card et al. [2018]; Kline et al. [2019]; and Lamadon et al. [2022]). Estimates of rent-sharing elasticities across owner-managers allow me to unpack whether pay differences are due to differences in firm-level rents (i.e. conditional on the same pass-through elasticity) or in differences in rent-sharing *conduct*, conditional on similar firm-level rents.

I estimate regressions of the following form at the worker-level:

$$\ln Earn_{it} = \xi_{OwnExp_j} \ln \frac{Rev_{jt}}{Emp_{jt}} + \gamma_i + \alpha_{jkt} + \varepsilon_{ikt}. \tag{2.3}$$

where i, j, k, and t index workers, firms, commuting zones, and years, respectively. The parameters of interest ξ_{OwnExp_j} dictate how firm-level revenue-per-worker relates to worker earnings and is allowed to vary by whether the firm's owner-manager was classified as high or low URE at the time of founding. In all regressions, I include worker fixed effects γ_i. I also include commuting zone-by-year fixed effects, which help isolate variation in firm-level conduct due to owner experiences that is uncorrelated with local time-varying shocks. Finally, I also interact these location controls with quartile bins of firm employment growth in their first five years of operation to ensure that my results are not biased toward high-growth firms.

Because firm level revenue-per-worker is likely to correlated with worker productivity, I follow Card et al. [2014] and instrument for firm-level rents-per-worker using the average value of $\frac{Rev_{jt}}{Emp_{jt}}$ at other firms in the same 4-digit NAICS industry but operating exclusively in separate commuting zones. Because I control for CZ-by-year shocks, my identification restricts attention to firms in different industries but operating in the same commuting zone in the same year, whose owners had different past URE exposure when they founded their firm. Identification of ξ_{OwnExp_j} parameters requires the assumption that the instrument variation only affects worker earnings via changes in firm-level rents, not, for example via labor market adjustments driven by same industry firms exposed to the same shock. Though potentially a strong assumption, I can interpret the difference in ξ_{OwnExp_j}'a across owner experiences by imposing the less restrictive assumption that these sources of bias are common across firms in the same commuting zone.

I estimate these regressions using an alternative sample of workers employed at firms that do not undergo ownership changes. I restrict attention to workers attached to their firm for at least four consecutive quarters and who earn at least 5000 in 2001 U.S. dollars in each year they are attached to the firm. This procedure yields 255,000 worker-year observations over the 1997 to 2007 period. As in my other sample, I only consider firms that maintain at least five employees, and I exclude owner-managers from this analysis.

2.4 Empirical Effects of Firm Ownership Changes Due to Owner Experiences

2.4.1 Firm-Level Effects

Figure 2.1 presents my headline result that firms acquired by high URE owners increase average worker compensation relative to those acquired by low URE owners. Log mean worker earnings at firms acquired by high URE owners display no differential trends prior to ownership transfer, and I estimate a precise zero effect in the year in which the transfer occurs. In the first full year of new ownership, mean earnings are 2.2% greater at high URE firms relative to low URE. This effect holds five years after the ownership transfer, when mean earnings are 4.0% higher on average. Panel (A) of Figure 2.2 shows that these effects are very similar when calibrating $E_{t_{Entry}}$ such that $\lambda \in 1, 3$. [7]

Panel A of Table 2.3 presents difference-in-differences estimates of the effects of high URE ownership transfers on worker earnings across alternative model specifications and outcome definitions. The average estimated effect of 2.5% associated with Figure 2.1 is statistically significant at the 5% level. This effect is of similar magnitude when including controls for owner finances or pre-period average county unemployment rates, and when using an alternative measure that aggregates worker-level earnings from the LEHD's Employment History Files (EHF) rather than using total firm payroll divided by employment.[8] All earnings effects are statistically significant at the 5 or 10% level. I also estimate the effects on the 75th percentile of within-firm earnings, shown in Panel C. These effects are of similar magnitude, but somewhat noisy, suggesting the effects on mean earnings are not driven solely by increased compensation for top earners. I additionally test for whether these earnings effects are driven by the largest firms, firms that experience large declines in employment, or high-growth firms. Table 2.5 presents estimates from specifications that interact difference-in-differences terms with indicators for each of these cate-

[7]Panel (B) of Figure 2.2 estimates a version of Equation 2.2 that defines an owner-manager's URE as the average unemployment exposure from ages 18 to 25. This model is motivated by work suggesting that economic experiences in these formative years can be particularly important for shaping adult behavior [Malmendier, 2021]. Though small and noisy, I find that such experiences predict a 2.5% increase in average earnings five years after the ownership transfer. The fact that I find larger and more precise effects using the full lifetime exposure measure suggests that the experienced-based learning model of Malmendier and Shen [2019] is more informative of managerial behavior. This discrepancy could also be explained by greater measurement error in Panel (B) of Figure 2.2.

[8]In each case, I exclude the labor income received by owner-managers.

gories. I define large firms as those in the top quartile of firm employment at the time of acquisition, "decline" firms as those that experience decreases of at least 25% in employment five years post-acquisition, and "boom" firms as those that experience growth of at least 100% in the same period. Large and high-growth firms exhibit no statistical difference difference in mean earnings, while earnings differences are approximately zero (though again noisy) for declining firms.

To further explore whether these earnings effects reflect changes in pay policy rather than changes to firm-level revenue or productivity, I also estimate the effects of high URE firm acquisitions on firm-level employment and revenue. Panel (A) of Figure 2.3 shows no differential trend in firm-level log employment prior to ownership change, and a small, insignificant positive effect for firms acquired by high URE owners. Panel (B) similarly shows flat pre-trends for log revenue. I estimate positive revenue effects for firms acquired by high URE owners. These effects are highly noisy, likely in part due to the fact that revenue data from the LBD must be imputed for some years. Difference-in-differences estimates of these outcomes in Table 2.4 show an approximate zero effect on log employment, and a noisy 4.8% effect on revenue in my baseline specification. This revenue effect could be explained by differences in unobserved ability across high and low URE managers, which in turn equates to higher productivity and earnings for workers. However, a positive revenue effect, holding employment fixed, could also be a product of a shift in pay policy toward higher worker compensation, allowing firms to either attract more capable workers or better retain existing employees. I now turn to worker-level estimates that explore these mechanisms directly.

2.4.2 Effects of Ownership Changes on Incumbent Workers

Though my prior results are suggestive of a change in firm-level pay policy, they do not rule out the possibility that changes in worker composition may fully explain these effects [Abowd et al., 1999]. To explore whether my results represent a change in pay policy conditional on worker conditions, I estimate worker-level analogues of Equation 2.2 on incumbent workers. I define incumbent workers as those who were employed at acquired firms at least two years prior to the ownership change, and who report positive earnings in the year after the ownership change occurred. Conditional on this incumbency status, I consider the total earnings of workers, including from other firms post-acquisitions, since changes in firm management may also affect worker mobility.

Figure 2.4 presents the effects of owner-manager URE on the earnings of these workers following ownership changes. The earnings of incumbent workers across firms exhibit no differential trends prior to firm acquisitions. In the first full year of new firm ownership, incumbents at high URE firms receive a statistically significant 4.0% increase in earnings relative to incumbents at control firms. Column (1) of Table 2.6 reports the corresponding difference-in-differences earnings estimate, showing that incumbent workers at high URE firms experience on average a 2.4% earnings increase. My event study estimates show that this earnings premia gradually decline after this initial increase, consistent with worker earnings converging across firms as workers at acquired firms reallocate toward similar jobs.

2.4.3 Worker Heterogeneity and the Firm Elasticity of Labor Supply

A natural question arising from these results is which incumbent workers benefit most from increases in firm pay policy following transitions to new owner-managers. Additionally, increases in pay should also affect workers' probability of leaving their firm if such pay increases reflect wage-setting power at the firm. I investigate these questions by estimating my difference-in-differences specifications while interacting URE indicators with indicators for worker age and education. Specifically, I use a binary variable for whether incumbent workers are above the age of 44 (the average age of new firm owners in my sample), and an indicator for whether workers possess at least a bachelor's degree or equivalent. Table 2.7 presents these results, with columns corresponding to specifications that use log earnings and yearly indicators for whether the worker remains at the acquired firm, respectively. Because the latter captures the probability of workers leaving their firm, I interpret these effects as informative of separation elasticities across workers.

My main result in this section is that the earnings effects on incumbent workers are largely driven by older and college-educated workers. Earnings effects for younger workers are approximately zero, while older workers receive a large, statistically significant earnings increases of 4.2% ($0.049 - 0.007$). Similarly, workers without a college degree receive a small positive earnings increase that is statistically indistinguishable from zero. In contrast, college educated workers receive large average increases of around 5.9% ($0.044 + 0.015$). My estimates of the effects of owner experiences on worker retention closely mirror these earnings effects: older and college educated workers at treated firms are 2.0% and 1.8% more likely to remain at the firm relative to other workers at their firm. Given my positive but noisy estimates on firm revenue for high URE owner acquisitions, these results could suggest that these results reflect the retention of "key personnel" in the acquisition of the firm.

Following Manning [2011], I use these estimates to calculate the elasticity of labor supply to the firm. By equating separation and recruiting elasticities, this parameter can be defined as two times the separation elasticity with respect to a given change in pay. In my setting, this translates to:

$$\varepsilon_{LS} = -2 \times \frac{-\hat{\gamma}_{HighExp}}{\hat{\phi}_{HighExp}},$$

where $\hat{\phi}_{HighExp}$ is the firm-level percent change in pay policy, as identified by differences in owner-manager URE, while $\hat{\gamma}_{HighExp}$ is my estimated effect on worker retention for subgroups. Because younger workers and those without a college degree have very small estimated retention elasticities, I calculate ε_{LS} using the effects for "key personnel" in Table 2.7 ($\hat{\gamma}_{HighExp}$.) Because demographic-specific pay increases may reflect selection on unobserved worker characteristics, I follow Bassier et al. [2021] and use my "firm-level" estimates of changes in pay policy in column (1) of Table 2.6 ($\hat{\phi}_{HighExp}$). Across values of $\hat{\gamma}_{HighExp}$, I calculate values of ε_{LS} ranging from 1.1-1.8. These estimates indicate high degrees of monopsony power at acquired firms and are very close to values found in the meta analysis of Sokolova and Sorensen [2021]. While more recent work by Bassier et al. [2021] finds larger elasticities (indicate less firm monospony power, my results can be interpreted as being specific to the setting of firm acquisitions, where worker preferences might dictate stronger attachments to their firms when an acquisition occurs.

As a final test relating my estimates to the literature on firm labor market power, I also estimate heterogeneous effects according to local labor market concentration, displayed in Table 2.11. I follow Arnold [2020] and use job flows across industries within the same commuting zone to calculate the Herfindahl-Hirschmann Index (HHI) of labor market concentration. I then interact an indicator denoting whether acquired firms where in a local labor market in the top quartile of HHI at the time of acquisition. Here, I find that the largest earnings and retention effects are at firms in highly concentrated markets. Though somewhat counter-intuitive, the fact that the largest earnings effects occur in such markets is consistent with the idea that managerial discretion is most likely to shape firm pay when firms have market power. In contrast, labor markets featuring greater competition for workers may apply greater discipline to firms to set wages. Additionally, these relative effects again suggest smaller labor supply elasticities to the firm in concentrated markets.

2.4.4 Earnings Effects for New Hires

To help reconcile my firm-level effects, which indicate persistent positive effects on firm earnings, and my incumbent effects, which indicate large but declining effects, I also investigate how the earnings of new hires evolve at acquired firms post acquisition. To do so, I track workers that undergo job-to-job transitions to the firms in my sample in the five years following the acquisition event. As in the preceding analyses, I compare workers hired at high URE firms to those hired at low URE firms. Additionally, this relatively larger sample permits me to include flexible controls for the year in which workers are hired, rather than the more coarse time controls used in other specifications.

Some caution should be taken in interpreting these estimates as reflective of firm-level pay differences, as these results only compare worker earnings relative to their levels at the prior firm. However, such changes are potentially reflective of firm pay policy, since the extent to which firms offer larger pay increases to prospective workers may also reflect motives to retain and attract productive employees. Figure

2.5 presents event-study regression estimates from these specifications, indicating that workers hired at high URE firms experience large, immediate, and persistent earnings increases over their previous jobs (relative to those hired at low URE firms). These results also hold when including controls for workers' earnings growth at their prior firm, which otherwise may induce selection on which kinds of workers are hired across firms. Columns (2) and (3) of Table 2.6 show that the average post-hire effects are significant and largely in line with those for incumbents. Overall, these results show that high URE firms also differ in how they pay new hires.

2.5 Pass-Through Estimates

This section carries out the alternative empirical design described in Section 2.3.4 to address several potential concerns regarding my interpretation of the preceding results as reflecting changes in managerial discretion around pay policy. First, while the firm-level revenue effects I estimate are noisy, they could reflected differences in the underlying productivity of acquiring owner-managers, via selection on prior labor market experience, that translates to higher worker earnings via higher marginal products of labor. Second, my prior results could reflect geographic heterogeneity that is correlated with recent local labor market conditions. I address these two issues by estimating pass-through elasticities for fixed changes in revenue while also flexibly controlling for local labor market-by-year fixed effects, respectively.

Table 2.12 presents both OLS and IV estimates of equation 2.3 over the 1997-2007 period. As my baseline estimates, I initially exclude the 2008-2013 recession years because my IV variation might otherwise be confounded by large, aggregate shocks which may induce firm closure or contraction. Column (1) shows no difference in firm pass-through by owner URE status using OLS. While unreliable as a measure of rent-sharing, since OLS likely conflates differences in worker productivity with differences in firm conduct, these results are still useful as descriptive measures of the firm-level labor share. In contrast, my IV design reveals a significant difference in pass-through elasticities according to owner experiences: high URE owner-managers increase worker earnings by approximately 0.22 dollars for every 1 dollar increase in firm rents, as defined by my IV measure, while low URE managers only increase worker earnings by 0.064 dollars for an equivalent increase in rents. These differences are statistically different at the 5% level. Together, these results provide evidence that my prior estimates reflect changes in conduct rather than only differences in firm productivity.

I also repeat this exercise using data from the 2008-2013 Great Recession in Table 2.13. While more suggestive, since the recession likely induces selection on firm and job survival, I interpret these IV estimates as reflecting how worker earnings responded to large, negative shocks. Interestingly, I find here that rent-sharing patterns reverse across owner URE status. This result is broadly consistent with the hypothesis that high URE owners may differ in how they provide implicit insurance contracts to workers to insulate them from shocks [Guiso et al., 2005]. Bolstering this interpretation is that the relative firm-level labor share (i.e. the OLS estimates in column (1)) are larger for firms operated by high URE managers.

To make these results more comparable to those in previous sections, I also estimate how the earnings of new hires at firms in this sample evolve relative to prior earnings, displayed in Figure 2.6. These specifications, which replicate the specifications used to generate Figure 2.5, indicate less immediate earnings effects, but still translate to significant positive earnings growth over time, as would be expected under greater rent-sharing.

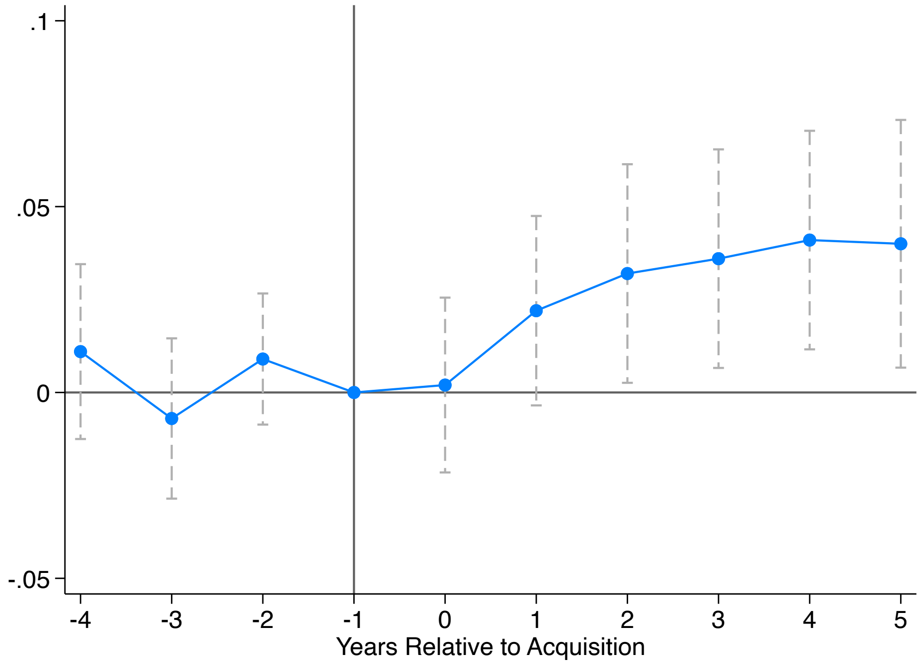
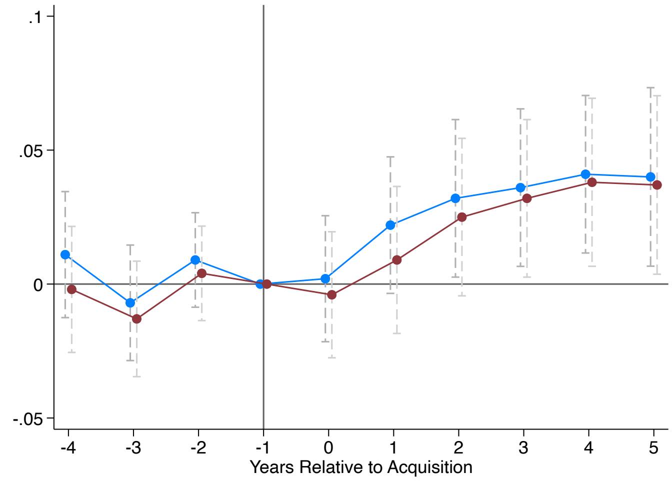


Figure 2.1: Effects of Owners' URE on Firm Mean Worker Earnings after Ownership Transfer

Notes: Figure 2.1 plots event-study coefficient estimates of the differential effects of firm ownership changes on average worker earnings across acquiring owner URE. *Source:* Authors' calculations based on LEHD and SBO data.

(a) Log Mean Earnings: Alternative Calibrations



(b) Log Mean Earnings: Formative Experiences

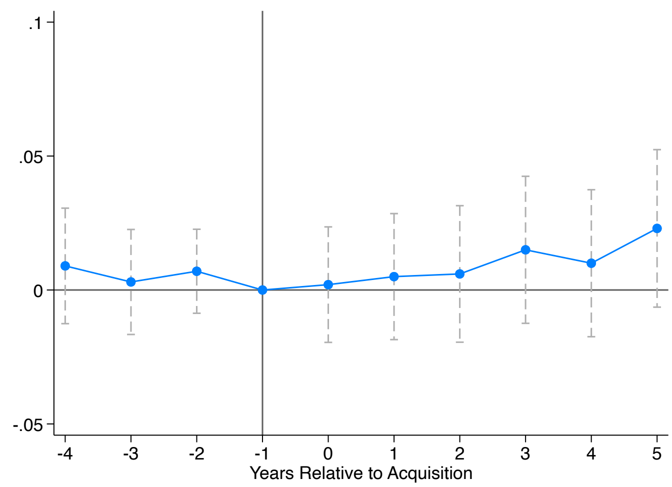
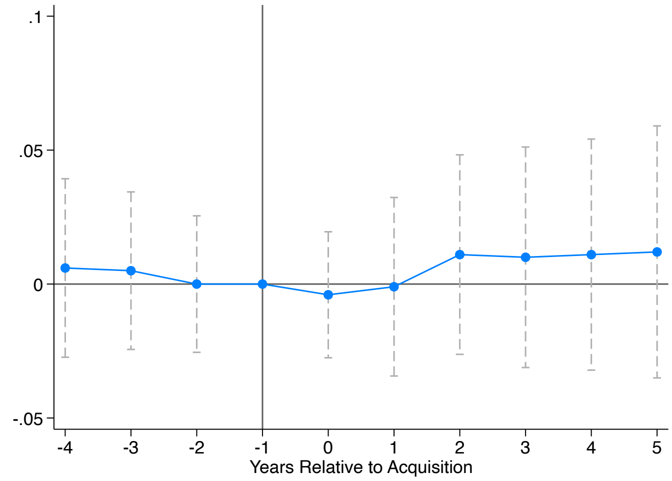


Figure 2.2: Effects of Owners' URE on Firm Mean Worker Earnings after Ownership Transfer: Alternative Treatments

Notes: Figure 2.2 plots event-study coefficient estimates of the differential effects of firm ownership changes on average worker earnings across acquiring owner URE using alternative treatment definitions. Panel (A) plots versions that calibrate $\lambda = 1$ (blue) and $\lambda = 3$ (red), respectively. Panel B only considers average URE during owner ages 18 to 25. *Source:* Authors' calculations based on LEHD and SBO data.

(a) Log Employment



(b) Log Revenue

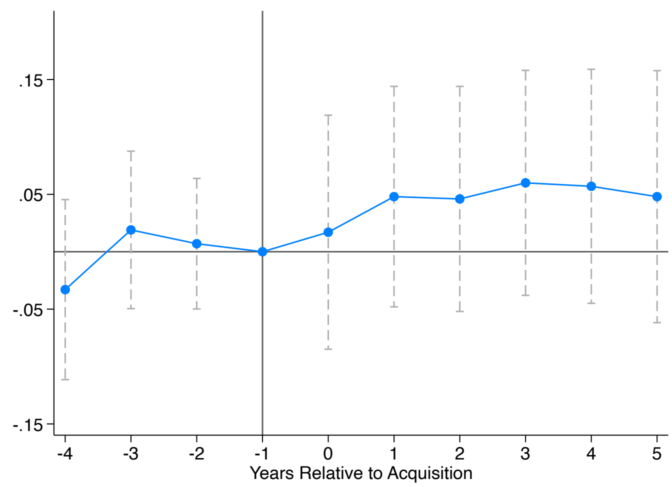


Figure 2.3: Effects of Owners' URE on Firm Outcomes after Ownership Transfer

Notes: Figure 2.3 plots event-study coefficient estimates of the differential effects of firm ownership changes on firm revenue and employment across acquiring owner URE. *Source:* Authors' calculations based on LEHD and SBO data.

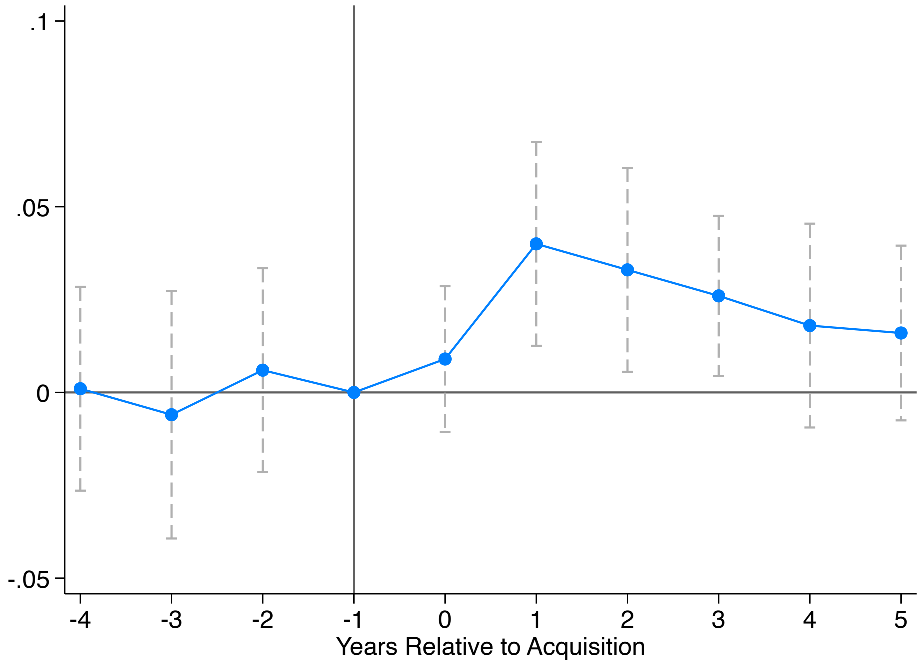


Figure 2.4: Effects of Owners' URE on Log Incumbent Worker Earnings
Notes: Figure 2.4 plots event-study coefficient estimates of the differential effects of firm ownership changes on the earnings of incumbent workers across acquiring owner URE. Incumbency status is defined as workers that were employed at the firm at least two years prior to the ownership change, and who report some positive earnings in the year after the change. *Source:* Authors' calculations based on LEHD, SBO, and BLS data.

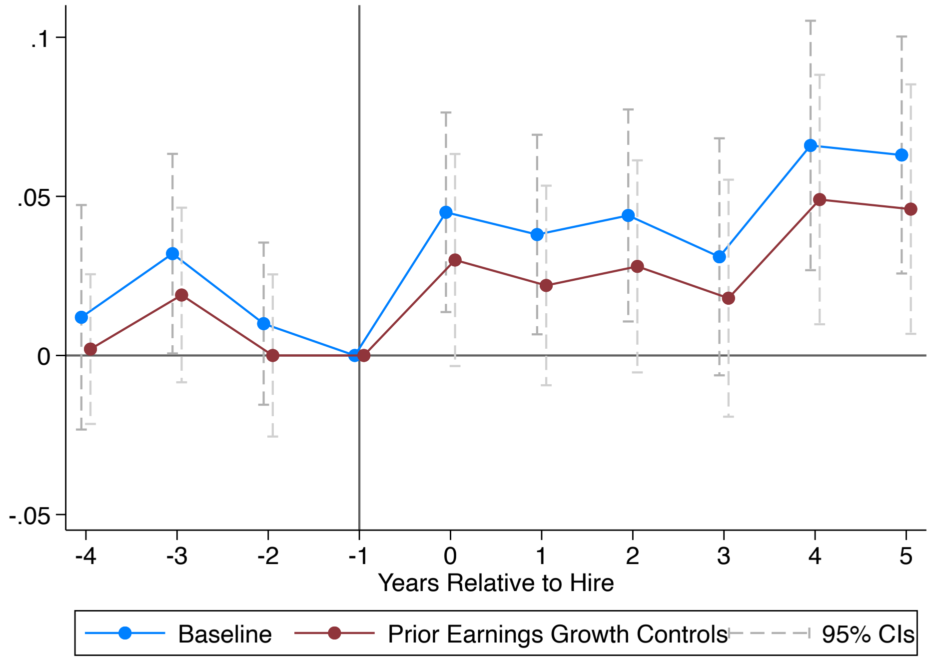


Figure 2.5: Effects of New Hires on Log Worker Earnings: Post Acquisition

Notes: Figure 2.5 reports event-study coefficient estimates of the differential earnings effects of job-to-job transitions of workers hired at acquired firms. *Source:* Authors' calculations based on LEHD, SBO, and BLS data.

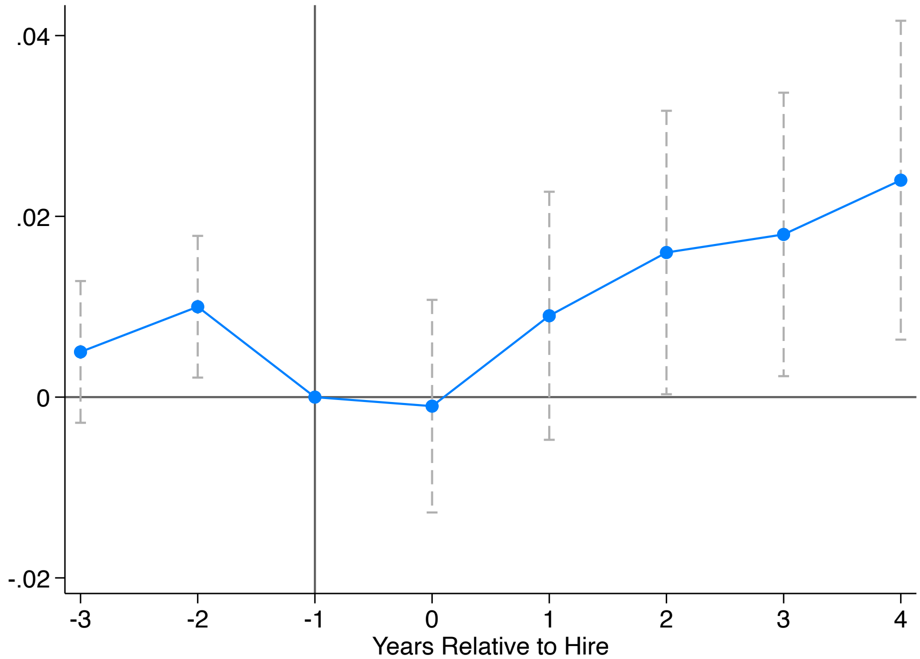


Figure 2.6: Effects of New Hires on Log Worker Earnings: Founder Firms

Notes: Figure 2.6 reports event-study coefficient estimates of the differential earnings effects of job-to-job transitions of workers hired at founder firms across owner URE. *Source:* Authors' calculations based on LEHD, SBO, and BLS data.

Table 2.1: New Firm Entry by Sector: 2007 Survey of Business Owners

	Share
Professional, Scientific, Technical Services	0.146
Construction	0.144
Health Care and Social Assistance	0.107
Retail Trade	0.105
Accommodation and Food Services	0.087
Administrative, Support, Waste Management	0.074
Manufacturing	0.053
Wholesale Trade	0.051
Finance and Insurance	0.043
Transportation and Warehousing	0.040
Observations	58923

Table 2.1 shows the share of new firms in the 2007 SB0 in the listed industries. New firms are defined as those founded after 2004. *Source:* Authors' calculations based on SBO data.

Table 2.2: Characteristics of Owner-Mangers and Acquired Firms by URE Exposure

	(1) URE	(2) Acq. Year	(3) Owner Age	(4) Prior Own.
Low Exp.	5.27	2004	43.95	0.46
	(0.46)	(2.62)	(8.67)	(0.5)
High Exp.	6.89	2003	42.48	0.46
	(1.09)	(2.84)	(8.2)	(0.5)
	(5) Firm Size	**(6) S-Corp**	**(7) Health Ins.**	**(8) ProfitShare**
Low Exp.	35.1	0.63	0.74	0.12
	(58.3)	(0.48)	(0.44)	(0.33)
High Exp.	33.2	0.61	0.74	0.13
	(52.7)	(0.49)	(0.44)	(0.34)

Table 2.10 reports characteristics of owner-managers of acquired firms in my baseline empirical sample. *Source:* Authors' calculations based on LEHD and SBO data.

Table 2.3: Effects of Owners' URE on Firm Earnings after Ownership Transfer

	(1)	(2)	(3)
	Panel A: Log Mean Worker Earnings		
HighExp × Post	0.025**	0.018*	0.024**
	(0.012)	(0.011)	(0.012)
	[0.037]	[0.100]	[0.046]
	Panel B: Log Mean Earnings (EHF)		
HighExp × Post	0.023**	0.019*	0.025**
	(0.011)	(0.01)	(0.011)
	[0.037]	[0.057]	[0.023]
	Panel C: Log 75th Percentile Earnings		
HighExp × Post	0.019	0.018*	0.027*
	(0.013)	(0.011)	(0.014)
	[0.144]	[0.100]	[0.054]
Base FE	✓		
Owner Finances FE		✓	
Unemp. Rate FE			✓
N	24,500	24,500	24,500

Table 2.3 reports difference-in-differences coefficient estimates corresponding to my firm-level event study designs. *Source:* Authors' calculations based on LEHD and SBO data.

Table 2.4: Effects of Owners' URE on Firm Employment and Revenue after Ownership Transfer

	(1)	(2)	(3)
	Panel A: Log Employment		
HighExp × Post	0.004	0.007	0.006
	(0.019)	(0.017)	(0.02)
	[0.833]	[0.681]	[0.764]
	Panel B: Log Revenue		
HighExp × Post	0.048	0.042	0.051
	(0.042)	(0.034)	(0.036)
	[0.253]	[0.217]	[0.157]
Base FE	✓		
Owner Finances FE		✓	
Unemp. Rate FE			✓
N	24,500	24,500	24,500

Table 2.4 reports difference-in-differences coefficient estimates corresponding to my firm-level event study designs. *Source:* Authors' calculations based on LEHD and SBO data.

Table 2.5: Mean Earnings Heterogeneity by Firm Growth Characteristics

	(1) Large Firm	(2) Emp. Decline	(3) Emp. Boom
HighExp	0.022	0.027**	0.020*
	(0.015)	(0.013)	(0.011)
	[0.069]	[0.069]	[0.069]
Int. × HighExp	0.000	-0.032	0.014
	(0.021)	(0.026)	(0.057)
	[1.000]	[0.218]	[0.806]
Base + Int. FE	✓	✓	✓
N	24,500	24,500	24,500

Table 2.5 reports difference-in-differences coefficient estimates corresponding to my firm-level event study designs, with treatment variables interacted with the specified firm characteristics. *Source:* Authors' calculations based on LEHD and SBO data.

Table 2.6: Earnings Effects for Incumbent Workers and New Hires

	(1) Incumbents	(2) New Hires	(3) New Hires
HighExp × Post	0.024**	0.033**	0.026**
	(0.012)	(0.014)	(0.013)
	[0.046]	[0.018]	[0.046]
Pre-Period Earn. Growth FE			✓
Observations	332,000	277,000	277,000

Table 2.6 reports difference-in-differences estimates corresponding to my worker-level event-study designs. *Source:* Authors' calculations based on LEHD and SBO data.

Table 2.7: Heterogeneity in Incumbent Outcomes by Worker Characteristics

	(1) Log(Earn)	(2) Tenure
	Panel A: Age	
HighExp × Post	-0.007	-0.007
	(0.024)	(0.009)
Age44 × HighExp × Post	0.049**	0.020**
	(0.022)	(0.009)
	Panel B: Education	
HighExp × Post	0.015	0.003
	(0.012)	(0.007)
College × HighExp × Post	0.044***	0.018*
	(0.017)	(0.010)
Observations	332,000	332,000

Table 2.7 reports difference-in-differences estimates of the differential effects of firm ownership changes on incumbent worker earnings and retention probabilities, with treatment indicators interacted with worker demographic characteristics. *Source:* Authors' calculations based on LEHD and SBO data.

Table 2.8: Heterogeneity in Incumbent Worker Outcomes by Age

	(1) Log(Earn)	(2) Tenure
HighExp × Post	-0.007	-0.007
	(0.024)	(0.009)
Age44 × HighExp × Post	0.049**	0.020**
	(0.022)	(0.009)
Observations	332,000	332,000

Table 2.8... *Source:* Authors' calculations based on LEHD and SBO data.

Table 2.9: Heterogeneity in Incumbent Worker Outcomes by Education

	(1) Log(Earn)	(2) Tenure
HighExp × Post	0.015	0.003
	(0.012)	(0.007)
College × HighExp × Post	0.044***	0.018*
	(0.017)	(0.010)
Observations	332,000	332,000

Table 2.9... *Source:* Authors' calculations based on LEHD and SBO data.

Table 2.10: Heterogeneity in Effects of Bonus Depreciation by Labor Market Characteristics

	(1) Log(Earn)	(2) Tenure
HighExp × Post	0.035**	0.013
	(0.016)	(0.012)
Male × HighExp × Post	-0.02	-0.013
	(0.019)	(0.010)
Observations	332,000	332,000

Table 2.10... *Source:* Authors' calculations based on LEHD and SBO data.

Table 2.11: Heterogeneity in Incumbent Worker Outcomes by Local Labor Market Concentration

	(1) Log(Earn)	(2) Tenure
HighExp × Post	0.012	-0.002
	(0.013)	(0.008)
HHI × HighExp × Post	0.048**	0.010
	(0.022)	(0.014)
Observations	332,000	332,000

Table 2.11 reports difference-in-differences estimates of the differential effects of firm ownership changes on incumbent worker earnings and retention probabilities, with treatment indicators interacted with local labor market HHI in the year of acquisition. *Source:* Authors' calculations based on LEHD and SBO data.

Table 2.12: Rent-Sharing Elasticity Estimates: 1997-2007

	(1) OLS	(2) IV
ξ_{LowExp}	0.106***	0.064
	(0.013)	(0.109)
$\xi_{HighExp}$	0.102***	0.219*
	(0.012)	(0.116)
p[$\xi_{HighExp}$ - ξ_{LowExp}]	0.796	0.021
Weak IV F-stat		183.9
Observations	255,000	255,000

Table 2.12 reports OLS and IV estimates of equation 2.3 over the 1997-2007 period. *Source:* Authors' calculations based on LEHD and SBO data.

Table 2.13: Rent-Sharing Elasticity Estimates: 2008-2013

	(1) OLS	(2) IV
ξ_{LowExp}	0.059***	0.581***
	(0.014)	(0.177)
$\xi_{HighExp}$	0.078***	0.263*
	(0.011)	(0.142)
p[$\xi_{HighExp}$ - ξ_{LowExp}]	0.283	0.045
Weak IV F-stat		136.7
Observations	223,000	223,000

Table 2.13 reports OLS and IV estimates of equation 2.3 over the 2008-2013 period. *Source:* Authors' calculations based on LEHD and SBO data.

Chapter 3

Capital Investment and Labor Demand: Evidence from 21st Century Tax Policy

Any views expressed are those of the authors and not those of the US Census Bureau. The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this information product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 1667. (CBDRB-FY21-255)

"Everybody must be sensible how much labour is facilitated
and abridged by the application of proper machinery.
It is unnecessary to give any example."
—Smith [1776]

How the adoption of capital impacts workers is one of the foundational questions of the economics discipline. This question is ever more relevant in the 21st century given widespread concerns that tax incentives for investment may unnecessarily accelerate the adoption of new machinery at the expense of workers. Empirical attempts to answer this question face a number of challenges: investment decisions are endogenous to productivity and demand shocks, capital accumulation is a slow process, and few datasets exist that can measure how capital accumulation impacts the demand for workers that interact with machinery.

This paper combines confidential data from the US Census Bureau and quasi-experimental variation in the cost of capital due to a tax policy called bonus depreciation to overcome these challenges. Bonus depreciation, or simply bonus, lowers the cost of investment by allowing plants to deduct equipment expenses more quickly. By comparing plants that benefit the most from bonus to those that benefit less, we isolate investment in capital equipment that is likely independent of other idiosyncratic shocks faced by a given plant. By following plants between 1997 and 2011, our results measuring the impact of capital adoption on workers allow plants to fully adjust along multiple margins.

The combination of detailed plant-level data and cross-sectional variation in the generosity of tax incentives reveals a number of interesting facts. Difference-in-differences analyses show that plants respond to the tax policy by increasing their capital stock and employment, leading them to increase their overall output. In contrast, capital investment did not increase average worker earnings or plant productivity. Using these facts, we estimate a structural model that elucidates the economic forces that drive the reduced-form estimates. The model separates the effects of the policy into substitution and scale effects. We estimate that the scale effect—the increase in the use of all inputs due to lower production costs—accounts for 90% of the employment effects of the policy. Because production employment increased by more than the scale effect, the model shows that capital and production labor are complements in modern manufacturing. We conclude that tax policies that incentivize capital investment lead manufacturing plants to increase their scale, but do not lead these plants to replace workers with machines.

The policy we study, bonus depreciation, is one of the largest incentives for capital investment in US history and has been in nearly continual use since its inception in 2001. The US Treasury [2020] estimates that the version of bonus depreciation that was implemented as part of the Tax Cuts and Jobs Act of 2017 will cost the federal government $285 billion between 2019 and 2028. Bonus depreciation allows plants to deduct capital investments from their taxable income more quickly, lowering the cost of investment. The extent to which the policy affects the cost of capital depends on tax rules that govern how quickly investments can be deducted in the absence of the policy. Assets that are typically deducted more slowly benefit more from the tax incentive because bonus accelerates deductions from further in the future. Importantly, the benefits are determined by IRS rules and not by the useful life of any particular asset. By comparing plants that benefit the most from this incentive—those that invest more in equipment that is deducted slowly according to IRS rules—to plants that benefit less, we isolate investment in equipment that is likely independent of other drivers of capital accumulation.

The identifying assumption underlying our difference-in-differences estimation strategy is that, in the absence of the policy, outcomes for treated plants—the third of plants that benefit most from the policy—would track those of the remaining plants that benefit less. We provide support for the validity of this identifying assumption in a number of different ways. First, we verify that outcomes at treated and control plants evolved in parallel prior to policy implementation. Second, responses to the policy are much larger for eligible than for ineligible capital. Third, responses to the tax policy are not due to forces responsible for the recent decline in US manufacturing employment, including trends in capital intensity and skill intensity and exposure to import competition and robotization. Finally, we confirm that the effects of the policy are present in multiple datasets and are robust across a battery of specification checks.

Our baseline results use confidential data from the Census of Manufactures and the Annual Survey of Manufactures to estimate the joint effects of the policy on capital and labor demand. We estimate that treated plants increased investment flows by 15.8% relative to non-treated plants after the policy was implemented. An advantageous feature of Census data is the ability to measure capital stocks. We estimate a relative increase in overall capital of 7.8% between 2001 and 2011. These findings reject the notion that the increases in investment flows reflected a re-timing of investment. The relative increase in capital stocks among treated plants allows us to study the effects of capital accumulation on labor demand.

In contrast to the concern that capital investment displaces workers, we find concurrent increases in employment that more than match the capital investment response. By 2011, plants that benefited more from bonus had a relative employment increase of 9.5%. These gains were concentrated among production workers, whose employment increased by 11.5%. Non-production employment also increased by 8.1%. That workers operating production machinery saw the largest gains suggests that capital complements labor in modern manufacturing.

The effects of bonus on employment are robust across various data sources and specification checks. First, plant-level results are robust to allowing for trends that differ by state or by pre-period measures of plant productivity, plant size, and firm size. Second, we find similar effects using employment data at the state-industry level from the Quarterly Workforce Indicators (QWI). These results based on aggregate data show that accounting for plant entry and exit does not alter our findings. We also obtain similar estimates when using alternate cutoffs to define treated units or continuous measures of treatment intensity. Our results are not driven by trends in industries facing concomitant shocks: we find similar effects when we allow for differential trends along financing costs, adoption of information and communication technology (ICT), or the production of capital goods. We also find similar effects when we exclude high-tech industries. Third, to show that our results are not driven by differential exposure to business cycles, we use NBER-CES industry-level data starting in 1990 to document that industries that benefit more from bonus did not differentially respond to past recessions. Finally, we use data from decennial Censuses and the American Community Survey (ACS) to verify that bonus has larger employment effects for workers whose occupations indicate they operate production capital. Overall, these checks limit concerns related to our identification strategy and suggest that our results measure the average effect of bonus on employment across the manufacturing sector.

A popular rationale for investment tax incentives is the belief that capital investment will raise productivity and workers' wages. We estimate that average earnings decreased by 2.7% at treated plants. Using QWI data, we show that bonus led to a relative increase in the shares of young, less educated, women, Black, and Hispanic workers. These composition shifts fully account for the observed decrease in average earnings; our estimates rule out average earnings increases greater than 1.7% at the 95% confidence level. Thus, while workers benefit from the availability of additional jobs, which are more likely to be filled by otherwise disadvantaged workers, the policy does not significantly increase average earnings. Finally, though we do not find an increase in plant-level productivity, the policy did allow plants to increase their output.

We use our reduced-form results to estimate a structural model of factor demands that illuminates the economic mechanisms underlying the responses to the tax policy.

We first implement the insight of Marshall [1890] and Hicks [1932] that policies that change the price of inputs impact both plants' choice of cost-minimizing inputs (substitution effect) and their profit-maximizing output level (scale effect). We show that the scale effect is identified by a linear combination of our reduced-form estimates. We estimate that, by lowering costs of production, the policy increased the use of all inputs by 10% ($p < 0.001$) and that this scale effect was responsible for 90% of the overall effect of the policy on the demand for production workers. To a first-order approximation, the policy allowed plants to increase their scale; on average, plants did not replace workers with machines.

Our model shows that the elasticities of substitution between capital, production labor, and non-production labor are identified by our reduced-form estimates.[1] Using a Classical Minimum Distance approach, we estimate that the Allen elasticity of substitution between capital and non-production labor is close to 0.73.[2] This result follows from the fact that the scale effect is larger than the 8% increase in non-production employment. In contrast, the 11.5% increase in production employment yields an elasticity of substitution between capital and production labor of -0.44, implying that capital and production labor are complements.[3] We reject values greater than 0.13 for this elasticity of substitution at the 95% confidence level. In a series

[1]Since the identifying variation is based on industry-level differences in the benefit of bonus, we estimate average elasticities of substitution across the manufacturing sector. As we discuss in Section 3.6, the benefit from bonus is not correlated with industry-level estimates of substitution elasticities.

[2]When production takes more than two inputs, there are multiple ways to define elasticities of substitution [Blackorby and Russell, 1981] and these elasticities may take negative values if inputs are complements [Hamermesh, 1996]. Allen elasticities capture substitution between labor and capital relative to all other inputs. Our results are robust to using Morishima elasticities, which capture substitution between labor and capital relative to capital. We rely primarily on Allen elasticities because they separate substitution from scale effects.

[3]We show that these estimates are compatible with popular models of production by estimating the parameters of a translog cost function as well as a nested constant elasticity of substitution (CES) production function.

of empirical tests, we verify the complementarity of production labor and capital by showing that bonus increased investment more in plants with lower labor costs, as measured by plant-level unionization, location in a right-to-work state, and by local labor market concentration.

Finally, we show that our model estimates are robust to allowing for alternative policy mechanisms and to incorporating reallocation effects of the policy. First, we extend our model to allow for cash flow effects of the policy to impact labor demand. This extended model delivers similar elasticities of substitution. Second, we show that accounting for reallocation to more capital intensive plants and industries does not substantively change our findings. Specifically, we estimate similar elasticities of substitution using industry-level data and we find similar aggregate measures of substitution using the model of Oberfield and Raval [2021].

Our results build on classic studies that have estimated the effects of accelerated depreciation on business investment [Hall and Jorgenson, 1967, Cummins et al., 1994, House and Shapiro, 2008, Edgerton, 2010]. Using tax return data and modern causal inference methods, Zwick and Mahon [2017] made a substantial leap forward in our understanding of the effects of bonus depreciation. They showed the policy was very effective at stimulating investment, especially among small firms and those who saw immediate cash flow benefits. A subsequent literature also finds large effects of accelerated depreciation policies on investment [Ohrn, 2018, 2019, Maffini et al., 2018, Fan and Liu, 2020, Guceri and Albinowski, 2021]. Less attention has been paid to the effects of these policies on employment outcomes.[4,5]

[4]Zwick and Mahon [2017] estimate effects of bonus on payroll but not employment, Garrett et al. [2020] estimate regional employment effects, and Ohrn [2021] studies executive compensation. Tuzel and Zhang [2021] study the effects of state accelerated depreciation policies on computer purchases and the mix of occupational employment.

[5]Criscuolo et al. [2019] and Siegloch et al. [2021] both explore joint capital and labor responses to place-based policies in the UK and Germany, respectively. LaPoint and Sakabe [2021] estimate responses to a geographically targeted Japanese version of bonus depreciation.

This paper improves our understanding of the effects of bonus depreciation in a number of ways. While prior research studied short-term effects using consolidated firm-level data, our results capture the decade-long effects of bonus on individual production units. Our rich production data also allow for a more complete understanding of the effects of bonus on the manufacturing sector. In particular, we estimate novel responses to bonus depreciation, including on the accumulation of capital stocks, plant sales, total factor productivity, labor earnings, overall employment, employment for production and non-production workers, and workforce demographics.

Since bonus was implemented during a period of employment decline, we evaluate the concern that bonus simply props-up non-competitive plants or industries. Contrary to this concern, we find large employment effects on a balanced panel of plants, in new and younger plants that are more likely to grow, and in plants and industries with high capital and skill intensities, that are more likely to adopt robots, and that maintain an international comparative advantage. Overall, we find that the effects of bonus depreciation are concentrated on the plants and industries most likely to thrive in the 21st century.

Our paper also contributes to the literature estimating elasticities of substitution between capital and different types of labor, which are fundamental economic parameters. Prior estimates suggest that capital and labor are highly substitutable, implying that policies that lower the cost of capital may increase income inequality [e.g., Zucman and Piketty, 2014].[6] Inequality may also increase if production workers are more substitutable with capital than non-production workers, as per the "capital-skill complementarity hypothesis" [Griliches, 1969, Goldin and Katz, 1998, Krusell

[6]Recent studies focusing on a single type of labor include Karabarbounis and Neiman [2014], Doraszelski and Jaumandreu [2018], Raval [2019], Benzarti and Harju [2021], and Oberfield and Raval [2021]. Chirinko [2008] concludes that this parameter is between 0.4 and 0.6. A recent meta-analysis yields an average estimate of 0.9 (close to Cobb-Douglas), but shows that correcting for publication bias lowers the estimate to 0.3 [Gechert et al., 2021].

et al., 2000, Lewis, 2011]. We contribute to this literature by using quasi-experimental variation in the cost of capital over a ten-year period, detailed plant-level data, and a multi-input structural model to estimate substitution elasticities between capital and different types of labor. Our estimates show that workers are not highly substitutable with machines and are not compatible with the capital-skill complementarity hypothesis.[7]

Our findings are consistent with the recent literature exploring the effects of technologically-advanced capital on labor demand. Multiple studies show that firm-level adoption of robots increases labor demand [Acemoglu et al., 2020a, Dixon et al., 2021, Koch et al., 2021].[8,9] Hirvonen et al. [2022] find that, in response to a technology subsidy, Finnish firms increased their technologically-advanced capital and employment in the same way as we find that US firms responded to bonus depreciation. Aghion et al. [2022b] find that French firms that invested in modern manufacturing capital and automation also increased their employment due to gains in productivity and consumer demand. Consistent with these studies, we show in heterogeneity analyses that bonus had larger employment effects in industries that were more likely

[7]Our results are consistent with the finding of Beaudry and Green [2003], that faster capital accumulation could have tempered the rise in income inequality experienced in the US since the 1980s.

[8]Using industry-level variation, Klenert et al. [2020] show that the adoption of robots led to increases in employment without substantially changing the share of low-skill workers. Using similar methods, Graetz and Michaels [2018] conclude robot adoption did not decrease employment.

[9]Acemoglu and Restrepo [2020] and Dauth et al. [2021] show robotization can decrease local labor demand by making highly automated firms more productive and shifting market share away from relatively more labor intensive firms. Acemoglu et al. [2020b] show that, due to bonus depreciation, the US tax code has increasingly favored capital over labor, raising the concern that bonus could reduce employment and wages. Garrett et al. [2020] find bonus depreciation increased employment in local labor markets suggesting capital investments stimulated by the policy, which may include robots, do not lead to similar effects. See Aghion et al. [2022a] for a survey of research on the effects of automation on labor demand.

to adopt industrial robots.[10,11]

Section 3.1 describes accelerated depreciation policies. Section 3.2 discusses our data sources. Sections 3.3 and 3.4 present our research design and results. We place our results in the context of the transforming US manufacturing sector in Section 3.5. Section 3.6 estimates our model of factor demands and Section 3.7 extends our model to explore the roles of reallocation and cash-flow effects of the policy.

3.1 Investment Tax Incentives in the 21st Century

Governments around the world have used accelerated depreciation policies for more than 100 years to stimulate business investment. These policies were initially used to spur defense spending during the First World War, were used again in the military buildup to the Second World War, and were used as a means to replenish industrial capital stocks in the aftermath of these wars.[12] While these policies gained popularity in the post-war years, base broadening tax reforms stymied additional applications of accelerated depreciation during the later years of the 20th century.

In 2001, the use of these policies came back into vogue when the US introduced "Bonus Depreciation." The policy allows firms to deduct a bonus percentage of the cost of equipment investment from their taxable income in the year the investment is made. Because costs are typically deducted slowly over time, bonus lowers the present

[10] Benmelech and Zator [2022] show robots account for less than 0.3% of equipment investment worldwide during our sample period. That robots likely account for only a small amount of all capital investment stimulated by bonus likely explains the divergence between our results and those of Lewis [2011] that suggest workers without high school degrees are substitutes for automative technologies.

[11] A number of studies show that adoption of ICT increased the relative demand for "skilled" workers who typically engage in non-routine, cognitive tasks [Autor et al., 1998, 2003, Akerman et al., 2015, Gaggl and Wright, 2017]. Interpreting our results in light of these findings suggests that bonus did not shift investment towards ICT or other types of skill-complementing capital.

[12] See Koowattanatianchai et al. [2019] for a historical account of accelerated depreciation policies.

value costs of new investments. For example, under 50% bonus, firms immediately deduct an additional 50% of investment costs. The remaining 50% of the costs are deducted according to normal depreciation schedules—usually the Modified Accelerated Cost Recovery System (MACRS). In addition to bonus, firms could also benefit from an accelerated depreciation policy referred to as §179 ("Section 179"), which allowed for full expensing of investment costs below a dollar limit.[13] Throughout the paper, we interpret our results as the combined effect of these policies.

Bonus and accelerated depreciation policies more generally have been politically popular because they only change the timing of tax deductions for businesses. Therefore, the cost of the policy appears very small over long time periods that do not account for the time value of money, such as in the case of the Congressional Budget Office's (CBO) ten-year forecasting window. Its popularity is, in large part, responsible for its near continuous use since 2001. Despite the CBO's generous measurement, bonus has real costs as a tax expenditure and real value as a subsidy because of the relative change in timing.

To understand the mechanics of bonus, consider a plant with a discount rate of 7% and a tax rate of 35% that purchases a computer for \$1,000, which would normally depreciate over five years.[14] With straight-line depreciation, the firm deducts \$200 each year from its taxable income, which lowers its tax liability by $\$200 \times 0.35 = \$70$. Under 50% bonus, the firm instead depreciates a bonus portion in the first year and receives an immediate deduction from taxable income of \$600 ($= \$500 + (\$500 \times 0.2)$), but only deducts \$100 in years two through five. In both cases, the firm deducts the full value of the asset over five years which, ignoring the time value of money,

[13] This dollar limit increased from \$24,000 to \$500,000 between 2001 and 2011. Between 2003 and 2011, the share of equipment investment that qualified for §179 was stable and averaged 12% [Kitchen and Knittel, 2016].

[14] This example ignores practical aspects of tax accounting, such as the the half-year convention.

lowers its total tax liability by $350. Using a discount rate of 7%, the depreciation deductions without bonus are only worth $307.10 in present value (PV) terms, while the deductions under 50% bonus have a PV of $328.55, 7% more than in the baseline. In this case, bonus decreases the after-tax cost of the investment by $21.45, or 3.1% relative to the original cost.

To see how bonus depreciation works in a more realistic setting, we start from the observation that the IRS sets tax depreciation schedules [IRS, 2002, see Table A.1 of Publication 946]. Figure 3.1 shows examples of MACRS schedules for a tractor trailer (a three-year asset) in Panel (A) and a barge (a ten-year asset) in Panel (B). The blue bars in this figure represent depreciation deductions over time in the absence of bonus depreciation. These schedules already partially front-load depreciation deductions. The orange bars show the schedule of deductions with 50% bonus depreciation. The benefit of bonus depreciation depends on the extent to which depreciation deductions are accelerated forward in time. Contrasting the two panels, it is clear that both assets benefit from bonus depreciation, but the asset that is depreciated more slowly according to IRS rules (i.e., the barge) benefits more. The fact that similar assets differentially benefit from bonus is at the heart of our identification strategy.

While this realistic example is instructive, it is useful to have a measure of the benefit of bonus depreciation that applies to all assets. Let z_0 be the original PV of depreciation deductions per dollar of investment and let b be the bonus depreciation percent. Under bonus, the PV of depreciation deductions per dollar of investment, z, is given by $z = b + z_0 \times (1 - b)$. The fact that $\frac{\partial z}{\partial b} = 1 - z_0$ shows that bonus provides a larger subsidy to capital that is depreciated more slowly according to IRS rules. As in Figure 3.1, assets such as a barge—those with lower z_0—benefit more from an increase in b.

In the US, each asset class is assigned a depreciation schedule, which determines

z_0. For equipment used in production, asset classes are defined by the activity for which a given piece of equipment is used. These classes align closely with NAICS industry definitions, instead of depending on the useful life of a specific asset.[15] For example, while equipment related to cutting timber is depreciated over a five year period, equipment used in the creation of wood pulp and paper is subject to a seven year schedule. Therefore, plants in different industries could use similar or identical equipment, but face different depreciation schedules. In Section 3.3, we discuss how we measure z_0 at the industry level.

It is important to consider that several real-world factors shape the application of accelerated depreciation policies. First, firms may not claim bonus if they have a tax loss or for other reasons [Kitchen and Knittel, 2016]. Our estimates therefore capture the effect on all firms, including those that are eligible for bonus but are not able to immediately benefit from the policy.

Second, while the generosity of bonus varied over time, accelerated depreciation policies were in nearly continuous use between 2001 and 2011 and significantly lowered the cost of investment. Panel (C) of Figure 3.1 shows the effective bonus rate for two levels of investment, \$400,000 and \$1,000,000. The \$400,000 investment benefits from accelerated depreciation in all years after 2001 while the \$1,000,000 investment benefits in all years after 2002 with the exception of 2006 and 2007. The average bonus rate between 2001 and 2011 was 45%.[16] Using this bonus rate and estimates

[15]Since 1986, class lives are formally defined in Revenue Procedure 87-56, 1987-2 C.B. 674 [IRS, 2002]. The procedure establishes two types of depreciable assets: (1) specific assets used in all business activities in Table B-1 and (2) assets used in specific business activities in Table B-2. For equipment used in manufacturing plants, most class lives are determined using Table B-2, which align closely with industry definitions. MACRS class lives were based on the Accelerated Cost Recovery System (ACRS) from 1981. Class lives in ACRS "were not intended to reflect actual useful lives, or even some percentage of the useful lives" [Brazell et al., 1989]. Appendix B presents more historical context of MACRS class lives.

[16]This rate combines 100% expensing for the 12% of §179 eligible investment with the average bonus rate between 2001 and 2011 of 38% for the remaining amount. Appendix C describes details of bonus depreciation and §179 expensing policies.

from Zwick and Mahon [2017] based on IRS data, we calculate that by increasing the PV of depreciation deductions, bonus lowered investment costs by 2.5%, on average.

Third, while the bonus amount varied over time, plants likely expected their investments to benefit from bonus in all years after 2001. These expectations were shaped by repeated extensions, increases in generosity, and several retroactive applications of the policy. In fact, Auerbach [2003] correctly predicted the 2003 increase in bonus depreciation generosity using an ordered probit model before it happened. Further supporting the view that firms expected to continually benefit from bonus, House and Shapiro [2008] estimate that in 2006, firms behaved as though the bonus depreciation rate was between 25% and 50% even when the statutory bonus depreciation rate was zero.

Finally, bonus impacts the cost of capital both by increasing the present value of depreciation deductions as well as by providing immediate cash flow. Bonus is economically equivalent to giving a firm that purchases a qualified asset an interest-free loan equal to the bonus portion multiplied by the tax rate and the value of the asset. The business *de facto* pays the loan back since it cannot take the tax deductions it would have taken under MACRS in later years. Recognizing the equivalence of bonus to an interest-free loan, Domar [1953] first theorized that accelerated depreciation policies could be especially valuable for financially constrained firms or those that would prefer to rely on retained earnings to finance capital investments. Edgerton [2010] and Zwick and Mahon [2017] provide evidence that financing constraints help shape the response of investment to bonus depreciation.[17] The total impact of bonus on the cost of capital is therefore likely to significantly exceed the value of depreciation deductions alone.

[17] Criscuolo et al. [2019] use similar logic to motivate the importance of credit constraints in shaping responses to industrial policies in the UK.

From the perspective of policy analysis, our reduced form estimates capture the ten-year cumulative effects of bonus depreciation on investment and employment, inclusive of these real-world factors surrounding the policy. In Section 3.6, we recover the implied effect of bonus on the cost of capital using our reduced form estimates that incorporate these factors.

After the US implemented bonus in 2001, a number of large economies have followed suit, using very similar instruments to decrease capital investment costs. These include the UK [Maffini et al., 2019], China [Fan and Liu, 2020], Canada, and Poland [Guceri and Albinowski, 2021]. Today, bonus and accelerated depreciation policies are being deployed to combat the world's largest economic crises, including global warming and the COVID-19 pandemic.[18] These trends highlight the importance of bonus depreciation and related policies in shaping investment and potentially labor demand in the 21st century.

3.2 Sources of US Manufacturing Data

This section describes the main datasets we use to measure the effects of bonus depreciation on various manufacturing outcomes; Appendix A precisely defines each of our variables.

We construct our primary dataset using the Census of Manufactures (CM), the Annual Survey of Manufactures (ASM), and the Longitudinal Business Database (LBD). The CM and the ASM are establishment-level manufacturing datasets containing detailed information on plants' inputs and outputs and are considered the

[18] The United Kingdom, Sweden, Russia, Germany, Ireland, Romania, and France have all relied on similar policies to speed the transition to environmentally sustainable production methods [Koowattanatianchai et al., 2019]. Australia, Austria, Germany, and New Zealand all included accelerated depreciation policies in their fiscal stimulus responses to the COVID-19 pandemic [Asen, 2020].

workhorse datasets of the US Census Bureau's Economic Census. The Census collects CM data quinquennially from the universe of manufacturing establishments in years ending in 2 and 7 (1997, 2002, 2007 in our data). The ASM collects annual data in all non-CM years for a sample of approximately 50,000 plants. Plants are selected to be part of the ASM in the year following the CM and are surveyed annually until the year after the following CM, when a new wave of ASM plants is selected. Larger plants are oversampled in the ASM and the largest plants are selected with certainty.

The ASM/CM data provide a unique opportunity to study how tax incentives for capital investment affect production. These data focus on plant-level production processes and include detailed measures of investment, materials cost, and total value of shipments (a proxy for plant-level revenue). CM data measure capital stocks directly and we integrate ASM data to construct capital stock measures using the perpetual inventory method in non-CM years [as in Cunningham et al., 2020]. The full picture painted by our data allows us to study how plants adjust production in response to the policy and our measure of output captures the scale effect of the policy. Another advantage of these data is that they include several measures of labor inputs: the number of workers (i.e., employment), total payroll, average worker earnings, and number of hours worked. We also observe whether labor was employed in production or non-production related tasks. This division of employment by tasks allows us to test the popular concern that production-related tasks are at risk of being automated, particularly in response to policies that lower the cost of capital. Finally, we combine information on employment, capital stock, and material inputs to estimate plant-level measures of total factor productivity (TFP).[19] To avoid sensitivity to outliers, we winsorize all variables at the 1% level.

[19]Following Criscuolo et al. [2019], we estimate residual TFP using industry-level cost shares. See Appendix A for details.

Our baseline regressions are performed on a balanced panel of establishments that are present in the ASM/CM between 1997 and 2011. A particular advantage of these data is that they allow us to track differences between treated and control plants for five years prior to policy implementation and to measure the effects of the policy over a 10 year horizon. To construct this sample we use establishment identifiers from the LBD that consistently track plants over time. Our final ASM/CM sample consists of approximately 160,000 plant-year observations. Our balanced sample sidesteps concerns that changes in the ASM sample construction across time could insert noise and discontinuous breaks in our results. Additionally, tracking capital accumulation and employment over a 15 year period eliminates concerns that plant responses may be constrained by adjustment frictions. By focusing on a balanced panel, our baseline results speak to how existing plants respond to the policy.

Due to the Census Bureau's ongoing concern with data privacy and disclosure risk [see, e.g., Abowd and Schmutte, 2019], we do not report summary statistics.[20] Chen [2019] and Giroud and Rauh [2019] relied on similar estimation samples using these data and disclosed summary statistics. The average plant in a similarly balanced panel has 165 employees, 77% of which are engaged in production-related tasks; capital investment averages $736,000 per year, of which 81% is in equipment [Chen, 2019].

In a number of analyses, we rely on complementary data from the publicly-available Quarterly Workforce Indicators (QWI) [see, e.g., Abowd et al., 2009, Curtis, 2018]. The underlying microdata for QWI come from the Longitudinal Employer Household Dynamics program. These data are primarily derived from state unemployment insurance systems and also include worker and firm characteristics from a

[20]It is common practice for papers relying on confidential Census Bureau data to not report variable means or other summary statistics for analysis samples [see, e.g., Foster et al., 2008].

variety of surveys and administrative sources. We collapse these data at the industry-state level. These data complement the ASM/CM data in three ways. First, they allow us to explore whether bonus had different employment effects on workers with different characteristics, including education, gender, age, race, and ethnicity. Accounting for the effects of bonus on the demographic composition of the workforce refines our understanding of the wage effects of the policy. Second, our state-industry analyses account for any potential effects of the policy on entry and exit. Third, we use these data to estimate the effects of bonus on plants that are not included or that are underrepresented in our ASM/CM sample, such as small and young firms.

Finally, we also use the NBER-CES Manufacturing Industry Database. These data rely on ASM/CM data to construct industry-level measures of employment and capital stocks. Relative to our balanced plant-level sample, our estimates using these data incorporate the effects of the policy inclusive of entry, exit, and reallocation across plants within industries.

3.3 Identifying Responses to Bonus Depreciation

Our research strategy compares how bonus depreciation impacted manufacturing outcomes across industries that differentially benefited from the policy. We first describe how we classify which industries benefited the most from bonus depreciation. We then describe our event study, difference-in-differences framework that uses this classification to identify how US manufacturing plants responded to the policy.

3.3.1 Treatment Variation in Bonus Depreciation

Recall from Section 3.1 that the plants that benefit the most from bonus are those that would depreciate assets over a longer time horizon in the absence of the incentive, i.e. those with lower values of z_0. We rely on industry-level (4-digit NAICS codes) measures of z_0 based on administrative tax return data from Zwick and Mahon [2017] and classify plants into the treatment group if they are in an industry j that benefits the most from bonus depreciation. Let Bonus_j be an indicator equal to one if the plant's z_0 is in the bottom tercile of the z_0 distribution.[21] Relying on the z_0 distribution also captures variation in the cost of capital due to §179 expensing. Like bonus, §179 most benefits plants that invest in assets that are depreciated more slowly according to IRS tax rules.

We rely on this binary treatment for two reasons. First, to calculate z_0, some assumption of discount rates must be made. By relying on this simple dichotomy, our treatment indicator is agnostic with regard to discount rates. Second, there is a clear break in the z_0 distribution at the 33rd percentile, making this a natural comparison of most- to less-treated units.[22]

Our indicator of bonus treatment is designed to mitigate endogeneity concerns. One specific concern in this context is that bonus depreciation may affect the mix of investments across asset classes. As a result, an industry's z_0 may be endogenous with regard to the policy. This concern is allayed by the fact that our measure of

[21] For each asset class, Zwick and Mahon [2017] calculate z_0 using a discount rate of 7%. Using data from IRS form 4562, they compute industry-level z_0s by aggregating the asset-class measures according to their importance in an industry's overall investment.

[22] We show this natural break in Panel (A) of Figure A1c, which presents a histogram of the z_0 distribution across industries. Zwick and Mahon [2017, §III.B, p.228] also classify plants in the bottom tercile of the z_0 distribution as treated in their dichotomous treatment definition. Garrett et al. [2020] obtain similar estimates of bonus on local labor markets when defining dichotomous treatments at the 25th, 33rd, and 40th percentiles. As we show below, we also obtain similar results when we define treatment status using these different thresholds or when using the continuous variation in z_0.

z_0 is calculated using only eligible investments made in the non-bonus periods of our sample. As these investments are less likely to be affected by bonus, the z_0 distribution and our bonus indicator should not be endogenous with respect to the policy.[23] Additionally, recall that IRS asset classes are defined by asset *use* and not *type*. A plant's z_0 is unlikely to change even when plants change the types of assets they purchase, because their use is unaffected by the policy.

3.3.2 Empirical Specifications

We estimate the effects of bonus on manufacturing outcomes using event study difference-in-differences regressions of the form

$$Y_{it} = \alpha_i + \sum_{y=1997,\ y\neq 2001}^{2011} \beta_y \big[\text{Bonus}_j \times \mathbb{I}[y=t]\big] + \gamma \mathbf{X}_{i,t} + \varepsilon_{it}, \tag{3.1}$$

where Y_{it} is an outcome of interest for plant i in year t and industry j. α_i is a plant-level fixed effect that captures all time-invariant components of manufacturing activity. $\mathbf{X}_{i,t}$ is a vector of fixed effects that varies across specifications. The coefficients β_{1997} through β_{2011} describe the relative outcome changes for plants that benefit most from bonus relative to 2001.

The identifying assumption behind this strategy is that outcomes at treated and control plants would evolve in parallel in the absence of bonus. This assumption is likely to hold because differences in z_0 are generated by the assignment of IRS depreciation schedules to different types of assets generally defined by their use rather than their useful lives. The primary threat to this assumption is that other trends during the time period correlate with bonus treatment. Because Bonus_j varies at

[23] We also address this endogeneity concern empirically by investigating the stability of z_0 over time in Appendix C. There, we use sector-level IRS SOI data on investment shares in each asset class to show that sector-level z_0s are stable over the years 2000–2011.

the industry level, we cannot include industry-year fixed effects to directly address this threat. Instead, we rely on a number empirical tests to support our identification assumption. First, we use the event study estimates to compare pre-period trends in outcomes between the treated and control units. In this context, the absence of differential trends suggests that the identifying assumption is likely to hold in the post-period. Second, we use the fact that, while equipment capital was eligible for bonus depreciation, investment in structures was generally not eligible. We separately estimate effects of bonus depreciation on eligible equipment capital and ineligible structures capital. Larger effects on treated equipment capital suggest we are precisely measuring the effect of bonus depreciation and not of other shocks that would violate our identifying assumption. Third, we show that our results are robust to including state-by-year fixed effects and flexible controls for trends related to plant characteristics. Specifically, we include plant size bins interacted with year fixed effects, firm size bins interacted with year fixed effects, and TFP bins interacted with year fixed effects.[24] These controls ensure that the effects of bonus are not confounded by trends that affect plants or firms of different sizes or productivity. Finally, in Section 3.5, we additionally show that our results are unrelated to major drivers of manufacturing transformation in the 21st century, including changes in capital and skill intensities, import competition exposure, and robotization.

We quantify the effects of bonus depreciation in two ways. First, we estimate the average effect of bonus over the full treatment period using pooled regressions of the form

$$Y_{it} = \alpha_i + \beta[\text{Bonus}_j \times \text{Post}_t] + \gamma \mathbf{X}_{i,t} + \varepsilon_{it}. \tag{3.2}$$

[24]Plant size is determined by the book value of assets in 2001 and firm size is defined as the count of employees in all establishments across a firm in 2001. We define four bins for each variable.

The difference-in-differences (DD) estimate, β, measures the average increase in an outcome for the treatment group relative to the control group. Second, because many of our outcome variables (such as capital and employment) are stocks that evolve slowly over time, we also report long-difference (LD) estimates, which correspond to β_{2011} in Equation (3.1). LD estimates measure the cumulative effect of accelerated depreciation policies on plant outcomes over the ten-year period 2002–2011.[25] One major benefit of measuring ten-year effects is that adjustment costs are unlikely to dramatically affect these results. Because federal bonus depreciation interacts with the design of state tax systems, we cluster standard errors at the 4-digit NAICS-by-state level following guidance in Bertrand et al. [2004] and Cameron and Miller [2015].[26]

3.4 Effects of Bonus Depreciation on US Manufacturing

This section presents our estimates of the effects of bonus depreciation on manufacturing outcomes. We first measure the effects of the policy on investment and capital stocks. Next, we estimate the effects of bonus on labor demand, as measured by employment and earnings per worker. Finally, we characterize how the policy affects plant output and productivity.

[25] To minimize the number of disclosed coefficients, we only report LD estimates for select specifications.

[26] Appendix D describes these interactions and shows that our results are generally robust to clustering at the industry level, which is more conservative.

3.4.1 Investment and Capital Stock Responses

We begin by exploring the effects of bonus depreciation on investment in physical capital. Panel (A) of Figure 3.2 shows the results of estimating Equation (3.1) when the outcome is log investment. Three results are immediately apparent. First, differences in investment between the treatment and control groups are small and stable in the pre-period, supporting the validity of our empirical strategy. Second, log investment for the treated group jumps by nearly 10% immediately upon policy impact in 2002 and remains elevated throughout the post period. These differences are statistically significant in all years after 2002. Third, while our baseline estimates include plant and state-by-year fixed effects, we obtain similar estimates when we flexibly control for time trends based on plant size, firm size, and productivity. The sustained relative increase in investment captured by each series suggests accelerated depreciation policies increase investment levels rather than only shifting capital expenditures across years. On the whole, these results show that bonus depreciation has a large and statistically significant effect on investment behavior in the manufacturing sector, confirming that the findings of House and Shapiro [2008] and Zwick and Mahon [2017] hold in our setting.[27]

Panel (A) of Table 3.1 presents estimates of the effects of bonus on log investment. Column (1) reports difference-in-differences (DD) estimates with only plant and year fixed effects and shows a relative investment increase of 17% ($p < 0.001$). Estimations that progressively include state-by-year fixed effects, plant size bins-by-year fixed effects, TFP bins-by-year fixed effects, and firm size bins-by-year fixed effects yield a

[27] As we discussed above, Zwick and Mahon [2017] use the same threshold for bonus treatment in their event study analyses, which show that investment in treated firms increased by 11.8% relative to firms in the control group between 2002-04. Over that same period, our event study coefficients indicate that investment for the treatment plants increased by 10.1%. See Appendix E for a detailed comparison to earlier estimates.

narrow range of estimates between 15.1 and 15.8%.[28]

Since investment data can include spells of non-investment, we consider alternative outcome variables that capture extensive margin responses. Panel (B) of Table 3.1 estimates the effect of bonus depreciation on the inverse hyperbolic sine (IHS, i.e., $\ln(x + \sqrt{x^2 + 1})$) of investment. The IHS of investment captures both intensive and extensive margins of response and takes similar values as the simple log outcome for large values of investment. The results in Panel (B) are nearly identical to those in Panel (A), suggesting that extensive margin responses to the policy are relatively unimportant in our sample of large plants. Panel (C) of this table reports the effects of bonus on investment scaled by the pre-period capital stock. This outcome also captures extensive margin responses and shows that bonus led to significant increases in investment.[29] In sum, across all three investment outcomes we find large, positive, and statistically significant effects of bonus depreciation on capital expenditure.

One strength of the ASM/CM data is that we observe measures of capital stock used in production. Given the large investment response, we also expect the policy to increase the capital stock of treated plants. We show that this is indeed the case in Panel (B) of Figure 3.2. Differences in the capital stock between treated and untreated plants are not statistically significant in the pre-period. The graph then shows that, relative to plants that benefited less from bonus, treated plants saw a persistent increase in their capital stock. This increase is robust to the inclusion of additional controls. Given this gradual increase, we focus on the long-differences (LD) estimates of bonus. Columns (1) and (2) of Table 3.2 show that by 2011 bonus

[28]Column (2) includes the same controls as the "Baseline" estimates presented in Panel (A) of Figure 3.2 and column (5) corresponds to the specifications with "Additional Controls."

[29]These estimates can be translated into percent increases by dividing the coefficient by investment as a share of pre-period capital. Assuming this fraction is 0.2, the estimate from column (5) in Panel (C) implies that bonus increased investment by 13.9%. Corresponding event study coefficients are presented in Figure A3.

depreciation led to a relative increase in the capital stock of between 7.78 and 8.04%.

ASM/CM data also allow us to separately estimate the effects on equipment and structures. Columns (3)–(6) of Table 3.2 show that the ten-year effect of bonus depreciation on equipment capital stock is three times larger than the effect on the stock of structures. Because bonus depreciation mostly applied to equipment investment during our period, finding a larger equipment response gives credence to our argument that estimated responses are due to the tax policy itself and not to other coincident unobservable shocks. In addition to serving as a useful validating exercise, these estimates are informative of how plants combine different types of capital in production. As we discuss in Section 3.6, bonus may influence investment in structures through both a scale effect and a substitution effect.

3.4.2 Labor Demand Response

Our results thus far verify that in our setting, bonus depreciation had large, positive impacts on investment and capital stocks in the US manufacturing sector. We now turn our attention to the important but under-explored question of whether plants used this increase in capital to replace workers, or if plants hired additional workers to interact with the new machinery.

Figure 3.3 shows event study coefficients depicting the effects of bonus on log employment. Both our baseline and additional controls specifications show that treated and control plants had similar employment trends before 2001. In 2002, we immediately observe that, relative to control plants, treated plants saw a large and statistically significant increase in the number of workers. This effect continues throughout the sample period and increases further in later years.

Panel A of Table 3.3 reports estimates of the effects of bonus on employment. Column (5) shows that employment at treated plants increased by 7.9% ($p < 0.001$), on average, between 2001 and 2011. Across our different sets of controls, this difference-in-differences estimate ranges between 7.85 and 8.5%. The long difference estimate in column (7) shows that, by 2011, the plants that benefited most from bonus had a relative employment increase of 9.5% ($p < 0.001$). Not only are the effects of bonus on the employment stock large and statistically significant, they are also larger than the effects of the policy on the capital stock. This finding is surprising given the popular concern that modern equipment investment is labor replacing and that the tax policy we study directly stimulates such investment.

An immediate question raised by this finding is whether the increase in employment is driven by production workers who directly interact with machines or by workers specializing in non-production tasks, such as management or sales. Relative to other administrative datasets that do not capture production tasks (e.g., the LEHD or IRS tax data), the ASM/CM data provide a unique opportunity to answer this question.[30] As we show in Panels (B) and (C) of Table 3.3, the point estimate of the effect of bonus on production employment is larger than that on non-production employment across all our specifications.[31] Comparing the long differences estimates in column (7), we find that the effect on production employment is more than 40% larger than the effect on the employment workers specializing in non-production tasks.[32] Our results are therefore not consistent with the hypothesis

[30] We follow Berman et al. [1994] in using the production/non-production task dichotomy in the ASM data when estimating labor demand. As we show below, we find similar results using Census data and task definitions related to manufacturing production in Acemoglu and Autor [2011].

[31] Using the DD specification in column (2), we reject the hypothesis that $\beta^{\text{Prod}} < \beta^{\text{Non-Prod}}$ with a p-value of 0.0214. We obtain a p-value of 0.14 for the same test using our LD estimates in column (6).

[32] Panels (A) and (B) of Figure A5 present event study graphs of the effects of bonus on production and non-production employment. As we show in Table A3, the result that the effect of bonus on production employment is larger than for non-production employment is robust to measuring employment in terms of hours worked. This table also shows that plants increase their use of

that bonus induced a shift from production employment to automated technologies or to technologies that are more likely to be complementary to non-production employment.

As we discuss in Section 3.2, the results above focus on a balanced panel of plants. One possibility that is not captured by our baseline results is that, facing a lower cost of capital, new plants may choose to engage in more capital-intensive forms of production. If this were the case, and if entry comprised an important share of overall capital investment, the large effect on employment could disappear when including the effect of bonus on new firms. To explore this possibility, we now estimate the effects of bonus on employment using QWI data at the state-industry level.[33] Importantly, these aggregated data capture extensive margins of response, such as plant exit or entry, that our balanced panel omits by construction. Figure 3.4 shows event study estimates of bonus depreciation on employment using quarterly data at the state-industry (4-digit-NAICS) level from QWI. We include state-by-industry and state-by-quarter fixed effects in this regression. We observe no differential pre-trends between treated and control industries and employment in treated industries increases shortly after the policy is implemented. The effect of bonus on employment grows through the end of the panel. Finally, the dynamics of the event study estimates are a near perfect match with the ASM/CM estimates presented in Figure 3.3.[34] These results suggest that entry and exit margins do not substantially alter our estimates of the effects of bonus depreciation on employment.

Due to the balanced panel nature of our ASM/CM data, our baseline results are not representative of smaller or younger firms. Panel (A) of Figure A6 estimates the

materials in response to bonus.

[33]All QWI regressions are weighted according to 2001 state-industry employment.

[34]Column (1) of Table A4 reports corresponding regression coefficients.

effects of bonus depreciation on smaller firms—those with 50 or fewer employees—and shows that bonus had similar effects on the employment of small firms. Panel (B) studies the effects of bonus on firms 0–5 years old and shows that bonus also elevated the employment of young firms. The similar results for small and young plants show that the effect of bonus on employment is not confined to the sample of large plants in our balanced panel and is generalizable to the full US manufacturing sector.[35]

Additional Robustness Checks

Before analyzing the impact of bonus on labor earnings and productivity, we demonstrate the robustness of the effects on employment.[36] First, in Panel (A) of Figure A7, we also show that we obtain similar results using the continuous variation in z_0.[37] We also estimate the effects of bonus on employment using alternative treatment cutoffs. Panel (A) of Figure A8 shows that we find similar employment effects when we define treatment using the 25th and 40th percentiles of the z_0 distribution.

We now show that our results are robust to controlling for a number of potential confounding factors. First, one potential concern is that producers of capital goods benefit from the policy both by a reduction in the cost of production and an increase in the demand for their products. If this were the case, our estimates would overstate the effects of a reduction in the cost of investment on labor demand. To assess this possibility, we measure the share of each industry's output that is used in non-

[35]The slightly larger effect for young firms is consistent with Isphording et al. [2021], who suggest that young firms are more likely to be financially constrained than small firms.

[36]Due to disclosure limits related to the use of Census Data, we rely primarily on QWI data at the industry-state level to perform these robustness checks.

[37]Additionally, Panel (B) of Figure A7 relates the treatment intensity z_0 to employment growth and shows that industries with lower values of z_0 experienced relatively larger increases in employment. The strong linear relationship between z_0 and employment growth explains why our results are not sensitive to how we define exposure to bonus in our analyses.

residential investment in 2001. In Panel (B) of Figure A8, we show that we find almost identical effects of bonus on employment when we include interactions of this measure with year fixed effects. Second, an additional concern is that plants that benefit most from bonus have different costs of capital, which could potentially bias our results. Panel (C) of Figure A8 shows that our results are robust to controlling for industry-level quintiles of effective interest rates from Compustat interacted with year fixed effects.

In Figure A9, we show that our results are not driven by growth in ICT intensive industries or "tech" industries. We use two separate measures of ICT intensity. First, we use BEA data to construct the share of ICT capital in the pre-period. Second, we use a measure of the share of workers engaging in ICT-related tasks during the period 2002–2016 from Gallipoli and Makridis [2018]. Panel (A) shows that we continue to find large employment effects when controlling for tercile bins of either measure interacted with year fixed effects. In Panel (B), we present event study plots after dropping "tech" industries.[38] All three series of estimates continue to show bonus depreciation has a large and statistically significant effect on employment, suggesting growth in ICT-intensive or high-tech industries does not substantially bias our estimates.

The result that the employment effect of bonus is concentrated on workers that interact with machinery relies on correctly identifying production tasks. In Appendix G, we map occupation data from the decennial Census and the American Community Survey to the routine/non-routine and cognitive/non-cognitive classifications from Acemoglu and Autor [2011]. As we show in Figure A10, when we use this definition of production occupations, we continue to find that bonus has larger effects on the

[38] Based on Heckler [2005], "tech" industries have more than 25% of workers in technology oriented occupations.

employment of production workers, who are primarily engaged in routine, manual tasks. We also find large effects for all routine-task workers, further reinforcing the conclusion that the benefits of modern capital investments are not solely absorbed by professional workers (i.e., those in non-routine, cognitive occupations).

Since bonus depreciation was enacted as a countercyclical fiscal measure, one concern is that the industries that benefit most from bonus also experience differential exposure to the business cycle. To show that our results are not driven by differential exposure to the business cycle, we use NBER-CES industry-level data to estimate the effects of bonus on investment and employment going back to the 1991 recession. As we show in Figure A11, industries that benefit most from bonus did not have differential trends during the 1991 recession. Moreover, these industry-level results confirm that bonus depreciation increased both investment and employment after 2001.

Finally, as shown in Garrett et al. [2020], bonus depreciation can have spillover effects on local labor markets. One potential concern is that our results may capture these spillover effects in addition to the reduction in the cost of capital. In Table A6, we show that we obtain similar plant-level effects of bonus on employment and investment when we additionally control for local exposure to bonus depreciation.[39] Overall, these robustness checks support the interpretation that our estimates capture the plant-level effects of a policy-driven reduction in the cost of capital on employment.

[39] As in Garrett et al. [2020], we measure local exposure to bonus using the share of workers in long duration industries in a given county. The finding that bonus has positive spillover effects on employment assuages the concern that the policy may hurt workers through negative market-level spillover effects [e.g., as in Acemoglu et al., 2020a]. In addition to showing that we obtain similar average plant-level effects, we do not find evidence that plant-level effects vary according to local exposure.

3.4.3 Labor Earnings

Policymakers often motivate the use of tax incentives for investment by arguing that worker pay will rise as plants increase investment [e.g., CEA, 2017]. To investigate this claim, we measure the effect of bonus depreciation on the log of total plant payroll divided by total plant employment. Figure 3.5 presents event study plots of the effects of bonus on average worker earnings. Relative to control plants, workers in treated plants saw a decrease in average earnings per worker. Columns (1)–(5) of Panel (A) of Table 3.4 show that relative earnings dropped by close to 2% in the post-period.[40] These results are especially surprising given the increase in labor demand we documented in the previous section.

A natural explanation for the negative effect of bonus on average earnings is that bonus changes the composition of the workforce. We use QWI data to show that this is the case. As in previous analyses using QWI data, we include state-by-industry and state-by-quarter fixed effects. In addition, we include flexible controls that ensure that our estimates are not contaminated by ongoing changes in the demographic composition of the manufacturing workforce during the period. Specifically, we include bins of changes in employment between 1997–2001 for a given demographic group at the state-industry-level interacted with year fixed effects.

Figure 3.6 presents event studies showing the effects of bonus on the employment of different demographic groups. Panel (A) presents two series of estimates, one for workers with a high school-level education or fewer years of education and another for workers with more than a high school-level education. This plot shows a stronger response for workers with fewer years of education. The difference-in-differences estimate on workers with fewer years of education is larger by 3.9 percentage points

[40]We find a similar negative effect when we estimate the impact of bonus on average earnings using QWI data; see column (2) of Table A4.

(p<0.001). REDO A4 AND FIX LATER These differential effects alter the composition of the workforce, increasing the share of lower education workers by 1.0%.[41] Panel (B) of Figure 3.6 presents analyses for workers above and below 35 years of age. We estimate larger employment effects on younger workers. The employment effects of bonus are larger by 9.7 percentage points (p<0.001), which increases the share of younger workers by 3.8%. Panels (C) and (D) also show stronger and statistically distinct responses to bonus depreciation for women (relative to men) and for Black and Hispanic workers (relative to white workers). We also find that bonus increased the share of female workers by 3.2%, the share of Hispanic or Latino workers by 8.5%, and the share of Black workers by 1.6%. Overall, we find that bonus had larger employment effects for workers that are paid, on average, relatively less. These results provide the first piece of evidence that the estimated decrease in average earnings per worker may be due to changes in workforce demographics induced by the tax policy.

We follow a two-step procedure to quantify the effect of these demographic changes on earnings. First, using pre-treatment data, we regress earnings-per-worker on demographic shares (young, female, less-education, Non-white). Using these estimates and observed demographic shares, we predict state-industry earnings during the full sample period. Therefore, these counterfactual earnings capture only the changes due to demographic shifts and hold earnings-per-worker constant within each demographic group. We then estimate the response of these counterfactual earnings to bonus depreciation. If the average earnings response we find is caused exclusively by demographic shifts due to the policy then would expect our counterfactual event study estimates to look very similar to those presented in Figure 3.5.

[41] Table A4 presents estimates of bonus on different demographic shares. For instance, column (3) of Table A4 shows that the fraction of workers with fewer years of education increased by 0.00259. We calculate the 1.0% increase by dividing this estimate by the base fraction of less educated workers of 25.3%.

Figure A13 compares the counterfactual event study estimates with earnings event study using ASM data (Figure 3.5). The counterfactual estimates closely track the ASM findings throughout the sample period. The similarity of the two series indicates that, indeed, the negative earnings response to bonus depreciation is due primarily to the policy-induced demographic shifts in the workforce we document above.

In Appendix J we provide further details on this procedure as well as present two other complementary analyses to explore connections between the earnings response and the demographic composition of the workforce. The first method estimates the effect of bonus depreciation on earnings while directly controlling for changes in workforce demographics. Second, we perform a modified Kitagawa-Oaxaca-Blinder decomposition to attribute the earnings response to either changes in composition or changes in average earnings for demographic groups. Findings from both analyses reinforce the conclusion that policy-induced shifts in workforce composition are responsible for the negative earnings response to bonus depreciation.

Overall, our results show that bonus depreciation did not increase average earnings per worker.[42] However, our employment results also show that bonus depreciation disproportionately helped disadvantaged workers at a time when their employment prospects in the manufacturing sector were dwindling [Gould, 2018].[43]

3.4.4 Productivity and Production Responses

The unique nature of the ASM data also allows us to explore the effect of bonus depreciation on productivity and total production. Panel (A) of Figure 3.7 presents results from an event study of the effects of bonus on our measure of plant-level TFP.

[42] This result is consistent with Fuest et al. [2018], who find that local tax cuts across German municipalities did not increase average earnings.

[43] Appendix G shows that the pattern of stronger employment effects for disadvantaged workers is most prevalent in production occupations (i.e., those primarily engaged in manual, routine tasks).

We do not find evidence that capital investment led to increases in plant productivity.[44] While this result does not suggest bonus depreciation increases productivity, it does show that adjustment costs due to incorporating new capital do not lead any measurable decrease in TFP. Panel (B) of Table 3.4 reports statistically insignificant estimates for both difference-in-differences and long differences analyses. Column (5) of this panel implies a 95% confidence interval of the effect of bonus on productivity between -1.4% and 0.8%.[45]

While bonus did not increase plant productivity, the mere fact that bonus decreased overall costs of production may have allowed plants to expand their operations. The event study in Panel (B) of Figure 3.7 shows that this was indeed the case. Column (5) of Panel (C) of Table 3.4 shows that the sales of treated plants (measured by the total value of shipments) saw a relative increase of 5.4%, on average, between 2001 and 2011. Since Panel (B) of Figure 3.7 shows that the effect of bonus on production grew over time, we also report long differences estimates in Panel (C) of Table 3.4. By 2011, the plants that benefited the most from bonus increased their sales by between 7.5 and 8.1%, relative to control plants. These findings suggest that bonus helped treated plants increase their overall scale. In Section 3.6, we show that the scale effect explains most of the capital and labor responses.

[44] We also rule out increases in labor productivity since the revenue effect does not exceed the effect on labor.

[45] As we show in the previous section, bonus impacts the composition of the workforce. One concern is that our TFP estimates are biased downwards since plants shift their employment to workers with fewer years of education and experience. However, this effect is likely to be quantitatively small. Assuming that these workers are paid their marginal product and using the average labor cost share of 25% and the unconditional decrease in average earnings of -2.73% (column (7) of Panel (A) of Table 3.4) would imply a correction to our TFP estimates of $+0.68\%$ ($= -2.73\% \times 25\%$). This correction would revise our -1.53% (column (7) of Panel (B) of Table 3.4) estimate to -0.85%, which still does not provide evidence in favor of the capital deepening hypothesis.

3.5 Tax Policy in a Transforming Manufacturing Sector

In analyzing the effects of bonus depreciation, it is crucial to place our findings in the context of the ongoing transformation of the US manufacturing sector. Doing so helps ensure that our results are driven by the effects of the tax policy and not by sector-level trends. Crucially, we explore whether bonus depreciation simply propped-up dying industries or whether it stimulated investment and employment in the industries most likely to thrive in the 21st century.

Charles et al. [2019] identify four main factors that led to significant transformation in the manufacturing sector between 2000–2017. First, they identify a marked increase in "skill intensity," as measured by the share of employment in non-production roles. Second, they note that this change is paired with an increase in "capital intensity," i.e., an increase in the share of productivity attributable to capital. The last two factors are the dramatic increase in import competition from China [e.g., Autor et al., 2013, Acemoglu et al., 2016, Autor et al., 2016, Pierce and Schott, 2016] and the increased adoption of automated production processes [e.g., Acemoglu and Restrepo, 2020].[46]

We first show that increases in skill and capital intensities, import competition from China, and automation are not correlated with bonus depreciation in ways that impact our empirical results. To do so, we use the ASM/CM plant-level data to re-estimate our main difference-in-differences estimates in the presence of controls for each of these four forces. As in Charles et al. [2019], we measure skill intensity at the plant-level as the share of employment in non-production roles in 2001. To operationalize this control, we create bins based on quartiles of the distribution of this

[46]Both of these forces could also cause or mediate changes in skill and capital intensity.

variable and we interact them with year fixed effects. Our capital intensity control is constructed in a similar manner, but is based on the 2001 plant-level ratio of total capital assets to total employment. We control for the "China Shock" using industry-level changes in import competition from China between 2000–2007 from Acemoglu et al. [2016] interacted with year fixed effects. Finally, we use data from Acemoglu and Restrepo [2020] on industry-level changes in the number of industrial robots per 1,000 workers between 1993–2007, which we also interact with year fixed effects.

Table 3.5 re-estimates our differences-in-differences parameters describing the effects of bonus on investment, employment, and mean earnings. For reference, columns (1), (3), and (5) display estimates we previously presented in columns (5) of Tables 3.1, 3.3, and 3.4. For comparison, columns (2), (4), and (6) include plant and state-by-year fixed effects as well as the four controls for skill intensity, capital intensity, Chinese import exposure, and robotization. As this table shows, the effect of bonus on investment is essentially unchanged when including these controls. Employment responses to bonus depreciation are slightly attenuated, decreasing from 7.9 to 6.9%. We also continue to find that bonus depreciation does not lead to significant gains in average earnings for the workers of more affected plants.[47] Overall, this table shows that our estimated effects of bonus are essentially unchanged in the presence of controls for salient drivers of the transformation of the US manufacturing sector.[48]

While our estimated effects of bonus depreciation are not generated by the major drivers of transformation in the manufacturing sector, it is still important to understand our results given this context. As Charles et al. [2019] note, the US

[47]Intuitively, controlling for skill intensity works in the same way as controlling for plant-level employment demographics. For this reason, we find similar null effects on average earnings as we do in Section 3.4.3.

[48]One possibility is that these controls may change the underlying variation from the tax policy. This could happen, for instance, by limiting the effect of the policy on skill or capital intensity. If this were the case, these specifications could risk over-controlling for some of the effects of bonus depreciation. For this reason, we do not view these results as our preferred estimates.

manufacturing sector lost 5.5 million jobs from 2000-2017. Figure A12 helps place our estimates in this context by comparing the magnitude of the effects of bonus depreciation to aggregate trends (see Appendix I for details). This figure shows that, while bonus attenuated the employment decline, it also stimulated positive growth in capital accumulation.

This context motivates the salient policy concern that bonus depreciation simply props-up non-competitive plants or industries. Contrary to this hypothesis, a number of our results suggest that the policy stimulated absolute increases in capital investment and labor demand for some plants. First, recall that we find that new and young firms—which are more likely to be growing—also respond to bonus depreciation by increasing employment. Second, our baseline results rely on a balanced sample of plants that survive through our analysis period. Whether bonus propped-up these plants is therefore not at the core of these results. Finally, the fact that we find similar results on our balanced sample of plants and when using the aggregate QWI data suggests that plant deaths are not a major component of the employment responses we observe.

To more directly explore this hypothesis, we estimate whether bonus had larger effects on plants and industries that are least likely to thrive in the future. We implement this analysis by including interactions between the difference-in-differences term and the cross-sectional continuous components of each control described above (e.g., 2001 capital stock per total employment). For comparability in interpretation, we normalize each interactor to have mean zero and divide it by its interquartile range. As such, the interaction terms are interpreted as differences in the effect of bonus depreciation between units in the 25th and 75th percentiles of each factor. Table 3.6 presents results from these analyses for our two main outcomes, log investment and log total employment.[49] Column (1) shows that investment responses to bonus depreciation are larger in plants with higher skill intensity. The interaction term in the employment regression is positive, but statistically insignificant at conventional levels. In column (2), we find that both investment and employment responses are larger in plants with high levels of capital intensity. These results imply that bonus depreciation did not encourage plants to swim against the current by investing in technologies characterized by low levels of capital and skill intensity. Two additional points related to this finding are worth mentioning. First, even if bonus contributed to the transition to capital intensive forms of production, the employment effects of bonus were larger in plants that were initially more capital intensive. Second, this result further validates the research design as capital intensive plants benefit the most from accelerated depreciation policies.

Column (3) of Table 3.6 estimates interaction effects of bonus and import competition. Increased import competition depresses the effects of bonus depreciation on both investment and employment. These results are intuitive; investment incentives have the least impact on the US industries that are most exposed to import competition from China. Finally, column (4) explores interaction effects between bonus and exposure to robotization. We find positive point estimates on the interactions with robotization, but only the employment interaction is statistically significant. Surprisingly, these results contradict concerns that capital investments stimulated by tax policy are labor replacing via the adoption of robots. The industries that automated most during the period also increased employment the most in response to bonus depreciation.

The results of Table 3.6 show that bonus depreciation did not simply prop-up non-

[49]Table A9 presents estimates from models in which all interaction terms are included together. Signs and magnitudes of all coefficients are the same.

competitive industries. Instead, we find that the policy has the largest impacts on the plants and industries that are the most skill-intensive, most capital-intensive, most automated, and least exposed to Chinese import competition. Bonus depreciation is most effective for the industries that are most likely to thrive in the transforming landscape of the US manufacturing sector.

3.6 Estimating Factor Demands Using Tax Policy Variation

While our reduced-form results yield novel insights into the effects of one of the largest tax incentives for investment in US history, these results alone are not sufficient to understand the economic mechanisms by which the policy impacts capital accumulation and labor demand. We uncover these mechanisms by estimating a structural model of factor demands. We incorporate the result of Marshall [1890] and Hicks [1932] that plants respond to changes in input prices by adjusting both their scale and input mix. The model allows us to estimate the relative importance of these mechanisms. The model also allows us to recover the implied effects of the policy on the cost of capital, which we use to compute cost of capital elasticities of capital and labor demand inclusive of financing and other constraints. Finally, the model leverages tax policy variation to estimate elasticities of substitution between capital and different types of labor.

3.6.1 Model Setup

The model considers the production and pricing decisions of plants in the manufacturing sector. Plants have a production function with constant returns to scale, which uses three inputs: capital K, production labor L, and non-production labor J. Plants first optimally choose inputs to minimize costs. Plants then maximize profits by choosing their output level. The output market is characterized by monopolistic competition where demand has a constant price elasticity [see, e.g., Hamermesh, 1996, Harasztosi and Lindner, 2019, Criscuolo et al., 2019]. Bonus depreciation lowers the cost of capital, which we denote by $\phi \equiv \frac{\partial \ln(\text{Cost of Capital})}{\partial \text{Bonus}} < 0$.[50] ϕ includes both the increased present value of depreciation deductions and reductions in financing and other frictions.[51] Since our identification strategy relies on cross-industry variation, our estimates of substitution elasticities capture the average value across the manufacturing sector.[52] Appendix K provides a detailed derivation of the model.[53]

These simple assumptions allow us to characterize the effects of bonus on plants'

[50]The model assumes that plants take input prices as constant. As we show above, we do not find that bonus impacts the wages of workers conditional on composition. In related work, Garrett et al. [2020] also estimate a null effect of bonus on average wages accounting for spillover effects within local labor markets. One possibility is that bonus impacts the pre-tax prices of capital goods. While classic papers show that tax incentives for investment can impact the prices of capital goods [e.g., Goolsbee, 1998], House et al. [2017] show tax incentives have not impacted capital goods prices in recent years, partly because of the growth of imported capital goods as a share of investment. Indeed, House and Shapiro [2008], Basu et al. [2021] show that capital goods prices did not increase in response to bonus depreciation.

[51]Appendix K provides a model consistent with Myers [1977], Bond and Meghir [1994], Bond and Van Reenen [2007] that shows that interactions with financing frictions amplify the effect of bonus on the cost of capital, ϕ. An alternative assumption is that bonus both lowers the cost of capital and provides additional cash flow that relaxes a financing or capacity constraint, which directly impacts labor demand. In Appendix K, we extend our model to allow for this possibility and show that our results are robust to allowing for cash flow effects.

[52]A potential concern is that industries with lower elasticities of substitution (σ_{KL}) benefit more from bonus. This concern is unlikely to impact our estimates since Table 3.5 and Figure A9 show that our reduced-form results are not sensitive to (1) controlling for capital intensity, (2) controlling for industry trends in ICT adoption, or to (3) removing high-tech industries, which are short duration industries with potentially high degrees of substitution. In addition, Panel (A) of Figure A14 shows that the benefit from bonus, z_0, is uncorrelated with industry-level estimates of σ_{KL} from Raval [2019]. Panel (B) further shows that we obtain similar effects on employment when we control for differential trends based on these industry-level estimates of σ_{KL}.

[53]Our framework abstracts away from adjustment costs that may limit plants from adjusting their capital inputs in any given year. Since we measure the effects of bonus depreciation over a ten-year period, it is reasonable to assume that plants will be able to adjust their capital inputs over this period.

demands for inputs of production. The reduction in the cost of capital ϕ impacts both the choice of cost-minimizing inputs (substitution effect) and the profit-maximizing output level (scale effect). To see this, note that the effect of bonus on the demand for capital is

$$\beta^K = \frac{\partial \ln K}{\partial \text{Bonus}} = (\underbrace{-s_J\sigma_{KJ} - s_L\sigma_{KL}}_{\substack{\text{Substitution} \\ \text{Effect}}} - \underbrace{s_K\eta}_{\substack{\text{Scale} \\ \text{Effect}}}) \times \underbrace{\phi.}_{\substack{\text{Bonus Lowers} \\ \text{Cost of Capital}}} \tag{3.3}$$

In their price-theoretic treatment of factor demands, Jaffe et al. [2019] interpret this equation as the production analogue of the Slutsky equation, since it separates substitution effects conditional on output from changes in the plant's scale. Plants increase their capital to the extent that lower production costs help each plant increase its sales. The strength of this scale effect depends on the cost share of capital s_K and the elasticity of product demand η. Plants also increase their capital by substituting away from other inputs J and L. The strength of this substitution effect depends on the input cost shares (s_J and s_L) and on the Allen partial elasticities of substitution (σ_{KJ} and σ_{KL}). Allen [1938] defines inputs K and J as complements in production whenever $\sigma_{KJ} < 0$, while $\sigma_{KJ} > 0$ implies that these inputs are substitutes. Both the scale and substitution effects depend on the degree to which bonus lowers the overall cost of capital, including financing and other frictions. We therefore interpret ϕ as the experienced reduction in the cost of capital inclusive of these frictions.

Consider now the model's prediction of the effect of bonus on the demands for labor

$$
\begin{aligned}
\beta^L = \frac{\partial \ln L}{\partial \text{Bonus}} &= s_K(\sigma_{KL} - \eta) \times \phi & (3.4)\\
\beta^J = \frac{\partial \ln J}{\partial \text{Bonus}} &= s_K(\sigma_{KJ} - \eta) \times \phi. & (3.5)
\end{aligned}
$$

Equation 3.4 shows that bonus increases labor demand when production labor and capital are complements, i.e., $\sigma_{KL} < 0$, or when the scale effect dominates the substitution effect, i.e., $\eta > \sigma_{KL} > 0$. Finally, consider the model's prediction of the effect of bonus on plant sales

$$
\beta^R = \frac{\partial \ln \text{Revenue}}{\partial \text{Bonus}} = s_K(1 - \eta) \times \phi. \tag{3.6}
$$

Equation 3.6 shows that the effect of bonus on revenue combines a price decrease of $s_K\phi$ with an increase in the quantity sold of $-\eta s_K \phi$.

As Blackorby and Russell [1981] discuss, there are alternative ways to define substitution elasticities when production takes more than two inputs. The elasticities of substitution in Equations (3.3)–(3.5) are Allen partial elasticities, which capture substitution between capital and a given input, relative to all other inputs. Our analyses require Allen elasticities for a number of reasons. First, they allow us to separate the scale and substitution effects of the policy and determine whether inputs are complements or substitutes.[54] Second, this framework provides a transparent link between our reduced-form estimates from Section 3.4 and the four model parameters that determine factor demands $\theta = (\sigma_{KL}, \sigma_{KJ}, \eta, \phi)$, which include the Allen elasticities. Third, as we show below, Allen elasticities allow us to isolate the effect of the policy on the cost of capital, ϕ, which we use to calculate demand elasticities

[54] While any two inputs may be complements, Allen [1938] shows that second-order optimization conditions require the total substitution effect to be negative, i.e, $s_J\sigma_{KJ} + s_L\sigma_{KL} > 0$.

for a given input J as follows: $\varepsilon_{\phi}^{J} = \frac{\beta^J}{\phi}$. Finally, by isolating ϕ and demand elasticities, Allen elasticities allow us to compute the Morishima elasticity [Blackorby and Russell, 1989]. This alternative measure captures substitution between capital and production labor, relative to capital, and can be calculated as: $\sigma_{KL}^{M} = \varepsilon_{\phi}^{L} - \varepsilon_{\phi}^{K}$.

3.6.2 Separating Scale and Substitution using Reduced-Form Estimates

We first use the model to decompose the effects of bonus depreciation on labor demand into scale and substitution effects. To do so, note that we can quantify the scale effect using our reduced-form estimates. This is because, regardless of the values of σ_{KL} and σ_{KJ}, the symmetry of Allen elasticities (i.e., that $\sigma_{KL} = \sigma_{LK}$) implies that:

$$\bar{\beta} \equiv s_J \beta^J + s_K \beta^K + s_L \beta^L = -s_K \eta \phi > 0. \tag{3.7}$$

This equation shows that the cost-weighted average of the effects of bonus on plants' inputs of production, $\bar{\beta}$, identifies the common scale effect in Equations 3.3–3.6, $-s_K \eta \phi$. Intuitively, the scale effect captures the common increase in the use all inputs, absent substitution effects. Constant returns to scale implies that the increase in quantity sold also equals the scale effect.

This equation makes it very easy to compute the common scale effect of the policy on the demand for plant inputs. Panel (A) of Table 3.7 reports estimates of the scale effect using the ten-year effects of the policy.[55] Assuming that the input cost shares are $s_K = 0.2$, $s_L = 0.5$, and $s_J = 0.3$, column (1) shows that the scale effect equals 0.10 (SE=0.01). Columns (2) and (3) of Table 3.7 show that varying the cost shares

[55]We use the following estimates in this calculation: β^K from column (1) in Table 3.2, and β^L and β^J from columns (6) of Panels (B) and (C), respectively, in Table 3.3.

has very small effects on our estimate of the scale effect. The scale effect is estimated with a high degree of precision and has a natural economic interpretation: the effect of the policy on the profit-maximizing output level led to an equal increase of 10% in the demand for all inputs.[56]

We now express elasticities of substitution as functions of our reduced-form moments and the elasticity of product demand, η. Taking the ratio of Equations 3.4 and 3.7 implies that

$$\sigma_{KL} = \eta \left(1 - \frac{\beta^L}{\bar{\beta}}\right). \tag{3.8}$$

Input L is a substitute for capital ($\sigma_{KL} > 0$) when the effect of the policy on labor demand β^L is smaller than the scale effect $\bar{\beta}$. Conversely, L complements capital ($\sigma_{KL} < 0$) when $\beta^L > \bar{\beta}$.

Panel (B) of Table 3.7 reports estimates of substitution elasticities under different assumed values for the cost shares and demand elasticity. Column (1) shows that $\sigma_{KL} = -0.515$ when the elasticity of product demand $\eta = 3.5$.[57] Columns (2)–(5) report estimates that vary the capital cost share $s_K \in [0.10, 0.30]$ or the demand elasticity $\eta \in [2, 5]$. We consistently estimate that $\sigma_{KL} < 0$, implying that production labor complements capital. This result follows from the fact that bonus increased the use of production labor by 11.6%, which is greater than the 10% scale effect. In contrast, since the estimated increase in non-production labor is smaller than the scale effect, we estimate that non-production labor and capital are substitutes ($\sigma_{JK} > 0$). Therefore, our results are not compatible with the capital-skill complementarity

[56] This would also be the total increase in factor demands in a Leontief production function without any substitution effects. Note that columns (4) and (5) vary η, which does not impact our estimate of the scale effect.

[57] Ganapati et al. [2020] estimate product demand elasticities using CM data. They report a central estimate of 3.42 and a range of estimates between 1.93 and 5.23 for selected industries.

hypothesis.[58]

Panel (C) of Table 3.7 formally evaluates the hypothesis that capital complements labor. We reject the null hypothesis that $\sigma_{KL} \geq 0$ with p-values ranging from 0.047 to 0.099, depending on the values of s_K and η. Because the effect of bonus on non-production labor is close to $\bar{\beta}$, we do not reject the hypothesis that non-production workers complement capital, even though these effects are precisely estimated.

The discussion above clarifies that the differences between the common scale effect and the total effect on a given input determine whether an input complements or substitutes for capital. Quantitatively, however, our calculations reveal that, for both production and non-production labor, the total effects are close to the scale effect. This result implies that the main mechanism driving the effect of bonus depreciation on labor demand is the scale effect; that is, the policy-driven reduction in the cost of capital allowed plants to expand both their output and their demand for all inputs. In the case of production labor, the 10% scale effect was responsible for close to 90% of the 11.6% total effect of the policy. The fact that the scale effect of the policy dominates the substitution effects we estimate allays concerns that bonus depreciation led plants to replace workers with machines.

Panel (D) of Table 3.7 presents estimates of the effect of bonus on the cost of capital, ϕ, and elasticities of capital and labor demand with respect to the cost of capital. Inverting Equation 3.7 implies that $\phi = -\frac{\bar{\beta}}{s_K \eta}$. Under our baseline parameterization, we estimate a semi-elasticity of the cost of capital with respect to bonus of $\hat{\phi} = -0.145$. This estimate reveals that—inclusive of interactions with financing and other frictions—bonus depreciation has a large effect on the cost of capital. Our

[58] Griliches [1969] defines the capital-skill complementarity hypothesis using Allen elasticities of substitution as follows: $\sigma_{KL} > 0$, $\sigma_{KL} > \sigma_{KJ}$, and $\sigma_{KL} > \sigma_{LJ}$. Appendix L shows that Allen elasticities of substitution can be used to estimate the parameters of a translog cost function [Christensen et al., 1971, 1973]. Our estimates are therefore consistent with models of production that allow for flexible patterns of substitution.

estimate of ϕ then implies an investment elasticity of $\hat{\varepsilon}^I_\phi = \frac{\hat{\beta}^I}{\hat{\phi}} = -1.40$.[59]

An advantage of our setting is the ability to estimate demand elasticities for capital stocks and for different types of labor. We estimate an own-price capital demand elasticity of $\hat{\varepsilon}^K_\phi = -0.55$ and cross-price elasticities of production labor of $\hat{\varepsilon}^L_\phi = -0.80$ and non-production labor of $\hat{\varepsilon}^J_\phi = -0.62$.[60] These relatively modest elasticities reinforce the importance of estimating ϕ inclusive of financing and other frictions. Appendix L discusses these elasticity estimates further.

Finally, these demand elasticities also allow us to estimate Morishima elasticities of substitution. Table A12 reports that $\hat{\sigma}^M_{KL} = \hat{\varepsilon}^L_\phi - \hat{\varepsilon}^K_\phi = -0.25$ (SE=0.14), which shows that the result that production labor complements capital is robust to using the Morishima elasticity. This estimate rejects the null hypothesis that $\sigma^M_{KL} \geq 0$ with a p-value=0.04. We also estimate a Morishima elasticity between non-production labor and capital of $\hat{\sigma}^M_{KJ} = \hat{\varepsilon}^J_\phi - \hat{\varepsilon}^K_\phi = -0.07$ (SE=0.19). To show that our results are consistent with a standard model of production, Appendix L uses these elasticities to estimate the parameters of a nested CES production function that nests non-production labor separately from other inputs.

[59] This estimate uses the long difference estimate on investment from Panel (A) of Figure 3.2. We relate this value to recent estimates from the literature in Appendix L and show that it has a similar magnitude to estimates that account for interactions between tax policies and financing and other frictions.

[60] Appendix L explores the dynamic patterns underlying these estimates. Panel (A) of Figure A16 shows that the scale effect grows over time as plants respond to the cumulative effects of the policy. While Panel (B) shows that the implied effect on the cost of capital ϕ also grows over time, Panels (C) and (D) show that the investment and employment elasticities are relatively constant over time. These results are consistent with our interpretation of ϕ as the effect of the policy on cost of investment inclusive of financing constraints as well as other frictions that may prevent plants from responding to the policy.

3.6.3 Structural Estimation of Capital-Labor Substitution

We now refine our estimation of capital-labor substitution elasticities in three ways. First, we jointly estimate the parameters of the model. Second, we incorporate the prediction of our model for the effect of the policy on plant revenue as an over-identifying moment. Finally, we ensure that the estimated parameters are consistent with axioms of cost-minimization. We incorporate these refinements by estimating our structural model via Classical Minimum Distance (CMD).

Identification and Estimation Approach

To identify η, first note that Equations 3.6 and 3.7 imply that $\beta^R = \frac{\eta-1}{\eta}\bar{\beta}$. Solving for η yields

$$\eta = -\frac{\bar{\beta}}{\beta^R - \bar{\beta}}. \tag{3.9}$$

The intuition for this expression is as follows. The effect of bonus on quantity sold is given by the scale effect since $\frac{\partial \log q}{\partial \text{Bonus}} = -\eta s_K \phi = \bar{\beta}$. The effect on prices can be decomposed from the revenue and quantity effects. Specifically, the plant lowers its price by $\frac{\partial \log p}{\partial \text{Bonus}} = s_K \phi = \beta^R - \bar{\beta}$. Equation 3.9 then shows that the elasticity of product demand η is the ratio of the percentage changes in quantity and prices.[61]

Equations 3.7 and 3.9 imply that $\phi = -\frac{(\bar{\beta}-\beta^R)}{s_K}$. To understand the identification of ϕ, note that the constant demand elasticity η implies that $\frac{\partial \log p}{\partial \text{Bonus}} = \frac{\partial \log \text{Unit Cost}}{\partial \text{Bonus}}$. Therefore, ϕ is identified by scaling-up the effects on prices (i.e., $\frac{\partial \log p}{\partial \text{Bonus}} = \beta^R - \bar{\beta}$) by the capital cost share, s_K.

Having identified each of the model parameters with the reduced-form estimates, we now discuss how we estimate the model using CMD. Let $\hat{\beta} = (\hat{\beta}^K, \hat{\beta}^L, \hat{\beta}^J, \hat{\beta}^R)'$ be

[61] Combining Equations 3.8 and 3.9, we have that $\sigma_{KL} = \frac{\bar{\beta}-\beta^L}{\bar{\beta}-\beta^R}$. A similar expression identifies σ_{KJ}.

the vector collecting the reduced-form estimates of the effects of bonus depreciation on inputs and plant revenue, and let $h(\theta)$ be the collection of model predictions from Equations 3.3–3.6. Our estimate $\hat{\theta}$ minimizes the criterion function $[\hat{\beta} - h(\theta)]'\hat{W}[\hat{\beta} - h(\theta)]$, where $\hat{W}$ is a weighting matrix.[62]

While the equations above show that the model parameters are closely related to our reduced-form estimates, the presence of the difference $\bar{\beta} - \beta^R$ in the denominator of the formula for η raises the concern that estimates of structural parameters may be sensitive to small differences between our reduced-form estimates. For this reason, we calibrate η in our baseline estimations; we show robustness to a range of calibrated values and to estimating η. Finally, to ensure that our estimated parameters are consistent with cost minimization, we require that the substitution elasticities satisfy the constraint: $s_J\sigma_{KJ} + s_L\sigma_{KL} > 0$ [Allen, 1938].

Estimated Parameters

To highlight the intuition behind our model, we present structural estimates of σ_{KL} graphically in Panel (A) of Figure 3.8 as a function of different values of η. The dot-dashed blue line plots Equation 3.8, which shows that $\sigma_{KL} < 0$ regardless of the value of η. The blue dots report estimates of σ_{KL} using the full model and calibrated values of η equal to 2, 3.5, and, 5. This figure also reports a model that estimates $\eta = 3.076$ as well as models that vary the share of capital in total costs between 10% and 30%. The full model estimates lie above the line that plots Equation 3.8 because we impose the constraint that the model be consistent with cost minimization (i.e., that $s_J\sigma_{KJ} + s_L\sigma_{KL} > 0$). Across these different variations, we consistently estimate

[62] In practice, $\hat{W}$ equals the inverse variance-covariance matrix $\hat{V}$ of the moments $\hat{\beta}$. Following Chamberlain [1984, §4.2], we estimate the variance of $\hat{\theta}$ with the matrix $[H(\hat{\theta})'\hat{V}^{-1}H(\hat{\theta})]^{-1}$, where $H(\hat{\theta}) = \nabla_\theta h(\theta)|_{\theta=\hat{\theta}}$ is the gradient of $h(\theta)$ at $\hat{\theta}$. We implement this procedure using code modified from Harasztosi and Lindner [2019] that relies on a finite difference approximation of $H(\hat{\theta})$.

that $\sigma_{KL} < 0$, implying that capital and production workers are complementary inputs.

Panel (A) of Table 3.8 reports estimates of σ_{KL} as well as all other model parameters across a range of model specifications. Our baseline estimate of σ_{KL} in column (1) equals -0.44. While this point estimate indicates that capital and production labor are complements, the full model estimates imply that 89% of the effect of bonus on production labor is due to the scale effect. The complementarity between these inputs is responsible for the remaining 11%. Panel (B) of Figure 3.8 plots the probability that $\hat{\sigma}_{KL}$ exceeds a given value. We reject values of σ_{KL} that are greater than 0.13 at the 95% confidence level.[63] Relative to prior estimates [e.g., Krusell et al., 2000, Karabarbounis and Neiman, 2014], our findings allay the concern that bonus depreciation led plants to replace workers with machines. Columns (2)–(3) of Panel (A) of Table 3.8 show that our estimates are not sensitive to calibrated cost shares, columns (4)–(5) show the effects of varying the elasticity of product demand η, and column (6) reports model estimates when we also estimate η. Across all specifications we find that non-production workers are substitutes with capital, $\sigma_{KJ} > 0$.

To gain intuition for these results, note that they follow directly from the fact that our estimates in Section 3.4 are such that $\hat{\beta}^{\mathrm{L}} > \hat{\bar{\beta}} > \hat{\beta}^{\mathrm{J}}$. In order to obtain an estimate of $\sigma_{KL} = 1$ (i.e., Cobb-Douglas), plants would have had to increase their capital use by 38%, which is almost 5 times larger than our estimated effect. Even a Leontief production function (i.e., $\sigma_{KL} = 0$) would require that plants increase their capital stock by 15.5%, which is twice as large as our estimated effect. Panels (B) and (C) of Table 3.8 show that the model predictions $h(\hat{\theta})$ are very close to our estimates $\hat{\beta}$. This result shows that the calibrated value of η and the restriction

[63] This figure also shows that we draw similar conclusions using models that only include capital and labor (orange line) or that separate capital into equipment and structures.

that our estimates are consistent with cost minimization are not in conflict with the reduced-form estimates of the effects of bonus depreciation.[64]

We briefly discuss additional robustness checks of our model; see Appendix L for details. Column (2) of Table A14 shows that our results are robust to using difference-in-differences estimates of $\hat{\beta}$ instead of long-differences estimates. Column (3) reports similar parameter estimates when we measure labor using production hours instead of number of workers. Column (4) shows that we also find a negative elasticity of substitution when we do not differentiate between different types of labor. Columns (5)–(6) show that we estimate similar elasticities of capital-labor substitution in models with one type of labor and that consider different roles for structures and equipment or that include materials as an additional input. Across all of our models, we consistently find that production workers complement capital in production.[65]

3.6.4 Empirical Implications of Capital-Labor Complementarity

The result that capital and labor are complements in production carries interesting testable hypotheses. Specifically, we would expect to see larger investment responses when plants face lower wages.[66] We test for heterogeneous responses by three proxies

[64]Table A15 shows that we obtain qualitatively similar results when we do not impose this constraint.

[65]Our baseline results are based on our LD estimates and allow plants to adjust their production over a 10 year period. Figure A17 explores the dynamics of capital-labor substitution. This figure shows that capital and labor are initially very complementary ($\sigma_{KL} \ll 0$). Over time, σ_{KL} tends toward our 10 year elasticity of -0.44. This pattern is consistent with the intuition that plants can only increase production by hiring workers when capital is fixed; workers become less complementary with machines as plants adjust their capital.

[66]This prediction follows from Equation (3.3), which implies that bonus depreciation will lead to stronger effects on investment when the labor cost share s_L is smaller. This implication is "Marshall's Second Law of Derived Demand," following the enumeration in Pigou [1920].

for lower labor costs: plant-level unionization, location in a right-to-work (RTW) state, and local labor market power. Our measure of "Union" is an indicator that equals 1 when more than 60% of workers at a plant are unionized.[67] RTW is an indicator equal to 1 for plants in RTW states (as of 2001), where employees have less bargaining power.[68] We measure labor market concentration using a NAICS 3-digit, commuting zone level Herfindahl-Hirschmann Index (HHI) based on 2001 market conditions.[69] In plants that operate in local labor markets that are highly concentrated, monopsony power may allow employers to set lower wages [see, e.g., Robinson, 1969, Manning, 2021].

Table 3.9 presents difference-in-differences estimates of the effects of bonus on investment, employment, and mean earnings that include interactions between bonus and each of these proxies for labor costs. The results in Panel (A) indicate that the investment responses are concentrated in less unionized plants, where we expect wages and bargaining power to be lower. Similarly, the estimates in Panel (B) show larger investment responses in RTW states. Finally, in Panel (C), we find larger investment responses in labor markets where wages are likely depressed due to monopsony power. Across all proxies of labor cost, we see that bonus induces more investment in plants

[67] Plant-level data on unionization are rare. Our measure is based on 2005 data from the Census Bureau's Management and Organizational Practices Survey (MOPS), which covers the majority of our sample.

[68] The RTW variable comes from Valletta and Freeman [1988]. RTW laws allow workers to opt out of union dues and agency fees. These laws decrease the power of unions because workers can free-ride on the efforts of the union, which is obligated to bargain and obtain benefits on behalf of all workers. Researchers have also found that RTW laws codify state-level anti-union sentiments [see, e.g., Farber et al., 2021, Footnote 43]. For these reasons, RTW laws lower workers' bargaining power and result in lower labor costs.

[69] We construct the HHI using data from the LBD. Given that local labor concentration is highly right-skewed in our sample, we measure concentration using the log of HHI. As with other continuous interaction variables, we demean the log of HHI before interacting it with bonus. The interaction has the convenient interpretation as the differential effect of bonus depreciation between a plant located in the average labor market concentration compared to a plant that is located in a highly concentrated labor market, according to FTC/DOJ guidelines (i.e., HHI>2500).

that face lower labor costs. These results are consistent with capital and labor being complements, which validates the results from our empirical model of factor demands. Further, these analyses highlight how labor market institutions can impact capital investment.

Table 3.9 also reports heterogeneous effects on employment and earnings. Two notable results stand out. First, negative interactions for both employment and earnings show that unions do not increase the benefits of bonus to workers. Second, bonus leads to a relative increase in average earnings in highly concentrated labor markets. Recent work by Yeh et al. [2022] finds evidence of significant monopsony power in the US manufacturing sector over the time period of our study. While our overall earnings result suggest minimal increases in earnings, Table 3.9 is consistent with the notion that in monopsonistic labor markets, plants must raise wages to increase employment.

Overall, the model of factor demands estimated in this section delivers a number of economic insights. First, the model shows that the scale effect is the main mechanism driving the increase in labor demand. Second, the implied reduction in the cost of capital delivers estimates of capital and labor demand elasticities with reasonable magnitudes. Third, we consistently estimate that capital and production workers are complements and our full model estimates rule out values of σ_{KL} greater than 0.13. Fourth, our estimates are compatible with standard production models. Finally, the model delivers testable predictions, which validate the complementarity between capital and labor.

3.7 Robustness of Model Estimates

In this section we extend our model to also allow for potential cash flow effects of the policy to relax financing or capacity constraints. Allowing for cash flow effects to directly impact labor demand yields similar model estimates. We also explore whether bonus led to significant reallocation toward more capital intensive plants and industries. Incorporating these forces delivers quantitatively similar results to our baseline model estimates.

3.7.1 Incorporating Cash-Flow Effects

As we discuss in Section 3.1, a particular feature of bonus depreciation is that it creates cash flows for firms that purchase large amounts of physical capital. These additional cash flows may impact the demand for all inputs, especially labor or other inputs that are harder to finance.

In Appendix K, we extend our model to allow for bonus to both lower the cost of capital and to increase cash flows. In this extended model, σ_{KL} is now identified by:

$$\sigma_{KL} = (1+\chi)\left(1 - \frac{\beta^L}{\bar{\beta}}\right), \tag{3.10}$$

where $\chi \geq 0$ captures the importance of the bonus depreciation cash flows relative to the decrease in overall input costs due to the policy. Just as in Equation 3.8, when we allow for cash-flow effects, the identification of σ_{KL} depends on the relative size of the increase in the use of production labor and the scale effect. The intuition for this result is that the scale effect is now governed by the extent to which the cash flows generated by the policy relax financing constraints.

Since we estimate that $\beta^L > \bar{\beta}$, Equation 3.10 shows that the cash flow adjusted model continues to produce estimates of $\sigma_{KL} < 0$. At the extreme, where $\chi = 0$ and there are no cash flow effects, we estimate that $\sigma_{KL} = -0.15$. As cash flow effects become more important, we find increasing degrees of complementarity between capital and production labor. Table A16 presents estimates of σ_{KL} using our baseline estimates of $\bar{\beta}$ and ϕ to calibrate χ. Across various parameterizations, we find estimates of σ_{KL} that are very similar to those presented in Table 3.7. This analysis shows that the result that capital and production labor are complements is robust to explicitly modelling the cash-flow effects of bonus.

3.7.2 Reallocation and Aggregation

Our estimates of elasticities of capital-labor substitution rely on plant-level data on employment and production. By relying on a balanced panel of plants, our estimates ensure that we compare how a stable group of plants combine capital and labor in response to changes in the cost of capital investment. While this is the right estimate for plant-level behavior, it does not account for reallocation across plants and industries or entry and exit. Intuitively, one may find larger aggregate elasticities of substitution if a reduction in the cost of capital leads new firms, or smaller firms that are underrepresented in our balanced sample, to adopt more capital-intensive forms of production. Similarly, a reduction in the cost of capital may lead to a reallocation of business activity toward plants and industries that are more capital intensive.[70] This section describes two ways in which we gauge the quantitative importance of these forces.

[70] In a classic paper, Houthakker [1955] showed that when this reallocation effect is substantial, plants with Leontief production functions can aggregate to a Cobb-Douglas production function at economy level.

Industry-Level Estimates of Substitution Elasticities

First, to account for the roles of reallocation and entry and exit, we now estimate our structural model using the NBER-CES Manufacturing Industry Database. Table A17 shows the reduced-form effects of bonus on capital and production and non-production labor. As with our plant-level results, we find significant increases in the use of all inputs and that the effect on production employment is larger than the effect on capital. Appendix L discusses how we use these results to estimate the industry-level counterparts of our plant-level models. Table A18 reports the implied scale effect of the policy and elasticities of capital-labor substitution using the industry-level data. Across all model specifications, we find very similar substitution elasticities and we estimate a capital-production labor elasticity of substitution $\sigma_{KL} = -0.59$ in our baseline specification. Because the data underlying these estimates account for entry and exit and for reallocation within industries, the fact that we estimate a similar value of σ_{KL} as when we use plant-level data indicates that reallocation and entry and exit within industries are quantitatively unimportant for our estimate of σ_{KL}.[71] These results are consistent with our results in Figure A6 showing that bonus stimulated employment growth in new and younger firms as well as with our results in Figure 3.4, which show that bonus had similar employment effects at the state-industry level of aggregation as at the plant level.

Aggregate Elasticities of Substitution

We now address the concern that the policy may lead to reallocation of business activity toward industries that are more capital intensive. If this were the case, our estimates of elasticities of substitution based on plant- or industry-level data could

[71] As with our plant-level results, we also find that the scale effect is the main driver of the effects of the policy.

lead us to underestimate the degree of capital-labor substitution at the economy-wide level. Oberfield and Raval [2021] develop a method to account for this reallocation that relies on the dispersion in capital intensity across plants and industries and on output elasticities of substitution.

The method of Oberfield and Raval [2021] is based on nested CES production functions. We estimate the parameters of a nested CES production function in Appendix L. Our plant-level results yield an implied (Morishima) elasticity of substitution between capital and production labor from our plant-level estimation of -0.248 (SE=0.141) (see Table A13). To account for within-industry reallocation, we re-estimate this same implied elasticity using the model results that rely on NBER-CES industry-level data presented in Table A18. We estimate an equivalent elasticity at the industry-level of -0.264 (SE=0.213) (see Table A19). The similarity of the plant- and industry-level elasticity estimates reinforces the conclusion that within-industry reallocation is not a substantial margin of response to bonus depreciation.

We now implement the method of Oberfield and Raval [2021] to map our industry-level estimates to an aggregate elasticity that further accounts for cross-industry reallocation.[72] Table A20 presents our aggregate elasticity estimates. Across all parameterizations, our point estimates are consistent with complementarity between capital and production labor. Our baseline parameterization yields an estimate of this aggregate elasticity of substitution of -0.186 (SE=0.199), which rejects values greater than 0.14 at the 5% level.

Overall, this section shows that our main results are not sensitive to whether cash flow effects of the policy directly impact labor demand and that incorporating plant entry and exit and reallocation within and across industries does not have large

[72] This method accounts for reallocation using an estimate of substitution between industries and a measure of the dispersion in capital intensity across industries. See Appendix L for additional details.

quantitative effects on our estimates of how capital and production labor jointly respond to bonus depreciation.

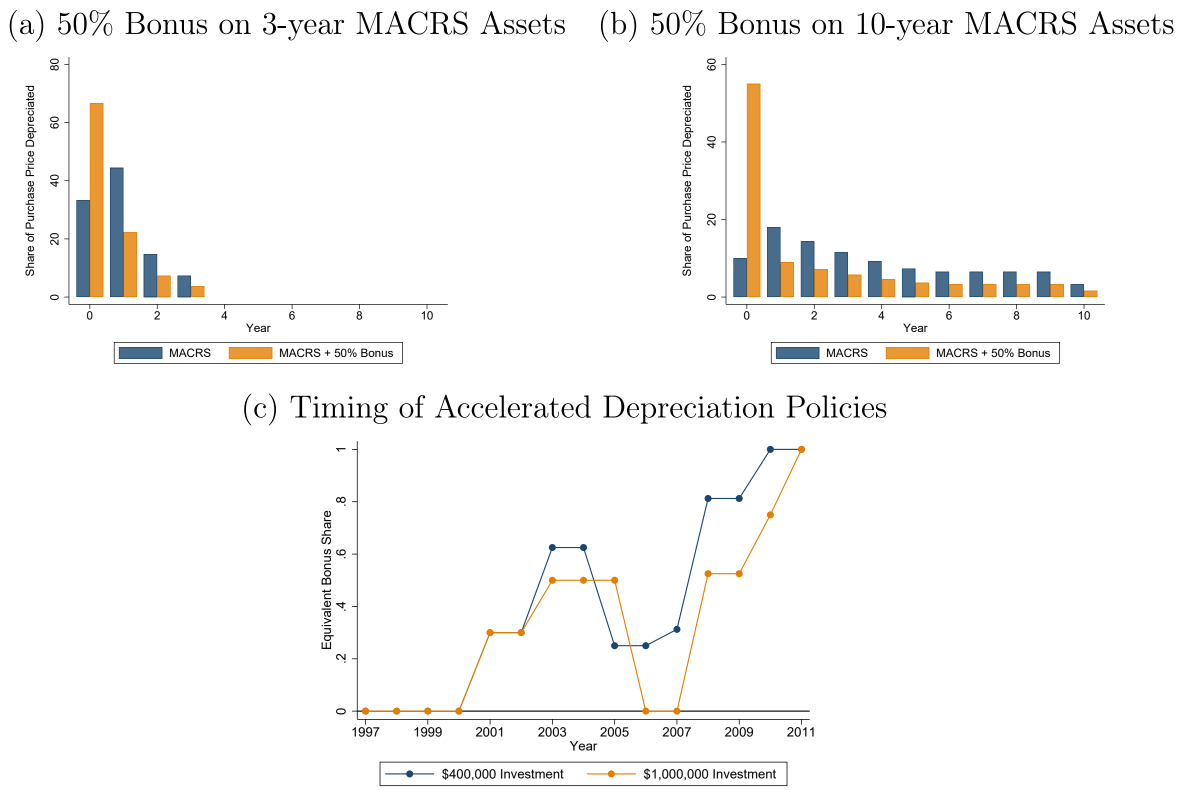
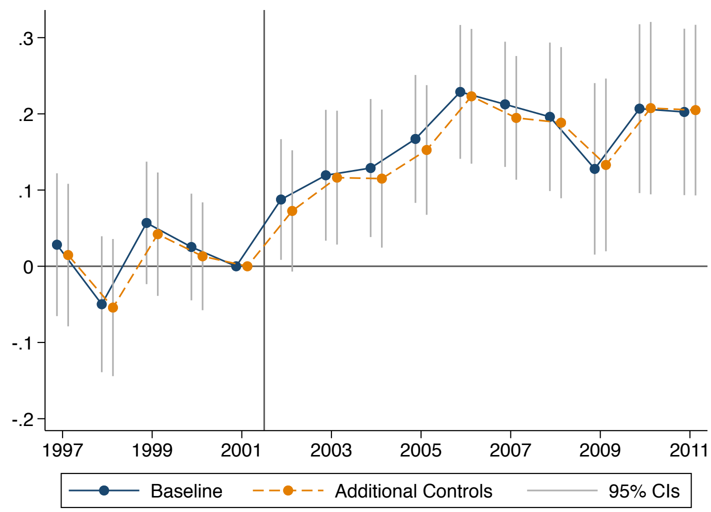


Figure 3.1: Bonus Depreciation Policy and Specific MACRS Assets

Notes: Panels (A) and (B) of Figure 3.1 show how 50% bonus changes the depreciation schedule for a three-year asset and a ten-year asset, respectively. See Appendix C for further explanation of these calculations. The bonus depreciation provision has a larger effect on the deduction schedule for a firm that invests in more assets that are depreciated more slowly for tax purposes. Panel (C) shows how the timing of §179 and bonus depreciation incentives affect the relative share of depreciation deductions that are accelerated into the first year of the investment. The two series plot the percent of purchase price accelerated for a $400,000 investment and for a $1,000,000 investment. The $1,000,000 investment only benefits primarily from bonus depreciation. The $400,000 begins benefiting from §179 expensing starting in 2003. *Source:* Panels (A) and (B), authors' calculations based on IRS [2002] data. Panel (C), authors' calculations based on the statutory §179 and bonus rates explained in Kitchen and Knittel [2016].

(a) Log Investment



(b) Log Total Capital

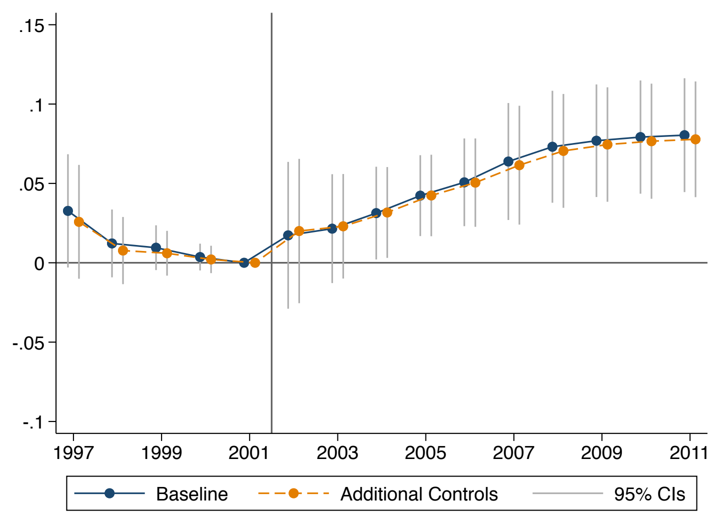


Figure 3.2: Effects of Bonus Depreciation on Capital Investment

Notes: Figure 3.2 displays estimates describing the effect of bonus depreciation on log investment in Panel (A) and log total capital in Panel (B). Plotted coefficients are estimates of β_y from Equation (3.1), which are the annual coefficients associated with bonus. The baseline specification in each panel includes state-by-year and plant fixed effects. The specifications with additional controls add plant size in 2001 bins interacted with year fixed effects, TFP in 2001 bins interacted with year fixed effects, and firm size in 2001 interacted with year fixed effects to the baseline specifications. These specifications correspond to columns (6) and (7) of Table 3.1, respectively. 95% confidence intervals are included for each annual point estimate with standard errors clustered by the 4-digit NAICS-by-state level. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

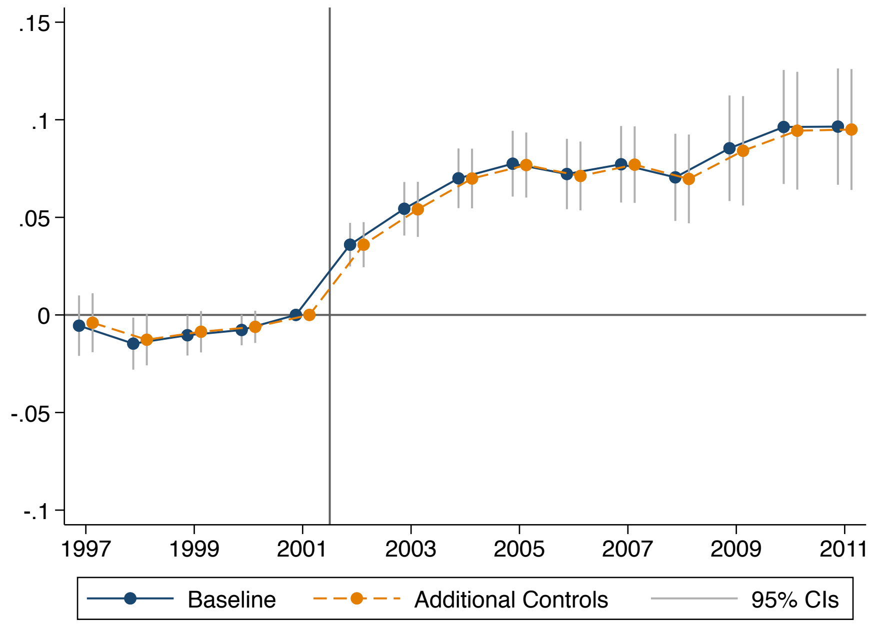


Figure 3.3: Effects of Bonus Depreciation on Log Employment

Notes: Figure 3.3 displays estimates describing the effect of bonus depreciation on log employment. Plotted coefficients are estimates of β_y from Equation (3.1), which are the annual coefficients associated with bonus. The baseline specification includes state-by-year and plant fixed effects. The specification with additional controls add plant size in 2001 bins interacted with year fixed effects, TFP in 2001 bins interacted with year fixed effects, and firm size in 2001 interacted with year fixed effects to the baseline specifications. These specifications correspond to columns (6) and (7) of Table 3.3, respectively. 95% confidence intervals are included for each annual point estimate with standard errors clustered by the 4-digit NAICS-by-state level. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

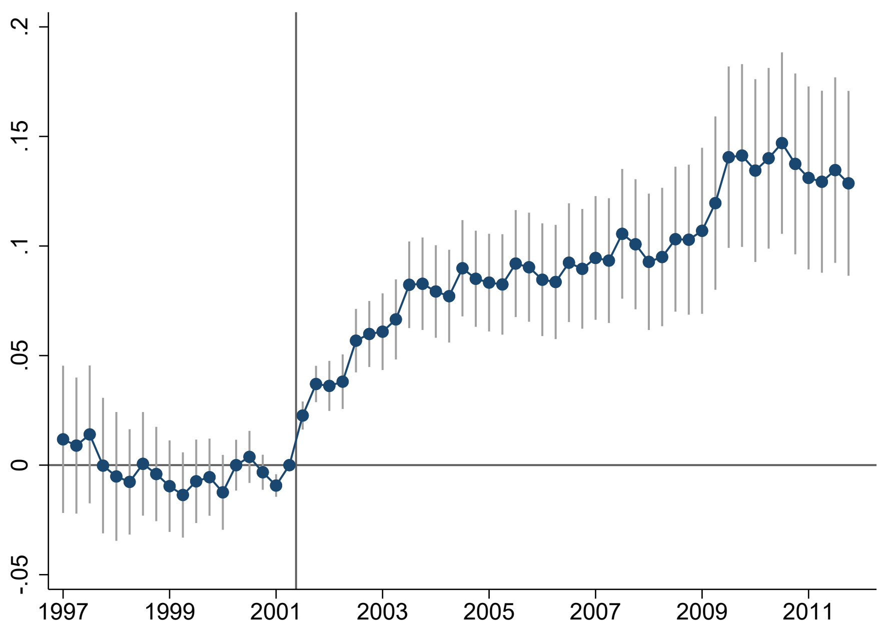


Figure 3.4: Effects of Bonus Depreciation on Log Employment; QWI Data

Notes: Figure 3.4 displays estimates describing the effect of bonus depreciation on log employment using state-by-industry QWI data. The regression estimates displayed in this figure correspond to a quarterly analogue of β_y from Equation (3.1), which is the change in log employment relative to 2001q2 in industries affected most by bonus relative to industries that are less affected by bonus. The regression includes 4-digit NAICS-by-state fixed effects and state-by-quarter fixed effects. The event study estimates in this figure correspond to column (1) of Table A4. 95% confidence intervals are included for each quarterly point estimate with standard errors clustered by the 4-digit NAICS-by-state level. *Source:* Authors' calculations based on QWI and Zwick and Mahon [2017] data.

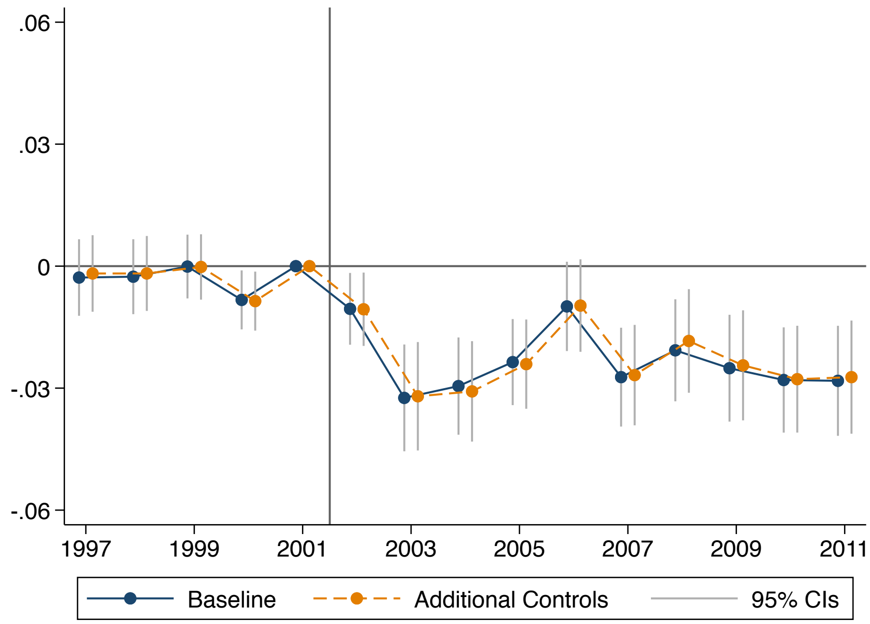


Figure 3.5: Effects of Bonus Depreciation on Log Mean Earnings Per Worker
Notes: Figure 3.5 displays estimates describing the effect of bonus depreciation on Log Mean Earnings per Workers. Plotted coefficients are estimates of β_y from Equation (3.1), which are the annual coefficients associated with bonus. The baseline specification includes state-by-year and plant fixed effects. The specifications with additional controls add plant size in 2001 bins interacted with year fixed effects, TFP in 2001 bins interacted with year fixed effects, and firm size in 2001 interacted with year fixed effects to the baseline specifications. These specifications correspond to columns (6) and (7) of Table 3.4, respectively. 95% confidence intervals are included for each annual point estimate with standard errors clustered by the 4-digit NAICS-by-state level. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

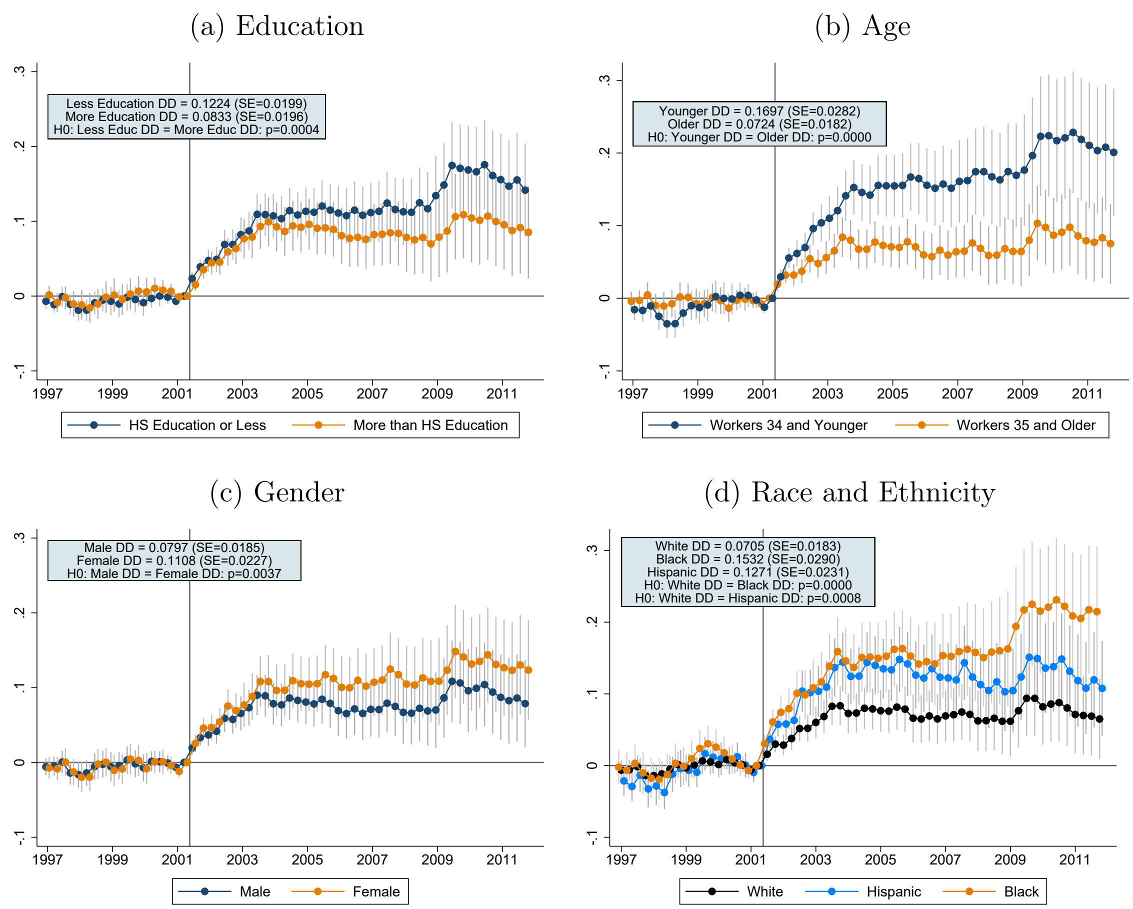
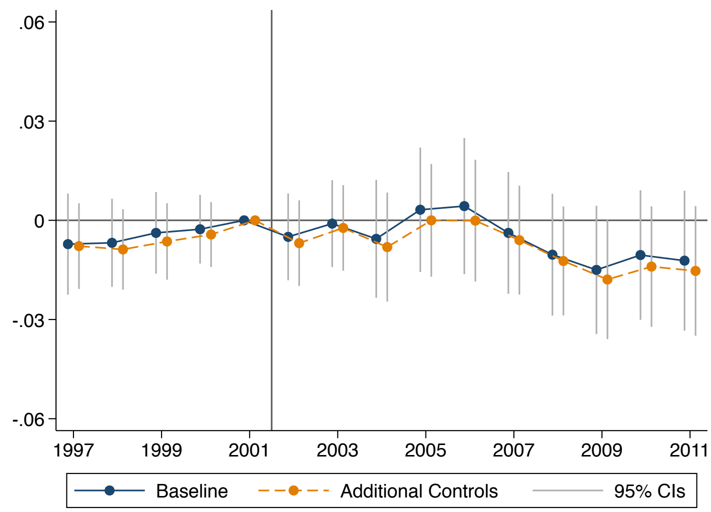


Figure 3.6: Effects of Bonus Depreciation on Employment by Demographic Group

Notes: Figure 3.6 displays estimates describing the effects of bonus depreciation on log employment for a number of demographic subgroups using QWI data. Panel (A) shows effects separately for workers with high school education or less and for workers with more than high school education. Panel (B) shows effects separately for workers 35 years of age and younger and 36 and older. Panel (C) shows effects separately for men and women. Panel (D) presents separate effects for white, Black, and Hispanic workers. All specifications used for each panel include 4-digit NAICS-by-state fixed effects, state-by-quarter fixed effects and controls for industry-level pre-period trends in employment for the focal group. 95% confidence intervals are included for each annual point estimate with standard errors clustered by the 4-digit NAICS-by-state level. The text box in each panel reports the associated DD estimates for each subgroup as well as the p-values from hypothesis tests comparing DD estimates for different subgroups. *Source:* Authors' calculations based on QWI and Zwick and Mahon [2017] data.

(a) Total Factor Productivity



(b) Log Total Value of Shipments

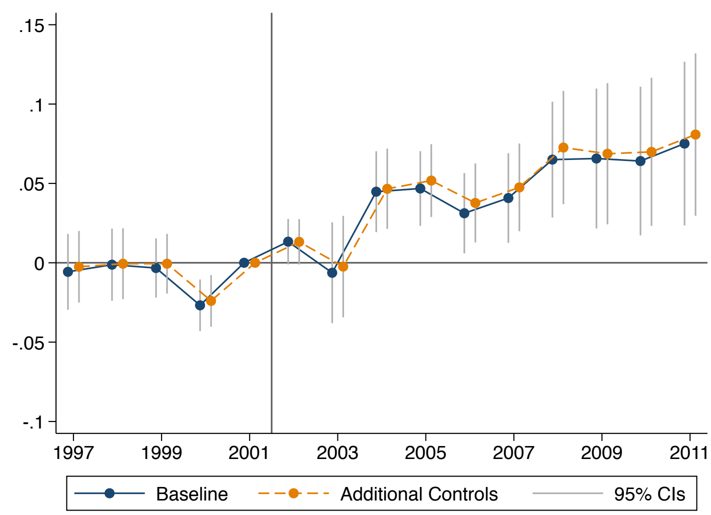


Figure 3.7: Effects of Bonus Depreciation on Productivity and Production

Notes: Figure 3.7 displays estimates describing the effects of bonus depreciation on total factor productivity in Panel (A) and log total value of shipments in Panel (B). Plotted coefficients are estimates of β_y from Equation (3.1), which are the annual coefficients associated with bonus. The baseline specification in each panel includes state-by-year and plant fixed effects. The specifications with additional controls add plant size in 2001 bins interacted with year fixed effects, TFP in 2001 bins interacted with year fixed effects, and firm size in 2001 interacted with year fixed effects to the baseline specifications. These specifications correspond to columns (6) and (7) of Table 3.4, respectively. 95% confidence intervals are included for each annual point estimate with standard errors clustered by the 4-digit NAICS-by-state level. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

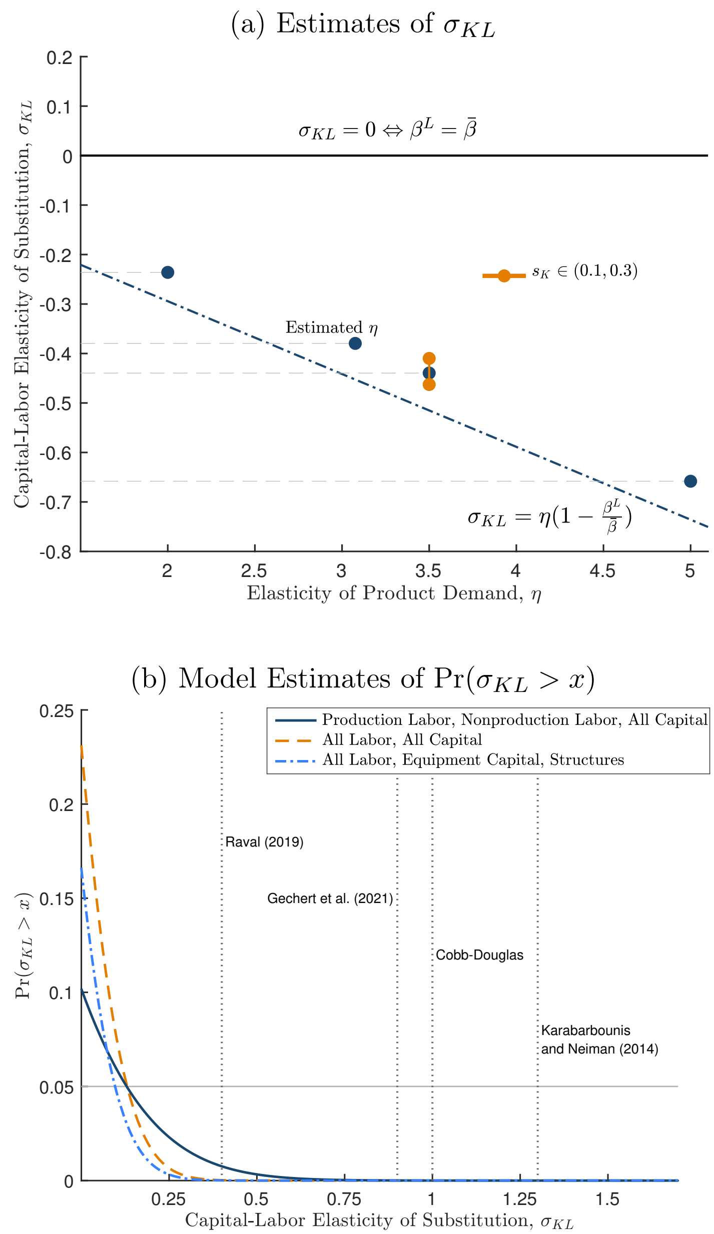


Figure 3.8: Model Estimates of Capital-Labor Elasticity of Substitution

Notes: Panel (A) of Figure 3.8 graphically displays our estimates of σ_{KL} based on our long-differences estimates of the effects of bonus depreciation on capital and labor demand for a range of values of η. The solid blue line in Panel (B) of Figure 3.8 displays the probability that the estimated capital-labor substitution parameter σ_{KL} in our baseline model (Column (1), Table 3.8) is greater than the values along the x-axis. The dashed orange line reports a similar probability for a model with one type of labor and capital (Column (4), Table A14) and the light-blue dot-dashed line reports the case of a model with one type of labor alongside equipment and structures (Column (5), Table A14). Vertical lines correspond to σ_{KL} values from Raval [2019], from Gechert et al. [2021], a $\sigma_{KL} = 1$ implied by a Cobb-Douglas production function, and from Karabarbounis and Neiman [2014], respectively. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table 3.1: Effects of Bonus Depreciation on Capital Investment

	(1)	(2)	(3)	(4)	(5)
	Panel A: Log Investment				
Bonus	0.1698***	0.1556***	0.1508***	0.1518***	0.1577***
	(0.0285)	(0.0276)	(0.0281)	(0.0279)	(0.0285)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
	Panel B: IHS Investment				
Bonus	0.1675***	0.1531***	0.1486***	0.1498***	0.1561***
	(0.0298)	(0.0289)	(0.0294)	(0.0292)	(0.0298)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
	Panel C: Investment over Pre-Period Capital				
Bonus	0.0309***	0.0288***	0.0267***	0.0272***	0.0278***
	(0.0044)	(0.0043)	(0.0044)	(0.0043)	(0.0045)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Year FE	✓				
Plant FE	✓	✓	✓	✓	✓
State×Year FE		✓	✓	✓	✓
PlantSize_{2001}×Year FE			✓	✓	✓
TFP_{2001}×Year FE				✓	✓
FirmSize_{2001}×Year FE					✓

Notes: Table 3.1 displays estimates describing the effects of bonus depreciation on log investment in Panel (A), log total capital in Panel (B), and investment over pre-period capital in Panel (C). Difference-in-differences subpanels show estimates of β from specifications in the form of Equation (3.2) while the long difference subpanels show estimates of β_{2011} from specifications in the form of Equation (3.1). Specification (1) estimates include year and plant fixed effects. Specification (2) estimates include state-by-year fixed effects and plant fixed effects. Specifications (3), (4), and (5) progressively add plant size in 2001 bins interacted with year fixed effects, TFP in 2001 bins interacted with year fixed effects, and firm size in 2001 interacted with year fixed effects, respectively, to the controls in the preceding column. Standard errors are presented in parentheses and are clustered at the 4-digit NAICS-by-state level. p-values are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table 3.2: Effects of Bonus Depreciation on Capital Stocks

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Total Capital		Log Equipment Capital		Log Structures Capital	
Bonus	0.0804***	0.0778***	0.1047***	0.0962***	0.0413**	0.032*
	(0.0183)	(0.0186)	(0.0192)	(0.0193)	(0.0181)	(0.0189)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.023]	[0.090]
Plant FE	✓	✓	✓	✓	✓	✓
State$\times$Year FE	✓	✓	✓	✓	✓	✓
PlantSize$_{2001}\times$Year FE		✓		✓		✓
TFP$_{2001}\times$Year FE		✓		✓		✓
FirmSize$_{2001}\times$Year FE		✓		✓		✓

Notes: Table 3.2 displays long differences estimates describing the effects of bonus depreciation on measures of capital stocks. For each measure of capital stock, the first specification includes year and plant fixed effects and the second specification includes plant size in 2001 bins interacted with year fixed effects, TFP in 2001 bins interacted with year fixed effects, and firm size in 2001 interacted with year fixed effects. Standard errors are presented in parentheses and are clustered at the 4-digit NAICS-by-state level. p-values are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table 3.3: Effects of Bonus Depreciation on Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Log Total Employment							
	Difference-in-Differences					Long Difference	
Bonus	0.0849***	0.0812***	0.0788***	0.0785***	0.0791***	0.0965***	0.095***
	(0.0097)	(0.0096)	(0.0096)	(0.0095)	(0.0097)	(0.0152)	(0.0158)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel B: Log Production Employment							
	Difference-in-Differences					Long Difference	
Bonus	0.1047***	0.1013***	0.0993***	0.0993***	0.0987***	0.1163***	0.115***
	(0.0108)	(0.0106)	(0.0106)	(0.0105)	(0.0107)	(0.0164)	(0.0168)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel C: Log Nonproduction Employment							
	Difference-in-Differences					Long Difference	
Bonus	0.0732***	0.0683***	0.064***	0.062***	0.0622***	0.0905***	0.0814***
	(0.0165)	(0.0163)	(0.0162)	(0.0163)	(0.0163)	(0.0249)	(0.0257)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.002]
Year FE	✓						
Plant FE	✓	✓	✓	✓	✓	✓	✓
State×Year FE		✓	✓	✓	✓	✓	✓
$\text{PlantSize}_{2001}\times$Year FE			✓	✓	✓		✓
$\text{TFP}_{2001}\times$Year FE				✓	✓		✓
$\text{FirmSize}_{2001}\times$Year FE					✓		✓

Notes: Table 3.3 displays difference-in-differences and long difference estimates describing the effects of bonus depreciation on log employment. The difference-in-differences subpanels show estimates of β from specifications in the form of Equation (3.2) while the long difference subpanels show estimates of β_{2011} from specifications in the form of Equation (3.1). Standard errors are presented in parentheses and are clustered at the 4-digit NAICS-by-state level. p-values are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table 3.4: Effects of Bonus Depreciation on Earnings, Productivity, and Revenue

	Panel A: Log Mean Earnings						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Difference-in-Differences					Long Difference	
Bonus	-0.0179***	-0.0208***	-0.0209***	-0.0205***	-0.0207***	-0.0282***	-0.0273***
	(0.0045)	(0.0043)	(0.0043)	(0.0043)	(0.0044)	(0.0069)	(0.0071)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
	Panel B: Total Factor Productivity						
	Difference-in-Differences					Long Difference	
Bonus	-0.0007	-0.0015	-0.0011	-0.0017	-0.0028	-0.0122	-0.0153
	(0.0062)	(0.0061)	(0.0061)	(0.006)	(0.0059)	(0.0108)	(0.01)
	[0.910]	[0.806]	[0.857]	[0.777]	[0.635]	[0.259]	[0.126]
	Panel C: Log Total Value of Shipments						
	Difference-in-Differences					Long Difference	
Bonus	0.0572***	0.0514***	0.0512***	0.0517***	0.0542***	0.0751***	0.0808***
	(0.0147)	(0.0138)	(0.0138)	(0.0136)	(0.0139)	(0.0263)	(0.0261)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.004]	[0.002]
Year FE	✓						
Plant FE	✓	✓	✓	✓	✓	✓	✓
State×Year FE		✓	✓	✓	✓	✓	✓
$\text{PlantSize}_{2001}\times$Year FE			✓	✓	✓		✓
$\text{TFP}_{2001}\times$Year FE				✓	✓		✓
$\text{FirmSize}_{2001}\times$Year FE					✓		✓

Notes: Table 3.4 displays estimates describing the effects of bonus depreciation on log mean earnings in Panel (A), log TFP in Panel (B), and log total value of shipments in Panel (C). Difference-in-differences subpanels show estimates of β from specifications in the form of Equation (3.2) while the long differences panel shows estimates of β_{2011} from specifications in the form of Equation (3.1). Standard errors are presented in parentheses and are clustered at the 4-digit NAICS-by-state level. p-values are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table 3.5: Effects of Bonus Depreciation, Controlling for Shocks to Manufacturing Sector

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Investment		Log Employment		Log Mean Earnings	
Bonus	0.1577***	0.1566***	0.0791***	0.0691***	-0.0207***	0.0001
	(0.0285)	(0.0315)	(0.0097)	(0.0104)	(0.0044)	(0.0048)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.983]
Plant FE	✓	✓	✓	✓	✓	✓
State×Year FE	✓	✓	✓	✓	✓	✓
Plant Controls ×Year FE	✓		✓		✓	
Sector Shocks ×Year FE		✓		✓		✓

Notes: Table 3.5 displays difference-in-differences estimates from specifications in the form of Equation (3.2) on log investment, log employment, and log mean earnings. All specifications include state-by-year and plant fixed effects. To control for trends in the manufacturing sectors, all specifications also include skill intensity bins interacted with year fixed effects, capital intensity bins interacted with year fixed effects, Chinese import exposure bins interacted with year fixed effects, and robotization bins interacted with year fixed effects. Standard errors are presented in parentheses and are clustered at the 4-digit NAICS-by-state level. p-values are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on ASM, CM, Zwick and Mahon [2017], Acemoglu et al. [2016], and Acemoglu and Restrepo [2020] data.

Table 3.6: Effects of Bonus Depreciation, Interactions with Shocks to Manufacturing Sector

Interaction Term	(1) Skill Intensity	(2) Capital Intensity	(3) Trade Exposure	(4) Robot Exposure
	Panel A: Log Investment			
Bonus	0.1801***	0.1565***	0.1249***	0.1584***
	(0.0337)	(0.0314)	(0.0313)	(0.0314)
	[0.000]	[0.000]	[0.000]	[0.000]
Bonus×Interaction	0.0978*	0.0316**	-0.0858***	0.0158
	(0.055)	(0.0152)	(0.0284)	(0.012)
	[0.075]	[0.038]	[0.003]	[0.188]
	Panel B: Log Total Employment			
Bonus	0.0743***	0.0691***	0.0538***	0.0705***
	(0.011)	(0.0104)	(0.011)	(0.0103)
	[0.000]	[0.000]	[0.000]	[0.000]
Bonus×Interaction	0.0215	0.0049*	-0.0415***	0.0125***
	(0.018)	(0.0029)	(0.0107)	(0.0038)
	[0.232]	[0.091]	[0.000]	[0.001]
Plant FE	✓	✓	✓	✓
State×Year FE	✓	✓	✓	✓
Skill Intensity×Year FE	✓	✓	✓	✓
Capital Intensity×Year FE	✓	✓	✓	✓
Trade Exposure×Year FE	✓	✓	✓	✓
Robot Exposure×Year FE	✓	✓	✓	✓

Notes: Table 3.6 displays difference-in-differences estimates and coefficients describing interactions between difference-in-differences terms and variables capturing manufacturing sector trends. The outcome variable in Panel (A) is log investment. The outcome variable in Panel (B) is log total employment. In Specifications (1)–(4), the difference-in-differences coefficient is interacted with measures of skill intensity, capital intensity, Chinese import exposure, and robotization respectively. All specifications include state-by-year and plant fixed effects. To control for trends in the manufacturing sectors, all specifications also include skill intensity bins interacted with year fixed effects, capital intensity bins interacted with year fixed effects, Chinese import exposure bins interacted with year fixed effects, and robotization bins interacted with year fixed effects. Standard errors are presented in parentheses and are clustered at the 4-digit NAICS-by-state level. p-values are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on ASM, CM, Zwick and Mahon [2017], Acemoglu et al. [2016], and Acemoglu and Restrepo [2020] data.

Table 3.7: Model-Based Implications of Reduced-Form Estimates

	(1) Baseline	(2) Low s_K	(3) High s_K	(4) Low η	(5) High η
	Panel A: Scale Effect Estimates				
Scale Effect, $\bar{\beta}$	0.101***	0.104***	0.099***	0.101***	0.101***
	(0.014)	(0.015)	(0.014)	(0.014)	(0.014)
	Panel B: Allen Elasticities of Substitution				
Production labor-capital, σ_{KL}	-0.515	-0.426	-0.608*	-0.294	-0.736
	(0.336)	(0.330)	(0.362)	(0.192)	(0.481)
Nonproduction labor-capital, σ_{KJ}	0.376	0.445	0.303	0.215	0.537
	(0.587)	(0.545)	(0.637)	(0.335)	(0.838)
	Panel C: p-values for Substitutability Tests				
Substitutability of production labor $H_0 : \sigma_{KL} \geq 0$	0.063	0.099	0.047	0.063	0.063
Complementarity of non-production labor $H_0 : \sigma_{KJ} \leq 0$	0.739	0.793	0.683	0.739	0.739
	Panel D: Cost of Capital Elasticity Estimates				
Effect on cost of capital, ϕ	-0.145***	-0.296***	-0.094***	-0.253***	-0.101***
	(0.021)	(0.044)	(0.013)	(0.036)	(0.014)
Capital, ε_{ϕ}^{K}	-0.555***	-0.271***	-0.852***	-0.317***	-0.793***
	(0.109)	(0.058)	(0.149)	(0.062)	(0.155)
Investment, ε_{ϕ}^{I}	-1.398***	-0.684***	-2.146***	-0.799***	-1.997***
	(0.357)	(0.180)	(0.532)	(0.204)	(0.509)
Production Labor, ε_{ϕ}^{L}	-0.803***	-0.393***	-1.232***	-0.459***	-1.147***
	(0.067)	(0.033)	(0.109)	(0.038)	(0.096)
Non-production Labor, ε_{ϕ}^{J}	-0.625***	-0.306***	-0.959***	-0.357***	-0.893***
	(0.117)	(0.055)	(0.191)	(0.067)	(0.168)
Cost shares:					
Production labor	0.50	0.55	0.45	0.50	0.50
Nonproduction labor	0.30	0.35	0.25	0.30	0.30
Capital	0.20	0.10	0.30	0.20	0.20
Demand Elasticity, η	3.50	3.50	3.50	2.00	5.00

Notes: Table 3.7 presents several results relating our reduced-form estimates to model outcomes across several alternative calibrations of cost shares and η. Panel (A) displays estimates of the scale effect defined in Equation (3.7). Panel (B) presents estimates of the Allen elasticities of substitution between capital and production labor and capital and non-production labor using equations (3.4) and (3.5), respectively. Panel (C) conducts hypothesis tests of the substitutability and complementarity of production and non-production labor, respectively. Panel (D) presents estimates of the effect of bonus depreciation on the cost of capital using the calculated scale effects in Panel (A) and Equation (3.7). It also presents estimates of the elasticity of capital, investment, production labor, and non-production labor with respect to this estimated change in the cost of capital. Standard errors are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table 3.8: Classical Minimum Distance Estimates of Production Elasticities

	(1) Baseline	(2) Low s_K	(3) High s_K	(4) Low η	(5) High η	(6) Est. η
	Panel A: Estimated Parameters					
Demand elasticity, η	3.500	3.500	3.500	2.000	5.000	3.076
						(2.123)
Production labor-capital, σ_{KL}	-0.440	-0.463	-0.410	-0.236	-0.658	-0.380
	(0.346)	(0.356)	(0.353)	(0.208)	(0.489)	(0.435)
Nonproduction labor-capital, σ_{KJ}	0.733	0.727	0.738	0.393	1.097	0.633
	(0.639)	(0.608)	(0.671)	(0.381)	(0.907)	(0.710)
	Panel B: Empirical Moments					
Revenue	0.075	0.075	0.075	0.075	0.075	0.075
Production labor	0.116	0.116	0.116	0.116	0.116	0.116
Nonproduction labor	0.090	0.090	0.090	0.090	0.090	0.090
Capital	0.080	0.080	0.080	0.080	0.080	0.080
	Panel C: Model-Predicted Moments					
Revenue	0.069	0.069	0.069	0.046	0.078	0.065
Production labor	0.109	0.109	0.108	0.103	0.110	0.108
Nonproduction labor	0.076	0.076	0.076	0.074	0.076	0.076
Capital	0.096	0.096	0.097	0.092	0.097	0.096
Cost shares:						
Production labor	0.50	0.55	0.45	0.50	0.50	0.50
Nonproduction labor	0.30	0.35	0.25	0.30	0.30	0.30
Capital	0.20	0.10	0.30	0.20	0.20	0.20
Effect on Cost of Capital, ϕ	-0.14	-0.27	-0.09	-0.23	-0.10	-0.16

Notes: Table 3.8 presents estimates of the structural parameters of the three input model of production labor, non-production labor, and capital in Section 3.6. All parameters estimated using a minimum distance estimator. Column (1) represents our baseline model featuring a calibrated value of $\eta = 3.5$ and cost shares of $s_L = 0.5$, $s_J = 0.3$, and $s_K = 0.5$. Columns (2) and (3) consider lower and higher capital cost shares, columns (4) and (5) consider lower and higher calibrated demand elasticities, and column (6) presents model estimates in which we estimate the value of η. Standard errors are presented in parentheses. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table 3.9: Heterogeneity in Effects of Bonus Depreciation by Labor Market Characteristics

	(1) Log Investment	(2) Log Employment	(3) Log Mean Earnings
Panel A: Interaction with highly unionized plant indicator			
Bonus	0.1966***	0.111***	-0.0158***
	(0.0338)	(0.0107)	(0.0053)
	[0.000]	[0.000]	[0.003]
Bonus×Union	-0.0854**	-0.0619***	-0.0103*
	(0.0385)	(0.012)	(0.0062)
	[0.027]	[0.000]	[0.097]
Panel B: Interaction with Right-to-Work indicator			
Bonus	0.0622*	0.0675***	-0.0232***
	(0.0364)	(0.0131)	(0.0058)
	[0.087]	[0.000]	[0.000]
Bonus×RTW	0.200***	0.0294	0.0052
	(0.0546)	(0.0191)	(0.0086)
	[0.000]	[0.124]	[0.545]
Panel C: Interaction with local labor market concentration			
Bonus	0.1498***	0.082***	-0.022***
	(0.0275)	(0.0096)	(0.0042)
	[0.000]	[0.000]	[0.000]
Bonus×log(HHI)	0.0381**	-0.0053	0.0081***
	(0.0183)	(0.0052)	(0.0029)
	[0.037]	[0.308]	[0.005]
State×Year FE	✓	✓	✓
Plant FE	✓	✓	✓

Table 3.9 displays difference-in-differences estimates and coefficients describing the interaction between difference-in-differences terms and variables capturing labor market characteristics. The outcome variables in Specifications (1)–(3) are log investment, log total employment, and log mean earnings. The treatment variable is interacted with an indicator for more than 60% union presence, an indicator for state Right-to-Work laws as of 2001, and a standardized measure of local HHI in Panels (A), (B), and (C) respectively. All specifications include state-by-year and plant fixed effects. Standard errors are presented in parentheses and are clustered at the 4-digit NAICS-by-state level. p-values are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on ASM, CM, Zwick and Mahon [2017], and Valletta and Freeman [1988] data.

Chapter 4

Competition and Entry in the Commercial Gambling Industry

In 2018, U.S. states raised over $33 billion in tax revenue from commercial and lottery gambling. Although tax revenue generation has typically served as the primary motivation for gambling legislation in the United States, legalization remains controversial due to the potential economic and social effects that accompany gambling. In the case of state-run lotteries, debates over legalization have often centered on regressivity, with opponents arguing that the adverse social costs of gambling fall disproportionately on low socioeconomic individuals, who purchase lottery tickets at higher rates than those from higher income groups [Oster, 2004].

Despite the broad range of concerns associated with legalized gambling, prior research has provided relatively little formal direction on how states may design regulations that balance these costs and benefits. Consequently, state legislatures have expanded the scale of gambling operations at a rapid pace in recent years. Currently, forty-five U.S. states run lotteries, up from thirty-six in 2000, while twenty-six allow the operation of commercial casinos. Most recently, twenty states have legalized sports wagering following a 2018 U.S. Supreme Court ruling permitting such legislation. This landscape is further complicated by the emergence of novel forms of gambling. Commercial gambling in seven states now includes so-called "video gambling," comprising slot machine-like terminals available in bars, restaurants and other establishments.

This paper aims to provide policy guidance for gambling regulations using data on the video gambling industry in Illinois, perhaps the foremost recent example of rapid gambling expansion in the United States. Beginning in September 2012, a broad class of establishments were allowed to apply for licenses to operate up to five video gaming terminals (VGTs). Local municipalities were given discretion to pass ordinances legalizing or prohibiting VGT operation. As of 2019, almost 7,000 establishments operated over 32,000 VGTs in Illinois. In the face of this dramatic expansion, critics have pointed to the relatively low rate of taxation on VGT profits: total tax rates were initially set at 30%, with only 5% of profits accruing to local municipalities.[1]. This rate remains lower than both Illinois casino gambling and video gambling industries in many other U.S. states [Grotto et al., 2019].

I investigate the role of taxation in the the Illinois VGT industry in two broad steps. In the first part of the paper, I estimate the effects of gambling legalization on local government finances. Despite receiving a small share of total tax revenue from VGT activity, this revenue may be substantial enough to stimulate increased local spending given the scope of the industry. Additionally, if VGT activity stimulates local economic activity, it may increase tax revenue from sales taxes and other sources. An accounting of these forces is important for evaluating the overall effects of gambling legalization, particularly because local tax revenue could offset the regressivity of gambling activity if appropriately reinvested.

[1] A 2019 law change increased the state tax rate to 29% but left municipal rates unchanged.

I use a event-study framework that compares the finances of municipalities that allow VGT operations both before and after legalization to municipalities that never implement policies or have yet to do so. I use the "imputation" estimator of Borusyak et al. [2022] to recover consistent estimates of event-study coefficients and flexibly control for population size and pre-period characteristics that might otherwise influence a municipalities' decision to permit gambling. In my preferred specification, I estimate that municipal-level legalization leads on average to a 3.0% increase in total tax revenue over a 5 year period. Local spending increases by 1.6% over the same period, though effects are noisy and occur gradually. On the other hand, I estimate a small but insignificant effect on revenue from non-gambling sources, suggesting that legalization does not produce spillovers to other sources of tax revenue.

In the second part of the paper, I show that establishment-level VGT adoption is highly concentrated in low-income communities, consistent with prior studies on the regressivity of gambling legalization. At the same time, these patterns are largely driven by establishment-level adoption decision and not on the extensive margin of which municipalities permit VGT operation. Motivated by these facts, I develop and estimate an equilibrium model of competition and entry among VGT establishments to study how taxes affect the scale and distribution of gambling.

The VGT industry is an attractive setting for studying how taxes shape the costs and benefits of gambling activity for several reasons. First, VGT activity in Illinois exhibits substantial geographic and time-series variation, driven by both municipal-level legalization and disparities in VGT adoption at the establishment level. In addition to generating useful estimating variation, this feature lends itself particularly well to studying whether the costs of gambling fall disproportionately on low-income communities. Second, the VGT industry experienced a high degree of entry and exit in response to increased competition over time, suggesting that a tax increase would likely induce exit in the industry. Finally, while the rate at which VGT profits are taxed is fixed throughout my sample period, state legislation effectively rules out price responses to an increase in taxation. Thus, entry is likely the only margin on which establishments may meaningfully respond to taxation. This particular feature motivates my modeling approach, which permits policy counterfactuals simulating the effects of alternative tax regimes.

Preliminary empirical analysis reveals several key features of the Illinois VGT industry as they relate to arguments for and against legalized gambling. First, gambling revenue exhibits significant heterogeneity across establishment types. While restaurants and bars make up the majority of VGT establishments, dedicated gambling locations known as "gaming parlors" make up another sizable portion of the market and average about three times the monthly gambling revenue as restaurants and bars. If these locations compete with non-parlor establishments for gambling services, then they may also attenuate positive spillovers to non-gambling expenditures in these locations. Gambling parlors also likely face higher implicit entry costs, since VGTs are their primary source of revenue. As a result, changes in establishment composition are likely an important feature to evaluate in response to a counterfactual tax change.

I find that the spatial distribution of VGT establishments is strongly predicted by local demographic characteristics. After controlling for population, VGT establishments tend to be found in lower median income local markets as defined by Census ZIP Code Tabulation Areas (ZCTA). These patterns hold across establishment types, but with heterogeneous magnitudes. The degree to which these patterns would persist under a counterfactual tax change ultimately depends on how competition and entry costs vary across ZCTAs. A proper accounting of these features of the gambling industry is thus crucial to understanding how lawmakers may leverage gambling taxes in the service of policy objectives.

Estimation of my equilibrium model of competition and entry is similar to the basic framework of Singleton [2019]. I first estimate the competitive effects of VGT establishment entry on the gambling profits of incumbent establishments using within-establishment variation in VGT income over time. With these estimates in hand, I utilize observed entry and exit behavior to estimate how establishment entry decisions relate to gambling profits. Recovery of parameter estimates reflecting these incentives permits counterfactual simulations that I can then relate to aggregate equilibrium outcomes.

Estimates of my model suggest that they entry costs associated with VGT operation are heterogeneous across establishment types, with the highest costs faced by gaming parlors. I also find significant competitive effects of entry within and across types. Finally, entry parameters related to local demographics suggests marginal entrants are most likely to locate in low income, low population communities. Taken together, these results suggest that a tax increase could shift gambling profits toward non-gaming parlor establishments by inducing exit by parlors while also reducing the regressivity of entry by inducing exit from poorer communities.

My counterfactual simulations suggest that existing tax rates lie far below the revenue-maximizing level. Specifically, increasing the tax rate on VGT profits to 50% leads to a 37.8% increase in total tax revenue, despite reducing gambling profits by 17.3%. Consistent with model parameter estimates, tax increases also reduce the degree to which VGT establishments concentrate in lower-income communities.

The rest of this paper is organized as follows. In the next section, I present evidence on the effects of VGT legalization on local government finances. Section II describes the key empirical features of the Illinois VGT industry that motivate the use of my entry model. In Section III, I present empirical evidence on the effects of

VGT legalization on local government finances. Section IV describes my empirical model of competition and entry, Section V presents model estimates, and Section VI simulates counterfactual tax changes. Section VII concludes.

4.1 Gambling and Local Government Finances

Illinois state legislatures passed the Video Gaming Act (VGA) in 2009, which allowed the operation of up to five video gaming terminals in a broad class of establishments starting in September 2012. The legislation specified a fixed division of net-of-winnings VGT profits, with 25% and 5% accruing to the state and local municipalities, respectively, for a total tax rate of 30%. In the face of public opposition, the legislation also empowered municipalities to "opt-out" of allowing VGT operation among local establishments [Grotto et al., 2019]. In practice, the roughly 1,000 municipalities in Illinois varied in whether they passed ordinances legalizing or prohibiting VGT operation, or neglected to pass special ordinances, effectively opting in.

In this section, I estimate how VGT adoption affects local revenue and spending for Illinois municipalities. I use information from the Illinois Gaming Board to determine the timing of VGT adoption among these municipalities. I then link this information to detailed local government data from the Illinois State Comptroller's Office. These data include information on local government revenues, expenditures, cash-on-hand, debt obligations, and other assets and liabilities. These values are disaggregated by fund types, accounting entities into which local government finances are organized for regulatory or functional purposes. Though potentially interesting for understanding how local governments might use VGT revenue, I aggregate across all fund types for my outcomes of interest.

As of 2019, approximately 25% of Illinois municipalities prohibited VGT operation. While legalization occurred throughout the 2012-2019 period, over 50% of legalizing localities did so by 2013, while another 33% did so in 2014. Despite being concentrated in this period, the staggered variation is still useful for controlling for potentially confounding time trends. I trim my sample to exclude municipalities with less than 100 people in 2012 and also exclude the city of Chicago, which does not permit VGTs and is significantly larger than all other Illinois municipalities. VGT localities have similar populations to prohibiting ones (approximately 6700 people in 2012). However, these municipalities feature populations rangings from 100 to 200,000 in 2012. As a result, I include granular population controls in all regressions.

4.1.1 Empirical Approach

My empirical approach aims to estimate the following relationship:

$$y_{it} = \gamma VGT_{it} + X_{it}'\beta + \mu_i + \delta_t + \epsilon_{lt}, \tag{4.1}$$

where VGT_{it} is an indicator for whether municipality i permits VGT operation in fiscal year t, X_{it}' denotes covariates, and μ_i and δ_t are unit and period fixed effects, respectively. The outcome of interest, γ describes the effects of legalization on municipal revenue and expenditure outcomes y_{it}. I estimate versions of this equation using the "imputation" estimator of Borusyak et al. [2022]. This approach recovers consistent estimates of γ while avoiding the negative treatment weights that conventional OLS estimation suffers from with staggered treatment and two-way fixed effects.

I first estimate event-study coefficients centered on VGT adoption. These results provide transparent visualization of the dynamics of estimated effects while also verifying that treated units do not violate the parallel trends assumption underlying my identification argument. In my baseline specifications, I flexibly interact time fixed effects with both population controls and controls for municipality cash-on-hand in fiscal year 2012. Population controls are important to ensure that my results are not biased by comparing treated and control municipalities of vastly different sizes and governmental structures. I operationalize these controls by fixing populations in 2012 and creating decile bins. I also consider two controls for municipal financial characteristics. First, I create quartile bins for the cash reserves of municipalities in 2012. This could plausibly influence selection into VGT adoption, since local governments facing liquidity constraints during the Great Recession may have faced pressure to find additional revenue sources. Formally, my identification argument is that, conditional on these controls, selection into VGT legalization is uncorrelated with time-varying trends in local government finances. I also consider a "fund mix" control, which splits categorizes municipalities based on whether they rely heavily on multiple accounting entities for public spending. Since alternative funds are often used because of regulatory or use constraints, this could also plausibly influence selection into VGT legalization. In practice, my estimates are very similar across specifications utilizing versions of these controls.

4.1.2 Results

Figure 4.1 presents event study coefficient estimates of the effect of VGT legalization on (a) log tax revenue, (b) log total expenditures, and (c) log tax revenue, excluding VGT revenue. All event study regressions feature relatively stable pre-trend terms prior to the first year in which VGTs are operational. Municipalities experience an immediate increase in tax revenue, with a maximum effect of around 5.0% three years after legalization, thought effects stabilize around 4.0%. Column (5) of Table 4.1

presents the difference-in-differences analog to these figures, showing that the average effect in this period is 3.0% for revenue. I find similar sized effects in specifications with alternative controls.

In contrast, effects on log expenditures are a precise zero in the first year of legalization. While these effects grow in a similar pattern to revenues, overall effects are smaller. This pattern equates to a 1.6% effect over the post period, though this effect is not statistically significant (p=0.136) in my preferred specification. Other specifications yield slightly larger expenditure effects, and are significant at the 10% level.

Finally, panel (c) of Figure 4.1 estimates effects on log tax revenue after substracting the amount accruing to municipalities according to gambling activity. While these effects also mirror that of revenue and expenditures, I imprecisely estimate a 1.2% effect, which is closer to zero in alternative specifications. As a result, I find limited evidence that VGT legalization stimulates local economies in a manner that raises additional tax revenue. These results suggest that the 5% share of VGT revenue accruing to municipalities can still equate to a significant increase in total tax revenue. At the same time, it is unclear how effectively Illinois municipalities use this revenue to fund public spending.

4.2 The Video Gambling Industry in Illinois

I now describe in greater detail the basic features of the Illinois gambling industry, and present descriptive evidence on the VGT industry to motivate my equilibrium model. Illinois instituted a lottery in 1974 and legalized casino gambling in 1991. Currently, ten casinos operate in the state. State legislatures passed the Video Gaming Act (VGA) in 2009, which allowed the operation of up to five video gaming terminals in a broad class of establishments. The first licenses for terminal operation were issued in September 2012, and the total number of VGT establishments has grown rapidly since. The left panel of figure 4.2 shows this growth over time. Many Illinois establishments adopted VGTs within the first year of license issuance, and the total number has grown steadily since 2014. As of June 2019, 6,987 establishments operate 32,023 VGTs in Illinois.

License issuance is subject to approval by the Illinois Gaming Board (IGB). Establishments seeking a license must submit an application under one four broad categories: retail establishments, veterans establishments, fraternal establishments, and truck stops. Eligibility language in the VGA is relatively broad. Establishments must possess a valid liquor license to operate VGTs and must have operated for at least forty-eight months prior to July 13, 2009, the date of the original legislation's passage.

As noted before, the VGA specifies a fixed division of net-of-winnings VGT profits that remains fixed through my sample period. Profits are taxed at a total rate of 30% with 25% accruing to the state and 5% to the municipality in which the establishment is located. The remaining profits are split evenly at 35% each to the operating establishment and the terminal operator, firms licensed with the IGB that supply VGTs to these establishments. Though the terminals take the form of different "games," with most establishments typically offering a variety of terminals, the VGA places strict requirements on wagers, winnings, and odds. As of June 2019, machines accept a maximum wager of $2 and yield a maximum payout of $500. The minimum expected payout for each wager is fixed at 80%. All VGTs are linked to the VGB by a centralized communication system that ensures compliance and proper reporting of monthly revenue and profits.

The right panel of figure 4.2 illustrates how state and municipality tax revenue has evolved over time. In 2013, the first full year of VGT operation, this revenue totaled approximately $95 million. In 2018, annual tax revenue had grown to $440 million. For context, this number is comparable to the $462 million the IGB collected in admissions and wagering taxes from Illinois casinos, while the Illinois Lottery collected $736 million in tax revenue in the same year. Excluding VGT and casino taxes that accrued to local municipalities, these three sources of gambling revenue accounted for approximately 4% of the total tax revenue collected by the Illinois state government in 2018.

Since 2012, 9560 establishments have operated VGTs at some point, while 2573 have exited the industry, either by ceasing VGT operation or closing permanently. Information on licensed establishments is listed online by the IGB. Specifically, periodic board meeting reports classify newly licensed establishments according to the four categories listed above, and include their address and name. I use these reports to collect more detailed information from Google Maps and Yelp on the types of establishments that operate VGTs.

Table 4.2 illustrates the composition of the industry, both by total and exiting licenses. Restaurants and bars make up the majority of all locations, though a broad range of establishment types have held licenses, including hotels, bowling alleys, country clubs, and gas stations.[2] Surprisingly, about 13% of current licenses are held by dedicated gaming parlors. The existence of these locations, almost all of which were established after September 2012, would seem to contradict the provision in the VGA regarding length of residency. The IGB website notes that an entity meeting the residency requirement may form a subsidiary that will subsequently meet the requirement as well. Though it is unclear if the 1047 gaming parlors that have

[2]"Unlisted" corresponds to establishments for which the IGB did not list an address or establishment name. "Miscellaneous" refers to a handful of establishments whose type did not fit easily into other categories.

opened in Illinois since 2012 received licenses due to this exception, their operation of VGTs is consistent with anecdotal evidence regarding the loose manner in which the regulatory provisions in the VGA have been applied [Grotto et al., 2019].[3]

The composition of exiting establishments follows a similar pattern to total licenses. Restaurants and bars constitute a higher proportion of exiting than total licenses, and gaming parlors have exited at a lower rate than suggested by their total share of the industry.

4.2.1 Establishment-Level VGT Data

Since September 2012, the IGB has posted monthly revenue reports that constitute the primary data source in this paper. These reports provide monthly, license-level information on the number of terminals, amount gambled, and the net terminal income for all licensed VGT establishments in Illinois. These net terminal profits are equal to the total amount gambled net of payouts. Table 4.3 presents basic summary statistics for these reports at the establishment-month level for the entire September 2012 to June 2019 sample period.

VGT locations average approximately $65,000 in monthly revenue, which translates, on average, to around $16,800 in net terminal income and $5,900 in profits collected by establishments. Table 4.4, which orders establishment categories by average monthly net terminal income, reveals considerable heterogeneity across these types. Truck stops earn the highest on average, while gaming parlors follow close behind at approximately $38,000 in monthly net terminal income. This total is nearly

[3] In 2015, the Chicago Tribune reported that the owner of three gaming parlors in Berwyn, Illinois was a convicted felon charged with tax fraud and the operation of illegal slot machines. The man had originally operated a bar in Berwyn until 2003, when it was fire bombed by the Chicago Outfit, an organized crime syndicate. He later went on to serve as a witness under a grant of immunity against Chicago Outfit members.

three times the average of restaurants and bars, the largest establishment category. These gaming parlors can be further categorized into those under ownership of one of seven gaming parlor chains. These 296 establishments average slightly more at $46,390 in monthly terminal income. Gas stations and liquor stores also rank highly at around $24,000 and $18,000, respectively, in monthly income. On the other hand, the 172 country clubs that have operated VGTs have only averaged $5,300 per month.

4.2.2 Selection on Local Demographics

Having laid out the basic features of the Illinois VGT industry, I now turn to the characteristics of the areas in which VGTs operate. On balance, I find evidence that on average VGT establishments select disproportionately into lower median income communities. To do so, I geocode establishments using the addresses listed in the IGB license reports. I then link locations to 5-year ACS estimates of local demographics at the ZIP Code Tabulation Area (ZCTA) level. This allows me to explore the extent to which the locations of VGTs select on income and other demographics that may reflect patterns documented by prior research on lottery regressivity. In 2017, the most recent year in which 5-year ACS estimates are available, VGTs were operational in 1008 of the 1440 ZCTAs in Illinois. This incomplete coverage largely reflects local municipalities, including the City of Chicago, that had in place ordinances banning VGTs. ZCTAs containing at least one VGT establishment corresponded to 71% of the Illinois population, and 87% of the population excluding Chicago.

To understand how these locations differ on the extensive margin, table 4.5 presents results from ZCTA-level regressions that regress binary variables for whether VGTs were operational in 2017 on local demographics. On average, VGT tend to operate in higher population, lower income communities, including specifications that control for the number SNAP-assisted households and the +16 unemployment rate. For context, the column (3) specification suggests that a 50% increase in median income, which is equivalent to moving from the 25th to 75th percentile ZCTA, corresponds to a 7.8% decrease in the likelihood of VGTs being operational.

Table 4.6 presents similar regressions that focus on the intensive margin of VGT selection. Conditioning on the presence of VGTs, I regress the logarithm of the number of VGT establishments on the same demographic variables in table 4.5. Columns (1) through (3) reveal that, in addition to median income, the number of SNAP households and the local unemployment rate also predict the number of establishments in a given ZCTA. On average, a one percent increase in the number of SNAP households is associated with an 0.10 percent increase in VGT households across most specifications. This result is consistent with the findings of Kearney [2005], who finds that the availability of lottery tickets leads to substitution away from non-gambling, rather than gambling, expenditures. As shown by column (4), the effects on income and SNAP households persist while also controlling for county fixed effects, though the significance of unemployment rate disappears.[4] This result suggests that establishments are not simply selecting into poorer, often rural, counties, but that, conditional on fixed county characteristics, they also select into poorer neighborhoods within counties. On average, a one percent change in median income across ZCTAs in the same county leads to a 0.89% decrease in the number of VGT locations. This estimate suggests that moving from the 25th to 75th percentile, as described before, ZCTA by median income would lead to a 44% decrease in the number of VGT establishments.

[4]ZCTAs do not always fall entirely within a single county. For the 26 Illinois ZCTAs in this sample that fall on county boundaries, I create separate ZCTA-county observations on which to collapse the number of VGT locations. I then assign to all ZCTA-counties the local demographics of the ZCTA.

Finally, these effects disappear when controlling for municipality-level fixed effects. Thus, conditional on the political boundary to which local VGT taxes accrue, there is no additional selection on poorer ZCTAs. Given the prior discussion on how local expenditures may mitigate the regressive effects of gambling, this result suggests that these municipalities are an appropriate destination for tax revenue if policymakers are worried about these regressive effects.

In light of the heterogeneity among establishments demonstrated by table 4.4, it is also useful to examine selection patterns by these establishment types. Table 4.7 presents regressions analogous to columns (3) and (4) from table 4.6 across four select types: restaurants and bars, gaming parlors, chain parlors, and "marts", which I have defined as all gas stations, convenience stores, and liquor stores. Restaurants and parlors exhibit similar selection patterns to all establishments, but income elasticities are larger for restaurants. Gaming parlor chains only select on median income after controlling for county-level fixed effects. This result suggests that they select, on average, into richer, more urban counties, but that conditional on county-level selection, they again choose to locate in poorer ZCTAs.

Marts, on the other hand, demonstrate the reverse pattern: they select on median income across all ZCTAs, but this effect disappears when controlling for county fixed effects. Also of note is that, unlike other types, marts do not select into more populous ZCTAs on average. Together, these result suggests that these locations tend to operate in poorer, less populated, likely rural counties rather than urban ones.

4.2.3 Competition and Entry

I now turn to the features of the VGT industry related to competition and entry. In the following analysis, I restrict attention to three types of establishments for simplicity: restaurants and bars, gaming parlors, and marts, defined earlier as gas stations, convenience stores, and liquor stores. Together, these categories account for 80% of total establishments in my sample.[5] As a starting point, I aggregate gross gambling revenue across these establishments at the ZCTA-month level explore how entry across establishment types affects these totals. Specifically, I estimate fixed effect models of the form

$$\log(\bar{D}_{Jlt} + 1) = Z_l + M_t + \sum_{e \in J} f(N_{elt})\beta_e + \epsilon_{lt}, \tag{4.2}$$

where $\bar{D}_{Jlt}$ is the total gambling revenue in the set of establishments of type $J \equiv [restaurant, parlor, mart]$ in ZCTA l in month t, N_{lt} is the number of establishments of type e operating VGTs, and $f(\cdot)$ is a quadratic polynomial function. By including the ZCTA-level fixed effect Z_l, I restrict identification to within-ZCTA variation in revenue over time.

Table 4.8 presents estimates from variations on this model. Columns (1) through (3) consider the effects of entry separately for each type on own-type aggregate revenue. For each type, the signs on the entry coefficients imply large effects for the initial entrant, though these effects are not statistically significant in the case of marts, likely due to its relatively smaller sample size. These magnitudes are largely mechanical, since type j gambling revenue is only reported if a type j establishment operates in the ZCTA in question. However, in each case the sign of the quadratic term implies these effects diminish as more establishments enter. The marginal effects implied by these estimates are consistent with substitution within establishment categories, since entry implies lower average monthly revenue across establishments. Column (4), which aggregates revenue across the three establishment type, shows

[5] The largest omitted categories are: veterans organizations, truck stops, hotels, bowling alleys, and fraternal organizations. Together they account for an additional 14% of establishments

similar implied marginal effects of entry by each type of establishment. Somewhat surprisingly, the marginal effects of entry of both gaming parlors and and marts on type-aggregated revenue is larger than the effects on own-type revenue, implying that gambling across establishment types are complements. Alternatively, these estimates could indicate that the timing of entry is endogenous to incumbent VGT profits, as Hak [2019] finds in the Illinois VGT industry. Ultimately, these aggregate results demonstrate why an establishment-level model that accounts for the endogeneity of entry is necessary to fully understand patterns of competition across establishments.

As noted before, the VGT industry in Illinois has also experienced significant exit. This exit behavior is essential to understanding how an increase in the tax rate applied to gambling profits may distort existing entry patterns, particularly in how establishments' monthly net-of-tax income inform their entry and exit decisions.

To explore these patterns, I estimate a linear probability model of observed exit behavior on ZCTA and establishment-type fixed effects, as well as a flexible function of net-of-tax income in the prior month:

$$Exit_{it} = \eta_l + \xi_j + \sum_{e=j} I_e g\big((1-\tau_j)D_{it-1}\big)\phi_e + \nu_{ijlt}, \tag{4.3}$$

where $Exit_{it}$ denotes whether establishment i ceased VGT operations in month $t+1$, D_{it-1} is the establishment's prior months' net terminal income, and τ_j is the implicit tax rate on this income. The vector-valued function $g(\cdot)$ categorizes establishments' net-of-tax income $(1-\tau_j)D_{it-1}$ into \$500 income bins from \$0 to \$10,000 (income greater than \$10,000 is the omitted category). "Exit" for restaurants or marts may either constitute ceasing operation of VGTs or closing permanently. Because the latter type of exit likely reflects factors other than VGT revenue, I also include a dummy variable that indicates if the exit of a non-parlor establishment was permanent. As such, the effects associated with these income bins can be interpreted as the marginal effects conditional on continued operation of the restaurant or mart.

Panels (A) through (C) of figure 4.3 plot estimates of these income bins, $\hat{\phi}_e$, separately by establishment type. Prior month net-of-tax profit generally predicts exit across establishment types, with lower profits implying non-decreasing marginal effects on the probability of exit. However, magnitudes vary greatly across establishment types: gaming parlors see much higher implied probabilities of exit across income bins than restaurants or marts. For instance, a decrease in prior months' income from \$10,000 to \$2,000 increases a gaming parlors' probability of exit in the next month by 6.76 percentage points, while the same change for a restaurant only implies a 0.31 percentage point effect. For marts, this change is associated with a similar effect of 0.36 percentage points. It is also useful to note that these marginal probabilities are at the monthly level, so the implied cumulative probabilities of exit over an extended period can be quite large in the presence of a permanent income shift such as a tax increase. Moreover, panel (D) shows that gaming parlors operating in June 2019 experience recent monthly profits that suggest a sizable number operate at or near statistically significant exit margins.

As noted before, gaming parlors likely face higher implied entry costs into the VGT industry because terminals constitute their primary source of income. Conversely, restaurants face little financial cost to VGT operation, though they may incur indirect costs if gambling activity deters potential customers. These reputation costs to VGT activity are likely smaller for gas stations, convenience stores, and liquor stores. The exit patterns in figure 4.3 are consistent with these heterogenous entry costs across VGT establishment categories.

Taken in isolation, this evidence suggests that a tax increase will induce exit across establishment types, with this exit behavior concentrated among gaming parlors, where the probability of exit is relatively more sensitive to net-of-tax income in the prior month. Under a modest tax increase, such as raising the implicit tax from 65% to 75%, a gaming parlor previously receiving $5,000 in monthly profit would see a 2 percentage point increase in their likelihood of exiting in the subsequent month. This effect translates to a 21.5% increase in the likelihood of exit over the next year. However, equilibrium outcomes may differ depending on the nature of competition among establishments. In particular, incumbents that would otherwise exit may remain in operation if the expected increase in revenue due to decreased competition from exiting establishments compensates them for the loss in revenue induced by a tax increase. Properly accounting for these mechanisms requires an equilibrium model.

4.3 Empirical Model of Competition and Entry

With these motivating facts in hand, I next describe the empirical model of establishment entry and competition that may be used to conduct taxation counterfactuals. The overall structure of my model is similar to the model of charter school supply and competition of Singleton [2019]. First, I estimate establishment-level gambling demand as a function of local competition. I then plug these "offline" estimates into profit expressions that determine establishment entry and exit decisions as a function of demand. These expressions are then used to estimate an equilibrium game of entry and exit following Bajari et al. [2010].

4.3.1 VGT Income

I model monthly income from VGT gambling at the establishment level as a function of the number of other VGT establishments in the local ZCTA. My estimating equation is given by

$$\log(P_{ijt} + 1) = \gamma \bar{P}_{jt-2} + \sum_{e=j} I_e N_{lt}^{'} \beta_e + \mu_i + \alpha_t + \epsilon_{it}, \tag{4.4}$$

where $\mathbf{N_{lt}}$ is a vector of the number of other gaming parlors, restaurants, and markets, respectively, in ZCTA l at month t and I_e is a same-type establishment indicator. Thus, the effects of competing establishment entry varies flexibly by each incumbent-entrant-type pair.

The primary threat to the validity of OLS estimates of β_e is the endogeneity of entry to demand shifters. To address this concern, I first include establishment and month fixed effects. The former accounts for establishment-average demand, which may be a function of local market characteristics, while the latter accounts for time-varying demand shifters that are common across markets.

Despite the inclusion of fixed effects, OLS estimates could still be confounded by time-varying, location-specific drivers of demand. I account for this possibility by including a two-month lag of total gambling income in the local market. I assume that local gambling profit is the only signal available to potential entrants regarding time-varying local demand shifters, while the two-month lag is consistent with the fact that entrants typically receive a license one to two months after applying for one. This choice is consistent with the findings of Hak [2019], who shows that incumbent profits are an important predictor of entry in the Illinois VGT industry.

4.3.2 Entry and Exit

I model establishment entry and exit as a static game of strategic interaction. I assume that in a given market, there are a finite number of players indexed as $i = 1, ..., n$ that choose an action $a_i \in [0, 1]$ to enter or exit the market. For simplicity, I assume that potential entrants may not choose the market in which they enter, nor may they choose their type. I drop time subscripts in the following section for ease of exposition, but in estimation I assume the entry game is played monthly. Period utility for an establishment of type $j \in [Restaurant, Parlor, Marts]$ is given by the expression

$$u_{ij}(a_i, a_{-i}, s, \epsilon_i; \theta) = \pi_{ij}(a_i, a_{-i}, s; \theta) + \epsilon_i(a_i), \tag{4.5}$$

where ϵ_i is a private information preference draw and s is an observed, common knowledge state across players in the same market. As is standard, I assume preference draws are distributed i.i.d type I extreme value.

I assume that establishments' type-specific profits are a function of observed post-tax revenue and local characteristics as captured by X_l. Importantly, I assume that the entry decisions of other establishments only affects profits via the competitive effects on pre-tax income as estimated in equation (4.4). By denoting a vector of strategies played by other players as $a_{-i} = (a_1, ..., a_{i-1}, a_{i+1}, ..., a_n)$, realized profits take the form

$$\pi_{ij}(a_i, a_{-i}, s; \theta) = f(X_l') + \delta(1 - \tau_j)P_{ij}(a_{-i}), \tag{4.6}$$

where δ is a parameter of interest describing how post-tax terminal income relate to realized profits, and τ_j is the fixed implicit tax rate on net terminal income chosen by policymakers. While this rate is fixed for all establishments in my sample, I allow it to vary by establishment type in the model for the purpose of later counterfactual simulations. Because I observe repetitions of the entry game over time, I assume for simplicity that the state vector is time-invariant and enters the profit function in the form

$$f(X_l') = \lambda_l + \omega_j, \tag{4.7}$$

$$\lambda_l = \lambda^1 Income_l + \lambda^2 Population_l. \tag{4.8}$$

Thus, the relevant states that influence establishment profits are local-specific characteristics as embedded in the ZCTA-specific term λ_l and fixed costs embedded in the establishment-type fixed effect ω_j. The former will capture how local demographics drive entry as explored in Section II.B. For simplicity, and because ZCTA-level real median income and population are relatively stable over the sample period, these are assumed to be fixed at their 2017 levels. The latter term then captures the heterogeneous costs likely faced by different establishment types.

Establishments choose an action in each period based on their expected utility from these choices. Since $f(s')$ is fixed, this constitutes forming expectations of next period net terminal income as a function of the actions of other players. I denote the probability of a given set of opponent actions a_{-i} as

$$\sigma_{-i}(a_{-i}|s) = \Pi_{k \neq i} \sigma_k(a_k|s), \tag{4.9}$$

where $\sigma_k(a_k|s)$ is the probability that player k chooses action $a_k \in a_{-i}$. Combining equations (4.6), (4.8), and (4.9) yields the expected utility expression

$$U_{ij}(a_i, a_{-i}, s, ; \theta) = \lambda_l + \omega_j + \delta(1 - \tau_j) \sum_{a_{-i}} P_{ij}(a_{-i}) \sigma_{-i}(a_{-i}|s). \tag{4.10}$$

I normalize the profits from choosing to exit to 0, which yields the standard expression of incumbent i's choice problem as choosing to remain if

$$U_{ij}(1, a_{-i}, s, ; \theta) + \epsilon_i(1) \geq \epsilon_i(0), \tag{4.11}$$

and exiting otherwise. Given my assumption on the distribution of the preference shocks, this choice problem leads to the standard closed-form expression of the choice probability of entry given by

$$\sigma(a_i = 1|s) = \frac{\exp\big(U_{ij}(1, a_{-i}, s, \epsilon_i; \theta)\big)}{1 + \exp\big(U_{ij}(1, a_{-i}, s, \epsilon_i; \theta)\big)}. \tag{4.12}$$

4.4 Model Estimation

I make several simplifying assumptions in the estimation of the equilibrium model of entry and competition. As noted before, I first restrict estimation to three types of establishments: restaurants and bars, gaming parlors, and marts. I also exclude the early period of the industry, specifically, the September 2012 to December 2014 period. These months were characterized by rapid growth in VGT licenses, so restricting the estimating sample to the latter periods is likely better suited to understanding the equilibrium state of the industry.

Estimation of the entry game parameters in equation (4.10) first requires estimates of establishments' ex-ante beliefs about the actions of the other establishments in their ZCTA, $\sigma_{-i}(a_{-i}|s)$. Following Bajari et al. [2010], I obtain these estimates by regressing observed entry and exit behavior at the establishment level on a ZCTA fixed effect, an establishment-type fixed effect and a function of net-of-tax establishment income in the previous month interacted with establishment type-fixed effects to control for heterogeneous income effects. As demonstrated by figure 4.3, such heterogeneity is relevant for predicting entry. However, utilizing the same specification would be computationally prohibitive due to the large number of income bins in equation (4.3). Instead, I estimate $\sigma_j(a_j|s)$ using a similar specification that replaces the granular income bins from figure 4.3 with two bins denoting "high" and "low" income establishments. In addition to making estimation easier computationally, this assumption likely better reflects how establishments form expectations over others' entry and exit behavior. While the monthly revenue reports released by the IGB could technically enable establishments to form such granular beliefs for individual establishments, Hak [2019] finds that aggregated monthly data is a better predictor of entry behavior and, consequently, establishment beliefs. Specifically, I estimate the probability of entry with the model described in equation (4.3) where the dependent variable is now $1 - Exit_{it}$ and the vector-valued function $g(\cdot)$ simply categorizes P_{it-1} as "high" or "low" income flexibly across establishment types. Using figure 4.3 as a guide, I define "low" prior months' income as less than \$4000 for gaming parlors, less than \$2000 for restaurants, and less than \$1000 for marts.

I use these model estimates to generate predicted entry probabilities $\hat{\sigma}_k(a_k = 1|s)$ where for a given ZCTA, s is an establishment type-income level pair, yielding six possible states for each establishment. I further assume that the number of potential entrants of each establishment type for all ZCTAs is set by the sample maximum number of incumbents in the ZCTA. Estimates of the probability of a given vector of player actions, $\hat{\sigma}_{-i}(a_{-i}|s)$, follow directly from equation (4.9). Given prior assumptions, each possible vector of player actions a_{-i} may be expressed as the number of entrants of each type-income level combination.

Combining this expression with equations (4.10) and (4.11) yields new closed-form choice probability expressions that I use for estimation:

$$\sigma(a_i = 1|s) = \frac{\exp\left(\lambda_l + \omega_j + \delta(1-\tau_j)\sum_{a_{-i}} \hat{P}_{ij}(a_{-i})\hat{\sigma}_{-i}(a_{-i}|s))\right)}{1 + \exp\left(\lambda_l + \omega_j + \delta(1-\tau_j)\sum_{a_{-i}} \hat{P}_{ij}(a_{-i})\hat{\sigma}_{-i}(a_{-i}|s))\right)}, \tag{4.13}$$

where $\hat{P}_{ij}(a_{-i})$ is an estimator of establishment i's predicted next-period profit generated from fixed effect estimates of equation (4.4). Equation (4.13) can be then be used to estimate the parameter vector $\theta \equiv [\lambda_l, \omega_j, \delta]$ by maximum likelihood.

To demonstrates how well the expected demand generated by the model predicts exit, figure 4.4 displays the marginal probability of exit for each establishment type as a function of the model-estimated expected net-of-tax income instead of the establishments' previous month net-of-tax income, as in figure 4.3. Marginal probabilities for restaurants and marts are very similar to those in figure 4.3, while gaming parlor marginal probabilities of exit are generally of larger magnitude. In general, this shows that my empirical model seems to be predicting a smooth, relatively monotonic increase in the probability of exit as a function of expected demand.

4.4.1 Results

I first present fixed effect estimates of equation (4.4). Table 4.9 displays semi-elasticities of establishment entry on realized, pre-tax income. Rows correspond to the entrant's type, while columns denote the establishment-type of incumbents. The diagonal entries of table 4.9 demonstrate that VGT entry from a same-type establishment leads to significant reduction in monthly pre-tax income across types. These estimates suggest significant substitution effects for restaurants and parlors, while marts exhibit statistically insignificant same-type substitution effects. Cross-type semi-elasticities, displayed on the off-diagonal entries, demonstrate that establishments compete across all types. The magnitudes of these competitive channels are generally strongest between parlors and other establishment types, where entry from a parlor leads to a decline in profits of between 3.5% and 3.8% at restaurants and marts. These magnitudes differ more substantially when considering the effects on incumbent parlors of entry by restaurants or marts, which lead to a 1.2% and 4.9% decline, respectively. Finally, cross-type semi-elasticities between restaurants and marts are almost identical for each type of entrant, suggesting a 2.7% decline in profits at an incumbent associated with the entry of a different-type establishment.

I next turn to estimates of the entry game parameters in equation (4.10). Consistent with prior discussions, this equation also includes an indicator for whether a restaurant or mart exited the market permanently. Table 4.10 presents estimates of the four key parameters in the entry model. The parameter δ represents the marginal effect of expected VGT profit on establishment utility, while the fixed effect terms ω_j represent fixed sources of income that may influence entry behavior across establishments. My estimate of δ indicates that expected profits have a significant effect on establishments' expected utility. The magnitude of this parameter is similar to previous reduced form estimates: for a gambling parlor, a decrease in take-home income from \$10,000 to \$2,000 leads to a 3.6 percentage point increase in the probability of exit in the subsequent month. In isolation, this estimate again suggests that an increase in the net-of-tax rate on profits, $(1 - \tau_j)$, would increase the likelihood of entry. However, this tax rate also enters into establishments' beliefs about the entry behavior of other establishments, as demonstrated by equation (4.4). Thus, counterfactuals that account for this channel are necessary to fully evaluate how such a change would alter equilibrium behavior.

Estimates of fixed cost parameters are also consistent with the reduced form evidence on exit behavior across establishment types. Gaming parlors have lower implied fixed income than both restaurants and marts, which in my model is equivalent to facing higher fixed costs. Restaurants have lower fixed income with marts, which could reflect higher reputation costs associated with gambling. Finally, estimating equation (4.10) using lagged take-home income instead of the expected income calculated within my model yields very similar parameter estimates.

Importantly, table 4.10 suggests significant heterogeneity in entry probabilities by local demographics. All else equal, incumbents in high income, low population ZCTAs face a lower implied probability of exit. These estimates suggest that marginal entrants are concentrated in poorer, high population communities. While competition could lead to different patterns in equilibrium, these estimates suggest that tax increases would lead to more exit in these communities, all else equal.

4.4.2 Counterfactual Simulations

This section presents results from counterfactuals that simulate entry and tax revenue under uniform tax increases. A tax increase affects equilibrium behavior through two channels. First, establishments will alter their beliefs about the entry and exit behavior of others, $\sigma_{-i}(a_{-i}|s)$. I estimate these counterfactual beliefs by altering the implied take-home income given observed prior period pre-tax income. Given my specification of s, this constitutes an expected shift of marginal-income establishments from high- to low- income states. Second, establishments will apply the new tax rate in their assessment of their expected next period profits. Equation (4.10) captures both channels, which I use to compute counterfactual choice probabilities. I use these counterfactual probabilities to simulate the market assuming a tax increases in January 2015, and compare aggregate outcomes to simulations in which taxes remain fixed. Specifically, I consider progressively higher counterfactual tax rates of 40%, 50% and 60%. While the split of tax revenues accruing to municipalities does not factor into the entry model, my results from section I suggest that increasing local municipalities' share of tax revenue could stimulate local spending.

Figure 4.5 shows how these tax increases affect the total number of VGT entrants, total VGT income, and total tax revenue. Increasing the total tax rate by 10pp implies a substantial increase in total tax revenue of 25.9% with comparatively small declines in the scale of VGT activity (-5.5% and -5.8% for total establishments and net terminal income, respectively). Despite larger declines in gambling activity for tax rates of 50% and 60%, these rates still suggest large gains in total tax revenue. However, the shape of these effects suggests that the revenue-maximizing rate lies between 40% and 60%.

Consistent with model estimates of the effects of income and population on entry, these increases also reduce the concentration of VGT activity in low-income communities. Figure 4.6 illustrates this graphically by regressing ZCTA- level establishment counts on population controls and income deciles and plotting the resulting income coefficients. While ZCTAs in the lowest income decile still contain the greatest number of VGT establishments, the slope of the income-entry gradient declines, with entry rates effectively flat from the 2nd to 5th deciles.

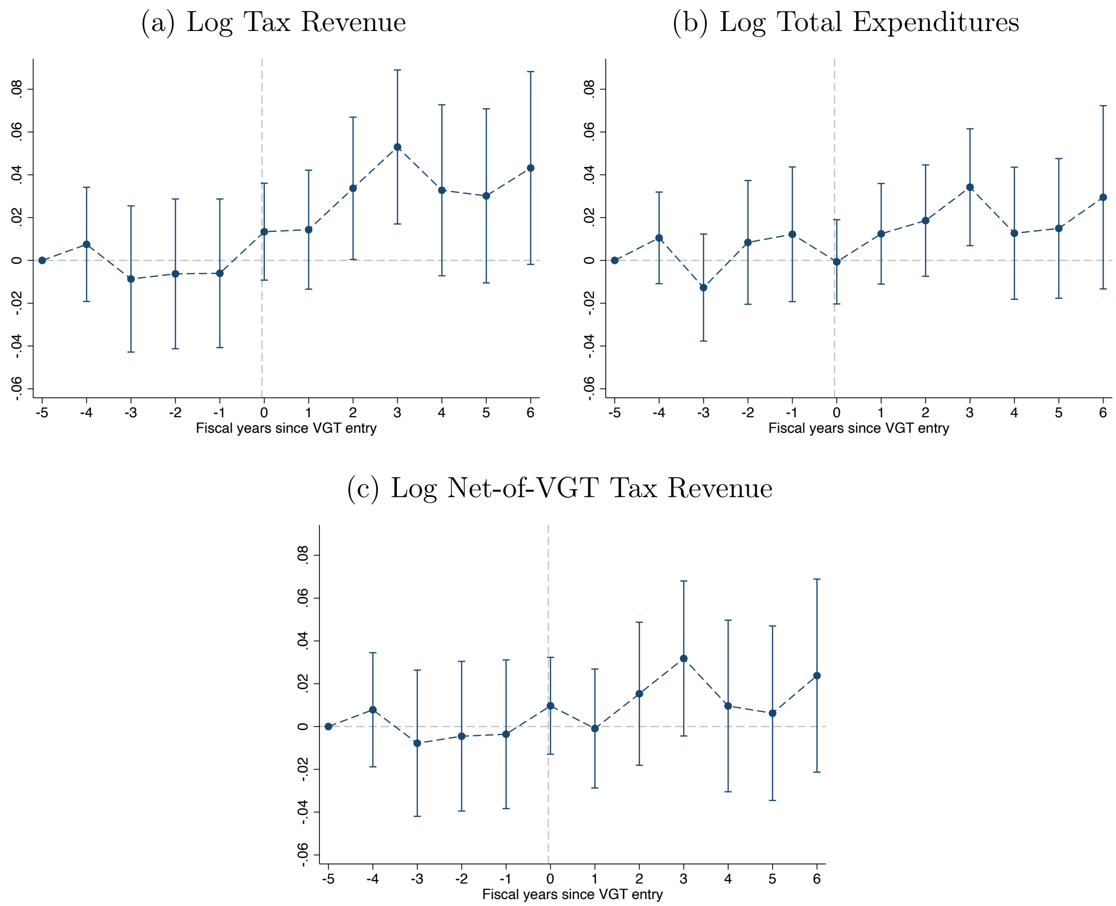


Figure 4.1: Effects of VGT Legalization on Municipal Finances: Event Study Estimates

Notes: Figure 4.1 presents event study coefficient estimates from a difference-in-differences framework of the effects of municipal-level legalization of VGTs using the estimator of Borusyak et al. [2022]. Event time 0 denotes the first year in which any VGT were operational. All specifications include municipality and fiscal year-by-cash reserve bin-by-population bin fixed effects. *Source:* Authors' calculations based on Illinois Gaming Board and Illinois Comptroller Office data.

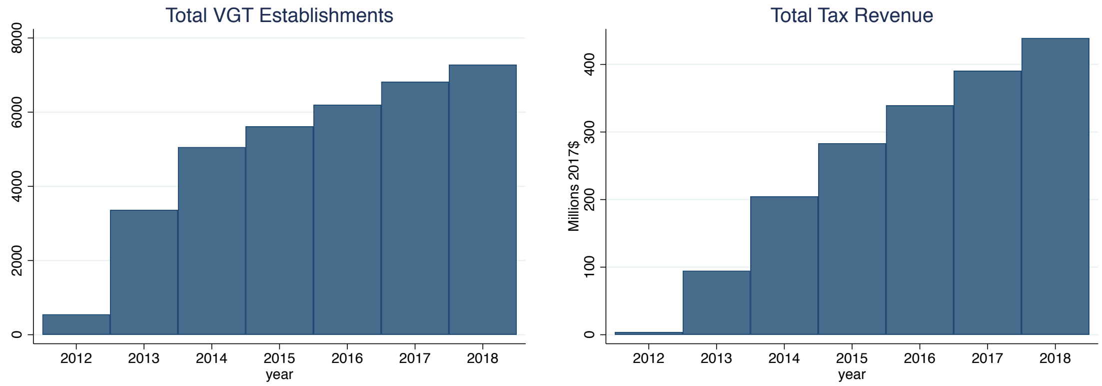


Figure 4.2: Evolution of Illinois Video Gaming: 2012-2018

Notes: Figure 4.2 shows the number of establishments ever operating video gaming terminals in the years 2012-2018 and the total tax revenue raised from VGT operation over the same time period. *Source:* Authors' calculations based on Illinois Gaming Board data.

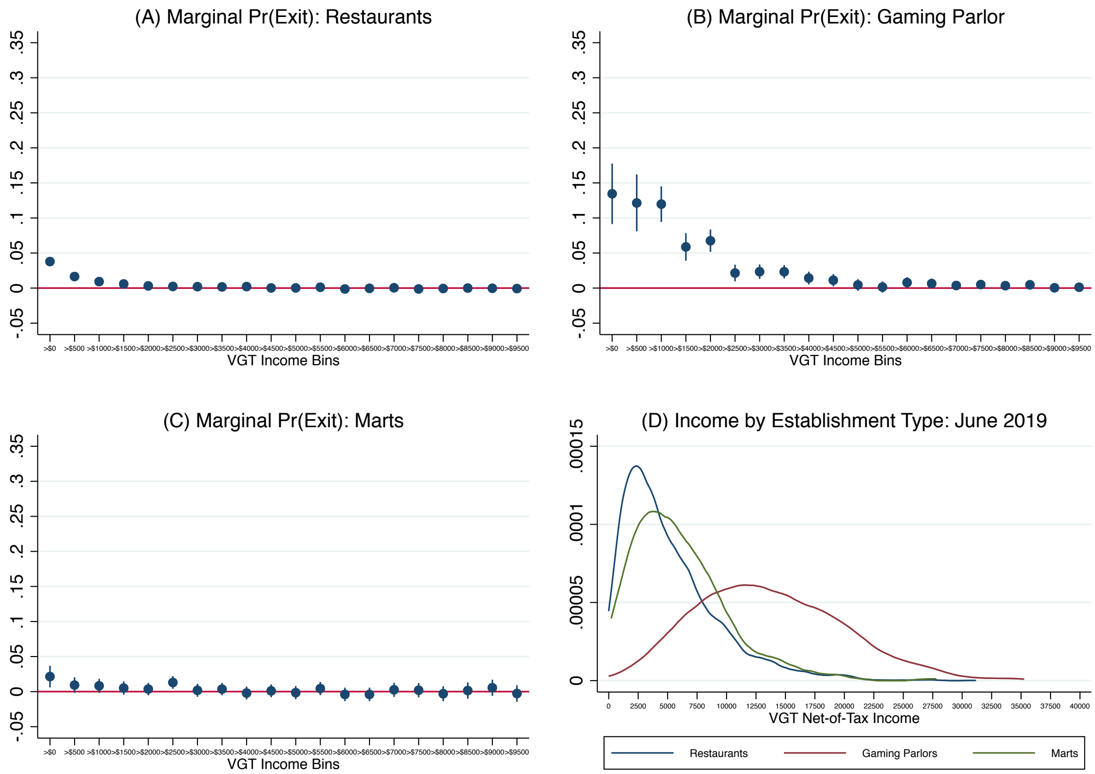


Figure 4.3: Exit Probabilities by Establishment Type: Prior Month VGT Income

Notes: Figure 4.3 presents the marginal change in the probability of exit by prior month net-of-tax income for VGT establishments. Estimates generated from establishment-level, monthly linear probability model of observed entry and exit regressed on ZCTA and establishment-type fixed effects, as well as prior month income bin dummies interacted with the establishment-type fixed effect. *Source:* Authors' calculations based on Illinois Gaming Board data.

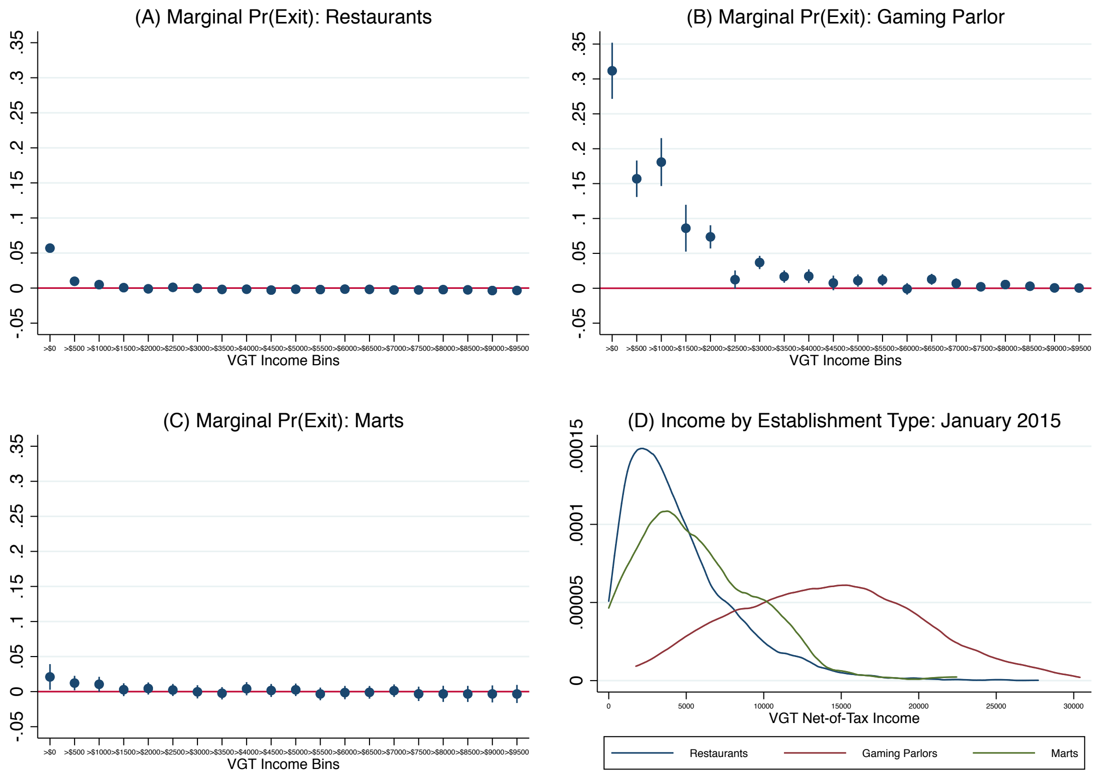


Figure 4.4: Exit Probabilities by Establishment Type: Expected VGT Income

Notes: Figure 4.3 presents the marginal change in the probability of exit by model-implied expected VGT take-home income for VGT establishments. Estimates are generated from establishment-level, monthly linear probability model of observed entry and exit regressed on ZCTA and establishment-type fixed effects, as well as expected income bin dummies interacted with the establishment-type fixed effect. *Source:* Authors' calculations based on Illinois Gaming Board data.

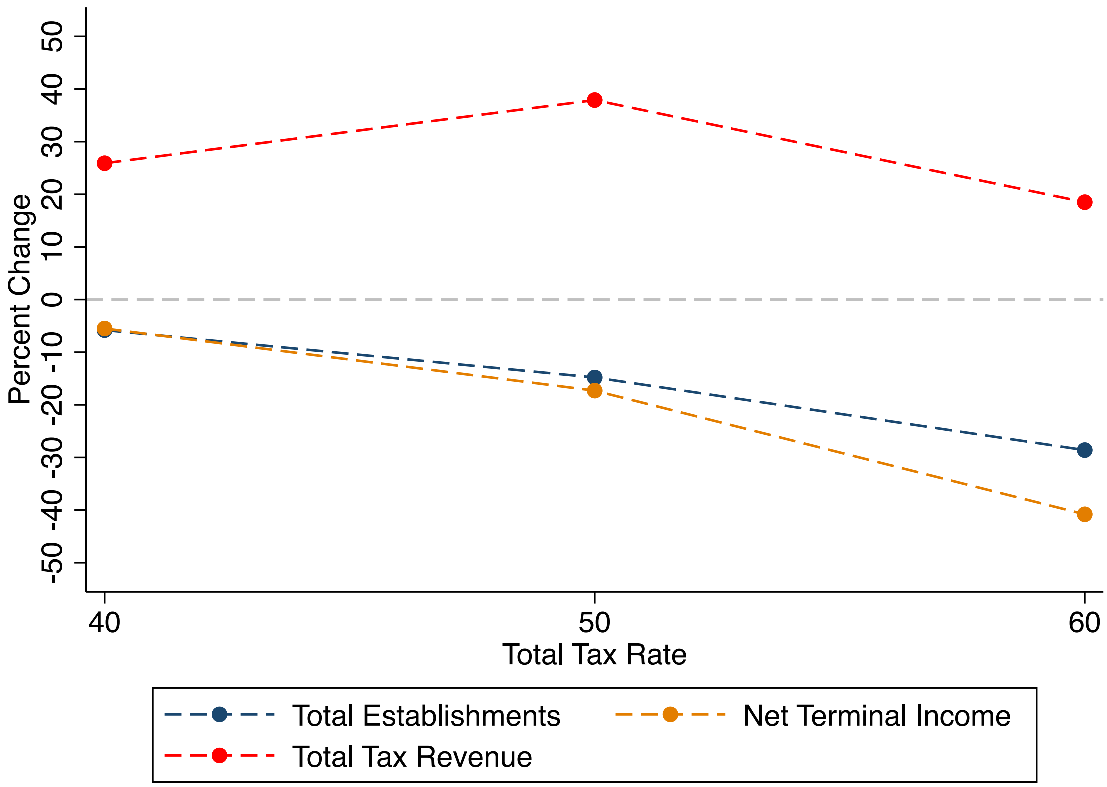


Figure 4.5: Simulated Effects of Tax Hikes on Revenue and Industry Scale

Notes: Figure 4.5 displays percent changes of total VGT establishments, total net terminal income, and total tax revenue from tax hikes relative to simulated data. *Source:* Authors' calculations based on Illinois Gaming Board data.

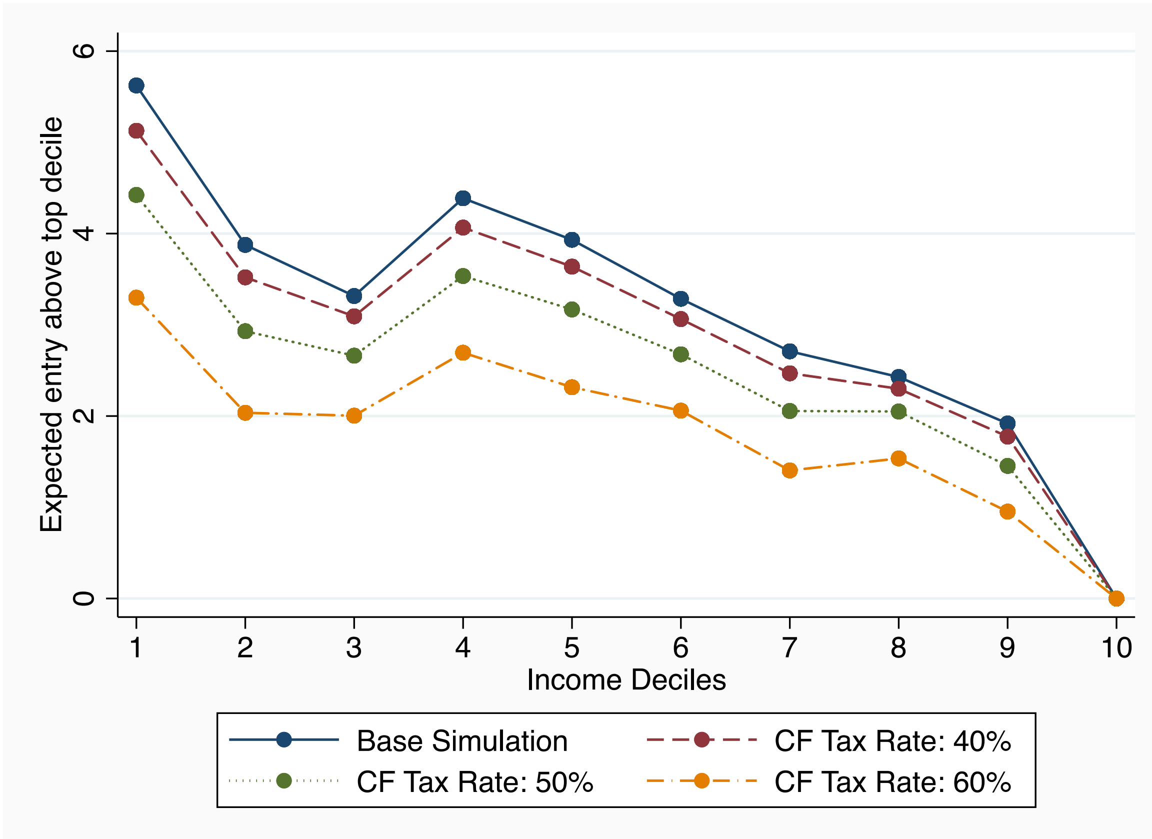


Figure 4.6: Simulated Distribution of VGT Entry by Income Decile

Notes: Figure 4.6 presents the distribution of VGT entry across the ZCTA income distribution. This figure regresses the average number of establishments resulting from simulated tax hikes in each ZCTA on controls for population and income decile indicators. Coefficients on these decile terms are then plotted for each simulation, with each coefficient describing the expected number of establishments relative to those in the top income decile. *Source:* Authors' calculations based on Illinois Gaming Board data.

Table 4.1: Effects of VGT Legalization on Municipal Finances: Difference-in-Differences Estimation

	(1)	(2)	(3)	(4)	(5)
	Panel A: Log Tax Revenue				
VGTLegal	0.0256*	0.0262*	0.0261*	0.0266**	0.0301**
	(0.0134)	(0.0135)	(0.0134)	(0.0135)	(0.0140)
	[0.0565]	[0.0522]	[0.0524]	[0.0482]	[0.0318]
	Panel B: Log Total Expenditures				
VGTLegal	0.0196*	0.0187*	0.0196*	0.0190*	0.0162
	(0.0106)	(0.0110)	(0.0106)	(0.0109)	(0.0108)
	[0.0660]	[0.0883]	[0.0640]	[0.0810]	[0.1360]
	Panel C: Log Net-of-VGT Tax Revenue				
VGTLegal	0.00876	0.00936	0.00918	0.00978	0.0128
	(0.0135)	(0.0135)	(0.0135)	(0.0135)	(0.0140)
	[0.5150]	[0.4880]	[0.4950]	[0.4690]	[0.3620]
Year × Population FE	✓	✓	✓	✓	
Year × Cash FE		✓		✓	
Year × FundMix FE			✓	✓	
Year × Population × Cash FE					✓
N	13174	13174	13174	13174	13008

Notes: Table 4.1 displays difference-in-differences estimates of the effects of municipal-level legalization of VGTs using the estimator of Borusyak et al. [2022]. p-values are presented in brackets. All specifications include municipality-level fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on Illinois Gaming Board and Illinois Comptroller Office data.

Table 4.2: Total and Exiting Licenses by Establishment Type

Total Licenses	Count	Pct.	**Exiting Licenses**	Count	Pct.
Restaurant or Bar	5944	62.18	Restaurant or Bar	1849	71.86
Gaming Parlor	1047	10.95	Unlisted	208	8.12
Gas Station	355	3.71	Gaming Parlor	114	4.43
Veterans Establishment	351	3.67	Hotel	73	2.84
Truck Stop	255	2.67	Country Club	55	2.14
Hotel	251	2.63	Bowling Alley	50	1.94
Liquor/Tobacco Store	251	2.63	Liquor/Tobacco Store	38	1.48
Bowling Alley	226	2.36	Miscellaneous	32	1.24
Unlisted	213	2.23	Gas Station	27	1.05
Fraternal Establishment	206	2.15	Veterans Establishment	27	1.05
Country Club	172	1.80	Fraternal Establishment	25	0.97
Miscellaneous	94	0.98	Athletic Club	21	0.82
Athletic Club	58	0.61	Event Center	20	0.78
Convenience Store	50	0.52	Convenience Store	14	0.54
Event Center	43	0.45	Grocery Store	8	0.31
Grocery Store	30	0.31	Truck Stop	7	0.27
Boat Club	14	0.15	Boat Club	4	0.16
Total	9560	100.00	Total	2573	100.00

Table 4.3: Establishment-Month Summary Statistics

	Mean	SD	Min	Max
VGT Count	4.33	0.94	0	5
Funds In	$64,597	$59,268	$0	$689,538
Funds Out	$47,762	$44,205	$0	$521,529
Net Terminal Income	$16,835	$15,697	-$5,340	$178,334
Establishment Profit	$5,892	$5,494	-$1,869	$62,417
Tax Revenue	$5,050	$4,709	-$1,602	$53,500
Observations	393917			

Net terminal income is the difference between "funds in," the amount paid in to VGTs, and "funds out," the amount paid out as winnings. Establishment profit and tax revenue follow from the 35%/30% split specified by the VGA. All amounts expressed in 2017 dollars.

Table 4.4: Monthly Net Terminal Income by Establishment Type

	Count	Mean	SD
Truck Stop	12322	$43,311	$26,314
Gaming Parlor	36309	$38,384	$19,297
Gas Station	8589	$23,833	$19,577
Liquor Store	8912	$18,422	$12,776
Bowling Alley	12113	$15,418	$13,320
Hotel	11988	$14,678	$11,653
Convenience Store	1467	$14,130	$12,229
Restaurant or Bar	247254	$13,730	$11,182
Veterans Establishment	21611	$12,655	$9,067
Grocery Store	661	$11,769	$12,679
Unlisted	7054	$12,273	$11,396
Fraternal Establishment	12241	$10,129	$7,186
Athletic Club	2137	$9,206	$8,492
Event Center	1352	$9,020	$8,305
Miscellaneous	3325	$7,880	$7,455
Boat Club	722	$5,745	$4,853
Country Club	5862	$5,303	$6,501
Total	393919	$16,835	$15,697

Table 4.5: VGT Selection on Demographics: Extensive Margin, 2017

	(1)	(2)	(3)
log(Income)	-0.244***	-0.158***	-0.157***
	(0.0366)	(0.0537)	(0.0551)
log(SNAP HHs)		0.0279	0.0277
		(0.0218)	(0.0219)
Unemp. Rate			0.000282
			(0.00295)
Constant	2.492***	1.762***	1.746***
	(0.384)	(0.522)	(0.546)
N	1408	1366	1366
R-Squared	0.152	0.121	0.121

Dependent variable is indicator for whether any VGT locations operated in the ZCTA in 2017. All specifications include total and black populations as controls.

Table 4.6: VGT Selection on Demographics: Intensive Margin, 2017

	(1)	(2)	(3)	(4)	(5)
log(Income)	-0.933***	-0.769***	-0.816***	-0.887***	-0.701
	(0.135)	(0.146)	(0.142)	(0.204)	(0.652)
log(SNAP HHs)		0.0953**	0.109**	0.102**	0.230
		(0.0457)	(0.0457)	(0.0495)	(0.272)
Unemp. Rate			-0.0135**	-0.00843	-0.00786
			(0.00664)	(0.00746)	(0.0338)
Constant	6.201***	4.708***	5.355***	5.584**	4.754
	(1.577)	(1.619)	(1.567)	(2.192)	(6.050)
County FE	no	no	no	yes	no
Municipality FE	no	no	no	no	yes
N	1003	999	999	999	999
R-Squared	0.650	0.652	0.653	0.707	0.944
Mean VGTs	7.26				

Standard errors clustered on county in parentheses. All specifications include total and black populations as controls.

Table 4.7: VGT Selection on Demographics by Establishment Type: 2017

	(1) Rest.	(2) Rest.	(3) Parlors	(4) Parlors	(5) Chains	(6) Chains	(7) Marts	(8) Marts
log(Income)	-0.516***	-0.664***	-0.238**	-0.376***	-0.0412	-0.113**	-0.246***	-0.0990
	(0.133)	(0.168)	(0.105)	(0.113)	(0.0554)	(0.0517)	(0.0676)	(0.0732)
log(Population)	0.301***	0.301***	0.174***	0.158***	0.0875***	0.0588**	0.0229	0.0580**
	(0.0455)	(0.0497)	(0.0421)	(0.0405)	(0.0255)	(0.0230)	(0.0232)	(0.0251)
log(SNAP HHs)	0.0831**	0.110**	0.0725**	0.0768**	0.0274	0.0438**	0.0620***	0.0564**
	(0.0409)	(0.0435)	(0.0365)	(0.0340)	(0.0197)	(0.0180)	(0.0214)	(0.0262)
Unemp. Rate	-0.0118*	-0.0146**	-0.00400	-0.0107**	-0.00378	-0.00877***	-0.00581*	0.000980
	(0.00626)	(0.00701)	(0.00400)	(0.00489)	(0.00241)	(0.00319)	(0.00322)	(0.00373)
Constant	4.127***	5.456***	1.168	2.730**	-0.250	0.605	2.469***	0.569
	(1.401)	(1.740)	(0.994)	(1.142)	(0.533)	(0.491)	(0.707)	(0.758)
County FE	no	yes	no	yes	no	yes	no	yes
N	999	999	999	999	999	999	999	999
R-Squared	0.417	0.516	0.346	0.424	0.257	0.337	0.107	0.248
Mean VGTs	2.91		0.56		0.19		0.28	

Standard errors clustered on county in parentheses. All specifications include black populations as controls.

Table 4.8: Effects of Establishment Entry on Log ZCTA Gambling Revenue

	(1)	(2)	(3)	(4)
# Restaurants (ZCTA)	1.083***			0.995***
	(0.142)			(0.143)
(# Restaurants)^2	-0.146***			-0.140***
	(0.0247)			(0.0261)
# Gaming Parlors (ZCTA)		0.660***		1.101***
		(0.135)		(0.154)
(# Gaming Parlors)^2		-0.196***		-0.428***
		(0.0420)		(0.0824)
# Marts (ZCTA)			0.654*	1.229***
			(0.377)	(0.306)
(# Marts)^2			-0.255*	-0.781**
			(0.143)	(0.311)
Constant	11.11***	12.23***	10.83***	11.26***
	(0.0503)	(0.0310)	(0.0497)	(0.0514)
N	46913	13172	7213	48541
R-Squared	0.902	0.828	0.839	0.914

Standard errors clustered at the ZCTA level in parentheses. All specifications include ZCTA- and period-level fixed effects.

Table 4.9: Entry Semi-Elasticities by Type

	Incumbent		
	Restaurant	Parlor	Mart
Restaurant	-0.00766***	-0.00447	-0.0108***
	(0.00105)	(0.00232)	(0.00169)
Parlor	-0.0265***	-0.0344***	-0.0434***
	(0.00126)	(0.00248)	(0.00196)
Other	-0.00549***	-0.0262***	-0.0175***
	(0.00173)	(0.00395)	(0.00243)
N	272835		
R-Squared	0.808		

Standard errors clustered at the establishment level in parentheses. Rows denote establishment type of entrant.

Table 4.10: MLE Estimates of Entry Parameters

δ	$\omega_{restaurant}$	ω_{parlor}	ω_{other}	λ_{inc}	λ_{pop}	$I_{permanent}$
0.000481***	2.074***	0.219*	2.620***	0.0314***	0.000777	-1.488***
(0.0001)	(0.046)	(0.114)	(0.069)	(0.00148)	(0.00135)	(0.104)

Chapter 5

Conclusions

The preceding chapters investigate three questions in economics where firm behavior plays an important role. First, Chapter 2 investigates how the personal labor market experiences of owner-managers influence firm pay policy, finding that owners with greater exposure to elevated local unemployment rates offer higher pay on average. I do so using a unique data linkage that improves upon existing methods used to study how entrepreneurs acquire firms. Using several complementary designs, I find that these effects on firm pay policy reflected several different potential motives, such as worker retention, more effective worker recruitment, and insurance provision for workers. Future work can use this data linkage to improve our understanding of both how workers select into firm ownership and how these forces shape pay and hiring policy.

Chapter 3 studies the effects of tax subsidies that lower the cost of capital. The question of whether policies that subsidize investment in physical capital help or hurt workers is pervasive in discussions about equitable and efficient fiscal policy. The chapter combines tax policy variation from bonus depreciation with confidential data to gain empirical leverage on this debate. We show that both capital and labor increased in response to the policy, and use a structural model to clarify the economic forces underlying this results.

Chapter 4 evaluates the effects of gambling taxation on local government spending and revenue and the scale and distribution of gambling activity. Motivated by evidence that establishment-level adoption decisions drive selection into lower-income communities, my equilibrium model of competition and entry demonstrates that marginal entrants are concentrated in these communities, with counterfactual simulations showing that substantial tax hikes can increase tax revenue while also reducing the scale and regressivity of gambling activity. At the same time, municipality-level analysis provides mixed evidence on whether appropriating revenue to local governments can stimulate local spending that may offset the social costs of gambling. Together, these results imply that state governments should evaluate alternative ways to target public spending that gambling tax revenue funds. Future work can better inform these policy questions by estimating the effects of gambling activity on potential benefits, such as local business stimulation, and potential social costs, such as crime or gambling addiction.

Appendix A

Appendix for Chapter 3

This appendix includes several sections of supplemental information. Appendix A contains definitions for all the variables used in the paper. Appendix C describes the variation in the net present value of depreciation deductions, z_0, across time and industries. We discuss the choice of standard error calculations in Appendix D. We compare our results on investment with those of Zwick and Mahon [2017] in Appendix E, and we present additional investment responses to bonus in Appendix F. Appendix G shows employment results by the task content of occupations using Census and ACS data. Appendix H provides additional employment results using QWI data. Appendix I places our results in the context of aggregate and long-run trends in the manufacturing industry. Appendix J decomposes the wage changes into compositional changes and other factors. Appendix K derives the complete model and presents extensions that add financing constraints and cash flow effects and cash flow effects. Finally, Appendix L discusses additional variations and extensions of the structural model.

A Variable Definitions

Variable Name	Description
Bonus	Indicator that the NPV of investment in industry j is less than 0.875. *Source:* Zwick and Mahon [2017].
Post	Post-2001 indicator.
Log Investment	Natural logarithm of investment in plus 1. Investment is defined as the total new and used machinery and equipment expenditures in \$1,000s by plant i in year t. *Source:* ASM/CM.
Log Total Capital	Natural logarithm of total capital plus 1. Total capital is defined as the value of total capital assets in \$1,000s of plant i in year t. Data is available in CM years 1997, 2002, 2007, and 2012. Interim years imputed using investment variable defined above. *Source:* ASM/CM.
IHS Investment	Inverse hyperbolic sine function of investment, as defined above, by plant i in year t. *Source:* ASM/CM.
$\Delta\text{PPENT}_t/\text{PPENT}_{1997-2001}$	Investment as Share of Pre-Period Capital. Pre-period capital defined as the average total capital, as defined above, in the 1997-2001 period. Investment in machinery and equipment as defined above by plant i in year t. *Source:* ASM/CM.

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Table A.1 – *Continued from previous page*

Variable	Description
Log Capital Equipment Stock	Natural logarithm of total capital equipment plus 1. Total capital equipment is defined as the value of total capital machinery and equipment assets of plant t in year j. Data is available in CM years 1997, 2002, 2007, and 2012. Interim years imputed using investment variable defined above. *Source:* ASM/CM and Cunningham et al. [2020].
Log Capital Structures Stock	Natural logarithm of total capital structures plus 1. Total capital equipment is defined as the value of total capital structures assets in $1,000s of plant i in year t. Data is available in CM years 1997, 2002, 2007, and 2012. Interim years imputed using investment variable defined above. *Source:* ASM/CM and Cunningham et al. [2020].
Log Employment	Natural logarithm of total employment plus 1. Total employment is defined as the total number of non-leased employees at plant i in year t. *Source:* ASM/CM.
Log Production Employment	Natural logarithm of production employment plus 1. Production employment is defined as the total number of non-leased employees working in production at plant i in year t. *Source:* ASM/CM.

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Table A.1 – *Continued from previous page*

Variable	Description
Log Non-production Employment	Natural logarithm of non-production employment plus 1. Production employment is defined as the difference between total employment and production employment, as defined above, at plant i in year t. *Source:* ASM/CM.
Log Mean Earnings per Worker	Natural log of average annual earnings plus 1. Average annual earnings defined as total payroll divided by total employment at plant i in year t. *Source:* ASM/CM.
Log Total Value of Shipments	Natural log of revenue plus 1. Revenue defined as the total value of shipments from plant i in year t. *Source:* ASM/CM.
TFP	Total Factor Productivity of plant i in year t. TFP calculated using a factor share approach following Criscuolo et al. [2019]: $TFP_{it} = \tau_{it} - \bar{\tau}_{jt}$ where $\tau_{it} = r_{it} - \bar{S}_{Mjt} m_{it} - \bar{S}_{Ljt} l_{it} - (1 - \bar{S}_{mjt} - \bar{S}_{Ljt}) k_{it}$. Here, r_{it} is log(total value of shipments), m_{it} is log(materials), l_{it} is log(total employment), k_{it} is log(total capital), and $\bar{S}$ terms denote average cost shares for the respective inputs in four-digit NAICS industry j. Finally, $\bar{\tau}_{jt}$ is the average value of τ_{it} in the three-digit NAICS sector. *Source:* ASM/CM and Cunningham et al. [2020].

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Table A.1 – *Continued from previous page*

Variable	**Description**
RTW	Indicator that plant i operated in a state with Right-to-Work laws in 2001. *Source:* Valletta and Freeman [1988].
Unionization	Indicator that for plant i, over 60% of total employment was unionized in 2005. *Source:* MOPS.
Log HHI	Natural logarithm of local labor market Herfindahl-Hirschmann Index (HHI) in 2001. Local labor market defined as the three-digit NAICS-commuting zone in which plant i operates in 2001. For local labor market m, HHI $= 10,000 \sum_{f \in F_t(m)} \left(\frac{l_{ft}}{L_{F(m)t}}\right)^2$, where l_{ft} is employment of firm f, $F_t(m)$ is the set of all firms operating in labor market m in time t, and $L_{F(m)t}$ is total employment in labor market m. *Source:* LBD.
Skill Intensity	Skill intensity of plant i defined as share of total employment classified as non-production employment in 2001. Skill intensity fixed effects defined as quartiles of skill intensity across plants in estimating sample. *Source:* ASM/CM.

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Table A.1 – *Continued from previous page*

Variable	**Description**
Capital Intensity	Capital intensity of plant i defined as total capital assets divided by employment in 2001. Capital intensity fixed effects defined as quartiles of capital intensity across plants in estimating sample. *Source:* ASM/CM.
ADH Exposure	ADH exposure for plant i defined as the change in exposure to Chinese import competition at the six-digit NAICS industry level from 2000 to 2007. *Source:* Acemoglu et al. [2016].
AR Robotization	AR Robotization for plant i defined as the change in robotization at the three-digit NAICS sector level from 1993 to 2007. *Source:* Acemoglu and Restrepo [2020].
Plant Size Fixed Effect	Plant size of plant i defined as total capital assets in year 2001. Plant size fixed effects defined as quartiles of plant size across plants in estimating sample. *Source:* ASM/CM.
Firm Size Fixed Effect	Firm size of plant i defined as total employment of firm to which plant is attached in year 2001. Firm Size fixed effects defined as quartiles of firm size across plants in estimating sample. *Source:* ASM/CM.

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Table A.1 – *Continued from previous page*

Variable	**Description**
TFP Fixed Effects	TFP of plant i defined above. TFP fixed effects defined as quartiles of TFP in 2001 across plants in estimating sample. *Source:* ASM/CM.
Log Employment, QWI	Natural logarithm of total employment in each four-digit NAICS industry × state × year. *Source:* QWI.
Log Mean Earnings, QWI	Natural logarithm of mean earnings in each four-digit NAICS industry × state × year. *Source:* QWI.
Fraction of Employees with High School Education or Less	Fraction of employees in each four-digit NAICS industry × state × year that report having a high school education or less. Reported education is observed for approximately one-seventh of the sample that completed the census long-form and is imputed for all other workers. *Source:* QWI.
Fraction of Employees 35 Years Old or Younger	Fraction of employees in each four-digit NAICS industry × state × year that are 35 years old or younger. *Source:* QWI.
Fraction of Female Employees	Fraction of employees in each four-digit NAICS industry × state × year that are female. *Source:* QWI.
Fraction of Non-White Employees	Fraction of employees in each four-digit NAICS industry × state × year with a reported race other than White. *Source:* QWI.

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Table A.1 – *Continued from previous page*

Variable	**Description**
Fraction of Hispanic or Latino Employees	Fraction of employees in each four-digit NAICS industry $\times$ state $\times$ year whose reported ethnicity is Hispanic or Latino. *Source:* QWI.
Fraction of Black Employees	Fraction of employment in each four-digit NAICS industry $\times$ state $\times$ year whose reported race is Black. *Source:* QWI.
Log Employment, Small Firms	Natural logarithm of employment in firms with 50 or fewer employees in each four-digit NAICS industry $\times$ state $\times$ year. *Source:* QWI.
Log Employment, Young Firms	Natural logarithm of employment in firms that are five or fewer years old in each four-digit NAICS industry $\times$ state $\times$ year. *Source:* QWI.
Log Employment, NBER-CES	Natural logarithm of total employment in each four-digit NAICS industry $\times$ year. *Source:* NBER and CES.
Log Investment, NBER-CES	Natural logarithm of total investment in each four-digit NAICS industry $\times$ year. *Source:* NBER and CES.
Log Capital Stock, NBER-CES	Natural logarithm of total capital stock in each four-digit NAICS industry $\times$ year. *Source:* NBER and CES.

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Variable	**Description**
ICT Asset Shares	Share of fixed assets in information and communication technology at the three- and four-digit NAICS industry level. Shares calculated as average over 1997-2001 period. *Source:* BEA.
Capital Producer Share	Share of output in 2001 that was used as investment in equipment capital from BEA Commodities by Industries - Summary, data item F02E divided by item T019. *Source:* BEA.
Cost of External Capital	Average cost of borrowing, defined as interest divided by debt, for publicly traded firms for each four-digit NAICS industry averaged over the 1997-2001 period. *Source:* Compustat.
Log Employment, Decennial Census and American Community Survey	Natural logarithm of total employment in each four-digit NAICS industry $\times$ state $\times$ year. *Source:* 1990/2000 Censuses and 2005/2010 ACS.

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Table A.1 – *Continued from previous page*

Variable	**Description**
Occupation-Task Definitions	Occupations are classified into four broad categories: (1) professional, (2) administrative, (3) production, and (4) services occupations. Professional occupations specialize in non-routine, cognitive tasks. Administrative occupations specialize in routine, non-cognitive tasks. Production occupations specialize in routine manual tasks. Services occupations specialize in non-routine manual tasks. *Source:* Acemoglu and Autor [2011]
Tech Industries	Industries with more than 25% of employment in technology oriented occupations. These include Aerospace Products and Parts (NAICS 3364), Other Chemicals (3259), Basic chemicals (3251), Pharmaceuticals (3254), Electrical Equipment and Components (3359), Audio and Video Equipment (3343), Navigational and Control Instruments (3345), Semiconductor and Component Manufacturing (3344), Communications Equipment Manufacturing (3342), Computer and Peripheral Equipment (3341). *Source:* Heckler [2005]
ICT z score	Normalized share of workers engaging in tasks involving ICT during the period 2002–2016. *Source:* Gallipoli and Makridis [2018].

B A Brief History of Depreciation Schedules

Brazell et al. [1989] and Brazell and Mackie III [2000] discuss the historical processes that lead to modern depreciation schedules in the US, which we briefly summarize in this section.

Depreciation deductions were first discussed by the Treasury in 1920 in *Bulletin F: Depreciation and Obsolescence*, which encouraged tax payers to choose depreciation rates based on their own experience and restricted the depreciation method to straight-line. This allowed depreciation schedules to vary by the specific *facts and circumstances* of individual taxpayers. By 1931, there was more discussion about how to raise revenue, and concern that depreciation deductions were not being claimed fairly across taxpayers, so the Treasury put out non-binding guidance for many different types of assets through Treasury Decision 4422. This new directive required taxpayers to file their own schedules for depreciation with the burden of justifying the reasonableness of depreciation placed on the taxpayer.

By 1960, there was a much larger concern that administrating different depreciation schedules for different taxpayers was leading to many disputes, so the Treasury started putting together studies of depreciation rates that firms were choosing according to their own facts and circumstances. In 1962, they published new guidelines built off of a series of such studies, Revenue Procedure 62-21 or *Depreciation Guidelines and Rules*, which started giving ranges based on several internal and external studies of asset lives being claimed by taxpayers under the earlier rules. Procedure 62-21 also introduced the Reserve Ratio Test (RRT), which was intended to check that the new guidelines were being appropriately used by all taxpayers by comparing accumulated depreciation to the cost basis of assets within a class. In 1965, the IRS estimated that almost 90% of filers would fail the RRT, so it was placed on a moratorium and never applied. The survey information from the early 1960s was later used in the Asset Depreciation Range (ADR) system started in 1971, which ended the potential use of the RRT.

In 1981, the US moved to the Accelerated Cost Recovery System (ACRS), which was intended to dramatically simplify the administration of income taxes and management of depreciation schedules by putting all assets into a few loose categories that were also generous enough that most filers would be better off. Put into the words of Brazell et al. [1989], "[ACRS] differed from previous changes, however, since the cost recovery periods were not intended to reflect actual useful lives, or even some percentage of the useful lives." MACRS only made a series of minor changes to ACRS, but did make categories a little bit more granular and closer to the class lives of ADR not updated for the 20+ years that had passed since the original Treasury studies based on discretionary depreciation schedules. Finally, the Technical and Miscellaneous Corrections Act of 1988 revoked the authority of the Treasury to adjust class lives, so the identifying variation in class life differences across industries dates to 1960 and was not been able to be modified easily since 1988.

C Context for the Present Value of Depreciation Deductions

The tax subsidy to long-duration capital investment during our sample period comes from both bonus depreciation and §179 incentives. The original round of 30% bonus depreciation applied to equipment installed after September 11, 2001 and was intended to be temporary. Bonus was increased to 50% in mid 2003. The policy was phased out beginning on January 1, 2005, but many large investments in long-lived assets qualified through January 1, 2006. In response to the 2008 financial crisis, bonus was reinstated at 50% and has continued with temporary extensions through the Tax Cuts and Jobs Act of 2017, which increased the policy to 100% bonus depreciation, also known as full expensing. §179 expensing began with a limit of $24,000 in 2001 increasing to $100,000 in 2003, $250,000 in 2008, and $500,000 in 2010. The §179 incentives are phased out dollar for dollar starting at four times the investment limit.

We display the time variation in how these incentives affected two different investments — one for $400,000 and one for $1,000,000 — and calculate the effective bonus rate in Panel (A) of Figure 3.1. First, §179 allows for investments under certain thresholds to be immediately deducted or expensed, which makes the present value of deductions for $1 of investments equal to one. After claiming any relevant §179 incentives, a firm can claim an additional "bonus" percentage of the remaining investment cost that wasn't covered, which is 38% on average during the sample period. For instance in 2004, the §179 threshold was $100,000 phasing out at $400,000 and the bonus rate was 50%. For a $400,000 investment, one first claims $100,000 of §179 incentives and then claims 50% bonus for the remainder of the investment cost. This leads to $250,000 of investment immediately deducted $(100,000 + 0.5 \times (400,000 - 100,000))$, which is equivalent to 62.5% bonus. Further, sometimes bonus is larger for larger investments such as the extension of 50% bonus for investments larger than one million dollar in 2005. The accelerated depreciation policies are mostly driven by §179 for smaller investments and by bonus for larger investments.

We rely on Zwick and Mahon [2017] replication data to measure which plants are most impacted by accelerated depreciation. They provide estimates of the net present value of depreciation deductions for non-bonus years derived from IRS Form 4562. The data provide variation at the 4-digit NAICS industry level. We plot the replication data in a histogram in Panel (A) of Figure A1c for manufacturing industries (NAICS 3111 to 3399). We find there is a structural break around 0.875, the scale of which is a function of several modeling assumptions regarding the appropriate discount factors. We use this structural break as the threshold to be considered treated by bonus. Plants with a NPV of depreciation deductions below the threshold are considered long duration industries and we count those industries as relatively treated and the rest as controls.

IRS SOI sector-level corporation depreciation data are used to calculate the NPV of depreciation deductions at the IRS sector level. The total sum of assets placed in service during the previous tax year for each sector and for each depreciation schedule is available in Table 13 of the "Corporation Complete Report" through IRS [2017]. As further evidence that firms are relatively unable to adjust the tax-duration of their investment, we plot the aggregate net present value of depreciation deductions for $1 of equipment investment by IRS sectors, which don't have perfect NAICS analogs. We show the results of these calculations in Panel (B) of Figure A1c. The longest duration businesses, the bottom tercile of firms weighted by equipment investment, always have z_0 calculations that are around 10%-15% lower than the medium and short duration firms. We show that the levels of these differences in IRS SOI data are stable from 2000 to 2011 before accounting for bonus depreciation incentives.

D Standard Error Clustering

Throughout the paper, we cluster standard errors at the level of treatment variation [e.g., Bertrand et al., 2004, Cameron and Miller, 2015]. To define this level, consider the impact of bonus on a firm's investment decision. The firm sets the marginal product of capital $f'(K)$ equal to the cost of capital as follows

$$f'(K) = r + \delta + \frac{1 - \tau z}{1 - \tau},$$

where r is the interest rate, δ is the economic rate of depreciation, and τ is the firm's combined corporate income tax rate. As we discuss in Section 3.1, the policy has differential benefits across industries since

$$z = b + (1 - b) \times z_0,$$

where z_0 is industry-specific. Additionally, the tax benefit from bonus depreciation depends on τ, which is a function of state and federal tax policies. Specifically,

$$\tau = \tau_f \times (1 - \tau_s) + \tau_s \times (1 - \tau_f \times \mathbb{I}[D_s]),$$

where τ_f and τ_s are the federal and state corporate income tax rates, respectively. The first term accounts for the fact that corporations are able to deduct state taxes from federal taxes. The second term in this equation captures the fact that some states allow for federal taxes to be deducted from state taxes, an event we denote by $\mathbb{I}[D_s]$. In this case, we assume that states allow for bonus depreciation at the state level and rely on the same tax base. Additional interactions between state tax systems and bonus depreciation arise when states depart from using the federal tax base or when they additionally provide further depreciation incentives [see, e.g. Ohrn, 2019, Suárez Serrato and Zidar, 2018]

The equations above clarify that the benefit from bonus depends on interactions between the federal bonus policy and federal and state tax systems. This motivates us to cluster standard errors at the industry-state level. Moreover, as we show in

Table A2, our primary investment, capital, employment, earnings, and productivity results have similar levels of statistical significance when we instead cluster standard errors at the industry level. Finally, we note that these levels of clustering are more conservative than those of previous papers that cluster at the firm level [e.g., Zwick and Mahon, 2017].

E Comparison to Investment Effects from Zwick and Mahon [2017]

This section compares our estimated effects of bonus on log investment with those reported by Zwick and Mahon [2017, ZM, henceforth]. ZM discuss their identifying variation in their §III.B on page 228. In a direct analogue to the exercise in this paper, this section of ZM compares investment outcomes in the 30% of firms in industries with the longest duration investment to the 30% of firms in the shortest duration of investment. Below we describe how we compare our results to those of ZM.

In Panels (A) and (B) of their Figure 1, ZM report yearly averages of log investment for both treated and control firms. We obtain the numerical values of these data points using the program WebPlotDigitizer (see https://apps.automeris.io/wpd/). Columns (1)–(4) of Table A1 report the extracted data. This table then creates a series that mirrors our event study estimates. To do so, we compute the difference between the average values of treated and control groups by year. We then normalize this difference to be zero in the year 2000 and we combine the data from the two times periods in ZM by making the assumption that differences in investment between these two groups are constant between 2004 and 2005. Table A1 details these operations.

Figure A2 plots the series in column (7) of Table A1 along with our estimates from the additional controls series in Panel (A) of Figure 3.2.[1] Similar to our results, ZM show that investment at treated firms increases immediately after the implementation of the policy. In the 2002-04 period and among those who had some positive investment, ZM show that treated firms had investment that was 11.8% higher than control firms. This corresponds to our event study estimates for the same time period which show an average increase in investment of 10.1%. This figure shows that we are not able to reject the hypothesis that the estimates in the orange line differ from those in the blue line for most years.

Overall, Figure A2 shows that our estimated effects of bonus on log investment are quite comparable with those reported by ZM. The similarity in these results is remarkable for several reasons. First, while we use census and survey data, ZM rely on data from corporate tax returns. Second, while we focus on plants in the manufacturing sector, ZM study data on firms in the overall economy. Third, while our results focus on a balanced panel that includes mostly larger plants, ZM study a non-balanced panel that includes many small firms. Finally, while our estimates only rely on the controls mentioned in Section 3.3, ZM produce the estimates in their Figure 1 using a two step process that first re-weights observations to address sampling changes over time and then residualizes the effects of a host of variables, including splines in assets, sales, profit margin, and age. Despite all these differences, Figure A2 shows that our investment results have a comparable magnitude to those of ZM.

[1] We normalize years to the survey year in ASM which is derived from a survey during the year while the tax data are retrospective from the following year. This means we plot 2000 in ZM as equivalent to 2001 in the ASM data.

F Additional Investment and Capital Results

This section shows two event studies for different constructions of the investment outcome variable as discussed in Section 3.4. Estimates for the first additional outcome, the inverse hyperbolic sine of investment ($\ln(x + \sqrt{x^2 + 1})$), are shown in Panel (A) of Figure A3. This outcome allows both the intensive and extensive margins to respond to bonus and has the same scale for interpretation as the natural log. The estimates are almost identical to the primary variable definition of log investment, which suggests the extensive margin is is not behaving differently than the intensive margin.

The third construction of the investment outcome is capital expenditure divided by pre-period capital. The interpretation of these coefficients are a change in investment as a share of original assets. The event study coefficients are shown in Panel (B) of Figure A3. The time patterns and increases are qualitatively similar to the other definitions. Difference-in-differences estimates for both of these variable definitions are shown in Panels (B) and (C) of Table 3.1.

As a final check to our investment and capital measures, we turn to the Quarterly Survey of Plant Capacity Utilization to test whether effective capital is changing in a way that may be different than reflected in the primary capital and investment measures. Capacity utilization (item 2, part C, also called "full production ratio" or FPR) is the share of revenue that was earned by a plant (item 2, part A) divided by counterfactual revenue as if operating at full capacity (item 2, part B) measured in the fourth quarter of each year, so we omit the 2000 treatment variable for normalization. We use the capacity utilization published from 1997-2011 to test whether industries more treated by bonus use more or less of their capital after increasing their investment in response to bonus using our long-difference design. 2007 is imputed as the average of 2006 and 2008 since we do not have data for that year. The results of this exercise are shown in Figure A4. Average capacity utilization is 67.6% with the 25^{th} and 75^{th} percentiles of 62.1% and 73.0% respectively. By 2011, we estimate plants in bonus-treated industries use 3.9pp. less of their total capacity, although the point estimate is not statistically significant. If anything, we find that our measures of capital could slightly overstate how much capital equipment increases relative to labor as an input in production processes.

G Additional Employment Effects by Task Content of Jobs

This section discusses the effects of bonus depreciation on employment for workers in various occupations defined as routine/non-routine and cognitive/non-cognitive as in Acemoglu and Autor [2011]. We also show how these results change for workers in different demographic groups.

To perform this analysis, we map occupation data from the U.S. Census and American Community Survey (ACS) to the broad task classifications of Acemoglu and Autor [2011]. They classify Census occupations into four broad categories: (1) professional, (2) administrative, (3) production, and (4) services occupations. Professional occupations are defined as managerial, professional, and technical occupations that specialize in non-routine, cognitive tasks. Administrative occupations are defined as sales, clerical and administrative support that specialize in routine, non-cognitive tasks. Production occupations are defined as production, craft, repair and operative occupations that specialize in routine, manual tasks. Services occupations specialize in non-routine manual tasks.

We construct counts of employment in each of these four categories at the state-by-industry level using microdata from the IPUMS samples of the 1990 and 2000

Censuses, the 2005 ACS, and the 2010 ACS five-year estimates. Our sample comprises adults between the ages of 18 and 64 that are not institutionalized and are employed in manufacturing industries. We drop imputed values for employment status. We define industries by their 1990 Census industry codes in order to maintain a consistent sample over time. Because exposure to bonus is defined at the 4-digit NAICS industry code, we utilize NAICS-Census code industry crosswalks to assign treatment status to Census industry codes. We exclude Census industries that cannot be mapped to a unique treatment status based on this crosswalk.

Figure A10 presents estimates from event study regressions that show the effect of bonus depreciation on workers in production occupations and routine occupations (production plus administrative) using data from the years 1990, 2000, 2005, and 2010. Estimates are weighted by employment in 2000 and standard errors are clustered at the industry-state level. The event study shows that bonus depreciation has large effects on production labor and on routine labor. The effects on production labor reinforce the conclusion that the effects of bonus depreciation are concentrated among those workers directly interacting with production machinery. The similar time pattern for routine work also suggests bonus depreciation increases demand for administrative labor.

Table A5 presents coefficients describing the effect of bonus depreciation on employment from 2000 to 2010 for groups of workers classified by the routine/non-routine, cognitive/non-cognitive, and across a number of different demographic groups. Each coefficient is taken from a different regression where the observation unit is a state-industry. All regressions include industry and state-year fixed effects, are weighted using 2000 employment, and use standard errors clustered at the state-industry-level.

The top line estimate in column (1) shows that bonus depreciation increased employment in most treated industries by 8.56% from 2000 to 2010. This estimate is close to our long-difference estimate presented in Panel A, column (6) from Table 3.3. Moving across the estimates presented in the table, we see large positive effects for routine work and smaller statistically insignificant effects on non-routine work.[2] Columns (4) through (7) show that the effect of bonus depreciation is largest for production workers, who perform manual routine tasks. The effect of bonus depreciation is also large and positive for administrative workers who perform cognitive routine tasks. Effects on professional and service workers are smaller and not statistically significant.

While bonus depreciation affects demand for all workers, column (1) also shows that the policy has outsized effects on young workers, workers with fewer years of education, female workers, Black workers, and Hispanic workers. These results reinforce the demographic analyses using QWI data presented in Section 3.4.3. Comparing the demographic subgroups results between column (1) and columns (2) and (6) suggests that the pattern of relatively larger effects of bonus depreciation on employment for traditionally disadvantaged groups is even stronger for routine and production workers.

In sum, this task-based analysis reinforces the conclusion that the effect of bonus depreciation on employment is largest for workers interacting with production machinery and engaging in manual-routine tasks. Among workers performing these types of tasks, the effect of bonus depreciation is larger for young workers, workers with fewer years of education, female workers, Black workers, and Hispanic workers.

[2] In 2000, production occupations accounted for approximately 80% of all routine employment in manufacturing.

H Additional Employment Results using QWI Data

This Appendix extends the employment results discussed in Section 3.4. In that section, we introduce state-industry level variation using QWI data to measure employment responses in settings that may not be well covered by the ASM sample that is balanced. First, the ASM sample can be tilted toward large and old plants by construction, so we use QWI state-industry variation to see whether the same trends show up in small and young firms.

We show QWI event study estimates for firms with 1-50 employees in Panel (A) of Figure A6. This sample restricts on plants being very small and aggregates up to the state level, so if a plant grows beyond 50 employees it will leave the sample and aggregate state employment in this category would decrease. This sample restriction still shows that long duration plants experienced more employment growth than short duration counterparts even selecting on being very small plants. Further, we replicate the employment analysis again restricting to plants that are in the first 5 years of operation. We find that employment in plants treated by bonus is increasing relative to untreated plants. Quarterly coefficients are shown in Panel (B) of Figure A6.

We also show extended robustness to a variety of industry level characteristics that could be correlated with the tax duration of investment. We do this using QWI data and state-industry variation instead of with ASM/CM data to limit the number of disclosures we have to make with the confidential Census data. The most important of these tests deals with our discrete definition of treatment. The variable z_0, which is defined as the PV of depreciation deductions for each dollar of investment, can be used as a continuous treatment instead of a discrete treatment. In Figure A7, we present results where we define treatment continuously as (1-z_0)τ*0.0375, which is the average treatment of accelerated depreciation due to bonus from 2002 to 2011. In

Panel (A), we show that the event study has the same sign and statistical significance as the discrete version. Panel (B) displays a binscatter of changes in employment as a function of z_0, where we see the continuous treatment does not show any effect driven by outliers, but a smooth decreasing of employment as industries enjoyed shorter depreciation schedules historically (i.e. higher z_0). Our formulation of the treatment as a discrete variable does not appear to have a material impact on our results.

Evidence presented in Panel (A) of Figure A8 suggests a similar conclusion. In Panel (A), we show how the QWI employment event study differs when we use 25th percentile and 40th percentile cutoffs to define bonus depreciation treatment. All three treatment definitions suggest large, positive effects of bonus depreciation on employment which suggest our baseline employment effects are largely unaffected by the choice of z_0 cutoff we use to define treatment.

Figure A8 presents a number of additional robustness checks. In Panel (B), we address the concern that our findings are driven by increased employment due to additional demand for capital goods rather than changes in the cost of capital due to the policy. To do so, we use the 2001 BEA Input-Output tables to identify industries that sell capital goods to other industries. These data, available as the Use of Commodities by Industries - Summary through the BEA Interactive Tables tool, describe the share of output in a given industry that is used in nonresidential private fixed investment in equipment at the 3-4 digit NAICS levels (data item F02E divided by item T019). For example, NAICS 333 covers many firms that manufacture machinery and this industry has 44.7% of output used as private fixed investment in equipment in 2001. On the other side of the spectrum, NAICS 326 businesses, those involved in manufacturing plastics and rubbers, have 0.1% of output used in fixed investment in equipment. We control for the share of output in each industry that is used in fixed investment in capital equipment and interact this control with year fixed effects. Our results in Panel (B) of A8 show the same pattern of increasing employment by 12.7% by 2011 for state-industries with the most benefit from depreciation incentives. We also test whether the final coefficient in 2011 is different with the capital good producer controls and fail to reject the null that the coefficient is the same as the baseline QWI analysis (12.9%) with a p-value of 0.89.

Next, we address the concern that the different mixes of assets and capital intensity across industries could lead to different costs of accessing external finance that requires some sort of collateral. As a proxy for the cost of external capital, we calculate the average cost of borrowing (interest divided by debt) for publicly traded firms in Compustat. We then include quintile bins of this external cost measure interacted with year fixed effects in Panel (C). Again, our results are very similar to baseline suggesting differences in the cost of external financing are not driving our results.

In Figure A9, we show that our results are not driven by growth in the use of information and communications technologies (ICT). We take two approaches. First, in Panel (A), we present additional estimates of the effect bonus depreciation on log employment controlling for two measures of ICT growth. For each control, we include tercile indicators interacted with year fixed effects. The first measure is ICT capital intensity measured as a share of capital stock in ICT goods using BEA Detailed Data for Fixed Assets and Consumer Durable Goods from 1997 to 2001. The second measure is the Gallipoli and Makridis [2018] Z-score, the normalized share of workers engaging in tasks involving ICT during the period 2002–2016. Both sets of estimates with these additional ICT controls are very similar to baseline suggesting growth in ICT usage is not biasing the results.

The second approach is account for ICT growth is simpler. In Panel (B), we present estimates after dropping "tech" industries from our regressions. We define "tech" industries as those with more than 25% of employment in technology oriented occupations following Heckler [2005]. These industries include Aerospace Products and Parts (NAICS 3364), Other Chemicals (3259), Basic chemicals (3251), Pharmaceuticals (3254), Electrical Equipment and Components (3359), Audio and Video Equipment (3343), Navigational and Control Instruments (3345), Semiconductor and Compoent Manufacturing (3344), Communications Equipment Manufacturing (3342), Computer and Peripheral Equipment (3341). These industries represent represent 21.4% manufacturing employment in 2001. Despite the smaller sample, we continue to find bonus depreciation has large and significant effects on employment.

I Aggregate and Long-Run Manufacturing Trends

This section provides additional context to the employment and capital investment results presented in Section 3.4. Figure A12 demonstrates that the positive effects of Bonus Depreciation on U.S. manufacturing plants that we estimate can be interpreted in the context of large sector-level declines in employment and an overall shift toward more capital-intensive production. We utilize data from the NBER-CES Manufacturing Industry Database to obtain sector-wide manufacturing time series. We then apply our event study estimates from Section 3.4 to these series to illustrate the aggregate effects implied by our results. We weight these regressions using 2001 employment counts at the industry level. Panel (A) demonstrates that manufacturing capital stock grew steadily for both long and short duration industries in the pre-period, but stagnated for short duration industries after 2001. On the other hand, long duration industry capital stock continued to grow in the treatment period, though less dramatically than in the pre-period. Panel (B) demonstrates that manufacturing employment experienced a stable post-2001 decline across both long and short duration industries. Long duration industries thus experienced relatively more positive employment growth than short duration industries, despite an overall decline in employment. Taken together, these figures demonstrate the well-established fact that U.S. manufacturing became more capital intensive over the 1997-2011 period.

Figure A12 replicates our main investment and employment event study regressions using data from the NBER-CES Manufacturing Industry Database over the 1990-2011 period to demonstrate that our results are not explained by long-run business cycle trends that the 1997-2011 sample period in our main analysis could otherwise mask. Event study coefficient estimates are obtained from regressions similar to Equation (3.1) using 4-digit NAICS industry-year level data. Panel (A) shows that despite some short-run fluctuations, log investment in our pre-period reveals no statistically significant differences across long and short duration industries in the 1990-2000 period. This coarse regression also produces post-2001 effects that are very similar to those derived from our plant-level regressions. Panel (B) shows that log employment in the pre-period was very stable across long and short duration industries, while we again find large positive effects in the post-2001 period.

J Worker Composition and Wage Decomposition

This section provides three complementary methods of assessing the impact of worker composition on the observed decrease in labor earnings at plants treated by bonus, relative to control plants. First, we predict earnings-per-worker at a state-industry level based on average earnings for each demographic group in 1997-2001. To implement this method, using pre-treatment data, we regress earnings-per-worker on demographic shares (young, female, less-education, Non-white). Using these estimates and observed demographic shares, we predict state-industry earnings during the full sample period. Therefore, these counterfactual earnings capture only the changes due to demographic shifts and hold earnings-per-worker constant within each demographic group. We then estimate the response of these counterfactual earnings to bonus depreciation. If the average earnings response we find is caused exclusively by demographic shifts due to the policy then would expect our counterfactual event study estimates to look very similar to those presented in Figure 3.5.

Figure A13 compares the counterfactual event study estimates with earnings event study using ASM data (Figure 3.5). The counterfactual estimates closely track the ASM findings throughout the sample period. The similarity of the two series indicates that the negative earnings response to bonus depreciation is due primarily to the policy-induced demographic shifts in the workforce we document above.

Second, we replicate the log earnings regression with QWI data while controlling for the various measurements of workforce composition at the state-industry level, which are "bad controls" but we still think is qualitatively insightful when viewed in conjunction the other methods. The results of these regressions are presented in Table A7. This table begins with the original log earnings regression coefficient indicating that bonus decreases earnings-per-worker at most-treated plants by 3.1%. The next four specifications sequentially add controls for each of the endogenous workforce characteristics that we find respond to bonus incentives: share young workers, share workers with highschool education or less, share of non-white workers, and share of female workers.[3] In the final column with all controls, we find that bonus leads to a statistically insignificant 0.7% increase in earnings. This indicates that the change in workforce composition explains the decrease in earnings.

Second, we apply a formal decomposition to measure the effect of each margin of

[3]The workforce characteristics are included in each regression interacted with year fixed effects to allow them to have different effects over time in an evolving market.

workforce composition directly. The Kitagawa-Oaxaca-Blinder decomposition follows the literature by estimating separate earnings regressions before and after bonus for the treatment and control samples to separate changes in observable characteristics from the changes in the predicted marginal effects associated with those characteristics [Kitagawa, 1955, Oaxaca, 1973, Blinder, 1973]. We begin with the fact that the wages in treated and control industries before and after the implementation of bonus can be described by a system of four equations, with each describing the relationship of wages to workforce characteristics for a different sample:

$$
\begin{aligned}
wage_{jst}^{\text{bonus, pre}} &= \alpha_{js}^{\text{bonus, pre}} + \gamma_{st}^{\text{bonus, pre}} + \beta^{\text{bonus, pre}} X_{jst}^{\text{bonus, pre}} + \varepsilon_{jst} \\
wage_{jst}^{\text{bonus, post}} &= \alpha_{js}^{\text{bonus, post}} + \gamma_{st}^{\text{bonus, post}} + \beta^{\text{bonus, post}} X_{jst}^{\text{bonus, post}} + \varepsilon_{jst} \\
wage_{jst}^{\text{control, pre}} &= \alpha_{js}^{\text{control, pre}} + \gamma_{st}^{\text{control, pre}} + \beta^{\text{control, pre}} X_{jst}^{\text{control, pre}} + \varepsilon_{jst} \\
wage_{jst}^{\text{control, post}} &= \alpha_{js}^{\text{control, post}} + \gamma_{st}^{\text{control, post}} + \beta^{\text{control, post}} X_{jst}^{\text{control, post}} + \varepsilon_{jst}.
\end{aligned}
$$

The controls X_{jst} in each regression include the share of young employees, share of employees with less than a high school education, share of non-white employees, and share of employees that are female. All regressions include state-by-year and industry-by-state fixed effects. In expectation under the assumption that $\mathrm{E}(\varepsilon_{jst}|X_{jst}) = 0$, we can restate these equations as OLS estimates. Taking differences of the first two equations describes the effect of bonus on average wages to be the difference in estimated fixed effects (Δ FE) plus the difference in average effects of workforce composition.

$$
\Delta w\bar{a}ge^{\text{bonus}} = \Delta\text{FE}^{\text{bonus}} + \hat{\beta}^{\text{bonus, post}} \bar{X}^{\text{bonus, post}} - \hat{\beta}^{\text{bonus, pre}} \bar{X}^{\text{bonus, pre}}.
$$

Adding and subtracting the estimated value of $\hat{\beta}^{\text{bonus, pre}} \bar{X}^{\text{bonus, post}}$ to the right hand side of this equation allows us to separate "quantity" or "composition" effects, changes in shares holding prices constant, from all other factors.

$$\Delta w\bar{a}ge^{\text{bonus}} = \underbrace{\Delta \text{FE}^{\text{bonus}} + \Delta\hat{\beta}^{\text{bonus}}\bar{X}^{\text{bonus, pre}}}_{\text{All Other Factors}} + \underbrace{\hat{\beta}^{\text{bonus, pre}}\Delta\bar{X}^{\text{bonus}}}_{\text{Composition}}.$$

To find the relative wage effects for treated plants relative to control plants, we perform the same calculation for the control equations and then take a difference between the wage decomposition for treated and control plants. Estimates of the four regressions explaining log earnings are shown in columns (1)-(4) of Table A8. The impact of the change in workforce composition is simply the difference between the quantity term for treated plants and for control plants and can be calculated separately for each characteristic:

- The increase in young workers accounts for 0.46 log points of the decrease,
- the increase in less educated workers accounts for 1.40 log points of the decrease,
- the increase in non-white workers accounts for 0.12 log points of the decrease,
- and the increase in female workers accounts for 0.85 log points of the decrease.

Taken in its entirety, this decomposition suggests that 2.83 log points of the 3.1 log point effect is explained by changes in composition, or close to 91% of the overall wage effect. Our analyses indicate that the change in the share of less educated workers and the share of female workers explain most of the decrease in earnings-per-worker, confirming our results from Table A7.

K Structural Model Derivation

Below we derive the model predictions presented in Section 3.6. The following exposition follows closely that in Harasztosi and Lindner [2019], which in turn follows Hamermesh [1996] to derive the output demand elasticity.

Consumer Problem

Consider a differentiated goods market and consumer preferences given by the constant elasticity of substitution function

$$U = \left(a \left[\left(\int_0^1 q(\omega)^{\frac{\kappa-1}{\kappa}} d\omega \right)^{\frac{\kappa}{\kappa-1}} \right]^{\frac{\theta-1}{\theta}} + (1-a) X^{\frac{\theta-1}{\theta}} \right)^{\frac{\theta}{\theta-1}},$$

where consumption of a variety ω from the differentiated goods market is given by $q(\omega)$ and X is spending on outside goods. Let $Q = \left(\int_0^1 q(\omega)^{\frac{\kappa-1}{\kappa}} d\omega \right)$. The consumer budget constraint is given by

$$\int_0^1 p(\omega) q(\omega) d\omega + X = I,$$

where consumer income is I and expenditures on the outside good X is set as a numeraire. Demand for variety ω may be derived by first solving the consumer's constrained optimization problem as represented by the Lagrangian below:

$$\mathcal{L} = \left(a \left[\left(\int_0^1 q(\omega)^{\frac{\kappa-1}{\kappa}} d\omega \right)^{\frac{\kappa}{\kappa-1}} \right]^{\frac{\theta-1}{\theta}} + (1-a) X^{\frac{\theta-1}{\theta}} \right)^{\frac{\theta}{\theta-1}} - \lambda \left[\int_0^1 p(\omega) q(\omega) d\omega + X - I \right].$$

Taking first-order conditions with respect to $q(\omega)$ and X

$$\frac{\partial L}{\partial q(\omega)} = \left(a\big(Q^{\frac{\kappa}{\kappa-1}}\big)^{\frac{\theta-1}{\theta}} + (1-a)X^{\frac{\theta-1}{\theta}}\right)^{\frac{\theta}{\theta-1}-1} a\big(Q^{\frac{\kappa}{\kappa-1}}\big)^{\frac{\theta-1}{\theta}-1} Q^{\frac{\kappa}{\kappa-1}-1} q(\omega)^{\frac{\kappa-1}{\kappa}-1} - \lambda p(\omega) = 0, \tag{A.1}$$

$$\frac{\partial L}{\partial X} = \left(a\big(Q^{\frac{\kappa}{\kappa-1}}\big)^{\frac{\theta-1}{\theta}} + (1-a)X^{\frac{\theta-1}{\theta}}\right)^{\frac{\theta}{\theta-1}-1} (1-a)X^{\frac{\theta-1}{\theta}-1} - \lambda = 0. \tag{A.2}$$

Relative demand for a given variety can be derived by taking the ratio of FOCs of two varieties ω_1 and ω_2, and rearranging:

$$q(\omega_1) = \left(\frac{p(\omega_1)}{p(\omega_2)}\right)^{-\kappa} q(\omega_2).$$

This expression may be further manipulated by multiplying both sides by $p(\omega_1)$ and integrating with respect to $p(\omega_1)$:

$$\int_0^1 p(\omega_1)q(\omega_1)d\omega_1 = p(\omega_2)^{\kappa} q(\omega_2) \int_0^1 p(\omega_1)^{1-\kappa} d\omega_1.$$

The left-hand side of this expression is equal to total expenditures on all varieties (that is, $(I-X)$). Defining the composite price index $P \equiv \left(\int_0^1 p(\omega_2)^{1-\kappa} d\omega_2\right)^{\frac{1}{1-\kappa}}$, we write this equation as

$$(I-X) = p(\omega_2)^{\kappa} q(\omega_2) P^{1-\kappa}.$$

We then solve for the optimal choice of $q(\omega_2) = (I-X)P^{\kappa-1}p(\omega_2)^{-\kappa}$. Utilizing this simplified expression, it is convenient to express $Q^{\frac{\kappa}{\kappa-1}}$ as

$$Q^{\frac{\kappa}{\kappa-1}} = \left(\int_0^1 q(\omega_2)^{\frac{\kappa-1}{\kappa}} d\omega_2\right)^{\frac{\kappa}{\kappa-1}} = (I-X)P^{\kappa-1}\left(\int_0^1 p(\omega_2)^{1-\kappa} d\omega_2\right)^{\frac{\kappa}{\kappa-1}} = (I-X)P^{-1}.$$

To derive the optimal quantity of X, combine the two FOCs:

$$a\left(Q^{\frac{\kappa}{\kappa-1}}\right)^{\frac{\theta-1}{\theta}-1} Q^{\frac{\kappa}{\kappa-1}-1} q(\omega)^{\frac{\kappa-1}{\kappa}-1} = (1-a)X^{\frac{\theta-1}{\theta}-1} p(\omega)$$

Multiplying both sides by $q(\omega)$ and integrating over ω simplifies the expression to

$$a\left(Q^{\frac{\kappa}{\kappa-1}}\right)^{\frac{\theta-1}{\theta}} = (1-a)X^{\frac{\theta-1}{\theta}-1} \int_0^1 p(\omega)q(\omega)d\omega.$$

Using the expressions $Q^{\frac{\kappa}{\kappa-1}} = (I-X)P^{-1}$ and $\int_0^1 p(\omega)q(\omega)d\omega = (I-X)$ implies that

$$X = \frac{\left(\frac{1-a}{a}\right)^{\theta} P^{\theta-1}}{1+\left(\frac{1-a}{a}\right)^{\theta} P^{\theta-1}} I \quad \text{and} \quad I - X = \frac{1}{1+\left(\frac{1-a}{a}\right)^{\theta} P^{\theta-1}} I.$$

We may now express the firm level demand for good $q(\omega)$ as

$$q(\omega_2) = I \frac{1}{1+\left(\frac{1-a}{a}\right)^{\theta} P^{\theta-1}} P^{1-\kappa} p(\omega_2)^{-\kappa}. \tag{A.3}$$

As a result, we can derive the elasticity of demand for a given variety ω with respect to its own price as

$$\frac{\partial \log q(\omega)}{\partial \log p(\omega)} = -\kappa.$$

Firm Problem

Firms first minimize production costs subject to constant returns to scale technology; let $c(w, R, p_j)$ denote the firm's unit cost function, which depends on the wage rate w, the rental rate of capital R, and the price of an arbitrary third input p_j. Given the elasticity of output demand derived in the previous section, we may utilize firm optimality conditions to derive the expressions in the main text that relate our empirical elasticities to structural parameters of interest. With constant returns to scale production technology, profit maximization for a firm producing variety ω is determined by the following expression:

$$\max_{q(\omega)} \quad p(q(\omega))q(\omega) - c(w, R, p_j)q(\omega).$$

Solving and rearranging yields the following first order condition:

$$\left(\frac{\partial p(\omega)}{\partial q(\omega)} \frac{q(\omega)}{p(\omega)} + 1\right) p(\omega) - c(w, R, p_j) = 0.$$

From the consumer problem, the inverse elasticity of demand is $\frac{\partial p(\omega)}{\partial q(\omega)} \frac{q(\omega)}{p(\omega)} = -\frac{1}{\kappa}$, which allows us to express the optimal price for ω as a function of a fixed mark-up μ and input prices:

$$p(\omega) = \underbrace{\frac{\kappa}{\kappa - 1}}_{\equiv \mu} c(w, R, p_j).$$

Using this expression, we first consider the effects of bonus depreciation on firm production. First, consider the effect of an arbitrary change in the cost of capital R on prices charged by affected firms. Taking logarithms and differentiating with respect to R gives

$$\frac{\partial \log p(\omega)}{\partial R} = \frac{\partial \log c(w, R, p_j)}{\partial R} + \frac{\partial \log \mu}{\partial R}$$

Given that the mark-up μ is constant, $\frac{\partial \log \mu}{\partial R} = 0$. Shephard's lemma $\left(c_R = \frac{K}{q}\right)$ then implies that the elasticity of output prices with respect to capital input prices is equal to the share of capital cost in total cost, s_K:

$$\frac{\partial \log p(\omega)}{\partial \log R} = \frac{R \times c_R}{c} = \frac{R \times K}{cq(\omega)} \equiv s_K.$$

We then utilize this expression to derive the analogous effect on total revenue:

$$\frac{\partial \log p(\omega)q(\omega)}{\partial \log R} = \frac{\partial \log p(\omega)}{\partial \log R} + \frac{\partial \log q(\omega)}{\partial \log p(\omega)}\frac{\partial \log p(\omega)}{\partial \log R}.$$

Letting $-\eta \equiv \frac{\partial \log q(\omega)}{\partial \log p(\omega)}$, the effect on total revenue of an arbitrary change in the cost of capital is

$$\frac{\partial \log p(\omega)q(\omega)}{\partial \log R} = (1 - \eta)s_K.$$

The scale effect, ηs_K, depends on the degree to which bonus depreciation impacts the quantity sold by a given firm, $q(\omega)$. Under the assumption that bonus depreciation only impacts one firm, Equation A.3 shows that $\eta = \kappa$. To the extent that bonus impacts the sector-level price index P, Equation A.3 shows that the relevant η also depends on substitution toward consumption on outside goods X.

Letting $\phi = \frac{\partial \log R}{\partial \text{Bonus}} < 0$ denote the effect of bonus on the cost of capital, we arrive at Equation 3.6:

$$\frac{\partial \log p(\omega)q(\omega)}{\partial \text{Bonus}} = (1-\eta)s_K \times \phi.$$

Next, we derive the effect of bonus on the input decisions of affected firms. For each input, we use Shepards' lemma to express the optimal choice of each input as a function of the optimal output quantity and the first derivative of the cost function. Taking logs and differentiating with respect to an arbitrary change in the cost of capital, we may arrive at expressions for the effect of bonus on optimal input decisions as a function of input elasticities of substitution, the output demand elasticity, and input cost shares. For the optimal choice of capital, Shephard's lemma gives $K = c_R q$. Therefore,

$$\frac{\partial \log K(\omega)}{\partial R} = \frac{c_{RR}}{c_R} + \frac{\partial \log q(\omega)}{\partial R}. \tag{A.4}$$

Multiplying both sides of this expression by $\frac{\partial R}{\partial \log R} = R$ and substituting for the previously derived expression for input cost shares yields

$$\frac{\partial \log K(\omega)}{\partial \log R} = R\frac{c_{RR}}{c_R} - \eta s_K.$$

To write $R\frac{c_{RR}}{c_R}$ in terms of elasticities of substitution, note that constant returns to scale and Shephard's lemma imply that:

$$\begin{aligned}
qc(w, R, p_j) &= wL + RK + p_j J \\
qc(w, R, p_j) &= wc_w q + Rc_R q + p_j c_{p_j} q \\
c(w, R, p_j) &= wc_w + Rc_R + p_j c_{p_j}.
\end{aligned}$$

Differentiating with respect to the cost of capital implies

$$\begin{aligned}
c_R &= wc_{wR} + c_R + Rc_{RR} + p_j c_{p_j R} \\
R\frac{c_{RR}}{c_R} &= -w\frac{c_{wR}}{c_R} - p_j\frac{c_{p_j R}}{c_R} \\
R\frac{c_{RR}}{c_R} &= -\frac{wL}{L}\frac{c_{wR}}{c_R} - \frac{p_j J}{J}\frac{c_{p_j R}}{c_R} \\
R\frac{c_{RR}}{c_R} &= -\frac{wL}{qc}\frac{cc_{wR}}{c_w c_R} - \frac{p_j J}{qc}\frac{cc_{p_j R}}{c_{p_j} c_R} \\
R\frac{c_{RR}}{c_R} &= -s_L\sigma_{KL} - s_J\sigma_{KJ},
\end{aligned}$$

where the second line solves for $R\frac{c_{RR}}{c_R}$, the third line manipulates each ratio by multiplying and diving by the respective input, and the fourth line uses Shephard's lemma and further multiplies and divides by c. The last line uses the definitions of cost shares $s_L = \frac{wL}{qc}$ and $s_J = \frac{p_j J}{qc}$ and of the Allen partial elasticity of substitution between inputs i and j, which is given by $\sigma_{ij} = \frac{cc_{ij}}{c_i c_j}$. Again letting $\phi = \frac{\partial \log R}{\partial \text{Bonus}} < 0$, we combine this expression with Equation A.4 to derive Equation (3.3) from the main text:

$$\frac{\partial \log K(\omega)}{\partial \text{Bonus}} = (-s_J\sigma_{KJ} - s_L\sigma_{KL} - \eta s_K) \times \phi.$$

We follow a similar procedure to derive Equation 3.4, the effect of bonus on the optimal labor choice. Taking logarithms of Shephard's lemma ($L = c_w q$) and differentiating with respect to R,

$$\frac{\partial \log L(\omega)}{\partial R} = \frac{c_{wR}}{c_w} + \frac{\partial \log q(\omega)}{\partial R}.$$

As before, we can write this expression as

$$\begin{aligned}\frac{\partial \log L(\omega)}{\partial \log R} &= \frac{Rc_R}{c}\frac{cc_{wR}}{c_Rc_w} - \eta s_K \\ \frac{\partial \log L(\omega)}{\partial \log R} &= \frac{RK}{qc}\frac{cc_{wR}}{c_Rc_w} - \eta s_K\end{aligned}$$

where the first line multiplies and divides by $\frac{c_R}{c}$ and the second line uses Shephard's lemma. Using definitions of the Allen partial elasticity of substitution and the share of capital in total costs, together with $\phi = \frac{\partial \log R}{\partial \text{Bonus}} < 0$, we arrive at Equation 3.4

$$\frac{\partial \log L(\omega)}{\partial \text{Bonus}} = s_K(\sigma_{KL} - \eta) \times \phi.$$

Equation 3.5 can be derived in a similar fashion.

Effects of Bonus under Financing Constraints

This section describes a simple model that shows that financing constraints can amplify the effects of bonus on the cost of capital. As in Domar [1953], suppose that plants would like to finance new investments, I, through a combination of retained earnings, RE, and the cash flow plants get from bonus, BCF. When $I \leq RE+BCF$ the firm pays $\frac{r(1-\tau z)}{1-\tau}$ to finance investment. Note that $BCF = \tau b I$, so that plants pay the interest rate $\frac{r(1-\tau z)}{1-\tau}$ if $I \leq \frac{RE}{1-\tau b}$. That is, retained earnings can finance larger investments when b is larger, since this allows plants to claim a larger share of the total tax deductions associated with the investment in the year the investment is made. Additionally, we consider that plants face uncertainty regarding the retained earnings that will be available at the time of investment, so that $RE \sim G(\cdot)$. As in Myers [1977], Bond and Meghir [1994], Bond and Van Reenen [2007], we assume that plants pay a transaction cost f when accessing financing mechanisms (e.g., by issuing stock) when investment costs exceed retained earnings.

The expected financing cost for an investment I is then

$$\begin{aligned}\text{Cost of Capital} &\equiv \frac{r(1-\tau z)}{1-\tau} + \frac{f}{1-\tau}\mathbb{P}r\left(I \geq \frac{RE}{1-\tau b}\right) \\ &= \frac{r(1-\tau(b+(1-b)z_0))}{1-\tau} + \frac{f}{1-\tau}G\left(I(1-\tau b)\right).\end{aligned}$$

The effect of bonus on the cost of capital is then:

$$-\frac{\tau}{1-\tau}\left[r(1-z_0) + fIG'(I(1-\tau b))\right].$$

Note that, since $G(\cdot)$ is a C.D.F., $G'(\cdot) \geq 0$. This expression shows that bonus lowers the cost of capital both by decreasing the standard user cost of capital term from Hall and Jorgenson [1967] and by reducing the likelihood that plants will pay transaction costs to access other forms of finance.

Let $\varepsilon_G = \frac{IG'}{G} \geq 0$ be the elasticity of the likelihood that the firm is constrained with respect to the size of the investment. We can then write ϕ as follows:

$$\begin{aligned}\phi &\equiv \frac{\partial \ln(\text{Cost of Capital})}{\partial \text{Bonus}} = \frac{-1}{\text{Cost of Capital}} \times \frac{\tau}{1-\tau}\left[r(1-z_0) + fG(I(1-\tau b))\varepsilon_G\right] \\ &= -\tau\left[s_r\frac{(1-z_0)}{(1-\tau z)} + (1-s_r)\varepsilon_G\right],\end{aligned}$$

where s_r is the share of financing costs explained by the opportunity cost of retained earnings.

When $s_r = 1$, $\phi = \frac{\partial \ln \frac{r(1-\tau z)}{1-\tau}}{\partial \text{Bonus}} = -\frac{\tau(1-z_0)}{(1-\tau z)}$. As an illustrative calculation, assume $\tau = 0.35$, $z_0 = 0.9$, and that $b = 0.5$. For investments financed with retained earnings (i.e., when $s_r = 1$), we calculate that $\phi = -0.052$. Assuming that about half of the investment cost is due to additional financing costs and that $\varepsilon_G = 0.25$ implies that

$\phi = -0.15$, while assuming that $\varepsilon_G = 0.5$ and $s_r = 0.5$ implies that $\phi = -0.276$.

Cash Flow Effects of Bonus under Capacity Constraints

The previous subsection showed that in our baseline model the term ϕ captures the impacts of bonus on the cost of capital including a role for financing constraints. A potential concern is that our baseline model is miss-specified by ignoring how cash-flow effects of the policy may impact the choice of all inputs. A particular worry is that this miss-specification may be placing too large a role on the cost of capital effect of bonus (i.e., that ϕ is too large) and that ignoring cash flow effects may bias the estimate of σ_{KL}.

In this section, we explore the possibility that cash-flow benefits from bonus depreciation may allow plants to expand their production capacity. As in Section K, plants choose the optimal mix of inputs to minimize costs of production. In contrast to that section—where plants chose the quantity produced to maximize profits—we instead assume that plants are constrained in the total production costs they can expend. Formally, assume:

$$\max_{q(\omega)} \quad p(q(\omega))q(\omega) - c(w, R, p_j)q(\omega) \quad \text{subject to} \quad c(w, R, p_j)q(\omega) \leq \bar{c} + \tau b I(w, R, p_j),$$

where total costs must not exceed the combination of a capacity constraint $\bar{c}$ plus the cash flow the plant receives from bonus depreciation, $\tau b I(w, R, p_j)$. Assuming that the constraint binds, we have:

$$q(\omega) = \frac{\bar{c} + \tau b I(w, R, p_j)}{c(w, R, p_j)},$$

so that

$$\frac{\partial \ln q(\omega)}{\partial \text{Bonus}} = -s_K\phi + \underbrace{\frac{\tau b I(w,R,p_j)}{\bar{c}+\tau b I(w,R,p_j)}}_{s^b}(1+\varepsilon_b^I)/b = -s_K\phi\left(1+\underbrace{\frac{s^b(1+\varepsilon_b^I)/b}{-s_K\phi}}_{\chi\geq 0}\right),$$

where s^b is the share of plant expenditures that comes from the cash-flow effect of bonus and where ε_b^I is the investment elasticity with respect to bonus. The last expression introduces the term χ as a measure of the relative importance of cash flow vis-a-vis cost of capital effects of bonus.

Following the derivations above, we obtain the effect of bonus on revenue as follows;

$$\frac{\partial \ln p(\omega)q(\omega)}{\partial \text{Bonus}} = \frac{\partial \ln p(\omega)}{\partial \ln q(\omega)}\frac{\partial q(\omega)}{\partial \text{Bonus}} + \frac{\partial q(\omega)}{\partial \text{Bonus}} = -s_K\phi(1+\chi)\left(1-\frac{1}{\eta}\right).$$

This expressions shows that, while the scale effect of the policy is now mechanical, the price and revenue effects depend on the elasticity of product demand, η.

Following the dichotomy of scale and substitution effects, note that since plants are still cost-minimizing, the substitution effects of bonus are the same as in our baseline model. In contrast, the scale effect of the policy is now given by the equation for $\frac{\partial \ln q(\omega)}{\partial \text{Bonus}}$ above. We thus obtain the following modified implications of the model:

$$\begin{aligned}
\frac{\partial \log K(\omega)}{\partial \text{Bonus}} &= (s_J\sigma_{KJ} - s_L\sigma_{KL} - s_K(1+\chi))\phi \\
\frac{\partial \log L(\omega)}{\partial \text{Bonus}} &= s_K(\sigma_{KL} - (1+\chi))\phi \\
\frac{\partial \log J(\omega)}{\partial \text{Bonus}} &= s_K(\sigma_{KJ} - (1+\chi))\phi
\end{aligned}$$

Note that these equations only differ from our baseline model in that $1+\chi$ has now replaced η. Intuitively, the scale effect in our baseline model is determined by profit maximization, which depends on the elasticity of product demand η. In contrast, in the capacity constrained model, the scale effect depends on the degree to which the cash flow effects of the policy allow plants to expand production.

As in our baseline model, the cost-weighted average of input effects continues to identify the scale effect:

$$\bar{\beta} = s_J \beta^J + s_K \beta^K + s_L \beta^L = -s_K \phi (1 + \chi).$$

Similarly, we can identify η by comparing the scale effect to the implied price effect of the policy, so that $\eta = \frac{\bar{\beta}}{\bar{\beta} - \beta^R}$.

To identify σ_{KL}, note that

$$\frac{\bar{\beta} - \beta^L}{\bar{\beta}} = \frac{\sigma_{KL}}{(1+\chi)} \Longrightarrow \sigma_{KL} = (1+\chi)\left(1 - \frac{\beta^L}{\bar{\beta}}\right).$$

Again, the only difference between our baseline model and the scale constrained case is that the term η is now replaced by $(1+\chi)$. A key implication of this expression is that, since $\chi \geq 0$ and $\beta^L > \bar{\beta}$, our estimates would also imply a negative value of σ_{KL} in this setting. That is, the conclusion that capital and production labor are complements in our setting is robust to allowing for cash flow effects to relax capacity constraints of manufacturing plants.

To obtain a plausible magnitude of χ, consider that we estimate that $\bar{\beta} = 0.10$ and that, in our baseline model, we estimate that $\phi = -0.14$. Together with the assumption that $s_K = 0.20$, the scale effect implies that $1+\chi = \frac{\bar{\beta}}{-s_K \phi} = \frac{0.10}{0.2*0.14} = 3.57$. This value of $1+\chi$ then implies a magnitude of σ_{KL} close to our baseline estimate of -0.54. To the extent that $1+\chi$ is greater than 3.57, we would obtain more negative estimates of σ_{KL}. The implied estimate of σ_{KL} is closer to zero when χ is small. At the extreme where $\chi = 0$, we have $\sigma_{KL} = -0.15$.[4] This value can be considered an upper bound for σ_{KL}, since the motivating assumption behind this analysis is that the cash-flow effect may play a significant role (i.e. $\chi \gg 0$).

To analyze this model more formally, we implement our estimate of the investment effects of bonus to estimate both σ_{KL} and s^b. We may identify σ_{KL} as follows:[5]

$$\sigma_{KL} = \frac{\bar{\beta} - \beta^L}{\bar{\beta} - s^b(1+\varepsilon_b^I)/b}.$$

This expression differs from an analogous expression in our baseline model by replacing β^R with $s^b(1+\varepsilon_b^I)/b$. Table A16 presents estimates of σ_{KL} utilizing this equation and estimates of $\bar{\beta}$ and ϕ. Across all columns, we find estimates of σ_{KL} that are very similar to those presented in Table 3.7. We can also use our long-difference estimate of the investment elasticity $\varepsilon_b^I = 0.20$ and $b = 0.45$—the average value across the sample period—to estimate s^b. Across all specifications, we estimate values no greater than 3.0%. That is, for plausible values of the model parameters we only require that at most 3% of plant expenditures are driven by the cash flow effects of the policy.

The alternative model in this section shows that allowing for the cash flow effects of bonus to help finance other plant costs—such as labor—does not alter the implication of our reduced-form estimates that $\sigma_{KL} < 0$—i.e., that capital and labor are complements in our setting. Indeed, for plausible values of the cash-flow effects of

[4]Note that when $\chi = 0$, the implied value of $\phi = -\frac{\bar{\beta}}{s_K} = -\frac{.10}{.20} = -0.50$. That is, low values of χ imply values of ϕ that are more negative than in our baseline model. Given the motivating concern that the baseline model puts too much weight on ϕ relative to cash flow effects, it is worth noting that for $\phi > -0.14$, it is necessary that $\chi > 2.57$, which then yields more negative estimates of σ_{KL}.

[5]From the scale effect, it follows that $-s_K\phi = \bar{\beta} - s^b(1+\varepsilon_b^I)/b$. Note also that, since $-s_K\phi > 0$, we have that $\bar{\beta} > s^b(1+\varepsilon_b^I)/b \geq 0$.

bonus, we find magnitudes of σ_{KL} that are similar to those in our baseline model.

L Additional Model Results

This section presents various model results in greater detail. First, we present estimates of both translog cost functions and constant elasticity of substitution production functions. These estimated functions allow us to test several hypotheses of interest. These results demonstrate that our conclusion that capital and labor are complements in production holds up across several alternative models. We utilize our event study estimates over the 2002-2011 period to calculate several model parameters over time. Finally, we estimate the model using industry-level data and compute aggregate elastcities of substitution that account for within and across industry reallocation of production toward more capital intensive production units.

Translog Cost Function Estimation

We now show that our estimates of substitution elasticities are compatible with a canonical model of production. In his treatise on labor demand, Hamermesh [1996] recommends that empirical researchers specify models that allow for flexible cross-price elasticities between capital and different types of labor. One such model is the transcendental logarithmic cost function, or "translog" for short, which is a second-order approximation to an arbitrary functional form [Christensen et al., 1971, 1973].

The translog cost function can accommodate an arbitrary number of inputs, is a second-order approximation to a general cost function, and nests several alternative production technologies. The general form is given by:

$$\log C = \log Y + a_0 + \sum_i a_i \log w_i + 0.5 \sum_i \sum_j b_{ij} \log w_i \log w_j, \tag{A.5}$$

where

$$\sum_i a_i = 1; \quad b_{ij} = b_{ji}; \quad \sum_i b_{ij} = 0, \ \forall j, \tag{A.6}$$

and where the parameters b_{ij} are the parameters of interest. For factor inputs i and j and associated cost shares s_i and s_j, the partial elasticities of substitution we estimate can be expressed as

$$\sigma_{ij} = [b_{ij} + s_i s_j]/s_i s_j, \quad i \neq j. \tag{A.7}$$

We can then estimate b_{lk} and b_{jk} using our estimated elasticities of substitution, σ_{KL} and σ_{JK}. In order to identify b_{lj}, we consider two values of σ_{LJ} relative to our estimates of σ_{KL} and σ_{JK} in Table 3.8. Specifically, first consider that cost minimization implies a lower-bound value of σ_{LJ} :

$$\begin{aligned} s_J \sigma_{LJ} + s_K \sigma_{KL} > 0, & \\ & \sigma_{LJ} > -(s_K/s_J)\sigma_{KJ}. \end{aligned}$$

As a second alternative, we consider the assumption that σ_{LJ} is as large as our largest estimated elasticity: $\max = \{\hat{\sigma}_{KL}, \hat{\sigma}_{JK}\} = \hat{\sigma}_{JK}$. Below, we present results using these two alternative values of σ_{LJ}, which we use to estimate b_{lj}.

To identify the parameters b_{ii} then requires values of σ_{LL}, σ_{JJ}, and σ_{KK}. These values can be obtained from the following identities:

$$s_L\sigma_{LL} + s_J\sigma_{LJ} + s_K\sigma_{LK} = 0,$$
$$s_L\sigma_{JL} + s_J\sigma_{JJ} + s_K\sigma_{JK} = 0,$$
$$s_L\sigma_{KL} + s_J\sigma_{KJ} + s_K\sigma_{KK} = 0.$$

Rearranging the first of these expressions, $\sigma_{LL} = [-s_J\sigma_{LJ} - s_K\sigma_{LK}]/s_L$. Equation (3.3) demonstrates that for an input j, σ_{jj} can be interpreted as the negative of the total substitution effect with respect to other inputs divided by the cost share s_j. We can then relate these parameters to their translog counterparts through the following equation:

$$\sigma_{ii} = [b_{ii} + s_i^2 - s_i]/s_i^2. \tag{A.8}$$

Equations (A.7) and (A.8) demonstrate that the partial elasticities of substitution we estimate are linear functions of the analogous translog parameters b_{ij}. Panels A of Tables A10 and A11 report translog parameter estimates for our two assumed values of σ_{LJ}.

An advantage of estimating these translog cost parameters is that we may derive simple testable restrictions on these parameters that correspond to different production technologies. We test the following hypotheses:

$$H_0 : b_{kl} = b_{kj} = b_{jl} = 0 \quad \text{(Cobb-Douglas)},$$

$$H_0 : b_{kl} = b_{kj} = 0 \quad \text{(Capital Separability)},$$

$$H_0 : b_{kj} = b_{lj} = 0 \quad \text{(J Separability)},$$

$$H_0 : b_{kl} = b_{lj} = 0 \quad \text{(L Separability)},$$

$$H_0 : b_{ij} = -s_i s_j \quad \forall i \neq j \quad \text{(Leontief)}.$$

Panels B of Tables A10 and A11 report p-values associated with the F-tests corresponding to these null hypotheses across the 3-input model estimates presented in Table 3.8. For both bounds on σ_{LJ}, we are generally able to reject the Cobb-Douglas production technology as well as capital and production labor separability at the 5% level and in many cases at the 0.1% level.

We also reject non-production labor separability when assuming $\sigma_{LJ} = -(s_K/s_J)\sigma_{KJ}$. This result makes intuitive sense since the lower bound that implies this value of σ_{LJ} corresponds to null total elasticity of substitution, which is closer to a Leontief production technology than a separable one. In contrast, we do not reject that non-production labor may be separable when we assume that $\sigma_{LJ} = \sigma_{KJ}$. This result also makes intuitive sense since $\sigma_{LJ} = \sigma_{KJ}$ implies that $b_{lj} = b_{kj}$, which by construction satisfies half of the conditions of test of J-separability.

In both cases, we are unable to reject a Leontief production technology across all models. This result is consistent with our finding in Section 3.6 that the most of the effect of the policy on factor demands was driven by the scale effect. Importantly, these results show that the estimated complementarity between capital and production labor is compatible with a standard model of production.

Elasticities of Capital and Labor Demand

While separating scale and substitution effects clarifies the mechanisms that drive responses to bonus, the effects of policies that change the cost of capital—e.g., changes in interest rates or other tax provisions—depend on elasticities of capital and labor demand. We now estimate these elasticities using our model to recover the implied effect of the policy on the cost of capital.

As we discuss in Section 3.1, the effect of bonus on the cost of capital depends on a number of real world factors, including the roles of depreciation deductions, tax losses, and financing constraints. One advantage of our model is that it links the estimated effects on inputs of production to the effects of the policy on the cost of capital. Equation 3.7 implies that

$$\phi = -\frac{\bar{\beta}}{s_K \eta}. \tag{A.9}$$

Column (1) of Panel (D) of Table 3.7 shows that the semi-elasticity of the cost of capital with respect to bonus $\phi = -0.145$ when the elasticity of product demand $\eta = 3.5$. Columns (2)–(5) show that varying s_K and η delivers estimates of $\phi \in [-0.25, -0.10]$.

Following the prior literature, we first consider the elasticity of investment with respect to the cost of capital. Column (1) of Panel (D) of Table 3.7 shows that $\varepsilon^I_\phi = \frac{\beta^I}{\phi} = -1.40$.[6] This elasticity lies in the range $[-2.1, -0.84]$ across columns (1)–(5). Through the lens of a simple investment model without financing frictions, the results in Zwick and Mahon [2017] imply an elasticity of -7.2. Our smaller estimate of this elasticity is due to the fact that our estimate of ϕ includes financing and other

[6]This estimate uses the long difference estimate on investment from Panel (A) of Figure 3.2.

constraints.[7]

An advantage of our setting is the ability to measure the effect of the cost of capital on the stock of capital used for production. Column (1) of Panel (D) of Table 3.7 reports our baseline estimate of $\varepsilon_{\phi}^{K} = \frac{\beta^K}{\phi} = \frac{0.080}{-0.145} = -0.55$.[8] For context, Equation 3.3 and our baseline values for s_K and η would imply that $\varepsilon_{\phi}^{K} = -1.5$ with Cobb-Douglas production. Thus, even though our estimated 8% increase in the capital stock is economically significant, we find a modest capital stock elasticity when we appropriately measure the effect of the policy on the cost of capital.

Our model-based estimate of ϕ also allows us to recover cross-price elasticities of labor demand with respect to the cost of capital. Column (1) of Panel (D) of Table 3.7 shows that we estimate an elasticity of $\varepsilon_{\phi}^{L} = \frac{\beta^L}{\phi} = \frac{0.116}{-0.145} = -0.80$ for production labor and $\varepsilon_{\phi}^{J} = \frac{\beta^J}{\phi} = \frac{0.090}{-0.145} = -0.62$ for non-production labor.[9] Both elasticities would equal -0.5 with Cobb-Douglas production. This comparison reinforces the dominance of the scale effect in our setting, since even a large degree of substitution would be overshadowed by the scale effect. In addition, since we estimate that $\varepsilon_{\phi}^{L} < \varepsilon_{\phi}^{J}$, our results are also not consistent with the hypothesis of capital-skill complementarity.

Our estimated elasticities of capital and labor demand highlight three policy-relevant insights. First, understanding how fiscal policies relax financing and other constraints is critical for forecasting the effects of fiscal policies on capital and labor demand. Second, the scale effect is the biggest driver of the effects of changes in the

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[7]In Appendix K, we calibrate values of ϕ under alternative assumptions. Including a role for financing constraints implies that ϕ is 2–4 times larger than when ϕ only accounts for the present value of depreciation deductions. These calculations are also consistent with calibrations in Zwick [2014] showing that bonus had large effects on investment due to high values of the shadow price of internal funds and high implied discounting rates. In a setting where tax policy is less likely to interact with financing constraints, Chen et al. [2019] estimate an investment tax elasticity of -2.2, which is comparable in magnitude to our estimates.

[8]This elasticity lies in the range $[-0.80, -0.32]$ across columns (1)–(5).

[9]Across our estimates in columns (1)–(5), $\varepsilon_{\phi}^{L} \in [-1.16, -0.46]$ and $\varepsilon_{\phi}^{J} \in [-0.90, -0.36]$.

cost of capital. Finally, this result alleviates the concern that lowering the cost of capital would reduce labor demand.

Constant Elasticity of Substitution Parameter Estimates

We now demonstrate that the elasticities in Panel (D) of Table 3.7 can be used to estimate key parameters from a nested constant elasticity of substitution (CES) production function. We consider a CES production function in which production labor and capital are nested separately from non-production labor:

$$F(K, L, J) = \left[\mu_1 J^{\rho_1} + (1-\mu_1)(\mu_2 L^{\rho_2} + (1-\mu_2)K^{\rho_2})^{\frac{\rho_1}{\rho_2}}\right]^{\frac{1}{\rho_1}},$$

where J represents non-production labor, L represents production labor, K represents capital, and ρ_1 and ρ_2 are our CES parameters of interest.[10]

The first-order conditions associated with cost minimization yield the following expression that relates the ratio of optimal L and K to the price ratio:

$$\frac{L}{K} = \left(\frac{(1-\mu_2)}{\mu_2}\frac{R}{w}\right)^{\frac{1}{1-\rho_2}}. \tag{A.10}$$

Taking logs and differentiating this expression with respect to the cost of capital ϕ leads directly to our identification result for ρ_2:

$$\varepsilon_\phi^L - \varepsilon_\phi^K = \frac{1}{1-\rho_2}, \tag{A.11}$$

[10] An alternative approach nests non-production labor and capital separately from production labor [e.g., as in Krusell et al., 2000]. This approach is not compatible with our findings. To see this, recall that we estimate $\sigma_{KL} < 0$. Because this approach assumes that $\sigma_{LJ} = \sigma_{KL}$, the production function would have two (out of three) negative elasticities of substitution and would therefore violate second-order sufficiency conditions of cost minimization [see, e.g., Allen, 1938, p. 505].

which can be rearranged to yield an expression for ρ_2.[11]

In order to derive an expression for ρ_1, we first note that cost minimization implies the following result that relates CES parameters to input cost shares:

$$\frac{RK}{RK+wL} = \frac{\mu_2\left(\frac{R}{\mu_2}\right)^{\frac{-\rho_2}{1-\rho_2}}}{\mu_2\left(\frac{R}{\mu_2}\right)^{\frac{-\rho_2}{1-\rho_2}} + (1-\mu_2)\left(\frac{w}{1-\mu_2}\right)^{\frac{-\rho_2}{1-\rho_2}}} = \frac{s^K}{s^K+s^L}. \tag{A.12}$$

As with Equation (A.10), we may also derive the following expression for the optimal quantity ratio of J and K using first-order conditions:

$$\begin{aligned}\frac{J}{K} = \left(\frac{R}{\mu_2}\right)^{\frac{1}{1-\rho_2}}\left(\frac{p_j}{\mu_1}\right)^{\frac{-1}{1-\rho_1}} \times \\ \left[\frac{1}{(1-\mu_1)}\left[\mu_2\left(\frac{R}{\mu_2}\right)^{\frac{-\rho_2}{1-\rho_2}} + (1-\mu_2)\left(\frac{w}{1-\mu_2}\right)^{\frac{-\rho_2}{1-\rho_2}}\right]^{\frac{\rho_1-\rho_2}{-\rho_2}}\right]^{\frac{1}{1-\rho_1}}\end{aligned} \tag{A.13}$$

Unlike Equation (A.10), taking logs of this expression and differentiating does not isolate ρ_1. Instead, we utilize expressions for the optimal quantities of J and K implied by cost minimization. Taking logs and differentiating these expressions with respect to R allows us to link ρ_1 to Morishima elasticities. Equation (A.12) and the definition of Morishima elasticities at the end of Section 3.6.2 yield the following result that relates ε^J_ϕ and ε^K_ϕ to an approximate expression around initial cost shares:

$$\varepsilon^J_\phi - \varepsilon^K_\phi \approx \frac{1}{1-\rho_2}\left[1 + \frac{\rho_1-\rho_2}{1-\rho_1}\frac{s^K}{s^L+s^K}\right]. \tag{A.14}$$

The expression holds locally since we use numerical values of s^K and s^L to approximate capital and labor cost shares, which are otherwise functions of prices and pro-

[11]Note that the left-hand-side expression in this equation and in Equation (A.15) below are also Morishima elasticities of substitution.

duction parameters. Rearranging this expression, combined with Equation (A.11), shows that ρ_1 is given by:

$$\varepsilon_\phi^J - \varepsilon_\phi^K \approx (\varepsilon_\phi^L - \varepsilon_\phi^K)\frac{s^L}{s^L + s^K} + \frac{1}{1-\rho_1}\frac{s^K}{s^L + s^K}. \tag{A.15}$$

According to Table 3.7, $\varepsilon_\phi^L - \varepsilon_\phi^K < 0$ and $\varepsilon_\phi^J - \varepsilon_\phi^K \approx 0$, implying that $\rho_2 > 1$ and $\rho_1 < 1$.[12] Panel (A) of Table A13 uses Equations (A.11) and (A.15) to show that we estimate $\rho_1 = -1.67$ and $\rho_2 = 5.03$. Panel (B) of Table A13 shows that our estimates imply that $\frac{1}{1-\rho_2} = -0.25 < 0.37 = \frac{1}{1-\rho_1}$. Thus, our results are not consistent with the capital-skill complementarity hypothesis. Panel (C) tests whether our results match the degree of capital-skill complementarity found in Krusell et al. [2000]. Our estimates reject the null of this high degree of capital-skill complementarity with a high degree of precision. This result is driven by the fact that bonus depreciation led to a substantial increase in the employment of production workers.

Model Estimates over Time

Our existing model results utilize either difference-in-differences or long difference estimates to recover estimates of scale effects, effects on the cost of capital, input elasticities with respect to changes in the cost of capital, and capital-labor substitution elasticities. Alternatively, we may utilize the event study estimates from Section 3.4 to recover these estimates for the entire 2002-2011 treatment period. Due to disclosure restrictions, we impute the covariances between reduced-form estimates in the 2002-2010 period where necessary by assuming that the correlations between any

[12] Table A12 reports that $\varepsilon_\phi^L - \varepsilon_\phi^K = -0.248$ (SE=0.141) and that $\varepsilon_\phi^J - \varepsilon_\phi^K = -0.070$ (SE=0.188). While Arrow et al. [1961] note that in two-input CES production functions, decreasing marginal returns requires that $\rho < 1$, the condition that $\rho_1, \rho_2 < 1$ is not necessary for a three-input production function to be consistent with cost minimization.

two regression estimates are constant and equal to their correlation in 2011.

Panels (A) and (B) of Figure A16 presents estimates of the scale effect and the effect on the cost of capital, respectively, over time. We estimate both the scale effect, $\bar{\beta}$, and the effect on the cost of capital, ϕ, by applying Equation (3.7) year-by-year. Consistent with the increasing effects over time across most outcomes in Section 3.4 we find that both of these effects increase in magnitude over time. Panels (C) and (D) display estimates of the investment and production employment elasticities presented in Table 3.7 over time. As in the main text, we define these elasticities as $\varepsilon^I_\phi = \beta^I/\phi$ and $\varepsilon^L_\phi = \beta^L/\phi$, respectively. These estimates are relatively stable over time. This result suggests that our estimates of ϕ capture the effects of the policy on the cost of capital, inclusive of financing and adjustment constraints that may prevent plants from adjusting their capital.

Lastly, we estimate σ_{KL} for each year over the 2004-2011 period by combining our event study estimates of the effect of bonus depreciation on production labor, an annualized long-difference estimate of the effect on total revenue, and Equations (3.4) and (3.6):

$$\sigma^t_{KL} = (1-\eta)\frac{\beta^L_t}{\beta^R_t} + \eta.$$

Figure A17 presents these estimates. While somewhat imprecise, these point estimates suggest a much larger, negative estimate of σ_{KL} that gradually attenuates over time. This pattern is consistent with labor being a more flexible input than capital in the short run, whereas over time, capital adjustments imply smaller degrees of complementary between labor and capital.

One way in which this dynamic pattern could be mis-measured is that capital is not always utilized at the same rate. To measure

Additional Model Estimates

To motivate the three input model presented in the main text, we consider a two input model with capital and labor. The two-input version of Equation 3.8 is:

$$\sigma_{KL} = \eta \left(1 - \frac{\beta^L}{s_L \beta^L + s_K \beta^K}\right). \tag{A.16}$$

To implement this equation, we set input cost shares so that $1 - s_K = s_L = 0.8$. Panel A of Figure A15 plots this equation using the estimated effects of bonus on capital and labor for a range of values of η. This figure shows that, regardless of the value of η, the fact that $\hat{\beta}^L > \hat{\beta}^K$ implies that capital and labor are complements, i.e., $\sigma_{KL} < 0$.[13] Column (4) of Table A14 implements the classical minimum distance approach to estimate σ_{KL}, finding an estimate of $\sigma_{KL} = -0.12$. In two input models, a negative elasticity of substitution is not consistent with cost minimization. One interpretation of these results is that the data are not consistent with a large degree of substitution between capital and workers.[14] A second interpretation is that plants in our data are not well approximated by a two input model.

We also consider several alternative models in which different inputs are used in production. Table A14 presents several three input alternatives to the baseline model estimates presented in the text, which we reproduce in column (1). Columns (2) and (3) of Table A14 again consider a three input production technology comprising production labor, non-production labor, and capital, but instead estimate labor relying on estimates of effects on employment using difference-in-differences (instead of long differences) and hours (instead of number of workers), respectively. In both cases,

[13] To be consistent with a Cobb-Douglas production function, Equation A.16 implies that $\hat{\beta}^K$ would have to be 2.25-times as large as $\hat{\beta}^L$, assuming $\eta = 5$; and 6-times as large if $\eta = 2$.

[14] Gechert et al. [2021] conduct a meta-analysis of estimates of σ_{KL} and show that, correcting for publication bias, one should expect to find a large number of negative estimates of σ_{KL}.

we estimates very similar values of σ_{KL}, suggesting that the finding that production labor and capital are complements is not driven by mismeasurement of labor inputs, nor by focusing on the ten-year effect of bonus depreciation on inputs. Column (5) of Table A14 considers an alternative production function that combines (all) workers with equipment capital, and structures. As discussed in the main text, structures were generally not eligible for bonus depreciation. This model finds that workers are complementary to equipment and that structures are substitutes with equipment. Since the model perfectly matches the estimated effect on capital structures, we interpret the estimated 4% increase in structures as being driven by a scale effect, though it is diminished by a substitution away from structures. Finally, column (6) considers a model with workers, capital, and materials. In this model, workers continue to be complements with capital, and we also find that materials and capital are substitutes.

Finally, we estimate a five input model that combines production labor, non-production labor, materials, capital structures, and capital equipment. Panel B of Figure A15 reports values of σ_{KL} implied by a five-input analogue of Equation A.16 across values of η. Once again, our estimates imply negative values of σ_{KL}.

Capital-Labor Elasticity of Substitution in Industry-Level Data

The model estimates so far are based on reduced-form estimates of capital and labor responses at the plant level from the ASM/CM data. Our baseline analyses focus on within-plant adjustments by relying on a balanced panel of plants. We now address whether entry and exit or reallocation to more capital intensive plants generate different substitution patterns at the industry level. To explore whether these margins impact our structural estimates, we estimate our model of factor demands using long difference estimates of the impact of bonus using the NBER-CES industry-level data.

We follow our main specifications as closely as possible, although we cannot control for geographic or plant-specific characteristics or trends. We weight these regressions using 2001 employment counts at the industry level.

We show estimates of the 2011 coefficient from Equation 3.1 in Table A17 for the outcomes log of production employment, log of non-production employment, and log of capital stock.[15] Our long difference estimates shows that bonus led to a relative increases in production employment of 17.9%, non-production employment increased of 13.2%, and capital stocks of 12.2% between 2001 and 2011. As with our plant-level results, we estimate larger effects on production employment than on non-production employment or capital.

Table A18 uses these industry-level results to estimate scale and substitution effects, and reports analogous statistics as those in Table 3.7. These tables show that our model has similar implications when we use industry or plant level data. In column (1), we estimate that σ_{KL} is equal to -0.59, and we reject the null hypothesis that $\sigma_{KL} \geq 0$ with a p-value of 0.072. For reference, Table 3.7 reports an estimate of -0.52 using plant-level estimates and the same parameterization. The similarity of the estimates of capital-labor substitution suggests that in our setting, entry, exit, and reallocation within industry are relatively minor factors.

Aggregate Capital-Labor Elasticity of Substitution

We now use the method developed in Oberfield and Raval [2021] to calculate the aggregate capital-labor elasticity of substitution. The method in Oberfield and Raval [2021] starts with a nested CES production function and generates an aggregate elasticity that accounts for reallocation toward more capital-intensive production units

[15] We directly observe production employment in the NBER-CES data. We define non-production employment as the difference between total and production employment. We obtain an industry price-adjusted capital stock by multiplying the capital stock by the investment price index.

within and across industries.

To apply this method, we begin by using the industry-level elasticity estimates discussed in Appendix L and presented in Panel (D) of Table A17. We use these estimates to compute the Morishima elasticities of substitution presented in Table A19. Because these elasticities are based on industry-level data, they already account for reallocation within industries. These estimates are very similar to their plant-level analogues presented in Table A12, suggesting that reallocation within industries is a not a substantial margin of response to bonus depreciation. As we discuss in Appendix L, these elasticites map to the parameters of a nested CES production function, as in the framework of Oberfield and Raval [2021].

Oberfield and Raval [2021] demonstrate that an aggregate capital-labor elasticity of substitution, σ_{KL}^{agg}, can be computed from our industry-level estimates of capital-labor elasticities of substitution, σ_{KL}^{N}. This aggregate elasticity of substitution is given by the following expression:

$$\sigma_{KL}^{agg} = (1-\chi^{agg})\sigma_{KL}^{N} + \chi^{agg}\left[(1-s_J)\varepsilon + s_J\sigma_{KJ}^{N}\right].$$

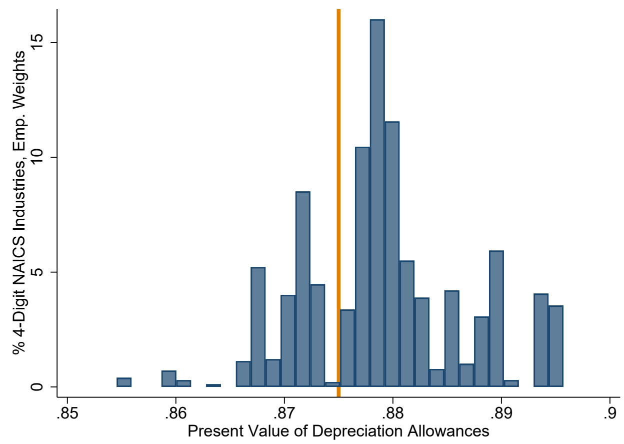
where σ_{KJ}^{N} denotes the mean industry-level elasticity of substitution between capital and non-production labor. The parameter χ^{agg} is a heterogeneity index that captures the dispersion of mean capital cost shares across industries. Letting $\alpha_n = \frac{rK_n}{rK+wL}$ be the cost share of capital in production inputs of industry n, α denote the economy-wide cost share, and $\theta_n = \frac{rK_n+wL_n}{rK+wL}$ denote industry n's share of economy-wide capital and production labor expenditures, the aggregate heterogeneity index is given by $\chi^{agg} = \sum \frac{(\alpha_n-\alpha)^2}{\alpha(1-\alpha)}\theta_n$. This quantity captures the degree to which aggregate capital-labor substitution will reflect within-industry substitution σ_{KL}^{N}; by substitution across industries of varying capital intensity, captured by the cross-industry demand elasticity ε; or by substitution toward non-production labor, captured by σ^N_{KJ}, which is in turn mediated by the cost share of non-production labor s_J. The relative importance of these forces thus depends on the degree of dispersion in capital intensities, with greater dispersion denoting greater degrees of cross-industry substitution.

Table A20 presents the results of this analysis for different calibrated values of η and s_{NL}.[16] The first row reports our industry-level elasticities of substitution, which account for within-industry reallocation. The second row calculates aggregate elasticities using oberfield2021micro estimated parameters: $\varepsilon = 1$ and $\chi^{agg} = 0.07$. Accounting for cross-industry reallocation yields aggregate substitution elasticities that are universally less negative. Across all specifications, we estimate aggregate elasticities consistent with complementarity between capital and production labor. Column (1) rejects values of σ^{agg}_{KL} greater than 0.14 at the 5% level; across all columns we reject values of σ^{agg}_{KL} greater than 0.21 at the same significance level.

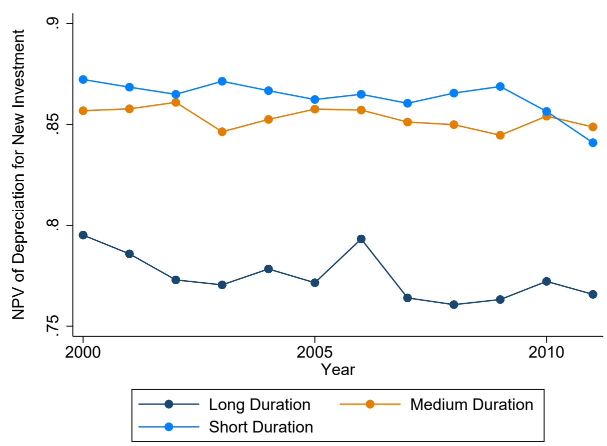
[16] Calibrations of the demand elasticity η affect estimates of industry-level and aggregate estimates through ϕ.

Appendix Figures

(a) Distribution of Depreciation NPV without Bonus



(b) Stability of Depreciation NPV Over Time



(c) Distribution and Stability of Depreciation Net Present Value without Bonus

Notes: Panel (A) of Figure A1c shows the distribution of the present value of depreciation deductions across manufacturing industries according to estimates in Zwick and Mahon [2017]. The vertical red line in this graph at 0.875 highlights the structural break that we take advantage of for defining plants that benefit most from Bonus. Panel (B) of Figure A1c displays the aggregate net present value of depreciation deductions for $1 of new investment in each year from 2000 to 2011 with an assumed discount rate of 7% without applying bonus depreciation. These represent annual estimates of z_0 discussed in Section 3.1. IRS sectors are aggregated into thirds based on weighted total investment in 2000 with the trends for each third graphed separately. The graph highlights that the sectors that invest in the longest tax-duration assets always have z_0 estimates less than 0.8 while the other two terciles have similarly stable z_0 estimates that are much higher. It does not appear that the non-bonus depreciation values of new investment are changing over time in response to bonus. *Source:* Authors' calculations based on Zwick and Mahon [2017] replication data and IRS SOI sector-level corporation depreciation data, derived from Form 4562.

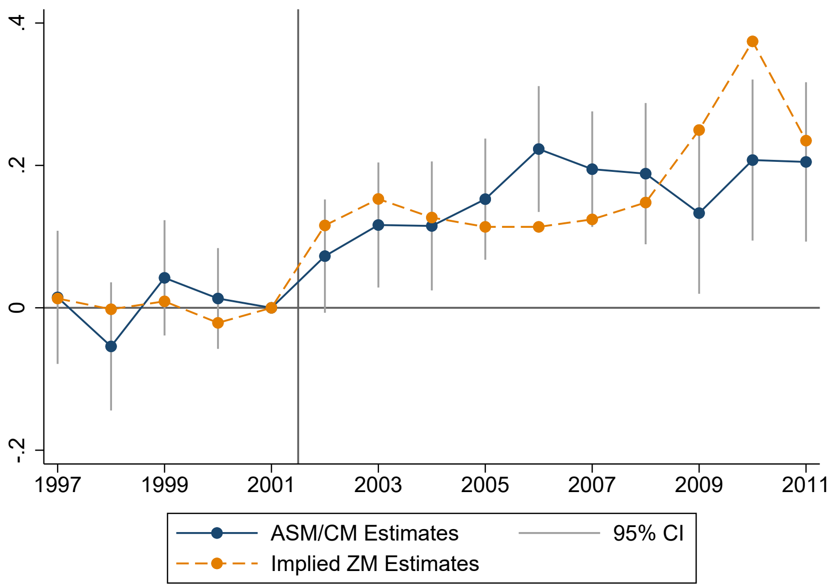
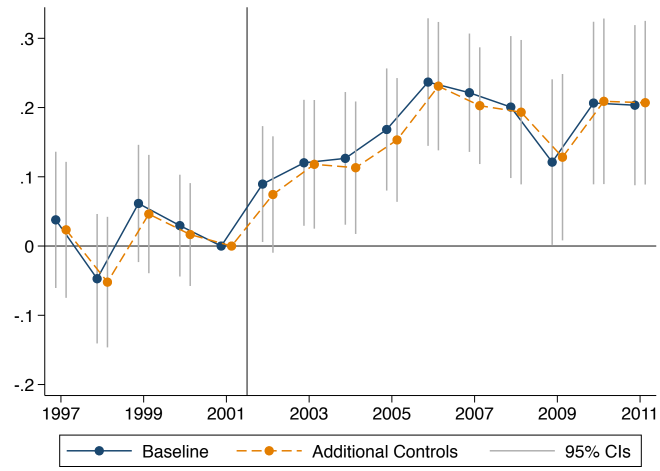


Figure A2: Comparison of Investment Event Study Results with Zwick and Mahon [2017]

Notes: Figure A2 compares our investment results to those of Zwick and Mahon [2017]. As we discuss in Section 3.3, we define exposure to treatment as a binary variable that takes the value of one when for firms with z_0 in the first three terciles of the distribution of z_0. Zwick and Mahon [2017] use the same definition of treated firms in their Figure 1 (see their §III.B, p.228). Using the reported values in their Figure 1, we construct a combined event study that mirrors our estimates. We describe this procedure in Appendix E. Table A1 lists the data and operations used to generate the orange series. Because IRS tax data report results from previous years and the ASM/CM data report production data in March of the current year, we align these two series to match economic activity in the same year. The blue series reproduce our estimates of the effects of bonus on log investment from Figure 3.2. This figure shows that our estimated effects of bonus on log investment are quite comparable with those reported in Zwick and Mahon [2017]. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

(a) IHS Investment



(b) $(\Delta \text{PPENT}_t / \text{PPENT}_{1997-2001})$

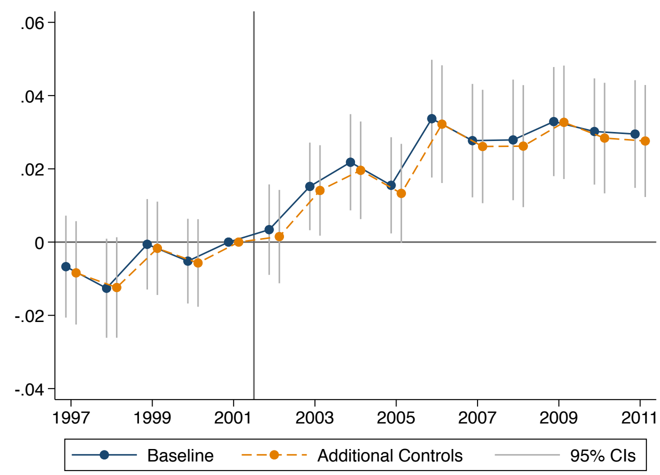


Figure A3: Effects of Bonus Depreciation on Alternative Investment Outcomes

Notes: Figure A3 displays estimates describing the effect of bonus depreciation on the Inverse Hyperbolic Sine of Investment in Panel (A) and PPENT expenditures divided by previous PPENT stock in Panel (B). Plotted coefficients are estimates of β_y from Equation (3.1), which are the annual coefficients associated with bonus. The baseline specification in each panel includes state-by-year and plant fixed effects. The specifications with additional controls add plant size in 2001 bins interacted with year fixed effects, TFP in 2001 bins interacted with year fixed effects, and firm size in 2001 interacted with year fixed effects to the baseline specifications. These specifications correspond to columns (2) and (5) of Table 3.1, respectively. 95% confidence intervals are included for each annual point estimate with standard errors clustered by the 4-digit NAICS-by-state level.
Source: Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

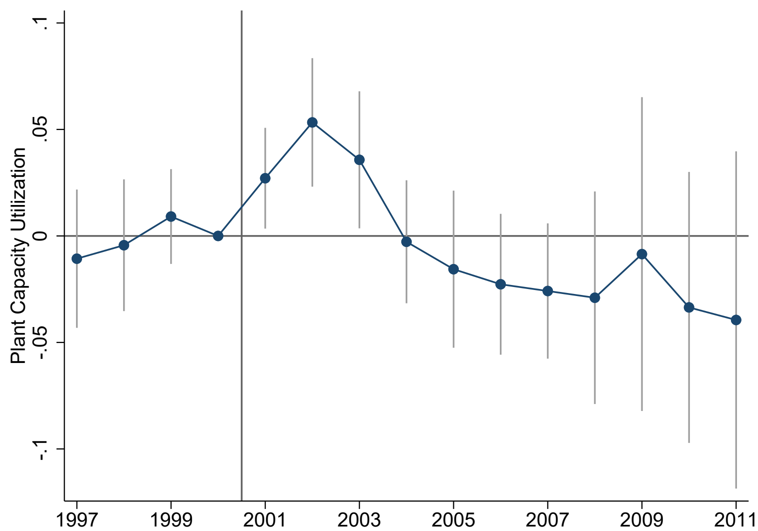
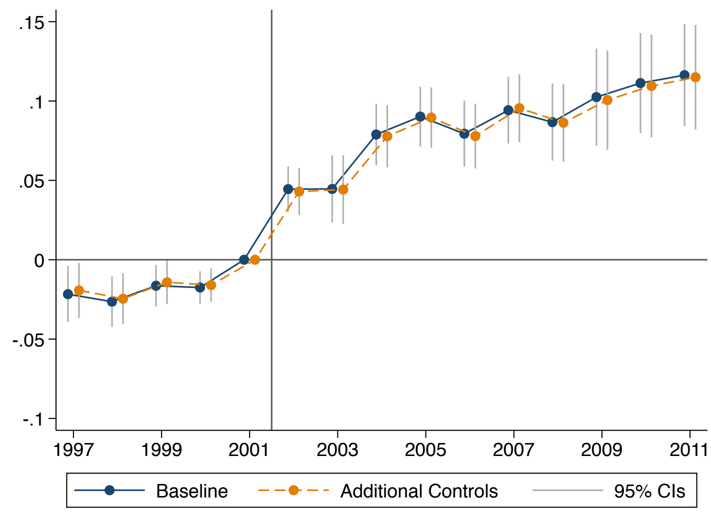


Figure A4: Effects of Bonus Depreciation on Plant Capacity Utilization
Notes: Figure A4 estimates how much capacity utilization changes for plants in industries most impacted by bonus. Plant capacity utilization averages at the industry level come from the Census' Quarterly Survey of Plant Capacity Utilization (QPC) fourth quarter estimates. Industry-year observations are weighted according to the inverse standard deviation of the industry average estimates. 1997-2006 data were converted using the Adobe Online converter from PDFs to excel files. 2008-2011 data are available directly on the QPC website while 2007 is imputed from the average of 2006 and 2008. The regression includes year fixed effects and standard errors are clustered at the industry level. While there is a short-run increase in utilization by treated industries through 2003, the long difference estimate shows a 3.9pp decrease in capacity utilization in bonus industries, which is statistically insignificant. *Source:* Authors' calculations based on the Quarterly Survey of Plant Capacity Utilization and Zwick and Mahon [2017] data.

(a) Log Production Employment



(b) Log Non-production Employment

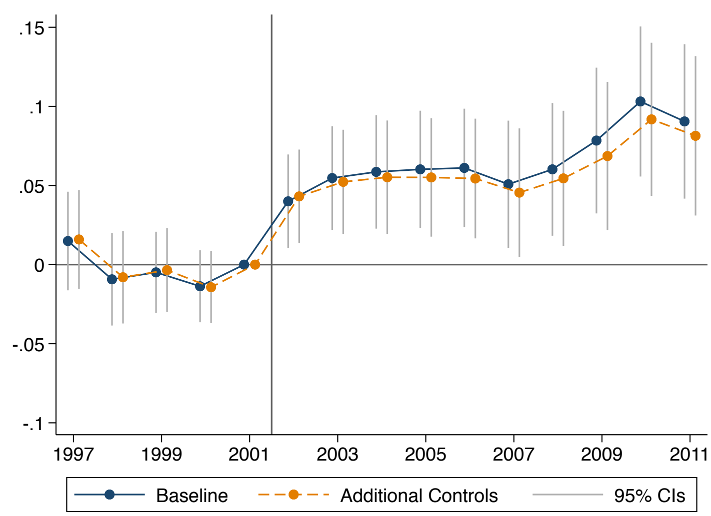
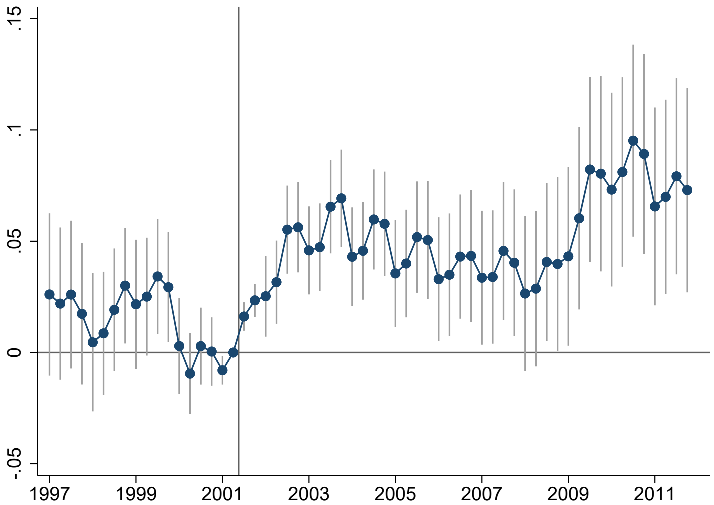


Figure A5: Effects of Bonus Depreciation on Production and Non-production Employment

Notes: Figure A5 displays estimates describing the effect of bonus depreciation on log production employment in Panel (A) and log non-production employment in Panel (B). Plotted coefficients are estimates of β_y from Equation (3.1), which are the annual coefficients associated with bonus. The baseline specification in each panel includes state-by-year and plant fixed effects. The specifications with additional controls add plant size in 2001 bins interacted with year fixed effects, TFP in 2001 bins interacted with year fixed effects, and firm size in 2001 interacted with year fixed effects to the baseline specifications. These specifications correspond to columns (6) and (7) of Table 3.3, respectively. 95% confidence intervals are included for each annual point estimate with standard errors clustered by the 4-digit NAICS-by-state level. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

(a) Firms with 1-50 Employees



(b) Firms 0-5 Years Old

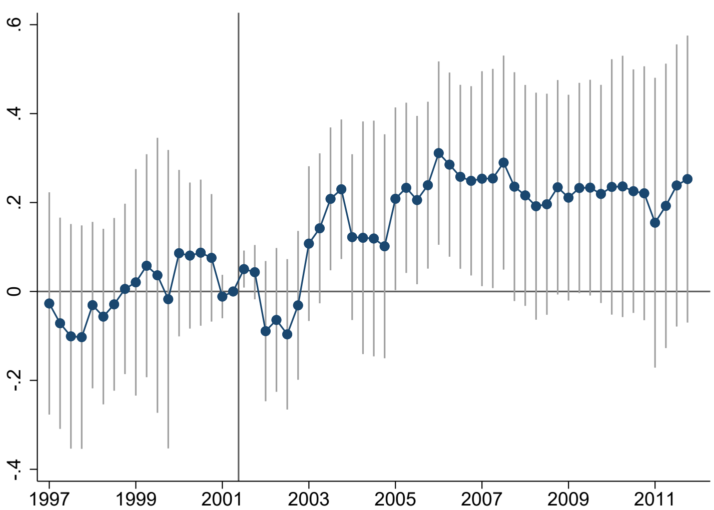
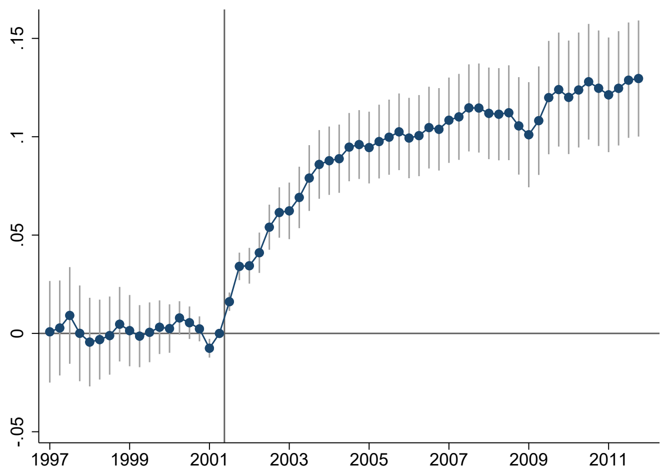


Figure A6: Effects of Bonus Depreciation on Smaller and Younger Firm Employment; QWI

Notes: Figure A6 displays estimates describing the effect of bonus depreciation on Log Employment for small and young firms using state-by-industry QWI data. Panel (A) restricts the sample to firms with 50 or fewer employees. Panel (B) restricts the sample to firms that are five or fewer years old. The regression estimates displayed in this figure correspond to a quarterly analogue of β_y from Equation (3.1), which is the change in log employment relative to 2001q2 in industries affected most by bonus relative to industries that are less affected by bonus. The regression includes 4-digit NAICS-by-state fixed effects and state-by-quarter fixed effects. 95% confidence intervals are included for each quarterly point estimate with standard errors clustered by the 4-digit NAICS-by-state level. *Source:* Authors' calculations based on QWI and Zwick and Mahon [2017] data.

(a) Effect of Bonus Depreciation on QWI Log Employment, Continuous Treatment



(b) Binscatter; Industry-Level Changes in Employment vs. z_0

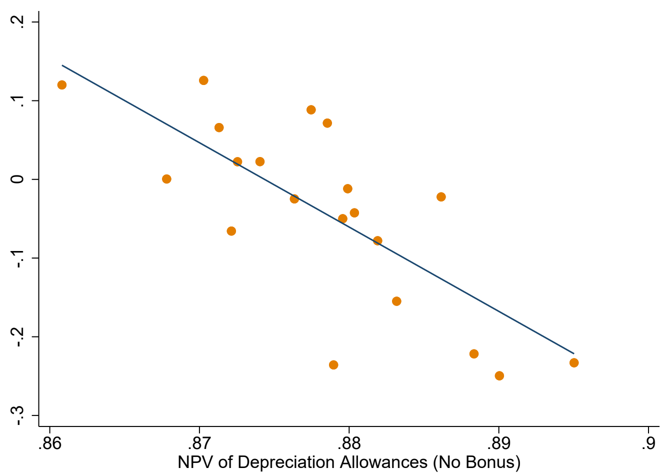


Figure A7: Effects of Bonus Depreciation on Employees, Continuous Treatment

Notes: Panel (A) of Figure A7 displays estimates describing the effect of bonus depreciation on log employment using state-by-industry QWI data as in Figure 3.4, but using the continuous (1-z_0)τ*0.0375 in place of the treatment indicator. Panel (B) presents a binned-scatter plot of industry-level changes between the pre- and post-periods in QWI Log Employment against z_0. Each industry-level change is derived from a regression in the form of Equation including an interaction term for the industry of focus. *Source:* Authors' calculations based on QWI and Zwick and Mahon [2017] data.

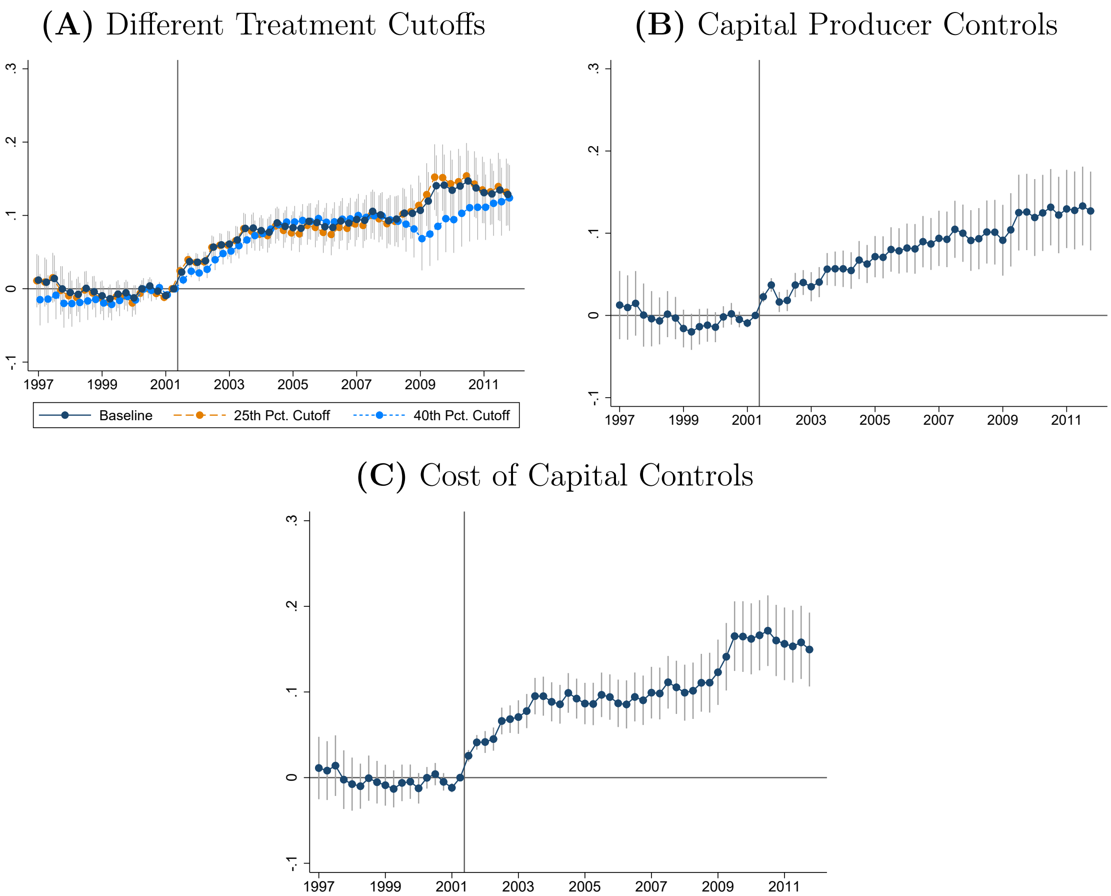
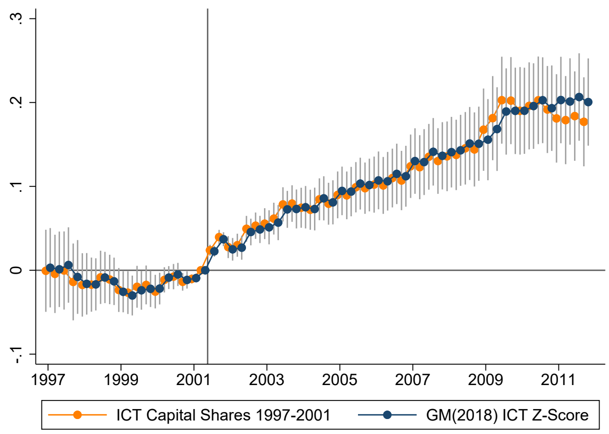


Figure A8: Effects of Bonus Depreciation, QWI Employment Robustness Checks

Notes: Figure A8 presents additional estimates of the effect of depreciation incentives on log employment in the state-by-industry QWI data as in Figure 3.4. Panel (A) shows the effects of bonus on employment using three different cutoffs in the z_0 distribution to determine treatment: 25^{th} percentile, 33^{rd} percentile, and 40^{th} percentile. Panel (B) includes a control for capital production as a share of output interacted with year fixed effects. Capital producing industries are identified using 2001 BEA Input-Output tables. Panel (C) includes quintile indicators for the cost of capital interacted with year fixed effects. We proxy for the cost of capital by taking the industry average of the cost of borrowing from Compustat firms in 2001, defined as xint / (dltt + dlc). *Source:* Authors' calculations based on QWI, BEA, Compustat, and Zwick and Mahon [2017] data.

(a) ICT Controls



(b) Dropping Tech Industries

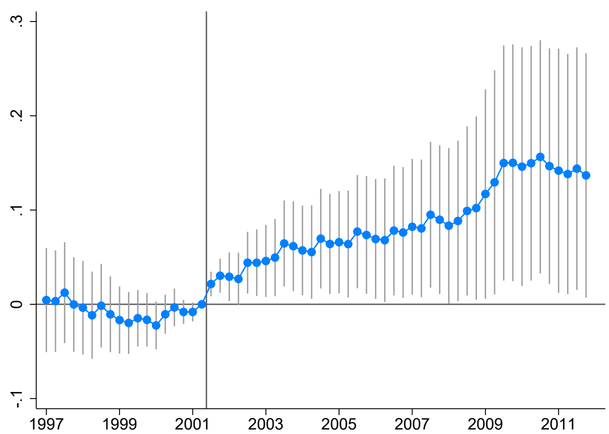


Figure A9: Effects of Bonus Depreciation, Controlling for ICT Growth

Notes: Figure A9 presents additional estimates of the effect of depreciation incentives on log employment in the state-by-industry QWI data as in Figure 3.4. Panel (A) includes tercile indicators for two measures of the use of information and communications technology (ICT) interacted with year fixed effects. The first is ICT capital intensity measured as a share of capital stock in ICT goods using BEA Detailed Data for Fixed Assets and Consumer Durable Goods from 1997 to 2001. The second is the Gallipoli and Makridis [2018] Z-score, which measures the normalized share of workers engaging in tasks involving ICT during the period 2002–2016. Panel (B) presents estimates that do not include tech industries. These include Aerospace Products and Parts (NAICS 3364), Other Chemicals (3259), Basic chemicals (3251), Pharmaceuticals (3254), Electrical Equipment and Components (3359), Audio and Video Equipment (3343), Navigational and Control Instruments (3345), Semiconductor and Compoent Manufacturing (3344), Communications Equipment Manufacturing (3342), Computer and Peripheral Equipment (3341). These industries represent 21.4% of 2001 manufacturing employment. *Source:* Authors' calculations based on QWI, BEA, Gallipoli and Makridis [2018], and Zwick and Mahon [2017] data.

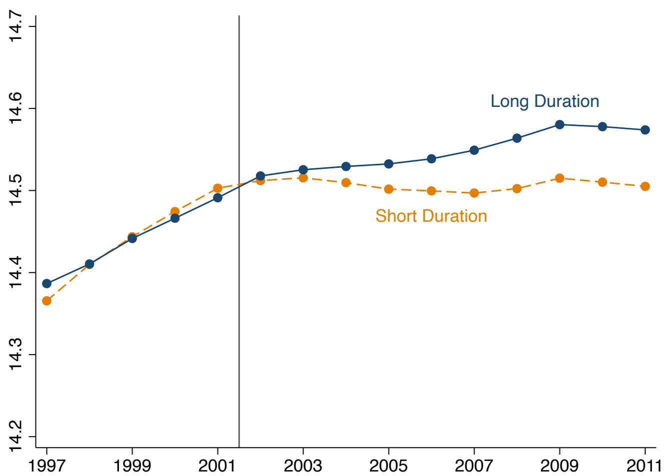
Figure A10: Effect of Bonus Depreciation on Employment by Task Content

Notes: Figure A10 displays estimates describing the effect of bonus depreciation on employment in routine occupations and production occupations based on event study regressions. Plotted regression coefficients in years 1990, 2005, and 2010 represent the difference in employment by long- vs. short-duration industries relative to the same difference in 2000. Employment is categorized by matching occupation definitions from the Census and ACS to production and routine categories from Acemoglu and Autor [2011]. Regressions are weighted by 2000 employment. Standard errors clustered at the state-industry level. *Source:* Authors' calculations based on Census, ACS, Zwick and Mahon [2017], and Acemoglu and Autor [2011] data.

Figure A11: U.S. Manufacturing Over the Business Cycle

Notes: Panels (A) and (B) of Figure A11 present event study regression coefficients representing the effect of bonus depreciation on log investment log employment and in 6-digit NAICS industries over the 1990 to 2011 period. Coefficients obtained from industry-year level regressions akin to Equation (3.1) with observations weighted by 2001 industry employment levels. Industry and year fixed effects are included in estimating equations, and standard errors are clustered at the 4-digit NAICS level. Shaded regions correspond to dates classified as business cycle contractions by the National Bureau of Economic Research. In Panel (C) and (D) the event study estimates are added or subtracted from the aggregate trends in each outcome based on NBER-CES data following the same procedure as in Figure A12. *Source:* Authors' calculations based on NBER-CES Manufacturing Industry Database, NBER Business Cycle Expansions and Contractions, and Zwick and Mahon [2017] data.

(a) Log Capital Stock



(b) Log Employment

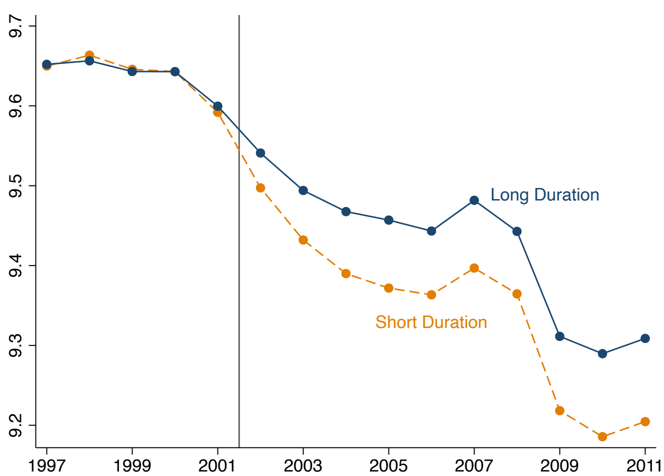


Figure A12: Effects of Bonus Depreciation on Aggregate Trends

Notes: Figure A12 presents the effect of bonus depreciation on aggregate trends over log employment and log capital stock over the 1997-2011 implied by our reduced form estimates. We construct aggregate series across bonus treatment by calculating aggregate time series of log capital stock and log employment, respectively, for all manufacturing industries and adding or subtracting estimates of event study coefficients from Equation (3.1) to the resulting series. *Source:* Authors' calculations based on NBER-CES Manufacturing Industry Database, ASM, CM, and Zwick and Mahon [2017] data.

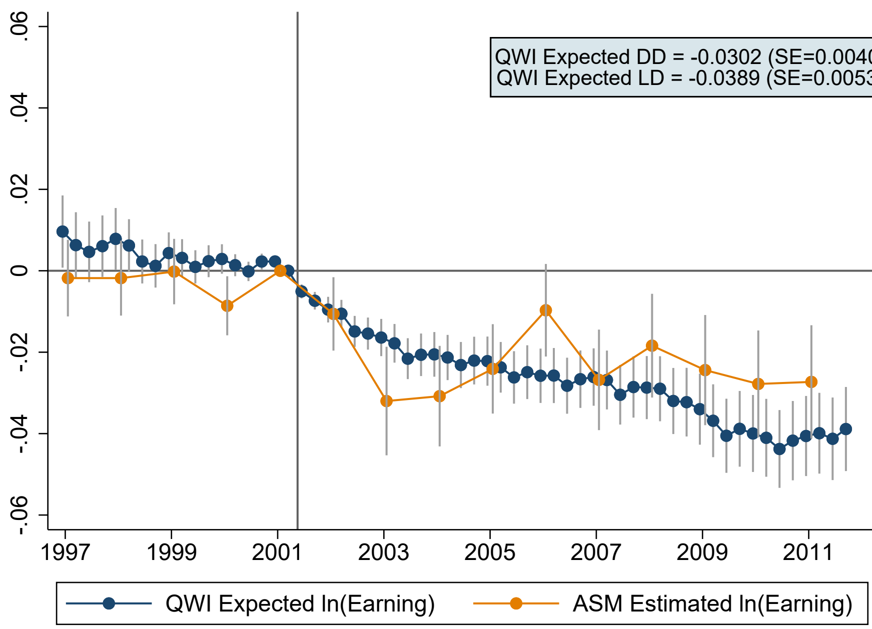
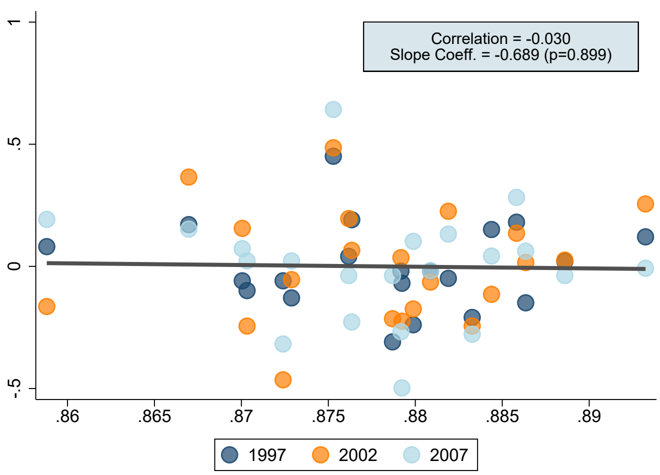


Figure A13: Comparing Counterfactual and Observed Earnings Changes

Notes: Figure A13 presents two series of plots. The first are event study estimates describing the effect of bonus depreciation on Log Mean Earnings per Workers. The ASM Estimated ln(Earning) replicates the estimates presented in Figure 3.5. The second plot, QWI Expected ln(Earnings) displays the effect of bonus depreciation on counterfactual earnings where the counterfactuals are based on average earnings predicted by worker demographic industry-state shares in the pre-period. The QWI Expected ln(Earning) estimates are made in the presence of industry-state and state-by-year fixed effects. DD and LD estimates of the effect of bonus depreciation on QWI Expected ln(Earning) and standard errors are noted in Figure. Standard errors are clustered at the state-industry level. *Source:* Authors' calculations based on ASM, CM, QWI, and Zwick and Mahon [2017] data.

(a) Correlation Between Raval [2019] σ_{KL} and Zwick and Mahon [2017] z_0



(b) Effect of Bonus Depreciation Employment Controlling for σ_{KL}

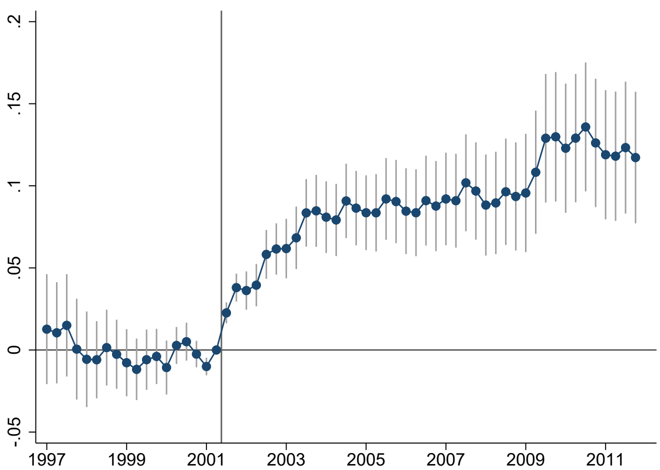
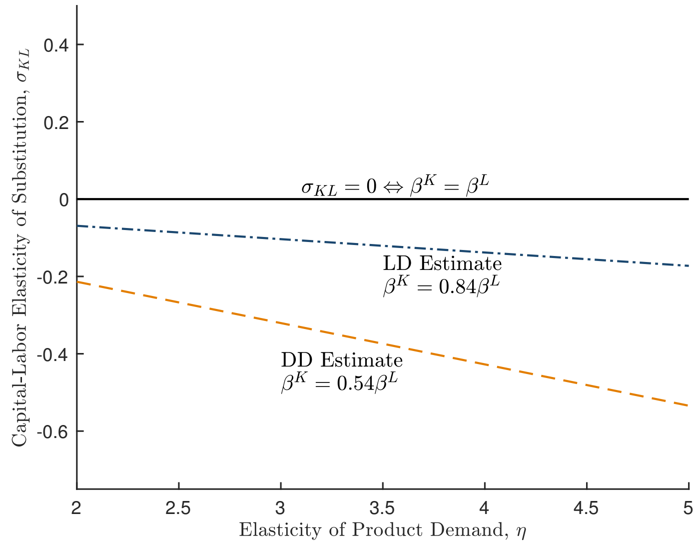


Figure A14: Bonus Depreciation Treatment and Differences in σ_{KL}

Notes: Panel (A) of Figure A14 shows how de-meaned σ_{KL} from Raval [2019] in years 1997, 2002, and 2007 vary across Zwick and Mahon [2017]'s z_0 measure averaged to the 3-digit NAICS level. The fitted linear relationship is based on year 2002 data. Panel (B) displays estimates describing the effect of bonus depreciation on log employment using state-by-industry QWI data as in Figure 3.4, controlling for tercile bins of 2002 σ_{KL} from Raval [2019] interacted with year fixed effects. *Source:* Authors' calculations based on data from the QWI, Zwick and Mahon [2017], and Raval [2019].

(a) σ_{KL} in a Two Input Model



(b) σ_{KL} in a Five Input Model

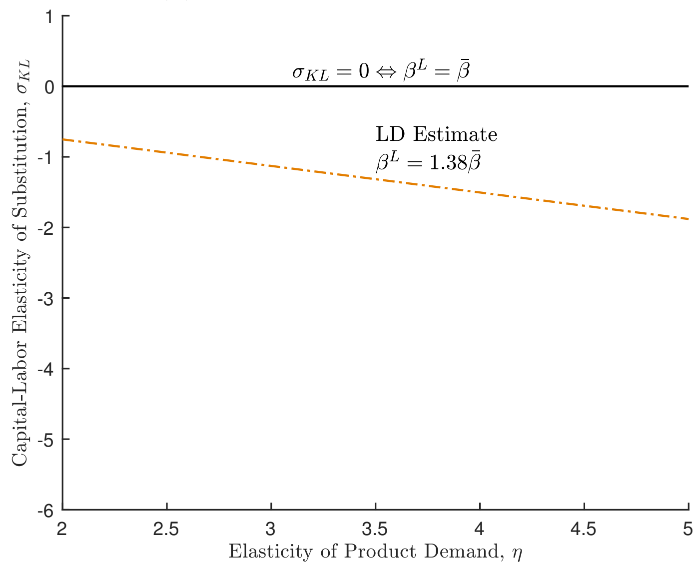


Figure A15: Additional Estimates of Capital-Labor Substitution

Notes: Figure A15 implements versions of Equation A.16 across two- and five-input models and for a range of values of η. Panel (A) shows that both our long-differences and difference-in-differences reduced-form estimates are not consistent with large degrees of substitution between capital and labor in a two-input model. This figure also motivates the estimation of three-input models since profit maximization requires a non-negative value of σ_{KL}. Panel (B) implements a five-input analogue of Equation A.16 using our long-differences estimates of the effects of bonus depreciation on capital and labor demand for a range of values of η. The inputs included are production labor (cost share $c_{l_1} = 0.15$), non-production labor (cost share $c_{l_2} = 0.10$), equipment capital (cost share $c_{k_1} = 0.06$), structures capital (cost share $c_{k_2} = 0.04$), and materials (cost share $c_m = 0.65$). *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

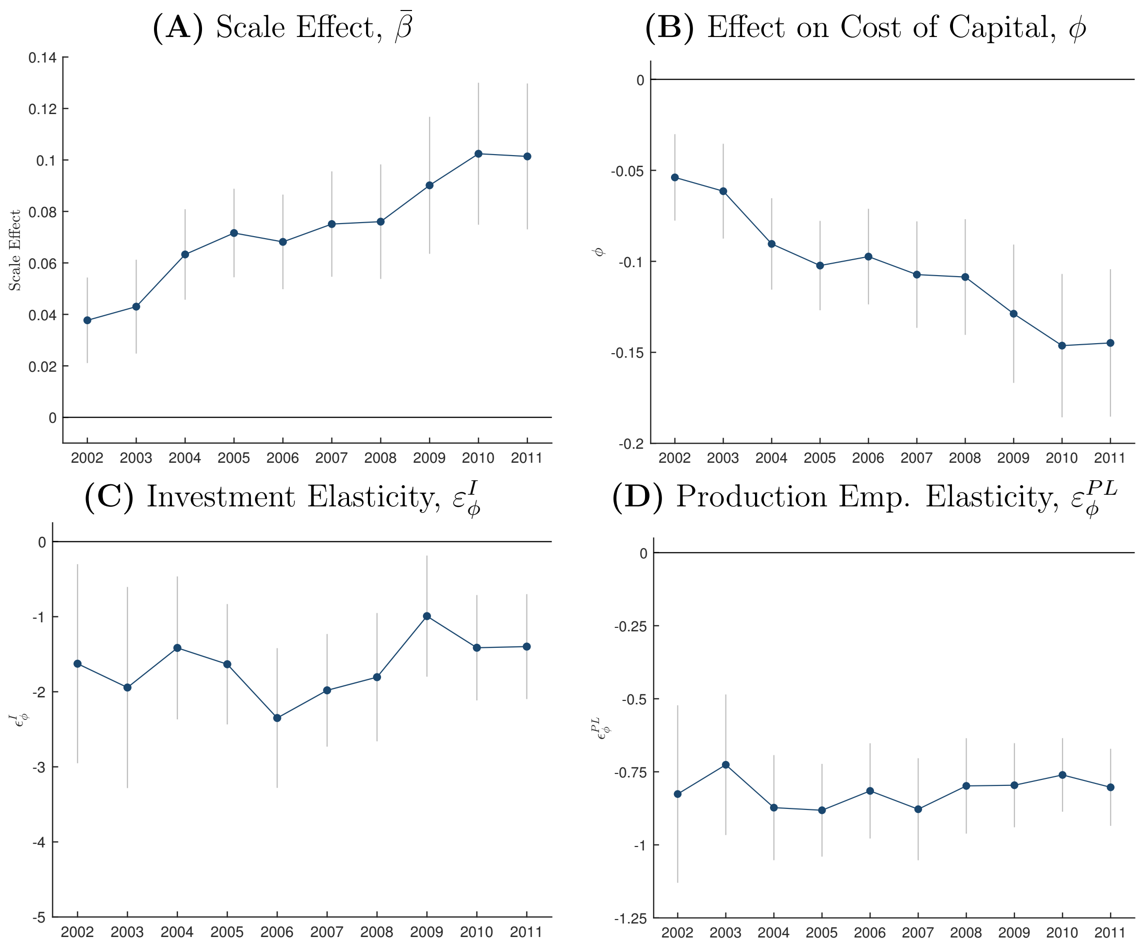


Figure A16: Scale, Cost of Capital, and Elasticity Estimates over Time
Notes: Figure A16 displays select model estimates over the 2002-2011 period using event study regression estimates from Equation (3.1). Panel (A) presents the scale effects implied by our reduced form estimates over the 2002-2011 period. Scale effects for year t are defined using equation 3.7 as $\bar{\beta}_t = s_J \beta_t^J + s_K \beta_t^K + s_L \beta_t^L$. Panel (B) displays estimates of the effect on the cost of capital. Effects for year t are defined using equation 3.7 as $\phi = -\hat{\bar{\beta}}_t/(s_K \eta)$. Panels (C) and (D) present estimates of the elasticity of investment and production labor, respectively, with respect to changes in the cost of capital over time. Elasticities are calculate as $\varepsilon_{\phi}^{I} = \beta^I/\phi$ and $\varepsilon_{\phi}^{L} = \beta^L/\phi$, respectively. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

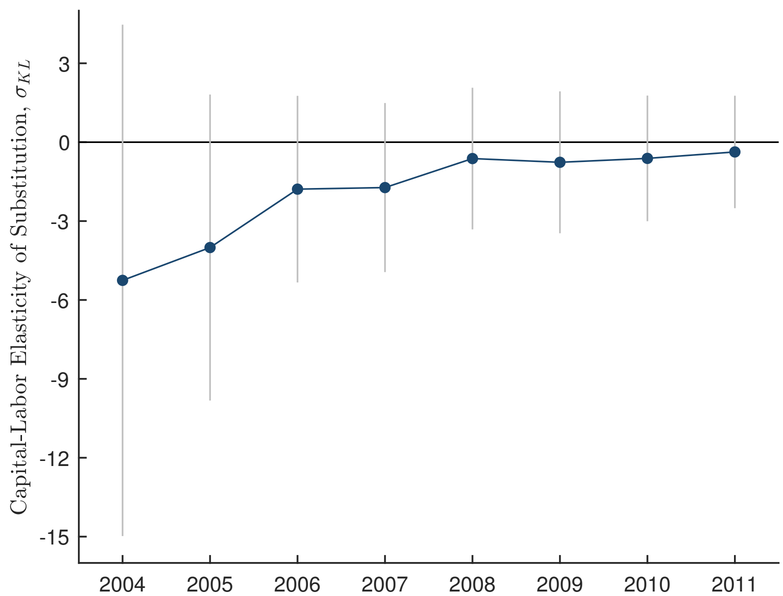


Figure A17: Capital-Production Labor Substitution over Time

Notes: Figure A17 estimates σ_{KL} over the 2004-2011 period. For each year t, σ_{KL} estimates are obtained using the estimated effects of bonus depreciation from Equation (3.1), an annualized long-differences estimate of the effect of bonus depreciation on revenue, and equations 3.4 and 3.6. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Appendix Tables

Table A1: Graph Data from Zwick and Mahon [2017]

	Figure 1, Panel A		Figure 1, Panel B		Differences (Bonus-Control)		Combined
	Control	Bonus	Control	Bonus	Panel A	Panel B	Event Study
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1996	6.553	6.553			0.013		0.013
1997	6.602	6.587			-0.002		-0.002
1998	6.482	6.478			0.009		0.009
1999	6.488	6.454			-0.021		-0.021
2000	6.480	6.467			0.000		0.000
2001	6.243	6.346			0.116		0.116
2002	6.078	6.218			0.153		0.153
2003	6.119	6.233			0.127		0.127
2004	6.251	6.352			0.114		0.114
2005			6.455	6.455		0.000	0.114
2006			6.604	6.614		0.010	0.124
2007			6.599	6.633		0.034	0.148
2008			6.569	6.705		0.136	0.250
2009			6.259	6.519		0.261	0.374
2010			6.398	6.519		0.121	0.235

Notes: Table A1 uses graph data from Zwick and Mahon [2017] as a way to compare our investment results. To construct this table, we first use the program WebPlotDigitizer (see https://apps.automeris.io/wpd/) to extract data points from Figure 1 in Zwick and Mahon [2017]. Columns (1)–(4) report the extracted data. Column (5) reports the differences between the first bonus and control series (i.e., column 2 minus column 1) normalizing the difference to 2000. Column (6) reports the differences between the second bonus and control series (i.e., column 4 minus column 3). Column (7) joins these two series making the assumption that there is no relative change between 2004 and 2005. We make this assumption given differences in how data are normalized between Panels A and B of Figure 1 in Zwick and Mahon [2017]. Figure A2 plots the series in column (7) of this table along with our estimates from Panel (A) of Figure 3.2. *Source:* Authors' calculations based on Zwick and Mahon [2017] graph data.

Table A2: Effects of Bonus Depreciation, Industry-Level Clustering

	(1) Log Investment	(2) Log Total Capital	(3) Log Employment	(4) Log Mean Earnings	(5) Log Total Revenue	(6) TFP
			Difference-in-Differences			
Bonus	0.1577**	0.0445	0.0791***	-0.0207**	0.0542	-0.0028
	(0.0642)	(0.0329)	(0.0224)	(0.0087)	(0.0344)	(0.0082)
	[0.014]	[0.176]	[0.000]	[0.017]	[0.115]	[0.733]
			Long Differences			
Bonus	0.2049	0.0778*	0.095**	-0.0273**	0.0808	-0.0153
	(0.1246)	(0.0416)	(0.04)	(0.0126)	(0.0717)	(0.0162)
	[0.100]	[0.061]	[0.018]	[0.030]	[0.260]	[0.345]
Plant FE	✓	✓	✓	✓	✓	✓
State×Year FE	✓	✓	✓	✓	✓	✓
PlantSize_{2001}×Year FE	✓	✓	✓	✓	✓	✓
TFP_{2001}×Year FE	✓	✓	✓	✓	✓	✓
FirmSize_{2001}×Year FE	✓	✓	✓	✓	✓	✓

Notes: Table A2 displays estimates describing the effect of bonus depreciation on various outcomes with standard errors clustered at the 4-digit NAICS level. Differences-in-differences subpanels show the Bonus×Post coefficient estimates from specifications in the form of Equation (3.2) while the Long Differences panel shows Bonus×$[t = 2011]$ coefficient estimates from specifications in the form of Equation (3.1). Outcome variables in Specifications (1)–(6) are Log Investment, Log Total Total Employment, Log Mean Earnings, Log Total Capital, Log Total Value of Shipments, and TFP. All Specifications include plant fixed effects, state-by-year fixed effects, plant size in 2001 bins interacted with year fixed effects, TFP in 2001 bins interacted with year fixed effects, and firm size in 2001 interacted with year fixed effects. Standard errors are presented in parentheses. p-values are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table A3: Effects of Bonus on Hours Worked and Materials

	(1) Log Prod. Hours	(2) Log Nonprod. Hours	(3) Log Materials
Bonus	0.0863***	0.0582*	0.0832**
	(0.0181)	(0.0311)	(0.0344)
	[0.000]	[0.061]	[0.016]
Plant FE	✓	✓	✓
State×Year FE	✓	✓	✓

Notes: Table A3 displays long differences estimates describing the effect of bonus depreciation on hours worked and on plants' use of materials. Standard errors are presented in parentheses and are clustered at the 4-digit NAICS-by-state level. p-values are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table A4: Effects of Bonus Depreciation, QWI Sample

	(1) Log(Emp)	(2) Log(Earn)	(3) % < HS	(4) % < 35 years	(5) % Female	(6) % Black	(7) % Hispanic
				Difference-in-Differences			
Bonus	0.097***	-0.031***	0.00259***	0.01285***	0.00822***	0.0012	0.00536***
	(0.0156)	(0.00547)	(0.000605)	(0.00151)	(0.0024862)	(0.00074)	(0.000969)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.105]	[0.000]
				Long Differences			
Bonus	0.135***	-0.0314***	0.00394***	0.0306***	0.0118***	0.00409***	0.00589***
	(0.0216)	(0.0078)	(0.000724)	(0.0022)	(0.00679)	(0.00153)	(0.0017)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.008]	[0.001]
Share2001			0.25	0.3	0.25	0.07	0.06
State×NAICS FE	✓	✓	✓	✓	✓	✓	✓
State×Quarter FE	✓	✓	✓	✓	✓	✓	✓
Pre-Period Growth FE					✓	✓	✓

Notes: Table A4 shows the effect of bonus depreciation on outcomes based on state-industry data from QWI. Differences-in-differences subpanels show the Bonus×Post coefficient estimates from specifications in the form of Equation (3.2) while the long difference subpanels show Bonus×$[t = 2011q3]$ coefficient estimates from specifications in the form of Equation (3.1). The outcomes across Specifications (1)–(4) are the Log of Total Employment, the Log of Mean Earnings, the fraction of employees with a high school degree or less Education, and the fraction of employees who are 35 years or younger. The outcomes across Specifications (5)–(8) are the fraction of female employees, the fraction of Black employees, and the fraction of Hispanic employees. All specifications include 4-digit NAICS-by-state fixed effects, State-quarter fixed effects, and pre-period growth rate bins in the outcome variable interacted with year fixed effects. Standard errors are presented in parentheses and are clustered at the 4-digit NAICS-by-state level. p-values are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* QWI and Zwick and Mahon [2017] data.

Table A5: Effects of Bonus Depreciation on Employment by Task-Content and Demographics: 2000-2010 Changes

	(1) All	(2) Routine	(3) Nonroutine	(4) Professional	(5) Admin.	(6) Production	(7) Services
				Cognitive Nonroutine	Cognitive Routine	Manual Routine	Manual Nonroutine
All Workers	0.0856***	0.126***	0.0264	0.0300	0.0876***	0.134***	0.0665
	(0.0201)	(0.0212)	(0.0217)	(0.0227)	(0.0235)	(0.0224)	(0.0467)
	Demographic Subgroups						
< HS Education	0.151***	0.159***	0.0906***	0.0806**	0.129***	0.159***	0.0873
	(0.0214)	(0.0220)	(0.0276)	(0.0335)	(0.0275)	(0.0228)	(0.0550)
Ages 18-35	0.143***	0.190***	0.0276	0.0262	0.113***	0.203***	0.0816
	(0.0272)	(0.0288)	(0.0349)	(0.0390)	(0.0400)	(0.0310)	(0.0942)
Female	0.126***	0.166***	0.0257	0.0545*	0.118***	0.143***	0.0406
	(0.0235)	(0.0251)	(0.0299)	(0.0317)	(0.0271)	(0.0316)	(0.0879)
Hispanic	0.154***	0.216***	-0.0398	0.0210	0.158	0.221***	-0.0197
	(0.0391)	(0.0427)	(0.0829)	(0.0971)	(0.106)	(0.0453)	(0.113)
Black	0.105**	0.158***	-0.0786	0.0977	-0.0134	0.162***	0.0408
	(0.0424)	(0.0448)	(0.0975)	(0.104)	(0.0952)	(0.0498)	(0.145)
Industry FE	✓	✓	✓	✓	✓	✓	✓
State×Year FE	✓	✓	✓	✓	✓	✓	✓

Notes: Table A5 displays coefficient estimates representing the effect of bonus depreciation on log employment at the state-industry level from 2000 to 2010. Specifications are estimated using subgroups of workers based on demographic categories and occupation task-content categories from Acemoglu and Autor [2011]. All regressions include industry and state-year fixed effects. Standard errors are clustered at state-industry level and presented in parentheses. *Source:* Authors' calculations based on Census, ACS, Acemoglu and Autor [2011], and Zwick and Mahon [2017] data.

Table A6: Effects of Bonus Depreciation, Interactions with Local Bonus Exposure

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Investment		Log Employment		Log Mean Earnings	
Bonus	0.1535**	0.1531**	0.0789***	0.0756***	-0.0206**	-0.0204**
	(0.0601)	(0.0642)	(0.0219)	(0.0222)	(0.0086)	(0.0087)
	[0.011]	[0.017]	[0.000]	[0.001]	[0.017]	[0.019]
Local Exposure	0.0349*	0.0407**	0.0127**	0.0149***	-0.0037	-0.0037
	(0.018)	(0.0178)	(0.0055)	(0.0049)	(0.0031)	(0.003)
	[0.053]	[0.022]	[0.021]	[0.002]	[0.233]	[0.217]
Bonus × Exposure	-0.0417	-0.0389	-0.0074	-0.0049	0.0045	0.0043
	(0.0283)	(0.0276)	(0.0083)	(0.0078)	(0.0037)	(0.0036)
	[0.141]	[0.159]	[0.373]	[0.530]	[0.224]	[0.232]
Plant FE	✓	✓	✓	✓	✓	✓
State×Year FE	✓	✓	✓	✓	✓	✓
PlantSize_{2001}×Year FE		✓		✓		✓
TFP_{2001}×Year FE		✓		✓		✓
FirmSize_{2001}×Year FE		✓		✓		✓

Notes: Table A6 displays difference-in-differences estimates and coefficients describing the interaction between difference-in-differences terms and variables capturing the share of local commuting zone exposure to bonus depreciation in 2001. Local exposure is defined as the percent of manufacturing employment in long duration industries in a given plant's commuting zone. Exposure variables are demeaned and standardized such that reported coefficients express the effect of moving from the 25th to the 75th percentile exposure across plants in our estimating sample. Due to disclosure restrictions, reported standard errors, displayed in parentheses, are clustered at the 4-digit NAICS level. p-values are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017].

Table A7: Effect of Bonus on Earnings, Controlling for Endogenous Worker Composition

	(1)	(2)	(3)	(4)	(5)
	Difference-in-Differences				
Bonus	-0.031***	-0.028***	-0.003	-0.003	0.007
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
	[0.000]	[0.000]	[0.495]	[0.549]	[0.126]
Industry × State FE	✓	✓	✓	✓	✓
State × Year FE	✓	✓	✓	✓	✓
Age Shares		✓	✓	✓	✓
Education Shares			✓	✓	✓
Race Shares				✓	✓
Sex Shares					✓

Notes: Table A7 displays difference-in-differences coefficients explaining the impact that bonus has on log earnings at the state-industry level. Column (1) does not include any controls for worker demographics and suggests bonus treatment lowered earnings by 3.1%. Columns (2)-(5) sequentially add controls for the share of young, less educated, non-white, and female workers in 2001, respectively, interacted with year fixed effects. Column (5) with controls for all demographic shares yields an estimate of 0.7, which is not statistically significant. Standard errors are presented in parentheses and are clustered at the 4-digit NAICS-by-state level. p-values are presented below in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* QWI and Zwick and Mahon [2017] data.

Table A8: Effect of Worker Composition on Observed Earnings, Decomposition Regressions

	(1) Treat Pre	(2) Treat Post	(3) Control Pre	(4) Control Post
Share Young	-0.548***	0.189**	-0.505***	-0.102
	(0.124)	(0.093)	(0.105)	(0.071)
	[0.000]	[0.043]	[0.000]	[0.149]
Share Highschool or Less	-3.298***	-3.683***	-4.436***	-3.810***
	(0.324)	(0.328)	(0.520)	(0.230)
	[0.000]	[0.000]	[0.000]	[0.000]
Share Nonwhite	0.096	0.078	0.893***	0.259***
	(0.132)	(0.080)	(0.247)	(0.082)
	[0.465]	[0.327]	[0.000]	[0.002]
Share Female	-0.549***	-0.644***	-0.904***	-0.390***
	(0.141)	(0.108)	(0.160)	(0.070)
	[0.000]	[0.000]	[0.000]	[0.000]
Industry × State FE	✓	✓	✓	✓
State × Year FE	✓	✓	✓	✓
Mean Share Young	0.308	0.254	0.303	0.236
Mean Share Highschool or Less	0.259	0.255	0.223	0.218
Mean Share Nonwhite	0.167	0.171	0.173	0.175
Mean Share Female	0.262	0.261	0.334	0.318

Notes: Table A8 presents regression estimates and independent variable means needed to to perform the Kitagawa-Oaxaca-Blinder decomposition in Appendix J. Each column estimates a panel earnings regression describing the impact of demographic shares on average wages with two-way fixed effects for a different sample. Column (1) displays estimates from 1997-2000 for treated industries. Column (2) displays estimates from 2001-2011 for treated industries. Columns (3) and (4) replicate the analysis of the first two columns for untreated industries. Standard errors are presented in parentheses and are clustered at the 4-digit NAICS-by-state level. p-values are presented below. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* QWI and Zwick and Mahon [2017] data.

Table A9: Effects of Bonus Depreciation and Manufacturing Trends

	(1) Log Investment	(2) Log Employment
Bonus	0.1457***	0.0577***
	(0.0339)	(0.0117)
	[0.000]	[0.000]
Treat×Skill Intensity	0.0577	0.0097
	(0.0541)	(0.0181)
	[0.286]	[0.592]
Treat×Capital Intensity	0.0259*	0.0028
	(0.0155)	(0.003)
	[0.095]	[0.351]
Treat×Trade Exposure	-0.0723**	-0.0413***
	(0.0296)	(0.0111)
	[0.015]	[0.000]
Treat×Robot Exposure	0.0187	0.0137***
	(0.012)	(0.0038)
	[0.119]	[0.000]
Plant FE	✓	✓
State×Year FE	✓	✓
Skill Intensity×Year FE	✓	✓
Capital Intensity×Year FE	✓	✓
Trade Exposure×Year FE	✓	✓
Robot Exposure×Year FE	✓	✓

Notes: Table A9 displays difference-in-differences estimates and coefficients describing the full set of interactions between the DD term and variables capturing all four manufacturing sector trends: Skill Intensity, Capital Intensity, Chinese Import Exposure, and Robotization. The outcome variable in Specification (1) is the Log of Investment. The outcome variable in Specification (2) is the Log of Total Employment. All specifications include state-by-year and plant fixed effects. To control for trends in the manufacturing sectors, both specifications include skill intensity bins interacted with year fixed effects, capital intensity bins interacted with year fixed effects, Chinese import exposure bins interacted with year fixed effects, and robotization bins interacted with year fixed effects. Standard errors are presented in parentheses and are clustered at the 4-digit NAICS-by-state level. p-values are presented in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on ASM, CM, Zwick and Mahon [2017], Acemoglu et al. [2016], and Acemoglu and Restrepo [2020] data.

Table A10: Translog Cost Function Estimation: σ_{LJ} Lower Bound

	(1) Baseline	(2) DD	(3) Hours	(4) Low s_K	(5) High s_K	(6) Low η	(7) High η
	Panel A: Estimated Parameters						
b_{ll}	0.250	0.250	0.250	0.247	0.247	0.250	0.250
b_{jj}	0.122	0.089	0.130	0.177	0.077	0.163	0.078
	(0.069)	(0.056)	(0.107)	(0.040)	(0.087)	(0.041)	(0.098)
b_{kk}	0.160	0.160	0.146	0.090	0.210	0.160	0.160
	(0.064)	(0.049)	(0.105)	(0.034)	(0.091)	(0.039)	(0.091)
b_{kl}	-0.144	-0.160	-0.133	-0.080	-0.190	-0.124	-0.166
	(0.035)	(0.030)	(0.050)	(0.020)	(0.048)	(0.021)	(0.049)
b_{kj}	-0.016	0.000	-0.013	-0.010	-0.020	-0.036	0.006
	(0.038)	(0.029)	(0.063)	(0.021)	(0.050)	(0.023)	(0.054)
b_{lj}	-0.106	-0.090	-0.117	-0.167	-0.057	-0.126	-0.084
	(0.035)	(0.030)	(0.050)	(0.020)	(0.048)	(0.021)	(0.049)
	Panel B: Production Function F-test p-values						
Cobb-Douglas	0.000	0.000	0.013	0.000	0.115	0.000	0.010
K Separability	0.000	0.000	0.000	0.000	0.000	0.000	0.000
J Separability	0.000	0.000	0.001	0.000	0.450	0.000	0.010
L Separability	0.000	0.000	0.007	0.000	0.000	0.000	0.001
Leontief	0.436	0.095	0.751	0.428	0.448	0.514	0.395
σ_{LJ}	0.29	0.40	0.22	0.13	0.49	0.16	0.44
	(0.15)	(0.14)	(0.22)	(0.03)	(0.51)	(0.09)	(0.22)
Demand elasticity	3.50	3.50	3.50	3.50	3.50	2.00	5.00
Cost shares:							
Production labor	0.50	0.50	0.50	0.55	0.45	0.50	0.50
Nonproduction labor	0.30	0.30	0.30	0.35	0.25	0.30	0.30
Capital	0.20	0.20	0.20	0.10	0.30	0.20	0.20
Effect on Cost of Capital, ϕ	-0.14	-0.12	-0.10	-0.27	-0.09	-0.23	-0.10

Notes: Table A10 presents estimates of translog cost parameters implied by estimated substitution elasticities corresponding to the columns in Table 3.8 and tests whether various production functions are consistent with the associated translog parameters. Panel A displays estimated translog cost parameters where σ_{LJ} is assumed to be equal to the lower bound implied by the model estimates in Table 3.8, $\hat{\sigma}_{LJ} = -(s_K/s_J)\hat{\sigma}_{KL}$. Panel B displays p-values from F-tests in which the null hypotheses are sets of conditions on the estimated translog parameters implying the specified production technologies. The null hypotheses tested are $H_0 : b_{kl} = b_{kj} = b_{jl} = 0$ (Cobb-Douglas), $H_0 : b_{kl} = b_{kj} = 0$ (Capital Separability), $H_0 : b_{kj} = b_{lj} = 0$ (J Separability), $H_0 : b_{kl} = b_{lj} = 0$, ($L$ Separability), and $H_0 : b_{ij} = -s_i * s_j \forall i \neq j$ (Leontief). Standard errors are presented in parentheses.
Source: Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table A11: Translog Cost Function Estimation: $\sigma_{LJ} = \max\{\sigma_{KJ}, \sigma_{KL}\}$

	(1) Baseline	(2) DD	(3) Hours	(4) Low s_K	(5) High s_K	(6) Low η	(7) High η
	Panel A: Estimated Parameters						
b_{ll}	0.184	0.159	0.165	0.133	0.220	0.215	0.151
	(0.071)	(0.055)	(0.119)	(0.099)	(0.061)	(0.042)	(0.101)
b_{jj}	0.056	-0.001	0.045	0.062	0.049	0.127	-0.020
	(0.134)	(0.103)	(0.219)	(0.138)	(0.126)	(0.080)	(0.191)
b_{kk}	0.160	0.160	0.146	0.090	0.210	0.160	0.160
	(0.064)	(0.049)	(0.105)	(0.034)	(0.091)	(0.039)	(0.091)
b_{kl}	-0.144	-0.160	-0.133	-0.080	-0.190	-0.124	-0.166
	(0.035)	(0.030)	(0.050)	(0.020)	(0.048)	(0.021)	(0.049)
b_{kj}	-0.016	0.000	-0.013	-0.010	-0.020	-0.036	0.006
	(0.038)	(0.029)	(0.063)	(0.021)	(0.050)	(0.023)	(0.054)
b_{lj}	-0.040	0.001	-0.032	-0.053	-0.029	-0.091	0.015
	(0.096)	(0.073)	(0.156)	(0.117)	(0.075)	(0.057)	(0.136)
	Panel B: Production Function F-test p-values						
Cobb-Douglas	0.000	0.000	0.000	0.000	0.000	0.000	0.000
K Separability	0.000	0.000	0.000	0.000	0.000	0.000	0.000
J Separability	0.676	0.991	0.837	0.653	0.696	0.111	0.915
L Separability	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Leontief	0.436	0.095	0.751	0.428	0.448	0.514	0.395
σ_{LJ}	0.73	1.01	0.79	0.73	0.74	0.39	1.10
	(0.64)	(0.49)	(1.04)	(0.61)	(0.67)	(0.38)	(0.91)
Demand elasticity	3.50	3.50	3.50	3.50	3.50	2.00	5.00
Cost shares:							
Production labor	0.50	0.50	0.50	0.55	0.45	0.50	0.50
Nonproduction labor	0.30	0.30	0.30	0.35	0.25	0.30	0.30
Capital	0.20	0.20	0.20	0.10	0.30	0.20	0.20
Effect on Cost of Capital, ϕ	-0.14	-0.12	-0.10	-0.27	-0.09	-0.23	-0.10

Notes: Table A11 presents estimates of translog cost parameters implied by estimated substitution elasticities corresponding to the columns in Table 3.8 and tests whether various production functions are consistent with the associated translog parameters. Panel A displays estimated translog cost parameters where σ_{LJ} is assumed to be equal the upper bound implied by the model estimates in Table A14, $\hat{\sigma}_{LJ} = \hat{\sigma}_{KJ}$. Panel B displays p-values from F-tests in which the null hypotheses are sets of conditions on the estimated translog parameters implying the specified production technologies. The null hypotheses tested are $H_0 : b_{kl} = b_{kj} = b_{jl} = 0$ (Cobb-Douglas), $H_0 : b_{kl} = b_{kj} = 0$ (Capital Separability), $H_0 : b_{kj} = b_{lj} = 0$ (J Separability), $H_0 : b_{kl} = b_{lj} = 0$, ($L$ Separability), and $H_0 : b_{ij} = -s_i * s_j \forall i \neq j$ (Leontief). Standard errors are presented in parentheses. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table A12: Morishima Elasticities of Substitution Parameter Estimates

	(1) Baseline	(2) Low s_K	(3) High s_K	(4) Low η	(5) High η
	Panel A: Morishima Elasticities of Substitution				
Production labor-capital, σ^M_{KL}	-0.248*	-0.121*	-0.380*	-0.142*	-0.354*
	(0.141)	(0.067)	(0.223)	(0.081)	(0.202)
Nonproduction labor-capital, σ^M_{KJ}	-0.070	-0.034	-0.107	-0.040	-0.100
	(0.188)	(0.091)	(0.290)	(0.107)	(0.268)
	Panel B: p-values for Substitutability Tests				
Substitutability of production labor	0.040	0.036	0.044	0.040	0.040
$H_0 : \sigma^M_{KL} \geq 0$					
Complementarity of non-production labor	0.355	0.354	0.356	0.355	0.355
$H_0 : \sigma^M_{KJ} \leq 0$					
Cost shares:					
Production labor	0.50	0.55	0.45	0.50	0.50
Nonproduction labor	0.30	0.35	0.25	0.30	0.30
Capital	0.20	0.10	0.30	0.20	0.20
Demand Elasticity, η	3.50	3.50	3.50	2.00	5.00

Notes: Panel (A) of Table A12 presents estimates of Morishima elasticities of substitution. Panel (B) presents p-values associated with tests of the substitutability and complementarity of the elasticities presented in Panel (A). Standard errors are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table A13: Constant Elasticity of Substitution Parameter Estimates

	(1) Baseline	(2) Low s_K	(3) High s_K	(4) Low η	(5) High η
	Panel A: CES Parameter Estimates				
Nonproduction Labor, ρ_1	-1.662	-1.248	-2.299	-3.659	-0.864
	(4.158)	(2.753)	(6.933)	(7.277)	(2.911)
Production Labor, ρ_2	5.034**	9.251**	3.628**	8.060**	3.824**
	(2.300)	(4.575)	(1.543)	(4.026)	(1.610)
	Panel B: Implied CES Substitution Elasticities				
Nonproduction Labor, $\frac{1}{1-\rho_1}$	0.376	0.445	0.303	0.215	0.537
	(0.587)	(0.545)	(0.637)	(0.335)	(0.838)
Production Labor, $\frac{1}{1-\rho_2}$	-0.248*	-0.121*	-0.380*	-0.142*	-0.354*
	(0.141)	(0.067)	(0.223)	(0.081)	(0.202)
	Panel C: p-values for Skill Complementarity Test				
$H_0 : \frac{1}{1-\rho_2} - \frac{1}{1-\rho_1} - 1 > 0$	0.004	0.003	0.006	0.000	0.016
Cost shares:					
Production labor	0.50	0.55	0.45	0.50	0.50
Nonproduction labor	0.30	0.35	0.25	0.30	0.30
Capital	0.20	0.10	0.30	0.20	0.20
Demand Elasticity, η	3.50	3.50	3.50	2.00	5.00

Notes: Panel (A) of Table A13 presents estimates of substitution parameters from a constant elasticity of substitution (CES) production function. Panel (B) presents the CES substitution elasticities implied by the results in Panel (A). Panel (C) tests the null hypothesis of $H_0 : \frac{1}{1-\rho_2} - \frac{1}{1-\rho_1} - 1 > 0$, consistent with the presence of skill complementarity of capital, across these models. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are presented in parentheses. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table A14: Additional Classical Minimum Distance Estimates of Production Elasticities

	(1) Baseline	(2) DD	(3) Hours	(4)	(5)	(6)
	Panel A: Estimated Parameters					
Demand elasticity, η	3.500	3.500	3.500	3.500	3.500	3.500
Labor-capital, σ_{KL}	-0.440	-0.603	-0.332	-0.106	-0.138	-0.474
	(0.346)	(0.305)	(0.498)	(0.144)	(0.142)	(0.952)
Nonproduction labor-capital, σ_{KJ}	0.733	1.006	0.786			
	(0.639)	(0.489)	(1.043)			
Equipment-structures, σ_{KS}					1.908	
					(0.603)	
Materials-capital, σ_{KM}						0.182
						(0.507)
	Panel B: Empirical Moments					
Revenue	0.075	0.051	0.075	0.075	0.075	0.075
Labor	0.116	0.101	0.086	0.097	0.097	0.097
Nonproduction labor	0.090	0.068	0.058			
Structures					0.041	
Materials						0.083
Capital	0.080	0.042	0.080		0.105	0.080
	Panel C: Model-Predicted Moments					
Revenue	0.069	0.060	0.052	0.065	0.064	0.057
Labor	0.109	0.098	0.080	0.094	0.094	0.091
Nonproduction labor	0.076	0.060	0.057			
Structures					0.041	
Materials						0.076
Capital	0.096	0.084	0.080		0.105	0.080
Cost shares:						
Labor	0.50	0.50	0.50	0.80	0.80	0.25
Nonproduction labor	0.30	0.30	0.30			
Structures					0.09	
Materials						0.65
Capital	0.20	0.20	0.20	0.20	0.11	0.10
Effect on Cost of Capital, ϕ	-0.14	-0.12	-0.10	-0.13	-0.23	-0.23

Notes: Table A14 presents classical minimum distance estimates across several alternative models. Column (1) reproduces column (1) of Table 3.8 for reference. Columns (2) and (3) demonstrate that these baseline results are robust to using difference-in-differences estimates and estimates on labor hours, respectively. Column (4) estimates a two input model of total labor employment and capital. Columns (5) and (6) consider three input models with either two types of capital or materials, respectively. Capital-labor substitution elasticities corresponds either to that of total capital and total labor, the elasticity of capital and production labor, or the elasticity of substitution between equipment capital and production labor. Standard errors are presented in parentheses. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table A15: Unconstrained Classical Minimum Distance Estimates of Production Elasticities

	(1) Baseline	(2) Low s_K	(3) High s_K	(4) Low η	(5) High η	(6) Est. η
	Panel A: Estimated Parameters					
Demand elasticity, η	3.500	3.500	3.500	2.000	5.000	3.858
						(3.115)
Production labor-capital, σ_{KL}	-0.509	-0.424	-0.594	-0.272	-0.759	-0.568
	(0.334)	(0.328)	(0.357)	(0.203)	(0.470)	(0.633)
Nonproduction labor-capital, σ_{KJ}	0.374	0.443	0.308	0.225	0.548	0.414
	(0.590)	(0.544)	(0.642)	(0.359)	(0.830)	(0.738)
	Panel B: Empirical Moments					
Revenue	0.075	0.075	0.075	0.075	0.075	0.075
Production labor	0.116	0.116	0.116	0.116	0.116	0.116
Nonproduction labor	0.090	0.090	0.090	0.090	0.090	0.090
Capital	0.080	0.080	0.080	0.080	0.080	0.080
	Panel C: Model-Predicted Moments					
Revenue	0.072	0.074	0.070	0.047	0.082	0.075
Production labor	0.115	0.116	0.115	0.108	0.118	0.116
Nonproduction labor	0.090	0.090	0.090	0.084	0.091	0.090
Capital	0.080	0.080	0.080	0.079	0.080	0.080
Cost shares:						
Production labor	0.50	0.55	0.45	0.50	0.50	0.50
Nonproduction labor	0.30	0.35	0.25	0.30	0.30	0.30
Capital	0.20	0.10	0.30	0.20	0.20	0.20
Effect on Cost of Capital, ϕ	-0.14	-0.30	-0.09	-0.24	-0.10	-0.13

Notes: Table A15 reproduces Table 3.8 from the main text by implementing an unconstrained classical minimum distance estimation procedure. Estimation is identical to that conducted in Table 3.8 with the exception that we do not impose the cost-minimization constraint $s_L\sigma_{KL} + s_J\sigma_{KJ} > 0$. Standard errors are presented in parentheses. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table A16: Capital-Labor Elasticity of Substitution with Cash Flow Constraints

	(1) Baseline	(2) Low s_K	(3) High s_K	(4) Low η	(5) High η
Production labor-capital, σ_{KL}	-0.515	-0.426	-0.608*	-0.294	-0.736
	(0.336)	(0.330)	(0.362)	(0.192)	(0.481)
Cash-flow expenditure share, s^b	0.027***	0.028***	0.026***	0.019***	0.030***
	(0.004)	(0.004)	(0.003)	(0.002)	(0.004)
Cost shares:					
Production labor	0.50	0.55	0.45	0.50	0.50
Nonproduction labor	0.30	0.35	0.25	0.30	0.30
Capital	0.20	0.10	0.30	0.20	0.20
Demand Elasticity, η	3.50	3.50	3.50	2.00	5.00

Notes: Table A16 presents estimates of elasticities of substitution between capital and production labor under financing constraints as described in Appendix K. Standard errors are presented in parentheses. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon [2017] data.

Table A17: Effects of Bonus Depreciation; NBER-CES Industry-Level Data

	(1) Log Prod. Emp.	(2) Log Nonprod. Emp.	(3) Log Capital
Bonus	0.179**	0.132*	0.122***
	(0.0731)	(0.0703)	(0.0434)
	[0.016]	[0.065]	[0.006]
Year FE	✓	✓	✓
NAICS FE	✓	✓	✓

Notes: Table A17 presents coefficient estimates representing the effect of bonus depreciation on manufacturing inputs at the aggregate industry level using data from NBER-CES. All coefficients are for the long difference, or the impact of bonus on outcomes by 2011 relative to 2001. Column (1) shows the impact on log production employment, column (2) shows the impact on log non-production employment, and column (3) shows the impact on log capital. All specifications include year and industry fixed effects. Standard errors clustered at the state-industry level are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on NBER-CES and Zwick and Mahon [2017] data.

Table A18: Model-Based Implications of Reduced-Form Estimates: NBER-CES Aggregate Version

	(1) Baseline	(2) Low s_K	(3) High s_K	(4) Low η	(5) High η
	Panel A: Scale Effect Estimates				
Scale Effect, $\bar{\beta}$	0.154**	0.157**	0.150**	0.154**	0.154**
	(0.064)	(0.068)	(0.060)	(0.064)	(0.064)
	Panel B: Allen Elasticities of Substitution				
Production labor-capital, σ_{KL}	-0.590	-0.501	-0.682	-0.337	-0.842
	(0.403)	(0.376)	(0.468)	(0.230)	(0.576)
Nonproduction labor-capital, σ_{KJ}	0.497	0.562	0.429	0.284	0.710
	(0.675)	(0.619)	(0.744)	(0.386)	(0.964)
	Panel C: p-values for Substitutability Tests				
Substitutability of production labor $H_0 : \sigma_{KL} \geq 0$	0.072	0.091	0.072	0.072	0.072
Complementarity of non-production labor $H_0 : \sigma_{KJ} \leq 0$	0.769	0.818	0.718	0.769	0.769
	Panel D: Cost of Capital Elasticity Estimates				
Effect on cost of capital, ϕ	-0.219**	-0.449**	-0.143**	-0.384**	-0.154**
	(0.091)	(0.193)	(0.057)	(0.159)	(0.064)
Capital, ε_{ϕ}^{K}	-0.554***	-0.271***	-0.850***	-0.317***	-0.792***
	(0.167)	(0.090)	(0.230)	(0.095)	(0.238)
Production Labor, ε_{ϕ}^{L}	-0.818***	-0.400***	-1.255***	-0.467***	-1.168***
	(0.081)	(0.038)	(0.140)	(0.046)	(0.115)
Non-production Labor, ε_{ϕ}^{J}	-0.601***	-0.294***	-0.921***	-0.343***	-0.858***
	(0.135)	(0.062)	(0.223)	(0.077)	(0.193)
Cost shares:					
Production labor	0.50	0.55	0.45	0.50	0.50
Nonproduction labor	0.30	0.35	0.25	0.30	0.30
Capital	0.20	0.10	0.30	0.20	0.20
Demand Elasticity, η	3.50	3.50	3.50	2.00	5.00

Notes: Table A18 presents several results relating our reduced-form estimates to model outcomes across several alternative calibrations of cost shares and η using aggregate manufacturing data. Panel (A) displays estimates of the scale effect defined in Equation (3.7). Panel (B) presents estimates of the Allen elasticities of substitution between capital and production labor and capital and non-production labor using equations (3.4) and (3.5), respectively. Panel (C) conducts hypothesis tests of the substitutability and complementarity of production and non-production labor, respectively. Panel (D) presents estimates of the effect of bonus depreciation on the cost of capital using the calculated scale effects in Panel (A) and Equation (3.7). It also presents estimates of the elasticity of capital, investment, production labor, and non-production labor with respect to this estimated change in the cost of capital. Standard errors are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on NBER-CES and Zwick and Mahon [2017] data.

Table A19: Industry-Level Estimates of Morishima Elasticities of Substitution

	(1) Baseline	(2) Low s_K	(3) High s_K	(4) Low η	(5) High η
	Panel A: Morishima Elasticities of Substitution				
Production labor-capital, σ^M_{KL}	-0.264	-0.129	-0.405	-0.151	-0.377
	(0.213)	(0.101)	(0.338)	(0.122)	(0.304)
Nonproduction labor-capital, σ^M_{KJ}	-0.046	-0.023	-0.071	-0.027	-0.066
	(0.255)	(0.124)	(0.393)	(0.145)	(0.364)
	Panel B: p-values for Substitutability Tests				
Substitutability of production labor	0.108	0.100	0.115	0.108	0.108
$H_0 : \sigma^M_{KL} \geq 0$					
Complementarity of non-production labor	0.428	0.427	0.428	0.428	0.428
$H_0 : \sigma^M_{KJ} \leq 0$					
Cost shares:					
Production labor	0.50	0.55	0.45	0.50	0.50
Nonproduction labor	0.30	0.35	0.25	0.30	0.30
Capital	0.20	0.10	0.30	0.20	0.20
Demand Elasticity, η	3.50	3.50	3.50	2.00	5.00

Notes: Panel (A) of Table A19 presents estimates of Morishima elasticities of substitution. Panel (B) presents p-values associated with tests of the substitutability and complementarity of the elasticities presented in Panel (A). Standard errors are presented in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations based on NBER-CES and Zwick and Mahon [2017] data.

Table A20: Industry and Aggregate Capital-Labor Elasticity of Substitution

	(1) Baseline	(2) Low s_K	(3) High s_K	(4) Low η	(5) High η
Industry Elasticity, σ_{KL}^{N}	-0.264	-0.129	-0.405	-0.151	-0.377
	(0.213)	(0.101)	(0.338)	(0.122)	(0.304)
Aggregate Elasticity, σ_{KL}^{agg}	-0.186	-0.061	-0.316	-0.085	-0.286
	(0.199)	(0.094)	(0.317)	(0.114)	(0.285)
Cost shares:					
Production labor	0.50	0.55	0.45	0.50	0.50
Nonproduction labor	0.30	0.35	0.25	0.30	0.30
Capital	0.20	0.10	0.30	0.20	0.20
Demand Elasticity, η	3.50	3.50	3.50	2.00	5.00

Notes: Table A20 reproduces industry-level Morishima elasticities of substitution from Table A19 and presents estimates of the aggregate elasticities of substitution between capital and labor implied by these estimates. Standard errors are presented in parentheses. *Source:* Authors' calculations based on ASM, CM, and Zwick and Mahon (2017) data.

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Biography

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