ARTICLE IN PRESS

Journal of Accounting and Economics xxx (xxxx) xxx

Contents lists available at ScienceDirect

Journal of Accounting and Economics

journal homepage: www.journals.elsevier.com/journal-of-accounting-and-economics

Fraudulent financial reporting and the consequences for employees[☆]

Jung Ho Choi [*], Brandon Gipper

Stanford Graduate School of Business, Stanford University, USA

ARTICLE INFO

JEL classification:
D83
J23
J31
M41
M48
M51

Keywords:
Wages
Employment growth
Accounting fraud
Information asymmetry

ABSTRACT

We combine U.S. Census data with SEC enforcement actions to examine employees' outcomes, such as wages and turnover, before, during, and after periods of fraudulent financial reporting. We find that fraud firms' employees lose about 50% of cumulative annual wages, compared to a matched sample, and the separation rate is much higher after fraud periods. Yet, employment growth at fraud firms is positive during fraud periods; these firms overbuild and hire new, lower-paid employees concurrent with the fraud, unlike firms in distress which tend to contract. When the fraud is revealed, firms shed workers, unwinding this abnormal growth and resulting in most of the negative wage consequences. Wage outcomes are particularly unfavorable in thin labor markets, and lower-wage employees, though unlikely to have perpetrated the fraud, experience more severe wage losses compared to higher-wage employees.

1. Introduction

Fraudulent financial reporting is an important issue in the economy. Large accounting scandals occur regularly (e.g., Enron, WorldCom, Waste Management, Toshiba, Luckin Coffee, Wirecard, and so on), and the consequences are usually significant. For

☆ 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 1668 (CBDRB-FY20-P1668-R8609). We thank Wayne Guay (Editor), Ray Ball, John Barrios, Phil Berger, Mary Billings, Nick Bloom, Hans Christensen, Steve Davis, Weili Ge (Discussant), Sophia Hamm, Mark Kim, Charles Lee, Sheffield E Lesure, Christian Leuz, Frank Limehouse, Brad Nathan (Discussant), Maureen McNichols, Darren Roulstone, Fiona Sequeira, Amit Seru, Catherine Schrand (Discussant), Sorabh Tomar, Lydia Wang, and workshop participants at American Accounting Association 2021 Annual Conference, Hawaii Accounting Research Conference, Ohio State University, New York University, Penn State FSRDC Conference, Santa Clara University, Stanford Summer Camp, University of Calgary, University of Chicago, and University of Southern California for helpful comments. We thank Mitchell Linegar, Caitlin McCarthy, Sara Malik, Nick Maletta, Fiona Sequeira, Xuan Su, and Eddie Yu for research assistance and Patty Dechow, Henry Laurion, and Richard Sloan for access to AAER data. 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. We thank Stanford University for funding and the Centers and Initiatives for Research, Curriculum & Learning Experiences for research assistance.

* Corresponding author.
E-mail address: jungho@stanford.edu (J.H. Choi).

https://doi.org/10.1016/j.jacceco.2024.101673
Received 5 March 2019; Received in revised form 26 October 2023; Accepted 8 January 2024
Available online 23 January 2024

Please cite this article as: Jung Ho Choi, Brandon Gipper, *Journal of Accounting and Economics* , https://doi.org/10.1016/j.jacceco.2024.101673

example, Karpoff et al. (2008b) find that firms lose about 29% of equity value when fraud is revealed. An extensive academic literature has also documented severe consequences of fraudulent reporting for other stakeholders, including customers, executives, and peer firms (e.g., Beatty et al., 2013; Desai et al., 2006; Sadka, 2006). However, prior papers rarely study labor market consequences, which can be large; for example, 17,000 workers lost jobs from the WorldCom fraud alone in June 2002 (Noguchi, 2002). We address two related questions. First, what are (the magnitudes of) the effects on employees and who is affected? Second, what types of real actions do executives take in conjunction with accounting fraud that result in these consequences for employees?

Accounting fraud has two distinct features—overbuilding and information frictions—that make it likely to have consequences for employees. First, executives take real actions, like overbuilding, to complement or decrease detection of accounting fraud. Prior literature shows that executives overinvest in physical capital during fraudulent reporting periods (McNichols and Stubben, 2008). Similarly, executives could show growth with excessive hiring while avoiding a high wage bill, for example, by replacing high-paid workers with lower-paid workers. Second, employees rely on firm-provided information when making career choices (e.g., Choi et al., 2023a; de Haan et al., 2023). If employees are misled about the prospects of the firm during the fraud, they could suffer along with other stakeholders. For example, excessive hiring could result in misled employees suffering later when these excesses are unwound, losing job-specific capital or job hunting in unfavorable conditions. A reasonable assumption is that executives use fraud to cover up distress. However, these overemployment and information friction features differ from those of firm distress, where executives fix problems with reasonable transparency, often with some contraction and a renewed focus on the core business (e.g., John et al., 1992). Observable distress enables workers to self-sort based on risk preferences and to demand wage premiums for risks imposed by distress (e.g., Brown and Matsa, 2016; Graham et al., 2023).

One important empirical challenge arises from our research questions; employee data are not commonly available. We use the Longitudinal Employer Household Dynamics (LEHD) and Longitudinal Business Database (LBD) datasets from the U.S. Census Bureau. These are an important data source for addressing questions related to employees in the United States (e.g., Hyatt and McEntarfer, 2012). These data contain workers' entire wage series across employers and a rich set of worker characteristics, such as age, education, gender, and employer location and industry. We combine this employer-employee data with the Securities and Exchange Commission's Accounting and Auditing Enforcement Releases (AAERs) to proxy for fraudulent financial reporting. Our final sample includes about 150 cases of fraud at firms employing a worker in one of 23 states over the period 1989–2008; we use wage data from 362 thousand workers who were employed at these firms in the years leading up to the accounting fraud. Although this U.S. Census data is powerful for addressing our questions, there are restrictions such that we can only show aggregate statistics (e.g., we cannot show percentiles) and cannot show aggregate statistics if the underlying count is low (e.g., we cannot show estimates which describe rare types of fraud).[1]

For our main tests, we examine employee wages and turnover during and after fraudulent financial reporting between fraud and control samples. To select the control workers, we propensity score match the fraud firms to control firms within industry and year prior to the misreporting. We find that existing employees at fraud firms, compared to the matched sample, have about 4% (9%) lower annual wages on average during (after) the fraudulent financial reporting. The present value of the average cumulative loss is 52% of a worker's annual wage, totaling $275 million for the average fraud firm.[2] As a basis of comparison, the present values of the average cumulative wage losses from the Clean Air Act, competition with China, and bankruptcy are 20%, 23%, and 59%, respectively (Autor et al., 2014; Graham et al., 2023; Walker, 2013).[3] The separation rate at fraud firms is higher during (after) the fraud period by 4% (18%) on average. After the fraud is revealed, displaced workers are more likely to leave the industry and even the county, taking their next job (if any) elsewhere. In addition, workers from fraud firms experience more multi-quarter spells without wages, suggesting more time spent between jobs.

These wage declines exist despite increased employment base growth at fraud firms *during* the accounting fraud, evidence of overbuilding by executives. Firms shed existing workers—i.e., those employed in the pre-fraud period—replacing them with new, lower-paid employees, causing a change to the employee mix. Plausibly, executives engineer this composition change to show headcount growth and keep the wage bill low. These results contrast with firms in distress, which lay off workers and have trouble attracting and retaining newer workers (Brown and Matsa, 2016; Caggese et al., 2019; Graham et al., 2023; John et al., 1992). Importantly, excluding fraud firms that go bankrupt does not affect our inferences. The bankruptcy-fraud and matched control firms subsample comprises only 7% of our main sample; these employees experience substantial wage losses, as both bankruptcy and fraud have compounding effects.[4]

[1] Moreover, we cannot provide a precise observation count in our analyses. In addition, the application process for using U.S. Census data for academic studies requires that individual states approve the project's use of data from that state. For an AAER case to enter our sample, the misreporting firm must have an employee in one of 23 participating states, among other sample criteria. About 26 approving states is typical (e.g., Walker, 2013; Tate and Liu, 2015, 2016; Dore and Zarutskie, 2017; Goldin et al., 2017; Graham et al., 2023; have 31, 23, 23, 25, 23, and 30 states, respectively).

[2] To compare magnitudes with other papers, we use estimates from Table 3 column 1. Our calculation is as follows: 52.4% cumulative wage losses × $50,340 average wages × 10,440 headcount = $275 million. This equals about 10% of estimated investor losses of $2819 million ($5550 total assets × 2.309 Tobin's Q × 22% losses of enterprise value estimated by Dyck et al., 2023).

[3] Walker's (2013) estimates range from 16% to 24% for a 9-year period starting with the implementation of the Clean Air Act. Autor et al.'s (2014) estimates range from 20% to 53% (with interquartile ranges and at the mean using both OLS and 2SLS estimates) for a 16-year period starting in 1992 as Chinese imports to the US increased. Graham et al.'s (2023) estimates range from 28% to 90% for a 7-year period starting in the year of bankruptcy. We compare our cumulative wage loss estimates to the specifications from these papers that most closely match our design.

[4] Refer to Internet Appendix (IA) Tables 1 and 2 (e.g., SEC, 2004; Hillegeist et al., 2004).

Ultimately, fraud firms have negative employment growth after the fraud concludes, unwinding the buildup.[5] This reversal affects both pre-fraud and newly hired employees. Sample splits show that employee displacement contributes substantially to the wage decline described above. Even the newly hired employees have cumulative wage losses equal to 30% of their annual wage.[6]

We exploit heterogeneity among employees to better understand the effects at both the labor market and the individual levels. First, we separately examine "thin" and "thick" labor markets, i.e., counties with few and many industry-specific employers before the fraud, respectively. The wage declines are stronger in thin labor markets, indicating that much of the effect likely comes from limited opportunities, consistent with the difficulty of job search in relatively sparse labor markets (e.g., Moretti, 2011). Second, we split on employee wages. We show the effects for the top 10% and bottom 90% of the pre-fraud wage distribution. Bottom 90% employees, who likely did not perpetrate the fraud (Schrand and Zechman, 2012), experience significantly negative wage losses from fraudulent financial reporting, unlike the top 10% employees who have mostly insignificant cumulative wage losses.

The results are robust to a variety of modeling choices. We vary our control sample in three ways to address several fraud-specific endogeneity concerns. First, using hand-collected data, we match firms in the fraud period with firms receiving similar temporal demand shocks. Second, we use employee characteristics to create a matched subsample of employees between fraud and control firms to show the results with comparable worker compositions at the two sets of firms. Third, we select a new control firm matched on firm fundamentals when the fraud is revealed (rather than commenced); this test shows how employees at fraud firms fare relative to employees at firms that are contracting in the year of the fraud's revelation. For our main tests, we use employer location and industry data within the LEHD to include specifications controlling for an extensive set of fixed effects to rule out county- and industry-specific pay levels, though we also show all results with additional specifications that utilize year-industry and, separately, year-industry-county fixed effects. Finally, we present all results controlling for sample attrition by imputing missing data with a new, nationwide panel from the Census Bureau, which indicates whether a worker has a job anywhere in the U.S. These various tests continue to show negative consequences of fraudulent financial reporting for employees.

We make several important contributions. First, our paper contributes to an extensive literature documenting other consequences of fraudulent financial reporting (e.g., Dyck et al., 2023; Erickson et al., 2004; Karpoff et al., 2008a, 2008b; Kedia and Philippon, 2009; McNichols and Stubben, 2008). Our analyses add to findings from these papers by measuring the costs of employee wages and turnover at the employee level, estimated at $275 million for the average fraud firm, an additional and previously unmeasured 10% on top of investor losses. Second, we contribute to another extensive literature documenting consequences for employees from a wide variety of shocks to firms (e.g., Autor et al., 2014; Couch and Placzek, 2010; Gibbons and Katz, 1991; Graham et al., 2023; Hummels et al., 2014; Jacobson et al., 1993; Jiang and Shen, 2018; John et al., 1992; Walker, 2013). We show evidence consistent with consequences from fraudulent financial reporting, where distinct mechanisms such as overbuilding then unwinding and changes to employee mix are relevant to employees. Third, our paper also generates policy considerations. The negative effects from fraud disproportionately impact workers in thin labor markets and low-paid workers, so policymakers might focus their prevention or damage mitigation efforts due to concerns about inequality or devastation of rural communities (e.g., as happened with WorldCom).[7]

2. Data and research design

2.1. Data

Our fraudulent financial reporting sample is the enforcement actions taken by the Securities and Exchange Commission (SEC), Accounting and Auditing Enforcement Releases (AAERs). We only require annual financial statements to be fraudulently misstated.[8] The AAER sample involves a tradeoff where Type I errors for identified misreporting are very low but sample size tends to be small and spread out over many years (Dechow et al., 2010; Karpoff et al., 2017). Our analyses focus on the misstated years cited in the AAER.

---

5 This result is generally consistent with evidence from Kedia and Philippon (2009) who use employment levels from Compustat and accounting misstatements. They find greater employee growth during the misstatement period and interpret it as overinvestment in labor. We build on their result by showing differences by employee cohorts and changes in employee mix. Additionally, we show wage effects.

6 Our tabulated and untabulated robustness tests indicate that the estimates of (post-period) cumulative wage losses for newly hired employees are negative, except for one specification. These wage losses are potentially ignored by executives who expect to complete the fraud undetected and avoid layoffs, which may benefit employees. We perform back-of-the-envelope calculations which compare the cumulative losses from our main analyses to alternative outcomes for workers using various assumptions about layoffs, employment growth, and fraud detection rates and find that frauds are ex ante negative for employees. We caution that these estimates are sensitive to assumptions, such as the detection rate.

7 In Senate testimony, Coates (2011) discusses another policy debate related to our paper, "While [various proposed policy reforms] have been characterized as promoting jobs, [these reforms change] the balance that existing securities laws and regulations have struck between the transaction costs of raising capital, on the one hand, and the combined costs of fraud risk and asymmetric and unverifiable information, on the other hand." Our findings provide evidence that costs to workers are likely to be among the costs of fraud, which adds nuance to a calculation of the net effect of how securities laws and regulations may promote jobs, although policymakers are likely to consider a package of policies instead of one policy alone (e.g., Isidro et al., 2020).

8 This sample identifies cases of accounting problems that can be connected to prosecutable, fraudulent behavior by executives (Schrand and Zechman, 2012). We use UC Berkeley CFRM's dataset. The CFRM database also excludes cases of simple bribes or disclosure violations without earnings misstatements. Many prior papers have used these enforcement actions across a range of topics, for instance, to estimate, describe, and measure effects of fraudulent financial reporting (e.g., Beneish, 1999; Dechow et al., 2011; Farber, 2005; Feroz et al., 1991; Groysberg et al., 2017; Holzman et al., 2021; Raghunandan, 2021; Zhou and Makridis, 2019).

That is, we test whether overbuilding (unwinding) begins in the year that the misreporting begins (after the misreporting concludes). We assume that misreporting is revealed to the public in the year after the misreporting concludes as well. This assumption is consistent with Karpoff et al.'s (2017) trigger event date, i.e., the date that the fraud is revealed to capital markets, which occurs 1.6 months after the misreporting period for the median case.[9]

We combine this AAER data with worker-firm matched data from the U.S. Census Bureau Longitudinal Employer-Household Dynamics (LEHD) to measure wage effects and worker displacement.[10] The LEHD data have a comprehensive coverage of workers, on average covering 96% of all private-sector jobs across years (e.g., Abowd et al., 2005). From the LEHD, we have two datasets. First, we have data from 23 states participating in the LEHD program. These data include wage data when the wages are covered by a state's unemployment insurance program and generally include salaries, bonuses, equity, tips, and other perquisites (e.g., meals, housing, and retirement contributions, among others) (US Bureau of Labor Statistics, 2016). We observe quarterly wages, which we aggregate to annual wages.[11] The data allow us to track the wages of workers who were employed at accounting-fraud firms but have since moved to other firms. Self-employed, unemployed, and workers who move to non-participating states are not observable in the LEHD data available to us. We also use the individual characteristics provided by the LEHD data to separate the effects of misreporting and employee characteristics on wages (e.g., gender, education, and experience). Second, we have data from all states in the U.S. (i.e., nationwide). These data include a variable indicating if a worker has positive wages from any state in a quarter; however, the data does not identify the wages nor the employer(s) where the worker earns wages.

For most analyses, we examine existing employees, i.e., those employed in the pre-fraud period. We require that employees be between 22 and 50 years old in the year prior to the fraud period, consistent with other papers (e.g., Autor et al., 2014; Davis et al., 2011); this requirement generally limits the sample to workers who are (or desire to be) full-time participants in the workforce. We require that a worker's annual real wages are higher than $10,000 to exclude temporary workers. We also use a sample of new employees, i.e., those hired during the fraud, that otherwise meet these same requirements. We match the fiscal year of the firm to the calendar year of the LEHD, which only measures data in calendar quarters.

To publicly report our results, Census policy requires that we test for the influence of large firms (in our case, large firms and large frauds). We also are restricted from mentioning any specific firm in our sample, so we cannot speculate which large frauds might give cause for concern. While the Census requirements may limit the analyses which we can present, these requirements also have the consequence such that any result in the paper is not driven by a small number of large frauds. If an analysis potentially reveals a firm's identity, for example, because fewer than 10 firms contribute to a presented statistic, we redesign the analysis or aggregate tabulated results to provide as much information as possible while meeting Census requirements.

2.2. Research design

Our main empirical tests measure wage effects for existing employees from the fraud using a difference-in-differences approach with coefficients of interest estimated in event time relative to employees of control firms from a matched sample. We estimate the following OLS specification characterizing workers' natural log of real wages (e.g., Benhabib and Alberto, 2018)[12]:

$$\text{Ln}(\textit{Annual Real Wages}_{j,\tau}) = \alpha + \textstyle\sum_{p=1,2,3}\beta_{1,p}\times \textit{Pre(t-p)}_{j,\tau} + \sum_{p=0,1,2}\beta_{2,p}\times \textit{Fraud(t+p)}_{j,\tau} + \sum_{p=0,1,2,3,4,5}\beta_{3,p}\times \textit{Post(T+p)}_{j,\tau} + \sum_{p=1,2,3}\beta_{4,p}\times \textit{Fraud Ind.}_j\times \textit{Pre(t-p)}_{j,\tau} + \sum_{p=0,1,2}\beta_{5,p}\times \textit{Fraud Ind.}_j\times \textit{Fraud(t+p)}_{j,\tau} + \sum_{p=0,1,2,3,4,5}\beta_{6,p}\times \textit{Fraud Ind.}_j\times \textit{Post(T+p)}_{j,\tau} + \sum\beta_m\,\text{Worker Controls}_{j,\tau} + \sum\beta_k\,\text{Fixed Effects}_{j,\tau} + \varepsilon_{j,\tau}. \quad (1)$$

Equation (1) indexes worker with j and calendar year with τ. Fraud periods vary in calendar time depending on the worker, so we include variables defined with t and T for fraud event-time indices. The period indicators (i.e., *Pre*, *Fraud*, and *Post*) span the sample except the baseline year, i.e., ***Pre(t-4)***, and serve as event-time fixed effects.

This research design allows measurement of effects from fraud to be dynamic over the misreporting's lifecycle. We treat the misreporting as having three distinct periods. (1) "Pre" is the three-year period prior to the beginning of the fraudulent misreporting, indicated by dummy variables ***Pre(t-3)***, ***Pre(t-2)***, and ***Pre(t-1)***, while ***Pre(t-4)*** is the baseline year.[13] (2) "Fraud" is the period identified

[9] In untabulated analyses, we match the trigger event dates from Call et al. (2018) by firm and misreporting period to AAERs, whose data is also derived from Karpoff et al. (2017). We confirm that about 78% of AAERs have trigger event dates that occur within a year of the last misreported annual financial statements.

[10] The Compustat-SSEL Bridge (CSB) (covering 1981–2005) and the Standard Statistical Establishment List (SSEL) (covering later years) primarily use CUSIPs to link Compustat to the Longitudinal Business Database (LBD). We supplement these links by matching Employer Identification Numbers and company name, address, and industry in both datasets. We merge the Computstat-LBD data with the LEHD files using the Employer Characteristics Files (ECF). These linking files are widely used in prior literature (e.g., Davis et al., 2011; Giroud and Mueller, 2017; Graham et al., 2023). Finally, we merge with CFRM using CIKs (current and historical).

[11] If a worker has multiple employers within a year that are observable, we aggregate wages across all employers to measure annual wages and use the highest annual wage among employers as the primary employer (i.e., the match) for the year. We do not drop any wage data, based on (e.g.) missing quarterly observations.

[12] Nominal wages from LEHD are scaled using the CPI to 2010 price levels to generate *Annual Real Wages*.

[13] We do not require existing employees to be at the fraud or control firms in years *Pre(t-4)* and *Pre(t-3)*. Thus, we have missing baseline periods for some employees. In IA Table 3, we adjust the specification to exclude indicators for pre-fraud periods *Pre(t-3)* and *Pre(t-2)*. This change effectively makes the average wage in three pre-fraud years, *Pre(t-4)*, *Pre(t-3)*, and *Pre(t-2)*, as the baseline. With this modification to the design, all sample employees have at least one observable wage in this modified baseline period. With this change, our inferences are unaffected.

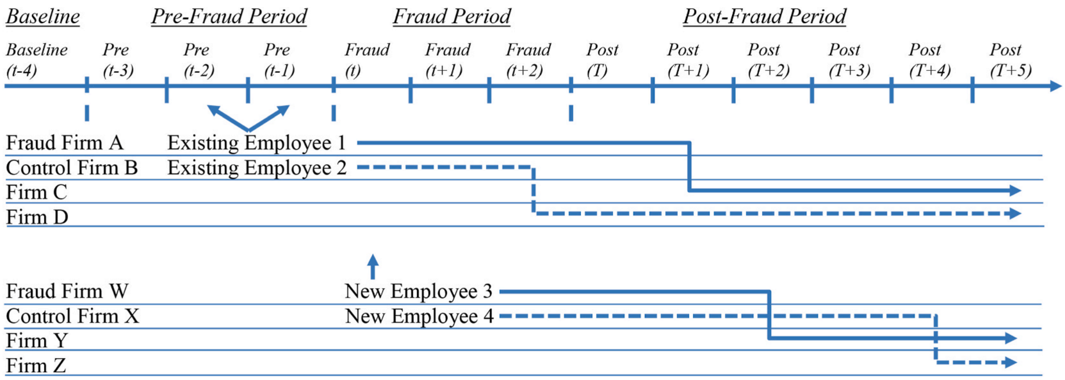

Fig. 1. A Fraud Example, Timeline, and Employees

This figure is a representation of the accounting-fraud timeline. The fraud is split into three periods. The "Baseline" period is the first year prior to the three periods of interest, $Baseline_{t\text{-}4}$. The "Pre-Fraud Period" extends for up to three years prior to the beginning of the fraud from the Accounting and Auditing Enforcement Release (AAER). We indicate these years as *Pre(t-3)*, *Pre(t-2)*, and *Pre(t-1)*. The "Fraud Period" extends for the length of the fraud and must result in misreporting of an annual financial statement (e.g., a single quarter of fraud that is corrected within a fiscal year would be excluded). The Fraud Period is determined by the start year and end year of financial misrepresentation from the AAER. We indicate these years as *Fraud(t)*, *Fraud(t+1)*, and *Fraud(t+2)*. Years relative to the first year of the fraud are indicated with lower case: *t*. For long-lasting frauds, we normalize this period to a maximum of three years by indicating additional fraud years as *Fraud(t+2)*. The "Post-Fraud Period" extends for up to six years after the conclusions of the fraud from the AAER. We indicate these years as *Post(T)*, *Post(T+1)*, *Post(T+2)*, *Post(T+3)*, *Post(T+4)*, and *Post (T+5)*. Years relative to the revelation year of the fraud are indicated with upper case: *T*.

We classify employees into two types. "Existing Employees" are workers at fraud (or control) firms prior to the beginning of the fraud indicated in the AAER. We require that existing employees worked for a fraud firm or a control firm for the last two years before a fraud firm engaged in accounting fraud, *Pre(t-2)* and *Pre(t-1)*. We do not require that we are able to observe the hire date if the employee works for the firm before our sample begins. Existing employees comprise our main sample across most tables, i.e., all except Table 5. "New Employees" are workers at fraud (or control) firms hired during the Fraud Period. We require that new employees were hired in the first year of a fraud period by a fraud firm or a control firm, *Fraud(t)*. We report results for new employees in Table 5 Panel C.

Our worker-year panel is not limited to the wage series at the treatment and control firms. We track employees as they move across firms. As an example, Existing Employee 1 might switch from Fraud Firm A to a third firm, Firm C, in event-time *Post(T+1)*. We show other examples above, i.e., Existing Employee 2 moving from Control Firm B to Firm D in *Fraud(t+2)*, New Employee 3 moving from Fraud Firm W to Firm Y in *Post(T+2)*, and New Employee 4 moving from Control Firm X to Firm Z in *Post(T+4)*, if Firms A, B, C, D, W, X, Y, and Z are in our sample states. We also have the wage series in the Baseline and Pre-Fraud Periods.

by the AAER, normalized to a maximum of three years with event-time sequenced by lowercase *t*, indicated by dummy variables *Fraud (t)*, *Fraud(t+1)*, and *Fraud(t+2)*. (3) "Post" is the six-year period after the fraud with event-time sequenced by uppercase *T*, indicated by dummy variables *Post(T)*, *Post(T+1)*, …, and *Post(T+5)*. This approach broadly follows McNichols and Stubben (2008).[14] Workers at the fraud and the matched control firms have identical treatment with respect to the event-time indicators, which correspond to sequential calendar years for one-, two- and three-year frauds (e.g., *Fraud(t)* for 2006 and *Post(T)* for 2007 for a one-year fraud misstating the 2006 financial report). We provide a detailed timeline in Fig. 1 that maps out the period indicators.[15]

$\beta_{4,p}$ represents estimated wages for workers at fraud firms incremental to those at control firms prior to the misreporting. These coefficients are, in essence, used to assess common trends with the presumption that the specific controls (e.g., fixed effects) remove appropriate variation from the two groups of workers in making the pre-period comparison. We expect the estimated coefficients to be insignificantly different from zero and not exhibit any pre-fraud trends. $\beta_{5,p}$ represents the incremental wages of fraud-firm employees for the fraud period. These are our first coefficients of interest; we infer the consequences for employees *during* the fraud from these coefficient estimates. $\beta_{6,p}$ represents the incremental wages for employees of fraud firms during the post-fraud periods. These are our second coefficients of interest; we infer the consequences for employees *after* the fraud from these coefficient estimates. Our

[14] In a departure from McNichols and Stubben (2008), we stack, rather than truncate, later years (more than three years) of the fraud. We stack these later years to document the complete employee trajectory during accounting fraud, to the extent allowed by the disclosure requirements of the U.S. Census Bureau. For measurement, a long-lasting fraud (four or more years) would have multiple calendar years where *Fraud(t+2)* equals one. For example, a seven-year fraud would have five calendar years coded as *Fraud(t+2)* for both the fraud firm and its matched control. *Post(T)* only equals one in the first year after the fraud ends, no matter its length. In IA Table 4, we replicate our main result with the McNichols and Stubben (2008) design, dropping later years of the fraud. Moreover, in IA Fig. 1, we replicate graphically the McNichols and Stubben (2008) result regarding excessive capital expenditures during the fraud period using the stacked design.

[15] Our assumption of the fraud sample period has an implication for the age range in our sample. We generally follow prior papers in selecting the age range between 22 and 50 (e.g., Davis et al., 2011; Graham et al., 2023). With the pre period window, the youngest a worker could be is 19 years old. With the average length of a fraud, two years, the oldest a worker could be is 58. Results are robust to using other age ranges, such as employees between 20 and 55 the year before the fraud period.

Table 1

Descriptive Statistics for Fraud and Control Firms This table compares fraud firms' to control firms' characteristics in the year prior to accounting fraud, *Pre(t-1)*. Accounting-fraud firms are identified by the AAER. Control firms are matched to fraud firms based on a propensity score estimated in Internet Appendix Table 5. The sample period is from 1989 to 2008. Appendix A defines variables. Statistical significance at the 10%, 5%, and 1% confidence levels is indicated by *, **, and ***, respectively. The number of observations is rounded to comply with disclosure requirements of the U. S. Census Bureau.

	(1)	(2)	(3)	
	Fraud Firms	Control Firms	T Tests of Differences (Fraud minus Control)	
			Difference	Standard Error
Size	6.482	6.310	0.172	(0.226)
Assets ($M)	5550	3359	2191*	(1266)
Sales Growth	0.228	0.237	−0.009	(0.036)
Return on Assets	0.080	0.087	−0.007	(0.012)
Leverage	0.257	0.233	0.024	(0.027)
Tobin's Q	2.309	2.259	0.050	(0.220)
Employee growth	0.140	0.151	−0.011	(0.045)
Avg. Annual Real Wages ($Th)	50.34	49.73	0.610	(3.905)
Employees (Headcount in U.S.)	10,440	8961	1479	(1908)
Observations	150	150		

Table 2

Descriptive Statistics for Employees of Fraud and Control Firms This table shows differences for averages of employees at fraud and control firms. Accounting-fraud firms in the sample commit financial misrepresentation from 1989 to 2008 according to the AAER. Fraud firms are matched with control firms using a propensity score estimated in Internet Appendix Table 5. Appendix A defines variables. Statistical significance at the 10%, 5%, and 1% confidence levels is indicated by *, **, and ***, respectively. The number of observations is rounded to comply with disclosure requirements of the U.S. Census Bureau.

	(1)	(2)	(3)	
	Fraud Firms	Control Firms	T-Test of Differences (Fraud minus Control)	
			Difference	Standard Error
Education	14.20	13.77	0.43	(0.279)
Age	37.90	36.68	1.22	(0.880)
Experience	17.70	16.91	0.79	(0.695)
Annual Real Wages	71,210	54,220	16,990**	(8310)
Ln(*Annual Real Wages*)	10.8	10.6	0.22	(0.158)
Female	0.449	0.439	0.01	(0.060)
Observations	361,800	404,400		

assumption for both coefficient sets is that wages would have evolved (in the absence of fraudulent financial reporting) for AAER-firm employees during and after the fraud as wages have evolved for control-firm employees. Moreover, while fraud is not exogenous, we use workers' wages at control firms to control for the wage impact of firm fundamentals and performance.

Worker controls include interactions among *Female Indicator*, *Education*, and *Experience* (e.g., Topel, 1991). In all specifications, we include worker and calendar year fixed effects as a part of difference-in-differences research design which control for each individual's wage level.[16] In our main specification, we include industry and county fixed effects matched to the worker's job in year τ to rule out industry- and county-level wage heterogeneity due to, for example, technology differences across industries or agglomeration differences across counties (e.g., Combes and Gobillon, 2015; Goldin and Katz, 1998). Appendix A defines all variables.

2.3. Present value calculations of wage effects

Besides these dynamic wage effects, we also use these coefficient estimates, $\beta_{5,p}$ and $\beta_{6,p}$, to calculate the present value of cumulative wage losses discounted to the year before the fraud (i.e., the end of *Pre(t-1)*) and the final year of the fraud (i.e., the end of

[16] The *Gender* and *Education* main effects are collinear with the worker fixed effects. *Experience* is also collinear with the main effects. When *Experience* is demeaned by worker, it is equivalent to a sequential count of the number of years in our sample; therefore, *Experience* is collinear with the worker and event-time fixed effects. The interactions are included in the regressions, but we do not report the estimates for parsimony. We find coefficients on these interaction terms, *Experience* × *Education* and *Female* × *Experience*, consistent with findings in prior literature (e.g., Heckman, 2003; Bertrand et al., 2010).

Table 3

Dynamics and Present Values of Earnings for Fraud Firm Employees This table reports estimates from OLS regression analyses estimating equation (1), presenting estimates for wage effects at fraud firms in the by-event-time years. Standard errors are in parentheses and calculated with clustering by pre-fraud employer (i.e., fraud firm or matched control firm). Present values and T-statistics are at the bottom of the table; Appendix B describes these calculations. Appendix A defines variables. Statistical significance at the 10%, 5%, and 1% confidence levels is indicated by *, **, and ***, respectively. All statistics are rounded to comply with requirements of the U.S. Census Bureau.

Dependent Variable = Ln(*Annual Real Wages*)	(1) Year Effects	(2) Year & Industry Effects	(3) Year & Industry & County Effects
Pre(t-3) × Fraud Ind.	-0.012	-0.001	-0.010
	(0.025)	(0.024)	(0.024)
Pre(t-2) × Fraud Ind.	-0.012	0.004	-0.009
	(0.029)	(0.029)	(0.029)
Pre(t-1) × Fraud Ind.	-0.021	-0.030	-0.024
	(0.032)	(0.037)	(0.034)
Fraud(t) × Fraud Ind.	-0.024	-0.028	-0.021
	(0.026)	(0.031)	(0.027)
Fraud(t+1) × Fraud Ind.	-0.032	-0.039	-0.033
	(0.039)	(0.042)	(0.038)
Fraud(t+2) × Fraud Ind.	-0.066	-0.064	-0.057
	(0.045)	(0.049)	(0.044)
Post(T) × Fraud Ind.	-0.058	-0.051	-0.044
	(0.041)	(0.039)	(0.039)
Post(T+1) × Fraud Ind.	-0.091*	-0.086**	-0.077*
	(0.047)	(0.044)	(0.042)
Post(T+2) × Fraud Ind.	-0.117**	-0.111**	-0.102**
	(0.050)	(0.045)	(0.044)
Post(T+3) × Fraud Ind.	-0.130**	-0.122**	-0.114**
	(0.055)	(0.053)	(0.049)
Post(T+4) × Fraud Ind.	-0.098*	-0.091*	-0.083*
	(0.058)	(0.052)	(0.049)
Post(T+5) × Fraud Ind.	-0.092	-0.081	-0.075
	(0.094)	(0.088)	(0.084)
Controls	Yes	Yes	Yes
Fixed Effects	Event-time, Year, Worker	Event-time, Year, Industry, Worker	Event-time, Year, Industry, County, Worker
Observations	8,720,000	8,720,000	8,720,000
R-squared	0.606	0.619	0.621
Pre-Fraud *(t-1)* Present Value	-0.524*	-0.495*	-0.445*
	(-1.82)	(-1.75)	(-1.67)
Pre-Revelation *(T-1)* Present Value	-0.475*	-0.437*	-0.400*
	(-1.95)	(-1.93)	(-1.83)

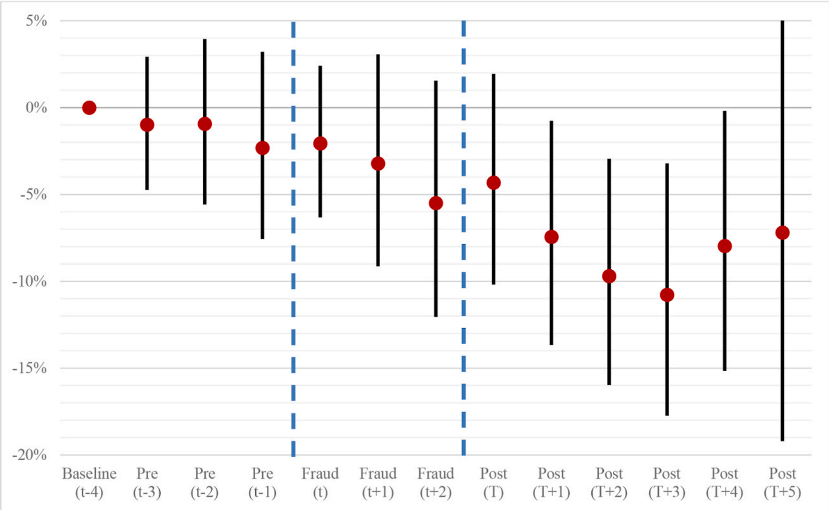

Fig. 2. Dynamics of Earnings for Fraud Firm Employees
This figure shows magnitude estimates from OLS regression analyses estimating equation (1): estimates for earnings effects at fraud firms (y-axis) in the by-event-time years (x-axis). Point estimates are incremental earnings of employees at fraud firms relative to those at matched control firms. We adjust the coefficient estimates from column (3) in Table 3 to percentages. We also show 90% confidence interval estimates as vertical bars through the point estimates. These standard errors are calculated with clustering by pre-fraud employer (i.e., fraud firm or matched control firm).

Fraud(t+2)). We convert the coefficients to percent wage changes relative to the baseline year and use an inflation-adjusted discount rate. Specifically, we discount using 4.47%—the inflation-adjusted average corporate BBB bond yield during our sample period—to the end of the pre-fraud period and the end of the fraud period. This discounted value approach and discount rate are like those in other papers (e.g., Davis et al., 2011; Graham et al., 2023; Sullivan and von Wachter, 2009). Corporate yield data come from the Bank of America and Moody's series maintained by the St. Louis Federal Reserve Bank. We use the variance-covariance matrix from the coefficient estimates and apply the delta method to calculate t-statistics for these present values. See Appendix B for an example.

2.4. Matching

We create a matched sample of fraud and non-fraud (control) firms for the difference-in-differences research design to control for economic characteristics of firms, which influence worker wages, except for the effects of accounting fraud. We require that control firms be covered by the LEHD data (i.e., these firms will have at least one employee hired in one of the 23 states for the pre-fraud and fraud periods). We perform a propensity score match within industry, using three-digit SIC industry codes from the firm-year prior to the misreporting. We follow Graham et al. (2023), who match on three-, two-, and one-digit SIC codes, although many prior papers examining financial reporting fraud match on two-digit SIC industry codes (e.g., Desai et al., 2006; Kedia and Philippon, 2009; Schrand and Zechman, 2012). If we do not find a matched, non-fraud firm within the same three-digit SIC-year group, we repeat the matching process to find a matched firm within the same two-digit or one-digit SIC-year group. In our main sample, 88% of matches are based on the three-digit SIC industry. Matches from two-digit or one-digit SIC industries comprise the remaining 12% of matches.[17]

We match on firm characteristics from the year prior to the fraud. We do so for two reasons. First, this is the common approach in prior literature (e.g., McNichols and Stubben, 2008; Schrand and Zechman, 2012). Second, executives already take real (and different) actions in the first year of the fraud, which would adversely impact the match given our intent to control for the wage impact of firm characteristics unrelated to fraud. For each fraud firm, we match to one non-fraud firm that has the closest propensity score to that matched fraud firm without replacement. We require that the propensity score difference be less than or equal to 0.25 (Imbens and Rubin, 2015). We estimate the following probit model on the CFRM-Compustat-LBD-LEHD sample to obtain firm-year scores for matching (indexing firms with i and fraud event-time with t):

$$\textit{Fraud-Firm Indicator}_{i,t-1} = \beta_0 + \beta_1 \times \textit{Size}_{i,t-1} + \beta_2 \times \textit{Sales Growth}_{i,t-1} + \beta_3 \times \textit{Return on Assets}_{i,t-1} + \beta_4 \times \textit{Leverage}_{i,t-1} + \beta_5 \times \textit{Tobin's } Q_{i,t-1} + \beta_6 \times \textit{Employee Growth}_{i,t-1} + \beta_7 \times \text{Ln}(\textit{Avg. Annual Real Wages}_{i,t-1}) + \varepsilon_{i,t-1}. \quad (2)$$

Our motivating principle in selecting matching variables for Equation (2) is to choose matched control firms that have economic characteristics which influence worker wages, similar to fraud firms, except for the effects of the fraud. We rely on prior literature that has shown firm size, growth, performance, leverage, and average pay levels are relevant for employee pay and composition (e.g., Berk, 2010; Chemmanur et al., 2013; Ouimet and Zarutskie, 2014; Dore and Zarutskie, 2017; Graham et al., 2023). For example, we control

[17] U.S. Census disclosure restrictions prevent additional granularity to disclose our match statistics. Results are qualitatively similar (i.e., same sign and significant at conventional levels with two-sided tests) when using only two-digit SIC industry codes.

Table 4

Dynamics of Displacement for Employees This table shows dynamics of displacement for employees. Panel A reports averages and differences of employee displacement at fraud and matched control firms. Columns (1)–(3) show percentages of employees who are displaced from either the firm, the industry, or the county. Columns (4)–(6) show percentages of employees who are displaced from the firm and leave either the industry or the county. Across all columns, displacement is measured with the employee's *next* job after matched employment during periods *Pre(t-1)* and *Pre(t-2)*, i.e., the next job after the fraud or matched control firms. Statistical significance at the 10%, 5%, and 1% confidence levels is indicated by *, **, and ***, respectively. All statistics are rounded to comply with requirements of the U.S. Census Bureau. This table shows dynamics of displacement for employees. Panel B reports estimates from OLS regression analyses estimating equation (1), presenting estimates for wage effects at fraud firms in the by-event-time years. In column (1) (column (2)), the sample includes workers who leave (remain with) the fraud or control firm prior to or in (through at least) period *Post (t+5)*, i.e., leaves (stays) at the latest three years after the fraud concludes. Standard errors are in parentheses and calculated with clustering by pre-fraud employer (i.e., fraud or control firm). Present values and T-statistics are at the bottom of the panel; Appendix B describes these calculations. Appendix A defines variables. Statistical significance at the 10%, 5%, and 1% confidence levels is indicated by *, **, and ***, respectively. For PVs, significance at the 10% confidence level for a one-tailed test is indicated by ˆ. All statistics are rounded to comply with requirements of the U.S. Census Bureau. This table shows dynamics of displacement for employees. Panel C reports estimates from OLS regression analyses estimating equation (1) with an alternative dependent variable: the number of quarters within the calendar year where the worker has zero wages (i.e., 0, 1, 2, 3, or 4). For these tests, we use the balanced sample of worker-years with nationwide data indicating whether the worker has wages in any state. Standard errors are in parentheses and calculated with clustering by pre-fraud employer (i.e., fraud or control firm). Appendix A defines variables. Statistical significance at the 10%, 5%, and 1% confidence levels is indicated by *, **, and ***, respectively. All statistics are rounded to comply with requirements of the U.S. Census Bureau.

Panel A: Absolute Displacement

		(1)	(2)	(3)	(4)	(5)	(6)
		% Employees Who Leave Firm / Industry / County			% Employees Who Leave Firm and Industry / County		
		Fraud	Control	Differences	Fraud	Control	Differences
Fraud(t)	*Firm*	17.7%	12.7%	5.0%**	-	-	-
	Industry	18.5%	11.8%	6.7%*	12.9%	10.7%	2.2%
	County	15.2%	12.7%	2.5%	10.1%	8.2%	1.9%
Fraud(t+1)	*Firm*	33.8%	31.3%	2.5%	-	-	-
	Industry	25.4%	27.7%	-2.3%	22.3%	24.9%	-2.6%
	County	27.5%	24.6%	2.9%	19.4%	19.9%	-0.5%
Fraud(t+2)	*Firm*	52.2%	46.5%	5.7%	-	-	-
	Industry	37.0%	42.9%	-5.9%	29.7%	37.6%	-7.9%**
	County	36.8%	35.8%	1.0%	25.7%	28.2%	-2.5%
Post(T)	*Firm*	51.0%	32.4%	18.6%**	-	-	-
	Industry	43.4%	29.4%	14.0%**	37.9%	27.7%	10.2%*
	County	37.6%	26.5%	11.1%**	31.7%	22.0%	9.7%**
Post(T+1)	*Firm*	59.2%	40.5%	18.7%**	-	-	-
	Industry	49.7%	35.7%	14.0%**	44.1%	34.2%	9.9%*
	County	43.8%	31.5%	12.3%**	38.1%	27.2%	10.9%**
Post(T+2)	*Firm*	65.6%	44.8%	20.8%***	-	-	-
	Industry	57.3%	40.4%	16.9%***	51.3%	38.6%	12.7%**
	County	49.2%	35.2%	14.0%**	44.3%	31.6%	12.7%**
Post(T+3)	*Firm*	68.4%	49.7%	18.7%**	-	-	-
	Industry	58.4%	44.2%	14.2%**	53.5%	42.7%	10.8%*
	County	53.0%	38.5%	14.5%**	46.0%	35.0%	11.0%**
Post(T+4)	*Firm*	71.9%	55.5%	16.4%**	-	-	-
	Industry	61.6%	48.5%	13.1%**	56.6%	47.0%	9.6%*
	County	55.3%	42.2%	13.1%**	49.1%	39.0%	10.1%**
Post(T+5)	*Firm*	75.5%	58.0%	17.5%**	-	-	-
	Industry	66.9%	50.9%	16.0%**	61.1%	49.7%	11.4%*
	County	59.7%	43.8%	15.9%***	52.4%	40.7%	11.7%**

Panel B: Wages for Leavers vs. Stayers

Dependent Variable = Ln(*Annual Real Wages*)	(1) Leavers	(2) Stayers
Pre(t-3) × Fraud Ind.	-0.026	-0.011
	(0.023)	(0.029)
Pre(t-2) × Fraud Ind.	-0.040	-0.014
	(0.029)	(0.035)
Pre(t-1) × Fraud Ind.	-0.028	-0.042
	(0.037)	(0.033)
Fraud(t) × Fraud Ind.	-0.006	-0.011
	(0.034)	(0.023)
Fraud(t+1) × Fraud Ind.	-0.013	-0.018
	(0.039)	(0.042)
Fraud(t+2) × Fraud Ind.	-0.053	-0.030
	(0.053)	(0.039)
Post(T) × Fraud Ind.	-0.005	-0.017
	(0.044)	(0.042)
Post(T+1) × Fraud Ind.	-0.047	-0.017
	(0.052)	(0.046)
Post(T+2) × Fraud Ind.	-0.100**	-0.048
	(0.049)	(0.052)
Post(T+3) × Fraud Ind.	-0.099*	-0.081
	(0.052)	(0.057)
Post(T+4) × Fraud Ind.	-0.105**	-0.040
	(0.052)	(0.055)
Post(T+5) × Fraud Ind.	-0.143*	-0.023
	(0.076)	(0.078)
Controls	Yes	Yes
Fixed Effects	Event-time, Year, Industry, County, Worker	Event-time, Year, Industry, County, Worker
Observations	4,633,000	4,087,000
R-squared	0.578	0.686
Pre-Fraud *(t-1)* Present Value	-0.399^	-0.212
	(-1.36)	(-0.77)
Pre-Revelation *(T-1)* Present Value	-0.387*	-0.184
	(-1.67)	(-0.77)

Panel C: Zero-wage Quarters within the Calendar Year

Dependent Variable = *Count of Zero-wage Quarters*	(1) Year Effects	(2) Year & Industry Effects	(3) Year & Industry & County Effects
Pre(t-3) × Fraud Ind.	0.096	0.103*	0.098
	(0.060)	(0.062)	(0.062)
Pre(t-2) × Fraud Ind.	0.046	0.062	0.046
	(0.062)	(0.066)	(0.068)
Pre(t-1) × Fraud Ind.	0.057	0.065	0.055
	(0.067)	(0.070)	(0.072)
Fraud(t) × Fraud Ind.	0.024	0.027	0.020
	(0.024)	(0.025)	(0.025)
Fraud(t+1) × Fraud Ind.	0.065	0.066	0.068
	(0.085)	(0.089)	(0.088)
Fraud(t+2) × Fraud Ind.	0.082	0.074	0.076
	(0.050)	(0.053)	(0.053)
Post(T) × Fraud Ind.	0.078	0.070	0.068
	(0.048)	(0.052)	(0.052)
Post(T+1) × Fraud Ind.	0.154***	0.148**	0.145**
	(0.058)	(0.060)	(0.060)
Post(T+2) × Fraud Ind.	0.226**	0.219**	0.216**
	(0.089)	(0.092)	(0.093)
Post(T+3) × Fraud Ind.	0.167**	0.159**	0.160**
	(0.066)	(0.067)	(0.068)
Post(T+4) × Fraud Ind.	0.119*	0.110*	0.110*
	(0.064)	(0.065)	(0.066)
Post(T+5) × Fraud Ind.	0.135*	0.126*	0.129*
	(0.071)	(0.070)	(0.071)
Controls	Yes	Yes	Yes
Fixed Effects	Event-time, Year, Worker	Event-time, Year, Industry, Worker	Event-time, Year, Industry, County, Worker
Observations	9,554,000	9,554,000	9,554,000
R-squared	0.417	0.422	0.424

for size because larger firms' employees tend to have higher wages but lower wage growth. In IA Table 5, we report the results of estimating Equation (2). Where these variables have been included in fraud models in prior literature, we find consistent results (e.g., Farber, 2005; Schrand and Zechman, 2012).

2.5. Sample description

Table 1 Panel A provides comparisons of our matched fraud and non-fraud (control) firms. In total, our sample contains about 150 fraud and 150 control firms. Our matching process described above generates a reasonably well-balanced sample, with only some difference in *Assets* between the two groups; this difference is perhaps unsurprising because we do not match firms using *Assets* (instead we match firms using *Size*, which is defined as natural log of *Assets,* due to skewness of *Assets*). We do not find significant differences between fraud and control firms when comparing *Size*, *Sales Growth*, *Return on Assets*, *Leverage*, *Tobin's Q*, *Employee Growth*, *Avg Annual Real Wages*, and *Employees*.[18] The average, firm-wide annual wages are comparable for fraud and matched control firms and equal to about $54 or $55 thousand normalized to 2010 CPI price levels.[19] IA Table 7 gives descriptive statistics of firm characteristics for fraud firms with LEHD data, i.e., our sample, and all fraud firms with Compustat data. Firms with employees in more states have a higher likelihood of entering the LEHD data, so we expect our sample to contain larger and more mature firms. This is consistent with the differences from IA Table 7; specifically, our sample fraud firms are larger, more profitable, have lower leverage, and have lower growth prospects than the typical fraud firm.

Table 2 presents descriptive statistics on the individual characteristics of employees of fraud and control firms. This sample includes

[18] Other pre-fraud period comparisons of the fraud and control firms indicate that the matches are good. We test differences in all firm characteristics in periods *Pre(t-4)*, *Pre(t-3)*, and *Pre(t-2)*; the characteristics between fraud and control firms are not different from each other in 18 out of 21 tests as shown in IA Table 6. We also matched on other subsamples of these variables, including total assets instead of the natural log of total assets, and our findings are qualitatively unaffected.

[19] As a comparison, average per capita income in the U.S. in 2019 was $33 thousand using the Census's Current Population Survey at 2010 price levels.

existing employees who work for the fraud or control firm in the two years prior to the fraud period, that is, *Pre(t-2)* and *Pre(t-1)*. These data (and calculated differences) are from the last year of the pre-fraud period, *Pre(t-1)*. Employees have similar education and gender compositions at fraud and control firms. While not statistically different, employees at fraud firms are older by a year and, consequently, have slightly more experience. The annual real wage for individual workers in our sample is equal to about $71 thousand at fraud firms and $54 thousand at control firms.[20] The significant wage-level difference is the result of skew; logged wages are not significantly different between fraud and control firms.[21]

3. Consequences of fraud for employees

3.1. Employee wages and displacement

We first analyze employee wages and displacement around accounting fraud. Table 3 contains our main results regarding employee wages, including estimates of dynamic wage effects during and after fraudulent financial reporting and calculations for the wage effect as present values before the fraud and before the fraud's revelation. Across columns, we increase the number of fixed effects; in columns 1, 2, and 3, we estimate models with worker effects and (1) year effects, (2) year & industry effects, and (3) year, industry, & county effects, respectively. For the dynamic wage effects, we consistently find negative wage coefficients in the post-fraud periods for employees who work(ed) at fraud firms. However, wage drops during the fraud period are not significant at conventional levels. The magnitudes for the post-fraud period are meaningfully negative, ranging from −4% to −13%. Therefore, the worst years for workers of fraud firms in terms of wage declines are *Post(T+1)* through *Post(T+4)*, with slight increases in the final years. We depict the coefficients from column 3 in Fig. 2.

In IA Fig. 2, we show our results separately for one-, two-, and three-year-and-longer frauds. Each subsample shows comparable wage effects, though year-by-year wage effects for frauds lasting three years and longer are only significant at conventional levels for year *Post(T+5)*. We attribute this to statistical power because wage loss point estimates are larger in some years, e.g., *Post(T+3)*, but still insignificant.

We also document evidence for common trends using the first three coefficient estimates. In all three columns, we observe that employees in the pre-fraud period have similar, insignificantly negative wage changes compared with workers at control firms. Overall, these tests indicate that the research design, including the various fixed effects, removes much of the variation from shocks that could pre-date the fraud. The onset of statistically significantly negative wage effects start after the fraud is revealed.

At the bottom of Table 3, we calculate present value calculations for cumulative, estimated losses as a percent of workers' annual wages using two different samples and two alternative dates. From the regression coefficients shown in Table 3, we estimate the present value of cumulative losses at the beginning of the fraud, "Pre-Fraud," and at the beginning of the post-fraud period, "Pre-Revelation." Using estimates from column 1, we find that workers experience cumulative wage losses of about 52.4% of their annual wages measured at the beginning of the fraud and 47.5% measured at the beginning of the post-fraud period. These cumulative wage losses are greater than those from economic shocks such as firms offshoring work (12%), being exposed to certain regulations (20%), and competition with China (23%) (Autor et al., 2014; Hummels et al., 2014; Walker, 2013), but smaller than one from economic shocks such as firm bankruptcy (59%) (Graham et al., 2023). Columns 2 and 3 show somewhat smaller estimates. The magnitudes are economically meaningful and primarily driven by wage losses after the fraud is revealed.

We analyze the displacement of employees in three dimensions: turnover, wages for leavers and stayers, and unemployment spells. In Table 4 Panel A, we examine whether employees of fraud firms are more likely to leave the firm, industry, and county during or after fraud periods. We calculate percentages of employees displaced from (i) the firm, (ii) the industry, or (iii) the county for all during fraud and post-fraud years. For industry and county, we calculate the change based on the industry and location of the employee's *next* job.[22] Similarly, we calculate percentages of conditional displacement, i.e., employees displaced from (iv) the fraud or control firm and the industry or (v) the fraud or control firm and the county.

We present these displacement statistics for employees of fraud firms in columns 1 and 4 and control firms in columns 2 and 5. Initial employee displacement rates from the firm, industry, and county for employees who leave the control firms are 13%, 12%, and 13%, respectively. The existing employees of fraud firms are more likely to leave the firm by 5% and leave the industry by 6.7% in the first year of the fraud. In later years of the fraud, employees who leave the fraud firm do not incrementally leave the industry for their next job, suggesting that before fraud revelation, fraud-affiliated employees find work within the industry. After revelation, fraud firm employee displacement increases substantially. Employees are significantly more likely to leave the fraud firm, the industry, and the county for their next job and more likely to leave the industry and county conditional on leaving their job.

In Table 4 Panel B, we show dynamics of worker wages with a sample split based on worker displacement at both fraud and matched control firms. We examine the subsample of employees who leave by the third year in the post-fraud period ("leaver"); this subsample includes leavers from both fraud and matched control firms. These results are shown in column 1. Leavers of fraud firms experience wage

[20] The employees in our sample have wages greater than the firm-wide averages. Our main sample focuses on existing employees with two years of work experience at the firm. The firm-wide averages include all employees in the LBD data, including temporary workers and those with shorter tenure, who have lower wages (Topel, 1991).

[21] In a test that addresses potential differences in employee types at fraud and control firms, we match employees using individual characteristics, including wage deciles. See Section 4.3.

[22] If the worker has a subsequent, missing observation, we consider them to have left the firm, industry, and county due to data limitations.

declines in the post-fraud period. In column 2, we examine the subsample of employees who stay through at least three years in the post-fraud period ("stayer"). While all coefficients in the fraud and post-fraud period are negative, these coefficients are not significant at conventional levels, indicating that stayers at fraud firms have similar wages as stayers at control firms. These leaver and stayer results show that employee separations are an important factor in wage consequences for fraudulent financial reporting.

Lastly, in Table 4 Panel C, we use our main research design, i.e., Equation (1), along with the LEHD nationwide data, which measures the number of calendar quarters within the year where workers experience zero wages. The coefficients measure the zero-wage quarters experienced incrementally by the fraud firm-affiliated employees. This measure, *Count of Zero-wage Quarters*, can represent job displacement, unemployment, and/or wage uncertainty, all of which adversely affect workers (e.g., Baily, 1974). Related to the decreases in wages in Table 3, we similarly find that the number of zero-wage quarters increases for fraud firm workers in the post-fraud period, peaking at *Post(T+2)*. The magnitude is meaningful, with the coefficient for this year indicating that these workers experience 5.4% more quarters with zero wages (0.216 divided by four quarters in a year). These results also show that quarters of zero wages extend further into the future for fraud firm employees where the results are significantly positive through *Post(T+5)*, suggesting difficulties in the labor market, e.g., periodic unemployment, for an extended time.

3.2. Employee growth and mix

Next, we show the dynamics of employee growth during the pre-fraud and fraud periods. In Table 5 Panel A, we present the trend of fraud firms' employment decisions measured as year-on-year employee growth. We include growth at control firms and the industry average as comparisons. Before the fraud, fraud firms have high, positive employee growth ranging between 14% and 16% annually (control firms' growth ranges between 11% and 15%; industry average growth rates are between 7% and 8%). During the fraud, employee growth rates remain quite high; fraud firms' rates average 9%, control firms' rates average 3% and the industry averages 5%. These percentages indicate that the fraud firms were hiring during the fraud period and hiring at rates greater than control firm or industry growth rates, consistent with overbuilding during the fraud.

In the post-fraud period, we find negative employee growth for fraud firms which is not significantly different from zero but is significantly lower compared with the control firms and the industry average. After the fraud, fraud firms' employee growth rates average −3%, while control firms average 0% and the industry averages 3%. Just as the positive growth rates during the fraud indicate net hiring, negative growth rates indicate that fraud firms shed employees after the fraud period (i.e., the fraud firms have net departures of workers after fraud revelation), while control firms neither shed nor hire, and the average firm in the industry hires. Moreover, there is an unwinding effect after fraud revelation where in the five years afterward, the fraud firm reverses all of its excess hiring in the fraud period and reverses the excess hiring from two-to-three years of the pre-fraud period as well. We illustrate columns 1, 2, and 4 in Fig. 3.[23]

In Table 5 Panel B and Panel C, we test employee mix of new versus leaving employee wages directly. Panel B calculates statistics of employee tenure using all available employee data, not just the sample of existing (pre-fraud) employees (i.e., at least two-year tenured employees) and shows that before and during the fraud period, fraud firms have fewer long-tenured workers (i.e., more new employees) than control firms. Following Caggese et al. (2019), long-tenured workers are those with cumulative tenure longer than two years. The proportion of newer workers peaks during the fraud period. Panel C uses only the subsample of employees who leave or join fraud firms during the fraud period (i.e., a single cross-section of transitioning employees) and shows that exiting employees at fraud firms earn more wages at the fraud firm than newly hired employees. These two findings are consistent with executives at fraud firms changing the labor mix away from experienced (long-tenured), higher-paid employees toward newer, lower-paid employees.[24]

Table 5 Panel D examines wage dynamics for newly hired employees that join fraud firms during the fraud period. We use a separate sample of "new employees" for this analysis; we require that she *not* work for the sample firm in the year prior to the fraud period, *Pre(t-1)*, and work for the firm for the first year of the fraud period, *Fraud(t)*. New employees at the control firms are also joining in the same year, and pre trends are similar in column 1 but show some differences in columns 2 and 3 in *Pre(t-2)*.[25] New employees have insignificantly positive wage effects during the fraud (i.e., wages do not drop when joining the fraud firm) and negative wage effects in the post fraud period (significant for *Post(T+1)*, *Post(T+2)*, and *Post(T+3)*) in the range of −6% and −8% per year. Present value calculations show cumulative wage losses of 30% in this period. Unlike specifications for existing employees, new employees have greater point estimates for the present value at the beginning of the fraud than at the end (e.g., −0.215 is greater than −0.300 in column 1); however, these are not statistically different from one another.

[23] As mentioned above, in IA Fig. 2, we separate out frauds by length of the fraud, i.e., one-year, two-year, and three-year (or more) frauds. One- and two-year frauds have patterns very consistent with the main results and our interpretation. Three-year and longer frauds exhibit long, consistent declines in employee growth and wages during and after the fraud. Due to Census disclosure requirements, we are unable to quantitatively examine employment growth at control firms nor additional subsamples of long-lasting frauds.

[24] After the fraud, we find that worker tenure converges between the fraud and control firms; this is consistent with fraud firms course correcting and, again, behaving like non-fraud firms.

[25] This is due to small differences between job transitions for new workers at fraud versus control firms in *Pre(t-2)* related to industry (untabulated). The worker groups are again not different from each other in *Pre(t-1)* before the fraud.

3.3. Interpretation

In this section, we interpret the findings about the consequences for employees of fraudulent financial reporting together. Shareholders receive meaningful and fair attention as victims of accounting fraud in the prior literature, having been misled by executives about the firm's performance (e.g., Dyck et al., 2023; Karpoff et al., 2008b). However, rank-and-file employees are also important stakeholders of firms and are not likely to be affected in the same way as executives or directors, who are penalized specifically via the legal system or corporate governance (e.g., Karpoff et al., 2008a; Srinivasan, 2005). Our findings of negative labor market outcomes for employees can be interpreted through two distinctive channels of fraud, overbuilding and information asymmetry—both detailed below—that affect worker wages and displacement.

First, executives overbuild when committing fraud. Our findings of employee growth suggest that executives in firms over-hire employees (and overinvest in physical capital), plausibly to bolster the perception of the firm (Kedia and Philippon, 2009; McNichols and Stubben, 2008). That is, executives take real actions which show growth and concurrently commit fraud, perhaps because they can be excessively optimistic (Schrand and Zechman, 2012). Yet, they still carefully manage reported performance, such as by investing in physical capital–which has costs spread out over the long run—but not by investing in research and development—which causes an immediate drag on profitability (McNichols and Stubben, 2008). Likewise, our employee mix findings suggest that executives grow the number of employees during fraud, perpetuating overbuilding, but they also change the employee mix by replacing high-paid, long-tenured workers with low-paid, new workers to keep costs down.

A second feature of fraudulent financial reporting is that executives falsify public information about the firm to show better performance than the hidden, unmanipulated data. This information asymmetry, combined with the overbuilding described above, affects workers through an unwinding effect in three ways. First, employees have risk preferences with respect to jobs, including distress and reporting quality, and use financial information from firms (e.g., Brown and Matsa, 2016; Choi et al., 2023a; Choi et al., 2023b; de Haan et al., 2023). Workers may take or keep a job at a fraud firm despite outside options that are preferable because of the misreporting, unwittingly contributing to the overbuilding. Second, when employees work for a firm, they accumulate firm- (and industry-) specific human capital (Becker, 1993). Because specific capital loses value when a worker is displaced, there may be heightened but unobserved turnover risk at a fraud firm which would distort the worker's investments upward in specific versus general capital. Third, due to overinvestment at the fraud firm (or even in the fraud firm's industry, e.g., Beatty et al., 2013; Li, 2016), many workers with similar skills are likely to lose jobs at the same time when the fraud is revealed. Workers will be searching for their next job in an unfavorable local labor market condition: the labor market will be "crowded," i.e., many, similarly skilled workers will be looking for a job at the same time. Thus, unwinding the overemployment causes incrementally harmful displacement for workers due to misinformation from fraud.

Conversely, when firms are distressed, executives take a different approach with different consequences for workers (e.g., Baily, 1974; Jacobson et al., 1993; Couch and Placzek, 2010; Falato and Liang, 2016; Bernstein et al., 2019). They acknowledge problems and restructure or refocus the business around core operations; some employees might be laid off, contributing to a successful recovery (e.g., John et al., 1992; Whitaker, 1999).[26] Acknowledging the distress could alert workers early such that job transitions are more successful, e.g., shorter and with lower wage drops (e.g., Malik, 2022). Further, distressed firms do not necessarily substitute long-tenured workers with short-tenured workers. Distressed firms could have trouble attracting new workers (Brown and Matsa, 2016) or be forced to pay wage premiums (Graham et al., 2023). Distressed firms also shed newer workers if firing costs for long-tenured workers or training costs are high (Caggese et al., 2019).[27]

4. Heterogeneity and supplemental analyses

4.1. Thick and thin labor markets

To better understand these wage changes, we descriptively split the result by whether the labor market in which the employee works is thick or thin, with many or few outside options, respectively. Moretti (2011), in reviewing local labor markets, points out that thick labor markets provide insurance to workers (and firms) against idiosyncratic shocks. He writes, "The presence of a large number of other employers implies a lower probability of not finding another job." This logic resonates in fraud cases that are particularly harmful to small communities, such as the impact of WorldCom's fraud on Clinton, Mississippi, where the labor market was thin for workers leaving

[26] In an untabulated test, we find that overbuilding (unwinding) at fraud firms is correlated with lower (higher) restructuring charges separately reported on the firms' income statements compared to non-fraud firms, consistent with executives using fraud to defer fixing core business issues.

[27] In IA Fig. 3, we find that workers who leave the fraud firm within a year have relatively smaller wage losses compared with those who leave the fraud firm later, which is consistent with those late leavers having more challenging and costly transitions.

Table 5

Employee Growth and Mix This table reports estimates for employment growth and mix. Panel A reports employee growth using Compustat data at fraud firms, matched control firms, and the industry average in columns (1), (2), and (4), respectively, in event-time years. The table also reports differences between fraud and matched control firms in column (3) and fraud firm and industry averages in column (5). Panel B presents regression estimates of firm-wide employee tenure on the Fraud Indicator and a constant to show differences between the fraud firms and matched control firms, in the pre-fraud, fraud, and post-fraud periods in columns (1), (2), and (3), respectively. Panel C presents differences in employee characteristics and standard errors for the final full year of employment for leaving employees versus the first full year of employment for new employees. Leaving employees are the existing employees who left the firm in the first year of a fraud period. Panel D reports estimates from OLS regression an analysis estimating equation (2), presenting estimates for wage effects at fraud firms in the by-event-time years. The analysis uses a separate sample of newly hired employees in period $Fraud_t$ at the fraud or control firm. Standard errors are in parentheses and calculated with clustering by pre-fraud employer (i.e., fraud or control firm). Present values and T-statistics are at the bottom of the panel; Appendix B describes these calculations. Appendix A defines variables. Statistical significance at the 10%, 5%, and 1% confidence levels is indicated by *, **, and ***, respectively. For PVs, significance at the 10% confidence level for a one-tailed test is indicated by ˆ. All statistics are rounded to comply with requirements of the U.S. Census Bureau.

Panel A: Employee Growth—Overbuilding and Unwinding

	(1) Fraud Firms	(2) Control Firms	(3) Fraud vs. Control	(4) Industry Average	(5) Fraud vs. Industry
Pre(t-4)	13.5%	13.6%	-0.1%	7.7%	5.8%***
Pre(t-3)	15.8%	11.1%	4.7%	8.1%	7.7%***
Pre(t-2)	14.7%	14.4%	0.3%	7.2%	7.5%***
Pre(t-1)	14.2%	14.5%	-0.3%	7.3%	6.9%***
Fraud(t)	12.9%	6.2%	6.7%**	6.4%	6.5%***
Fraud(t+1)	8.7%	2.1%	6.5%**	4.2%	4.5%
Fraud(t+2)	5.8%	0.7%	5.1%*	3.8%	2.0%
Post(T)	-2.8%	3.2%	-5.9%*	3.8%	-6.6%**
Post(T+1)	-4.7%	-2.2%	-2.5%	2.4%	-7.1%***
Post(T+2)	-2.9%	0.2%	-3.0%	2.2%	-5.0%***
Post(T+3)	-4.2%	1.6%	-5.8%**	3.6%	-7.7%***
Post(T+4)	-4.2%	0.2%	-4.4%	2.3%	-6.5%***
Post(T+5)	1.0%	-1.9%	3.0%	2.1%	-1.1%

Panel B: Employee Tenure Mix in Event Time

	(1)	(2)	(3)
Fraud Period:	*Pre*	*Fraud*	*Post*
Dependent variable: *Employee Tenure Mix*			
Fraud Indicator	-0.027**	-0.047***	-0.013
	(0.013)	(0.016)	(0.011)
Constant	0.316***	0.451***	0.540***
	(0.009)	(0.011)	(0.008)
Observations	1,300	800	1,600
R-squared	0.003	0.011	0.001

Panel C: Characteristics of Leaving Employees versus New Employees

	(1) T-Test of Differences Fraud Firm Employees Only (New minus Exiting) Difference	(2) Standard Error
Education	-0.237	(0.226)
Age	-0.563	(0.420)
Experience	-0.326	(0.359)
Annual Real Wages	-11,720***	(4,260)
Ln(*Annual Real Wages*)	-0.319***	(0.090)
Female	0.019	(0.022)
Observations	295,000	

Panel D: New Employee Wages

Dependent Variable = Ln(*Annual Real Wages*)	(1) Year Effects	(2) Year & Industry Effects	(3) Year & Industry & County Effects
Pre(t-3) × Fraud Ind.	0.009	-0.008	-0.009
	(0.015)	(0.019)	(0.019)
Pre(t-2) × Fraud Ind.	-0.025	-0.057***	-0.061***
	(0.015)	(0.019)	(0.019)
Pre(t-1) × Fraud Ind.	0.026	-0.011	-0.017
	(0.030)	(0.029)	(0.030)
Fraud(t) × Fraud Ind.	0.044	0.001	0.018
	(0.052)	(0.043)	(0.036)
Fraud(t+1) × Fraud Ind.	0.011	-0.021	-0.014
	(0.036)	(0.028)	(0.026)
Fraud(t+2) × Fraud Ind.	-0.006	-0.015	-0.019
	(0.029)	(0.032)	(0.031)
Post(T) × Fraud Ind.	-0.026	-0.029	-0.029
	(0.033)	(0.034)	(0.034)
Post(T+1) × Fraud Ind.	-0.061*	-0.061*	-0.061*
	(0.035)	(0.037)	(0.037)
Post(T+2) × Fraud Ind.	-0.081**	-0.080**	-0.082**
	(0.032)	(0.034)	(0.034)
Post(T+3) × Fraud Ind.	-0.070*	-0.068*	-0.070*
	(0.037)	(0.039)	(0.039)
Post(T+4) × Fraud Ind.	-0.057	-0.057	-0.056
	(0.046)	(0.046)	(0.045)
Post(T+5) × Fraud Ind.	-0.067	-0.065	-0.065
	(0.052)	(0.053)	(0.053)
Controls	Yes	Yes	Yes
Fixed Effects	Event-time, Year, Worker	Event-time, Year, Industry, Worker	Event-time, Year, Industry, County, Worker
Observations	3,306,000	3,306,000	3,306,000
R-squared	0.606	0.615	0.616
Pre-Fraud *(t-1)* Present Value	-0.215	-0.287^	-0.269^
	(-1.19)	(-1.46)	(-1.46)
Pre-Revelation *(T-1)* Present Value	-0.300*	-0.295^	-0.296*
	(-1.71)	(-1.61)	(-1.82)

WorldCom (e.g., Bayot, 2002). We expect the consequences of these frauds in thin labor markets to be particularly devastating for workers who are competing with many other, simultaneously unemployed workers when few jobs or other employer options are available.

We separately examine thick and thin labor markets, measured by counties with many and few industry-specific employers, respectively.[28] Table 6 shows this sample split in columns 1 and 2. In column 1, we present estimates where the county-level labor market has many industry-specific employers, i.e., thick labor markets (e.g., Dore and Zarutskie, 2017). In the post-fraud period, the fraud firms in thick labor markets pay less than the control firms, but no coefficients are statistically significant. The estimated magnitudes range between −6% and −9%, through *Post(T+3)*. In column 2, we present estimates where the county-level labor market has few industry-specific employers, i.e., thin labor markets. Employees in these labor markets do very poorly. The negative wage effects in the fraud and post-fraud periods are large, e.g., point estimates more negative than −10% for years after *Post(T+2)*. Overall, the wage declines are *much* stronger (present values of −0.141 (thick market) vs −0.620 (thin market), z-statistic = −1.77) in thin

[28] We use the number of establishments in the year before the fraud (*Pre(t-1)*) as the measure of market thickness. We preserve this split across other event-time years because our research design measures wage changes for an employee across years, and we do not want a partial series of an employee's wages in both thick and thin labor market subsamples. We define the number of establishments at each county and industry (2-digit SIC code). This research design choice allows us to examine how the initial county-industry labor market condition before the fraud (*Pre(t-1)*) influences the labor market outcomes of employees of fraud and control firms as fraud evolves. In untabulated tests, we also measure thick labor markets based on the size of industry-specific labor supply (i.e., the number of employees at each county and industry). Our results are qualitatively the same with this alternative sample split.

Table 6
Employee Earnings and Market and Pre-fraud Wage Level Heterogeneity This table reports estimates from OLS regression analyses estimating equation (1), presenting estimates for wage effects at fraud firms in the by-event-time years. In columns (1) and (2), the sample is divided into "thick" and "thin" markets that have above and below median, respectively, within-industry employers in the same county. In columns (3) and (4), we present subsamples of employees in the top 10% and bottom 90% of the pre-fraud wage distribution, respectively. Standard errors are in parentheses and calculated with clustering by pre-fraud employer (i.e., fraud firm or matched control firm). Present values and T-statistics are at the bottom of the table; Appendix B describes these calculations. Appendix A defines variables. Statistical significance at the 10%, 5%, and 1% confidence levels is indicated by *, **, and ***, respectively. All statistics are rounded to comply with requirements of the U.S. Census Bureau.

Dependent Variable = Ln(*Annual Real Wages*)	(1) Thick Markets	(2) Thin Markets	(3) Top 10%	(4) Bottom 90%
Pre(t-3) × *Fraud Ind.*	0.017	-0.027	-0.012	-0.011
	(0.022)	(0.029)	(0.030)	(0.023)
Pre(t-2) × *Fraud Ind.*	0.028	-0.042	0.006	-0.012
	(0.035)	(0.030)	(0.033)	(0.029)
Pre(t-1) × *Fraud Ind.*	-0.009	-0.035	0.043	-0.032
	(0.034)	(0.040)	(0.040)	(0.034)
Fraud(t) × *Fraud Ind.*	-0.008	-0.025	0.008	-0.025
	(0.035)	(0.028)	(0.032)	(0.028)
Fraud(t+1) × *Fraud Ind.*	0.055	-0.085*	0.002	-0.037
	(0.039)	(0.045)	(0.042)	(0.039)
Fraud(t+2) × *Fraud Ind.*	-0.015	-0.081	0.024	-0.066
	(0.033)	(0.059)	(0.048)	(0.045)
Post(T) × *Fraud Ind.*	-0.038	-0.050	0.002	-0.050
	(0.036)	(0.045)	(0.041)	(0.039)
Post(T+1) × *Fraud Ind.*	-0.050	-0.097*	-0.033	-0.083*
	(0.041)	(0.052)	(0.043)	(0.044
Post(T+2) × *Fraud Ind.*	-0.068	-0.124**	-0.034	-0.111**
	(0.049)	(0.055)	(0.041)	(0.046)
Post(T+3) × *Fraud Ind.*	-0.063	-0.138**	-0.031	-0.125**
	(0.045)	(0.061)	(0.050)	(0.051)
Post(T+4) × *Fraud Ind.*	-0.025	-0.116**	-0.030	-0.091*
	(0.046)	(0.057)	(0.053)	(0.051)
Post(T+5) × *Fraud Ind.*	0.015	-0.136*	-0.042	-0.080
	(0.088)	(0.076)	(0.076)	(0.087)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Event-time, Year, Industry, County, Worker	Event-time, Year, Industry, County, Worker	Event-time, Year, Industry, County, Worker	Event-time, Year, Industry, County, Worker
Observations	3,670,000	5,050,000	870,000	7,850,000
R-squared	0.621	0.621	0.556	0.583
Pre-Fraud *(t-1)* PV	-0.141	-0.620**	-0.089	-0.489*
	(-0.58)	(-2.03)	(-0.34)	(-1.78)
Pre-Revelation *(T-1)* PV	-0.196	-0.522**	-0.137	-0.435*
	(-0.97)	(-2.10)	(-0.64)	(-1.93)

Table 7

Robustness: Different Matching Approaches This table reports estimates from OLS regression analyses estimating equation (1). Each column uses separate samples of control employees from matches described in the column header. Standard errors are in parentheses and calculated with clustering by fraud-period employer (i.e., fraud firm or matched control firm). Present values and T-statistics are at the bottom of the table; Appendix B describes these calculations. Appendix A defines variables. Statistical significance at the 10%, 5%, and 1% confidence levels is indicated by *, **, and ***, respectively. For PVs, significance at the 10% confidence level for a one-tailed test is indicated by ˆ. All statistics are rounded to comply with requirements of the U.S. Census Bureau.

Dependent Variable = Ln(*Annual Real Wages*)	(1) Fraud Period, Unmanaged Sales Growth	(2) Within-Firm Employee Characteristics	(3) Revelation Period, *Post (T)*, Match
Pre(t-3) × Fraud Ind.	0.007	0.001	0.0231
	(0.023)	(0.019)	(0.0167)
Pre(t-2) × Fraud Ind.	0.009	0.007	-0.0037
	(0.030)	(0.024)	(0.0252)
Pre(t-1) × Fraud Ind.	-0.024	-0.025	-0.0362
	(0.034)	(0.030)	(0.0280)
Fraud(t) × Fraud Ind.	0.012	-0.041	-0.0407
	(0.033)	(0.026)	(0.0351)
Fraud(t+1) × Fraud Ind.	-0.058	-0.050	-0.0822**
	(0.036)	(0.036)	(0.0350)
Fraud(t+2) × Fraud Ind.	-0.048	-0.058	-0.0855*
	(0.049)	(0.046)	(0.0506)
Post(T) × Fraud Ind.	-0.047	-0.033	-0.032
	(0.035)	(0.038)	(0.0474)
Post(T+1) × Fraud Ind.	-0.092**	-0.076**	-0.0677
	(0.037)	(0.037)	(0.0478)
Post(T+2) × Fraud Ind.	-0.109**	-0.098**	-0.0807*
	(0.048)	(0.042)	(0.0488)
Post(T+3) × Fraud Ind.	-0.125***	-0.090*	-0.0558
	(0.047)	(0.046)	(0.0467)
Post(T+4) × Fraud Ind.	-0.112**	-0.073	-0.0325
	(0.043)	(0.046)	(0.0532)
Post(T+5) × Fraud Ind.	-0.162**	-0.121*	-0.0471
	(0.064)	(0.070)	(0.0476)
Controls	Yes	Yes	Yes
Fixed Effects	Event-time, Year, Industry, County, Worker	Event-time, Year, Industry, County, Worker	Event-time, Year, Industry, County, Worker
Observations	8,310,000	8,693,000	10,970,000
R-squared	0.628	0.627	0.647
Pre-Fraud *(t-1)* PV	-0.521**	-0.472*	-0.409^
	(-2.05)	(-1.93)	(-1.46)
Pre-Revelation *(T-1)* PV	-0.507***	-0.391**	-0.262
	(-2.63)	(-1.96)	(-1.16)

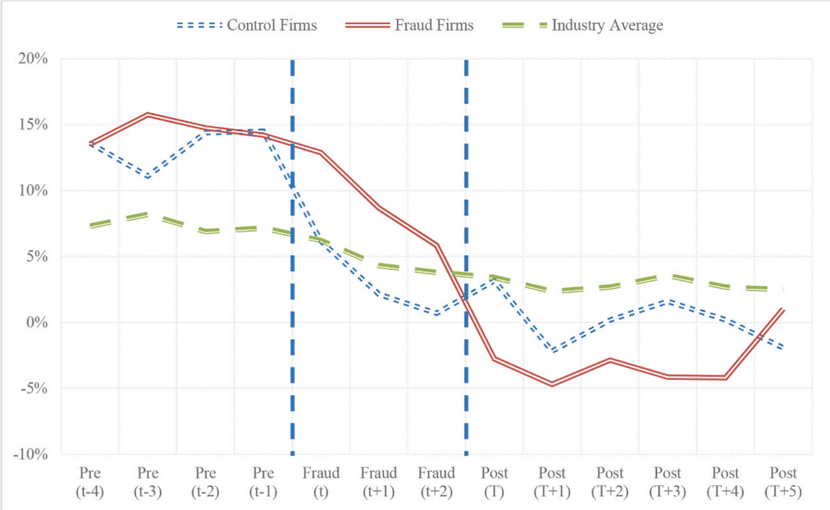

Fig. 3. Employee Growth Levels
This figure shows levels of employee growth (y-axis) in the by-event-time years (x-axis). Point estimates are growth levels at matched control firms, fraud firms, and the industry average, using Compustat data. Matched control firms have employment growth trends plotted with short dashes. Fraud firms have employment growth trends plotted with the solid line. Industry average employment growth trends are plotted with long dashes.

labor markets, indicating that the effect is driven by displacement into sparse labor markets and frictions to effective job-searches (e.g., Moretti, 2011).[29] This result has an important implication for SEC enforcement. Kedia and Rajgopal (2011) find that SEC enforcement actions concentrate on firms in big cities. However, some negative consequences are more severe for fraudulent firms in small cities, i.e., where there are thin labor markets.[30]

4.2. *Pre-fraud wage levels*

Another source of variation that can help inform which employees suffer these negative wages around fraud comes from employee characteristics. We use pre-fraud variation in pay to provide some evidence on whether there are wage effects for the highest paid employees (e.g., potentially executives or culpable accountants being penalized for the fraud) or if lower paid employees also suffer wage drops around fraudulent financial reporting. This analysis is rarely found in prior papers measuring wage losses (e.g., Hummels et al., 2014; Jacobson et al., 1993), but it is important in our context because of frauds being attributable to someone within the firm, usually executives (Schrand and Zechman, 2012). Following prior literature (e.g., Autor et al., 2008), we split out the workers in the top 10% of the wage distribution from the rest of the workers.

For columns 3 and 4 in Table 6, we present analyses that condition on the pre-fraud period wage level across firms, splitting the sample into workers who are in the top 10% of the wage distribution ("top 10%") and the bottom 90% of the distribution ("bottom 90%"). In column 3, employees in the top 10% do not suffer significant negative consequences during or after the fraud period. Bottom 90% employees, however, experience significant, negative wage effects in the fraud and post-fraud periods, as severe as −7% during the fraud and between −5% and −13% after the fraud. Workers in the bottom 90% of the wage distribution have worse wage consequences from fraudulent financial reporting despite the lower likelihood that they are involved with the fraud (present values of −0.089 (top 10%) versus −0.489 (bottom 90%), z-statistic = −1.55).[31]

Although fraudulent financial reporting decisions are made at the management level, the results indicate that mainly non-management workers bear the costs. Low-wage workers suffer more even though they are not likely the executives who commit fraud. In addition, this analysis has some connection to the thick and thin labor markets analysis. The bottom 90% can be the employee side of the same story. These workers could face greater job search frictions in labor markets with less portable skills. That is, high wage workers have greater mobility because high wage workers have more opportunities in the labor market (e.g., Machin and Kjell, 2012) and, thus, can avoid wage losses from job displacement. Consistent with this, in IA Table 8, we find that the bottom 90% of workers in thin labor markets have the worst wage outcomes among these subsamples. Further, in IA Table 9, we find similar patterns to the top 10%

[29] The z-statistic is estimated with the standard errors that we calculate using the delta method and covariance estimates from the estimated variance-covariance matrix of our main result. The correlation is estimated based on the most conservative (relative) correlation from the coefficients used for PV calculation in Table 3 Column 1, which is 0.54. The specific numerical equation for −1.77 is $\frac{-0.620-(-0.141)}{\sqrt{0.24^2+0.31^2-2*0.54*0.24*0.31}} = -1.77$.

[30] In an untabulated analysis, we confirm with our data that SEC offices are located in thick labor markets.

[31] We define the top 10% of the wage distribution within industry. Different methods for calculating the wage distribution, e.g., measuring the wage distribution across industries or within fraud vs. control subsamples, do not qualitatively change the results.

versus bottom 90% comparison when examining the top 25%, middle 50%, and bottom 25%, where the outcomes in percentage terms are progressively worse moving down the wage groups, though still significantly negative for those in the middle of the wage distribution.

Overall, our conclusion from Table 6 is that there is meaningful, expected heterogeneity in the total wage effects of fraudulent financial reporting. These findings are consistent with an explanation where some workers are less affected because they live in regions with thick labor markets which can protect against shocks (e.g., Moretti, 2011). The findings are also consistent with an explanation where some workers are likely to have skills that are highly sought after and command high wages. Thus, these workers are also likely protected against severe negative wage effects if these skills are highly valued by other companies (e.g., Machin and Kjell, 2012).

4.3. Robustness: different matching approaches

For our next tests, we vary the control samples in three ways and present these results in Table 7. For our main result, we use a parsimonious set of variables from prior literature that influence wages to match to control firms (e.g., Chemmanur et al., 2013). Below, we use different matching approaches as robustness to address various endogeneity issues specific to fraud and those endogenous issues' possible effect on worker wages. Moreover, these alternative control samples can also provide additional evidence regarding the mechanism of managers overhiring employees concurrent with the fraud and then unwinding this overhiring after the fraud is revealed.

First, we match fraud firms using hand-collected data from the fraud period to control for potential contemporaneous shocks which may have a direct effect on hiring and wages leading into the subsequent fraud years.[32] This match addresses a concern that the fraud firm has a contemporaneous (and negative) shock that both (1) managers want to cover up with fraud and (2) would already lead to workers having lower wages even without fraud. These results are shown in column 1 and are quantitatively similar to the main sample's results. That is, when controlling for the demand shock in the first year of the fraud where we use the fraud firm's unmanaged sales and match to control firms with similar sales growth, we still find comparable, negative outcomes for fraud firm employees.

Second, we use the employee characteristics data from the LEHD to match subsamples of employees from fraud and control firms. This is a matching innovation compared with prior accounting papers that often only match on firm characteristics. It is important in our case because fraud and control firms may have different types of employees in the years leading up to the fraud. We perform coarsened matches (i.e., match within groups rather than precise matches) based on age, employee education (four groups based on pre-high school, high school, some college, college graduate), gender (two groups based on male and female), and pre-fraud wage decile. The findings are shown in column 2. Again, the results are similar to the main sample's results in Table 3. Employee composition at fraud and matched control firms does not seem to play a major role in the total wage effects that we find in our main analysis, which is important because without this matching, we found significantly different pre-fraud wage levels between the groups of workers.

Third, we match fraud firms to control firms during the fraud's revelation (i.e., *Post(T)*) rather than based on the fraud's commencement. This final matched sample tests wage outcomes for employees at fraud firms compared to employees at control firms that have ex post outcomes that are like discovered fraud firms because revelation could be the relevant shock to employees, rather than the commencement of fraud. Fraud firms may be unwinding some fraud-related overbuilding of employees; these matched controls will similarly have characteristics like sharply reducing headcount, etc. Thus, matching on a similar outcome for a non-fraud firm generates a control group that could also be unwinding excessive growth (but without fraud). Thus, if this alternative sample controls for a mechanism that we discuss in Section 3 (overbuilding and unwinding), measured wage effects could be muted. Any incremental wage effects that we might find for fraud firm employees might instead capture mechanisms such as a rushed job search or information frictions (Malik, 2022). In column (3), we continue to find negative wage consequences during the fraud (i.e., *Fraud(t+1)* and *Fraud(t+2)*) and after (i.e., *Post(T+2)*). Other years have negative coefficients but are not significant at conventional levels. We find cumulative wage losses of 41%, similar to the 44% from Table 3, though only significant with a one-sided p-value less than 10%.[33]

4.4. Robustness: different fixed effects

We use specifications with year, industry, and county fixed effects, although we acknowledge a trade-off compared with the use of specifications with year-industry-county interacted fixed effects (e.g., Graham et al., 2023). On one hand, our main fixed effect choice allows us to measure the effect of accounting fraud on employees' labor market outcomes, including local- and industry-related spillovers of the fraud. If frauds have negative spillovers in the local labor market, year-county or year-industry-county fixed effects would attenuate the post-fraud coefficients of interest, adversely impacting what we want our research design to measure, the

[32] We separate the fraud sample into revenue and non-revenue misreporting firms. For the revenue misreporting subsample, we gather unmanaged sales data from, in order: (i) differences between Compustat-Snapshot "As First Reported - Annual" and "Most Recently Restated - Annual", (ii) AAER reported annual misstatement amounts, (iii) restatements on SEC EDGAR database, and (iv) a Factiva and Google search for archival news documents reporting on the fraud. We use this hand-collected data to construct a *Sales Growth* variable measured from *Pre(t-1)* to *Fraud(t)* and include this variable in our propensity-score-matching model along with the other variables noted in equation (1). The matching model and descriptive statistics are presented in IA Table 10.

[33] In IA Table 11, we match fraud firm employees to random employees within industry following Graham et al. (2023); results are comparable to the main result.

Table 8

Robustness: Different Fixed Effects This table reports estimates from OLS regression analyses estimating equation (1) using alternative research designs or missing employee wage imputation. Panel A presents estimates for wage effects at fraud firms in the by-event-time years using specifications with fixed effects noted in each column header. Standard errors are in parentheses and calculated with clustering by pre-fraud employer (i.e., fraud firm or matched control firm). Present values and T-statistics are at the bottom of the table; Appendix B describes these calculations. Appendix A defines variables. Statistical significance at the 10%, 5%, and 1% confidence levels is indicated by *, **, and ***, respectively. For PVs, significance at the 10% confidence level for a one-tailed test is indicated by ^. All statistics are rounded to comply with requirements of the U.S. Census Bureau. Panels B and C show key analyses from each table and, except as otherwise noted, follow the approach from the respective tables, panels, and columns as noted in each column header. Panel B presents estimates for wage effects as present values using specifications with Year × Industry fixed effects. Panel C presents estimates for wage effects as present values using specifications with Year × Industry × County fixed effects. Standard errors are in parentheses and calculated with clustering by pre-fraud employer (i.e., fraud firm or matched control firm). Appendix B describes the present value calculations. Appendix A defines variables. Statistical significance at the 10%, 5%, and 1% confidence levels is indicated by *, **, and ***, respectively. Significance at the 10% confidence level for a one-tailed test is indicated by ^. All statistics are rounded to comply with requirements of the U.S. Census Bureau.

Panel A: Main Result with Different Fixed Effects

Dependent Variable = Ln(*Annual Real Wages*)	(1) Year × Industry	(2) Year × County	(3) Year × Industry, Year × County	(4) Year × Industry × County
Pre(t-3) × Fraud Ind.	-0.029*	0.021	-0.006	0.001
	(0.016)	(0.020)	(0.014)	(0.013)
Pre(t-2) × Fraud Ind.	-0.042	0.019	-0.013	0.020
	(0.025)	(0.026)	(0.021)	(0.023)
Pre(t-1) × Fraud Ind.	-0.063**	0.000	-0.025	-0.007
	(0.030)	(0.021)	(0.025)	(0.026)
Fraud(t) × Fraud Ind.	-0.050	-0.010	-0.015	-0.007
	(0.031)	(0.026)	(0.021)	(0.021)
Fraud(t+1) × Fraud Ind.	-0.057	0.003	-0.005	0.006
	(0.042)	(0.028)	(0.023)	(0.023)
Fraud(t+2) × Fraud Ind.	-0.080**	-0.035	-0.030	-0.025
	(0.038)	(0.039)	(0.026)	(0.025)
Post(T) × Fraud Ind.	-0.081**	-0.021	-0.030	-0.025
	(0.037)	(0.043)	(0.030)	(0.025)
Post(T+1) × Fraud Ind.	-0.110***	-0.056	-0.060*	-0.055*
	(0.036)	(0.046)	(0.032)	(0.031)
Post(T+2) × Fraud Ind.	-0.143***	-0.087*	-0.096***	-0.094***
	(0.038)	(0.048)	(0.033)	(0.034)
Post(T+3) × Fraud Ind.	-0.122***	-0.079*	-0.073**	-0.074**
	(0.041)	(0.048)	(0.034)	(0.035)
Post(T+4) × Fraud Ind.	-0.119***	-0.061	-0.072**	-0.062*
	(0.043)	(0.051)	(0.036)	(0.037)
Post(T+5) × Fraud Ind.	-0.140**	-0.091	-0.116**	-0.107***
	(0.056)	(0.068)	(0.049)	(0.039)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	Event-time, Year × Industry, County, Worker	Event-time, Year× County, Industry, Worker	Event-time, Year × Industry, Year × County, Worker	Event-time, Year × Industry × County, Worker
Observations	8,720,000	8,720,000	8,720,000	8,720,000
R-squared	0.627	0.614	0.633	0.656
Pre-Fraud *(t-1)* PV	-0.667***	-0.313	-0.360*	-0.318*
	(-2.80)	(-1.21)	(-1.94)	(-1.73)
Pre-Revelation *(T-1)* PV	-0.574***	-0.318^	-0.360**	-0.338**
	(-3.22)	(-1.43)	(-2.35)	(-2.22)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Table:	Table 4	Table 4	Table 5	Table 6	Table 6	Table 6	Table 6	Table 7	Table 7	Table 7
Panel & Column:	B: Col. 1	B: Col. 2	D: Col. 3	Col. 1	Col. 2	Col 3.	Col. 4	Col. 1	Col. 2	Col 3.
Analysis Summary:	Leavers	Stayers	New Workers	Thick Markets	Thin Markets	Top 10%	Bottom 90%	Fraud Year Match	Employee Match	Revelation Match
Panel B: Year × Industry Fixed Effects										
Pre-Fraud *(t-1)*	-0.482**	-0.606**	0.054	-0.349*	-0.600**	-0.155	-0.701***	-0.637***	-0.534**	-0.476**
	(-1.99)	(-2.31)	(0.32)	(-1.83)	(-2.09)	(-0.75)	(-2.89)	(-2.97)	(-2.39)	(-2.27)
Pre-Revelation *(T-1)*	-0.489**	-0.422**	-0.074	-0.368**	-0.512**	-0.175	-0.603***	-0.577***	-0.443**	-0.329^
	(-2.49)	(-2.19)	(-0.50)	(-2.51)	(-2.39)	(-1.13)	(-3.30)	(-3.54)	(-2.54)	(-1.60)
Panel C: Year × Industry × County Fixed Effects										
Pre-Fraud *(t-1)*	-0.292^	-0.17	0.187	-0.167	-0.454**	0.179	-0.354*	-0.407**	-0.262^	-0.257^
	(-1.47)	(-1.00)	(1.25)	(-0.82)	(-2.20)	(1.07)	(-1.89)	(-2.11)	(-1.50)	(-1.47)
Pre-Revelation *(T-1)*	-0.344**	-0.156	0.003	-0.25^	-0.422**	0.101	-0.371**	-0.378**	-0.247*	-0.208^
	(-1.97)	(-1.17)	(0.03)	(-1.52)	(-2.56)	(0.74)	(-2.38)	(-2.50)	(-1.74)	(-1.53)

total effect of fraud on wages.[34] Additionally, finer fixed effects (e.g., year-industry-county fixed effects) would reduce the variation of wages across fraud and control firms' workers when year-industry-county subsamples contain only fraud or control workers but not both, especially in cross-sectional analyses. On the other hand, our estimates may include some time-varying industry conditions that are unrelated to fraud and not controlled by the matched sample given that our primary control for (industry-level) pay trends is the matched control sample of firms. Although the total effect of fraud on wages is interesting, these possible confounding factors indicate that year-industry-county interacted fixed effects may provide narrow but refined estimates for negative consequences of fraudulent financial reporting for employees.

To examine the robustness of our results, we include specifications with finer fixed effects to show our results with specifications that include time-series controls for average wages within industry and industry-by-county. We show these results in Table 8. Panel A has a structure very similar to Table 3, our main result, including estimates of dynamic wage effects during and after fraudulent financial reporting and calculations for the wage effect as present values before the fraud and before the fraud's revelation. Across columns, we use increasingly dense fixed effects. In columns 1, 2, 3, and 4, we estimate models with year-industry, year-county, year-industry and year-county, and year-industry-county, respectively. For the dynamic wage effects, we consistently find negative wage coefficients in the post-fraud periods for employees who work(ed) at fraud firms in *Post(T+2)* and *Post(T+3)*. The wage drops estimated using present value are significantly negative in columns 1, 3, and 4. For year-county effects in column 2, only the post-revelation present value is significantly negative with a one-sided test. Moreover, the magnitudes in the first column with year-industry effects should be interpreted carefully. Because some of the pre-fraud years have significantly negative coefficients, the present value calculation may overestimate the effect because wages have already trended downward prior to the fraud and post-fraud years used to calculate the present value.

We extend our robustness tests to all the tables with PV calculations. In other words, we repeat analyses from our paper with present values using both Year × Industry and Year × Industry × County fixed effects.[35] Panel B and Panel C of Table 8 show these present values for our other analyses using these year-industry and year-industry-county fixed effects, respectively. While the results exhibit some variation compared with our main approach, we note four results that support our inferences across the other specifications using at least one-sided tests.[36] First, leavers have consistent wage losses; stayers do not. Second, workers in thin labor markets have consistent wage losses; workers in thick labor markets do not. Third, workers in the bottom of the wage distribution have consistent wage losses; workers in the top of the wage distribution do not. Fourth, other matching approaches also show wage losses for workers from fraud firms.

4.5. Robustness: missing wages and imputation

Next, we estimate the present value of cumulative wage losses with a balanced sample with imputed wages (if missing) that accounts for potential biases driven by worker-years with missing wages. Because our wage data is limited to 23 states, our sample is subject to attrition due to worker movements to states not in our sample. This is an important issue because we have shown that workers at fraud firms are more likely to be displaced, and displaced workers may move across state borders to find a new job. We may overestimate the total wage effects of fraud if our data contains biases that could arise due to unobservable wages for former employees of fraud firms versus former employees of control firms, like high wage employees of fraud firms moving out-of-state while high wage employees of control firms staying within state.

We begin by analyzing the extent of the issue. We generate a balanced panel for workers in our sample and create an indicator variable to measure whether a worker-year is missing from our main sample. We regress this indicator as a modification of Equation (1) on all independent variables from our final specification in Table 3. We report these results in Table 9 Panel A. We do not find statistical differences in the number of missing worker-years between fraud firms and matched control firms in pre-, during-, or post-fraud periods. Although we observe more displaced employees from fraud firms than from control firms (e.g., Table 4), we do not have a statistically greater number of missing worker-years, even in the high-displacement post-fraud periods.

To mitigate this issue, we perform missing wage imputations to understand the potential for sample attrition to change our estimates. We use the LEHD nationwide data that indicate whether and when a worker is employed *anywhere* to impute missing wages. In the fraud and post-fraud periods, we replace (1) missing wages with the sample-wide, bottom 10th percentile of wages when a worker is not employed anywhere (a conservative benchmark relative to zero wages and an important breakpoint in the wage distribution from prior literature, e.g., Acemoglu and Autor, 2011) and (2) replace missing wages with the worker's most recently observed annualized wage if the worker is employed somewhere but we do not see wages. In the pre-fraud period, we (3) backfill a worker's missing wages with her wages from *Pre(t-1)*, which are required for sample employees. Because we scale all wages by the CPI, these imputations are not affected by inflation.

From untabulated regression coefficients, we again estimate the present value of cumulative losses at the pre-fraud and pre-revelation dates. Results are shown in Table 9 Panel B. With the balanced panel of worker-years with imputed wages, workers

[34] Fixed effects can act as mediators, removing variation of interest in wage regressions (Gow et al., 2016, p. 484).

[35] This approach enhances convergence with the Graham et al. (2023) paper (over many of its iterations) as well. We use both the main specification from the previous version of Graham et al. (2023) (i.e., Industry × Year) and the main specification from Graham et al. (2023) (i.e., Industry × County × Year) for all of our analyses that examine wages. Use of present values follows Graham et al. (2023) closely, who use present values for all analyses except their main table (Table 4 on p. 2106).

[36] Although the new employee results are weaker in Column 3 of Panels B and C of Table 8, our tabulated and untabulated robustness tests indicate that the estimates of (post-period) cumulative wage losses for newly hired employees are negative, except for one specification.

Table 9

Robustness: Missing Wages and Imputation This table reports estimates from OLS regression analyses estimating a modification to equation (1), presenting estimates for an indicator variable equal to one when wages are imputed for the "Balanced, Imputed Wage Sample", estimating effects at fraud firms in the by-event-time years in column (1). The dependent variable, *Wage Imputation Indicator*, is analogous to an indicator for a missing observation such that our main sample is not balanced; however, this variable is *not* analogous to an indicator variable for zero wages. Standard errors are in parentheses and calculated with clustering by pre-fraud employer (i.e., fraud firm or matched control firm). Appendix Table A defines variables. Statistical significance at the 10%, 5%, and 1% confidence levels is indicated by *, **, and ***, respectively. All statistics are rounded to comply with requirements of the U.S. Census Bureau. Panels B shows key analyses from each table and, except as otherwise noted, follow the approach from the respective tables, panels, and columns as noted in each column header. Panel B presents estimates for wage effects as present values using a balanced panel sample where missing wages have been imputed using a combination of (1) observed wages in non-missing years for the same employee, (2) wage-earning status in missing-wage years for the same employee across the United States, and (3) the 10th percentile of sample-wide wages. At the bottom, real, imputed, and total observations are enumerated in thousands. Standard errors are in parentheses and calculated with clustering by pre-fraud employer (i.e., fraud firm or matched control firm). Appendix B describes the present value calculations. Appendix A defines variables. Statistical significance at the 10%, 5%, and 1% confidence levels is indicated by *, **, and ***, respectively. Significance at the 10% confidence level for a one-tailed test is indicated by ^. All statistics are rounded to comply with requirements of the U.S. Census Bureau.

Panel A: Missing Wages

Dependent Variable = *Wage Imputation Indicator*	(1)
Pre(t-3) × Fraud Ind.	0.012
	(0.011)
Pre(t-2) × Fraud Ind.	-0.002
	(0.016)
Pre(t-1) × Fraud Ind.	-0.008
	(0.016)
Fraud(t) × Fraud Ind.	-0.013
	(0.015)
Fraud(t+1) × Fraud Ind.	0.005
	(0.012)
Fraud(t+2) × Fraud Ind.	0.001
	(0.016)
Post(T) × Fraud Ind.	-0.003
	(0.014)
Post(T+1) × Fraud Ind.	0.004
	(0.015)
Post(T+2) × Fraud Ind.	0.029
	(0.019)
Post(T+3) × Fraud Ind.	0.015
	(0.016)
Post(T+4) × Fraud Ind.	0.005
	(0.017)
Post(T+5) × Fraud Ind.	0.019
	(0.018)
Fixed Effects	Event-time, Year, Industry, County, Worker
Observations	9,554,000

Panel B: Missing Wage Imputation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Table:	Table 3	Table 4	Table 4	Table 5	Table 6	Table 6	Table 6	Table 6	Table 7	Table 7	Table 7
Panel & Column:	Col. 3	B: Col. 1	B: Col. 2	D: Col. 3	Col. 1	Col. 2	Col 3.	Col. 4	Col. 1	Col. 2	Col 3.
Analysis Summary:	Main Result	Leavers	Stayers	New Workers	Thick Markets	Thin Markets	Top 10%	Bottom 90%	Fraud Year Match	Employee Match	Revelation Match
Pre-Fraud *(t-1)*	-0.565**	-0.530*	-0.242	-0.357*	-0.272	-0.734***	-0.362	-0.597**	-0.500**	-0.376*	-0.259
	(-2.37)	(-1.90)	(-0.94)	(-1.78)	(-1.15)	(-2.63)	(-1.45)	(-2.36)	(-2.14)	(-1.78)	(-1.12)
Pre-Revelation *(T-1)*	-0.546***	-0.552**	-0.235	-0.381*	-0.348*	-0.666***	-0.405*	-0.571***	-0.527***	-0.329*	-0.216
	(-2.69)	(-2.28)	(-1.05)	(-1.92)	(-1.72)	(-2.86)	(-1.78)	(-2.65)	(-2.90)	(-1.75)	(-1.24)
Real Observations	8,720	4,633	4,087	3,306	3,670	5,050	870	7,850	8,310	8,693	10,970
Imputed Observations	834	731	103	433	448	386	83	751	863	828	1280
Total Observations	9,554	5,364	4,190	3,739	4,118	5,436	953	8,601	9,173	9,521	12,250

Table 10
Worker-Focused Rationale for Committing Fraud This table reports estimates of the ex ante present value (PV) for workers to commit fraud as a percentage of annual wages. Benefit and cost inputs to the PV calculation are derived from our analyses, prior literature, and flexible assumptions. The discount rate is equal to PV calculations elsewhere in the paper, i.e., 4.47%. Also presented are subjective probabilities of uncaught fraud where the PV equals zero; if the probability is calculated as negative, we indicate that it is not meaningful ("nm"). We use an alternative scenario where 5% of workers depart as opposed to committing fraud, i.e., an avoided cost with fraud is not laying off 5% of the existing employees (John et al., 1992). Lost wages for departing workers are calculated over a 6-year period using initial and final year estimates from Couch and Placzek (2010) with linear interpolation. Column (1) indicates the chance of getting caught (latent fraud detection rates) using various estimates from Zakolyukina (2018) and Dyck et al. (2023). Columns (2)–(4) and (5)–(7) use match control firms and an industry average, respectively, as the benchmark (counterfactual) for estimating normal employment growth. Columns (2) & (5), (3) & (6), and (4) & (7) calculate benefits for new employees with assumptions of 1-, 3-, and 5-year horizons, respectively, when reverting from excess hiring during the fraud period (using the gross, average employment growth of 9.1%) to the benchmark in the post-fraud period. See additional details of the calculation at Internet Appendix Table 14.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Benchmark Firm:		**Matched Control Firms**			**Industry Average**		
Time to revert to steady state if fraud is not caught:		1 year	3 years	5 years	1 year	3 years	5 years
Estimations of latent detection rates of accounting misrepresentation in—	Chance caught:	**Ex ante PV of fraud for average worker as a percentage of annual wages**					
Zakolyukina (2018) caught misstatements in any one year	3%	4.9%	6.9%	8.3%	−3.7%	−2.3%	−1.3%
Zakolyukina (2018) caught misstatements over 5 years	16%	−2.1%	−0.4%	0.8%	−10.1%	−8.9%	−8.0%
Dyck et al. (2023) caught fraud by auditor	29%	−9.0%	−7.6%	−6.5%	−16.3%	−15.3%	−14.6%
Dyck et al. (2023) caught AAER fraud by SEC	52%	−21.3%	−20.3%	−19.6%	−27.4%	−26.8%	−26.3%
		Subjective probabilities of uncaught fraud with breakeven PV (i.e., ex ante PV = 0%)					
		12.1%	15.4%	17.5%	nm	nm	0.4%

experience cumulative wage losses of about 56.5% of their annual wages measured as a pre-fraud present value and 54.6% measured as a pre-revelation present value, using the estimates from column 1. In the Internet Appendix, IA Table 12 shows five other imputation approaches using (1) the 10th percentile of wages regardless of the employee's work status in unobserved states, (2) an industry-year adjusted 10th percentile of wages, (3) zero wages, (4) estimates of unemployment insurance payments when eligible and zero when ineligible (e.g., Graham et al., 2023), and (5) estimates of unemployment insurance payments when eligible and observations left missing when ineligible. PV estimates across these approaches are significantly negative, and losses range from −38.1% to −104.9% in column (3). We believe that our approach in Table 9 is reasonable in that we use all available data, i.e., the nationwide panel indicating wages, and avoid aggressive approaches that deepen estimated losses. For completeness, we reperform all of our main analyses and matching robustness in the remaining columns of Table 9 Panel B with the balanced, imputed wage sample.[37,38]

4.6. Simulation of the Executive's fraud decision

Presumably, executives commit fraud considering (expected) benefits and costs (Amiram et al., 2020; Becker, 1968). Given some executives may argue that fraud would protect their workforce from layoffs or even firm failure, we evaluate the net employee benefits and costs of fraud using reasonable parameter assumptions (Couch and Placzek, 2010; Dyck et al., 2023; John et al., 1992). With this approach, we also estimate the subjective probability of detection where the net cost (and net benefit) to employees is zero and the expected costs for workers of all fraud, caught *and* uncaught.

These net costs and subjective probabilities of detection (i.e., likelihood of the fraud going uncaught) calculations are described in IA Table 15. We ignore all other costs and benefits attributable to fraud not specific to the (potential) fraud firm's workforce. These calculations require material assumptions that cannot be validated with our data because uncaught fraud (or an unchosen alternative, like layoffs) often entails unobservable counterfactuals. However, we borrow measures and estimates from prior literature for, e.g., layoffs at distressed firms (John et al., 1992) and fraud detection rates (Dyck et al., 2023; Zakolyukina, 2018).

We present a simulation of an executive's worker-focused rationale for the decision to commit fraud in Table 10. With a variety of

[37] In IA Table 13, we use the dense fixed effects from Section 4.4 with the imputation approach from Section 4.5. The pattern of the results is similar in that most signs and significance are equivalent to Table 8. A few of the PVs become more significant and two lose significance at conventional levels: the pre-fraud and pre-revelation PV estimates with employee-based matching using year-industry-county fixed effects.

[38] Related to worker displacement, perhaps many frauds are revealed during recessions, like the financial crisis, and so negative wage effects could be driven by tough labor market conditions from these macro trends. We prefer to retain all frauds in our sample because the average labor market condition when fraud is revealed matters for estimating the average effects for employees from fraudulent financial reporting. In IA Table 14, we conduct a test that excludes frauds revealed in years 2000, 2001, 2007, 2008, and 2009 and find some attenuation in the present value calculations. This is consistent with more negative effects for employees when fraud is revealed during recessionary periods. The magnitude excluding these years is still large, with estimated cumulative wage losses of 35%–53%, larger than estimates from, e.g., trade competition from China of 23% (Autor et al., 2014). On average, 28% of all AAER fraud cases are revealed during these recession years. These cases are worse for employees and are highly relevant for our estimates.

possible inputs, only the more optimistic assumptions (i.e., lowest chances of being caught and longest lasting benefits to newly hired employees) produce estimates that are potentially beneficial for employees. An example calculation suggests that a combination of avoided layoffs of 5%, continued abnormal growth for three years if uncaught, and fraud detection rates of 52% have a cumulative expected cost for workers of 20% of annual wages. Fixing the expected cost at zero indicates that executives, if only considering the net benefit to workers, act as if the probability of (AAER) fraud detection is 15% or lower, much more favorable than estimates from academic literature (e.g., Dyck et al., 2023).

This simulation of the executive's fraud decision contains another useful calculation, the expected cost to employees of all fraud, both caught and uncaught. As with other papers examining fraud, we only include detected and penalized fraud. Importantly, a finding that rank-in-file employees suffer wage losses when firms are discovered to have committed fraud does not necessarily imply that rank-in-file employees suffer, on average, when firms commit fraud because some firms commit fraud and go undetected (or unpenalized by authorities). In the simulation, the executive is comparing the net cost or net benefit of fraud to an alternative cost, like immediate layoffs. If we drop this alternative cost, the remaining estimate is the cost to employees of all fraud. Across the scenarios presented in Table 10, the net benefit of all fraud for workers ranges between 2% and −34% (untabulated). The assumptions above (three years of continued abnormal growth and detection rates of 52%) result in the average employee, between both newly hired and existing, incurring 27% in expected, cumulative wage losses.

5. Conclusion

This paper provides evidence on the consequences for employees from fraudulent financial reporting. We use employer-employee matched data from the U.S. Census Bureau combined with SEC enforcement actions against firms with serious misreporting, i.e., fraud, to examine employee wages and displacement. Compared to the employees at non-fraud control firms, we find that employees at fraud firms have lower wages after periods of fraudulent financial reporting even though fraud firms have higher employment growth during the fraud. During the fraud, executives appear to overbuild and change employee composition. We also find that employees at fraud firms are more likely, compared to a matched sample of employees at non-fraud firms, to leave the firm, industry, and county of employment after the fraud is revealed. Overall, fraud firms unwind this overbuilding and have negative employee growth, driving the negative wage consequences as workers are displaced.

We discuss and show evidence consistent with mechanisms for these wage effects. The negative change in wages combined with employee displacement and negative employment growth at fraud firms indicates workers suffer negative labor market outcomes, for instance losses arising from firm-specific investments, job search inefficiency, and/or entering crowded labor markets. In particular, wage losses are worse in thin labor markets. Finally, we examine workers in the bottom 90% (top 10%) of the pre-fraud wage distribution and perhaps surprisingly (do not) find negative wage effects during and after fraudulent financial reporting.

This paper is among the first to measure wage and turnover effects for employees at fraud firms. We note some caveats and potential avenues for future research. First, we show evidence that is consistent with certain real actions taken by executives that relate to fraud, such as overbuilding during the fraud; however, isolating any specific channel is a challenge and other unexplored channels could exist. For instance, the job search frictions after the fraud and disruption to labor markets are both related to the severity of economic shocks to the firm. Second, matched difference-in-differences designs do not necessarily show causation (Roberts and Whited, 2013). We find the labor market consequences that happen concurrently, with little evidence for pre-period trends, so we are confident these effects are associated with the fraud but not necessarily caused by it. Third, the (unexplored) heterogeneous consequences of fraudulent financial reporting for employees might deserve more attention. Furthermore, other types of financial misconduct could be consequential as well but are not in our sample (Egan et al., 2019). Finally, we do not study the effects spreading beyond the affected employees (e.g., their families and neighborhoods) (Holzman et al., 2021).

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jacceco.2024.101673.

References

Abowd, John M., Stephens, Bryce E., Vilhuber, Lars, Andersson, Fredrik, McKinney, Kevin L., Roemer, Marc, Woodcock, Simon, 2005. The LEHD Infrastructure Files and the Creation of Quarterly Workforce Indicators. U.S. Census Bureau, Suitland, MD.
Acemoglu, Daron, Autor, David, 2011. Skills, tasks and technologies: implications for employment and earnings. In: Handbook of Labor Economics, 4. Elsevier, 1,043-1,171.
Amiram, Dan, Huang, Serene, Rajgopal, Shiva, 2020. Does financial reporting misconduct pay off even when discovered? Rev. Account. Stud. 25 (3), 811–854.
Autor, David H., Katz, Lawrence F., Kearney, Melissa S., 2008. Trends in U.S. Wage inequality: revising the revisionists. Rev. Econ. Stat. 90 (2), 300–323.
Autor, David H., Dorn, David, Hanson, Gordon H., Song, Jae, 2014. Trade adjustment: worker-level evidence. Q. J. Econ. 129 (4), 1799–1860.
Baily, Martin Neil, 1974. Wages and employment under uncertain demand. Rev. Econ. Stud. 41 (1), 37–50.
Bayot, Jennifer, 2002. Turmoil at WorldCom: the employees; misery in Mississippi, reward on maui. The New York Times June 29.
Beatty, Anne, Liao, Scott, Jeff Jiewei, Yu, 2013. The spillover effect of fraudulent financial reporting on peer firms' investments. J. Account. Econ. 55, 183–205, 2-3.
Becker, Gary S., 1968. Crime and Punishment: an Economic Approach." *the Economic Dimensions Of Crime*. Palgrave Macmillan, pp. 13–68.
Becker, Gary S., 1993. Human capital revisited. In: Human Capital: A Theoretical and Empirical Analysis with Special Reference to Education, third ed. The University of Chicago Press, pp. 15–28.
Beneish, Messod D., 1999. Incentives and penalties related to earnings overstatements that violate GAAP. Account. Rev. 74 (4), 425–457.

Benhabib, Jess, Alberto, Bisin, 2018. Skewed wealth distributions: theory and empirics. J. Econ. Lit. 56 (4), 1,261-1,291.
Berk, Jonathan B., Stanton, Richard, Zechner, Josef, 2010. Human capital, bankruptcy, and capital structure. The Journal of Finance 65 (3), 891–926.
Bernstein, Shai, Colonnelli, Emanuele, Giroud, Xavier, Iverson, Benjamin, 2019. Bankruptcy spillovers. J. Financ. Econ. 133 (3), 608–633.
Bertrand, Marianne, Goldin, Claudia, Katz, Lawrence F., 2010. Dynamics of the gender gap for young professionals in the financial and corporate sectors. Am. Econ. J. Appl. Econ. 2 (3), 228–255.
Brown, Jennifer, Matsa, David A., 2016. Boarding a sinking ship? An investigation of job applications to distressed firms. The Journal of Finance 71 (2), 507–550.
Caggese, Andrea, Cunat, Vincente, Metzger, Daniel, 2019. Firing the wrong workers: financing constraints and labor misallocation. J. Financ. Econ. 133 (3), 589–607.
Call, Andrew C., Martin, Gerald S., Sharp, Nathan Y., Wilde, Jaron H., 2018. Whistleblowers and outcomes of financial misrepresentation enforcement actions. J. Account. Res. 56 (1), 123–171.
Chemmanur, Thomas J., Cheng, Yingmei, Zhang, Tianming, 2013. Human capital, capital structure, and employee pay: an empirical analysis. J. Financ. Econ. 110 (2), 478–502.
Choi, Bong-Geun, Choi, Jung Ho, Malik, Sara, 2023a. No just for investors: the role of earnings announcements in guiding job seekers. J. Account. Econ., 101588
Choi, Jung Ho, Gipper, Brandon, Malik, Sara, 2023b. Financial reporting quality and wage differentials: evidence from worker-level data. J. Account. Res. 61, 4, 1,109-1,158.
Coates, John C., 2011. Testimony of John C. Coates IV before the U.S. Senate Subcommittee on Securities, Insurance and Investment on Proposed Securities Law Reforms. Working Paper. Available at SSRN 1973258.
Combes, Pierre-Philippe, Gobillon, Laurent, 2015. The empirics of agglomeration economies. In: In Handbook of Regional and Urban Economics, 5. Elsevier, pp. 247–348.
Couch, Kenneth A., Placzek, Dana W., 2010. Earnings losses of displaced workers revisited. Am. Econ. Rev. 100 (1), 572–589.
de Haan, Ed, Nan, Li, Zhou, Frank, 2023. "Financial reporting and employee job search.". J. Account. Res. 61 (2), 571–617.
Dechow, Patricia, Ge, Weili, Schrand, Catherine, 2010. Understanding earnings quality: a review of the proxies, their determinants and their consequences. J. Account. Econ. 50, 2–3, 344-401.
Dechow, Patricia M., Ge, Weili, Larson, Chad R., Sloan, Richard G., 2011. Predicting material accounting misstatements. Contemp. Account. Res. 28 (1), 17–82.
Desai, Hemang, Hogan, Chris E., Wilkins, Michael S., 2006. The reputation penalty for aggressive accounting: earnings restatements and management turnover. Account. Rev. 81 (1), 83–112.
Dore, Timothy E., Zarutskie, Rebecca, 2017. "Firm Leverage, Labor Market Size, and Employee Pay." Finance and Economics Discussion Series 2017-078. Washington: Board of Governors of the Federal Reserve System.
Dyck, Alexander, Morse, Adair, Zingales, Luigi, 2023. How pervasive is corporate fraud? Rev. Account. Stud. 1–34.
Egan, Mark, Matvos, Gregor, Amit, Seru, 2019. The market for financial adviser misconduct. J. Polit. Econ. 127 (1), 233–295.
Erickson, Merle, Hanlon, Michelle, Maydew, Edward L., 2004. How much will firms pay for earnings that do not exist? Evidence of taxes paid on allegedly fraudulent earnings. The Accounting Review 79 (2), 387–408.
Falato, Antonio, Liang, Nellie, 2016. Do creditor rights increase employment risk? Evidence from loan covenants. J. Finance 71 (6), 2545–2590.
Farber, David B., 2005. Restoring trust after fraud: does corporate governance matter? Account. Rev. 80 (2), 539–561.
Feroz, Ehsan H., Park, Kyungjoo, Pastena, Victor S., 1991. The financial and market effects of the SEC's accounting and auditing enforcement Releases. J. Account. Res. 29 (Suppl. ment), 107–142.
Gibbons, Robert, Katz, Lawrence F., 1991. Layoffs and lemons. J. Labor Econ. 9 (4), 351–380.
Giroud, Xavier, Mueller, Holger M., 2017. Firm leverage, consumer demand, and employment losses during the Great Recession. Q. J. Econ. 132 (1), 271–316.
Goldin, Claudia, Katz, Lawrence F., 1998. The origins of technology-skill complementarity. Q. J. Econ. 113 (3), 693–732.
Goldin, Claudia, Kerr, Sari Pekkala, Olivetti, Claudia, Barth, Erling, 2017. The expanding gender earnings gap: evidence from the LEHD-2000 Census. Am. Econ. Rev. 107 (5), 110–114.
Graham, John R., Kim, Hyunseob, Li, Si, Qiu, Jiaping, 2023. Employee costs of corporate bankruptcy. J. Finance 78 (4), 2087–2137.
Davis, Steven J., von Wachter, Till M., 2011. Recessions and the Cost of Job Loss. Working Paper. Available at NBER 17638.
Groysberg, Boris, Lin, Eric, George, Serafeim, 2017. Does financial misconduct affect the future compensation of alumni managers? Working Paper. Available at SSRN 3069937.
Heckman, James J., Lochner, Lance, Todd, Petra E., 2003. Fifty years of Mincer earnings regressions. National Bureau of Economic Research Working Paper No. 9732.
Hillegeist, Stephen A., Keating, Elizabeth K., Cram, Donald P., Lundstedt, Kyle G., 2004. Assessing the probability of bankruptcy. Rev. Account. Stud. 9 (1), 5–34.
Holzman, Eric R., Miller, Brian P., Williams, Brian M., 2021. The local spillover effect of corporate accounting misconduct: evidence from city crime rates. Contemp. Account. Res. 38 (3), 1,542-1,580.
Hummels, David, Jorgensen, Rasmus, Munch, Jakob, Xiang, Chong, 2014. The wage effects of offshoring: evidence from Danish matched worker-firm data. Am. Econ. Rev. 104 (6), 1597–1629.
Hyatt, Henry, McEntarfer, Ericka, 2012. Job-to-Job flows in the great recession. Am. Econ. Rev.: Papers & Proceedings 102 (3), 580–583.
Imbens, Guido W., Rubin, Donald B., 2015. Causal Inference in Statistics, Social, and Biomedical Sciences. Cambridge University Press.
Isidro, Helena, Nanda, Dhananjay (DJ), Wysocki, Peter D., 2020. On the relation between financial reporting quality and country attributes: research challenges and opportunities. Account. Rev. 95 (3), 279–314.
Jacobson, Louis S., LaLonde, Robert J., Sullivan, Daniel G., 1993. Earnings losses of displaced workers. Am. Econ. Rev. 83 (4), 685–709.
Jiang, John, Shen, Michael, 2018. Labor Market Outcomes of Restatements for Corporate Accountants. Working Paper. Available at SSRN 3224481.
John, Kose, Lang, Larry HP., Netter, Jeffry, 1992. The voluntary restructuring of large firms in response to performance decline. J. Finance 47 (3), 891–917.
Karpoff, Jonathan M., Scott Lee, D., Martin, Gerald S., 2008a. The consequences to managers for financial misrepresentation. J. Financ. Econ. 88 (2), 193–215.
Karpoff, Jonathan M., Scott Lee, D., Martin, Gerald S., 2008b. The cost to firms of cooking the books. J. Financ. Quant. Anal. 43 (3), 581–611.
Karpoff, Jonathan M., Allison Koester, D., Scott, Lee, Martin, Gerald S., 2017. Proxies and databases in financial misconduct research. Account. Rev. 92 (6), 129–163.
Kedia, Simi, Philippon, Thomas, 2009. The economics of fraudulent accounting. Rev. Financ. Stud. 22 (6), 2169–2199.
Kedia, Simi, Rajgopal, Shiva, 2011. Do the SEC's enforcement preferences affect corporate misconduct? J. Account. Econ. 51 (3), 259–278.
Li, Valerie, 2016. Do false financial statements distort peer firms' decisions? Account. Rev. 91 (1), 251–278.
Machin, Stephen, Salvanes, Kjell G., Pelkonen, Panu, 2012. Education and mobility. Journal of the European Economic Association 10 (2), 417–450.
Malik, Sara, 2022. "Without a Word of WARN-Ing: Advance Notice, Information Quality, and Labor Market Outcomes." Working Paper. Available at SSRN 4211875.
McNichols, Maureen F., Stubben, Stephen R., 2008. Does earnings management affect firms' investment decisions? Account. Rev. 83 (6), 1571–1603.
Moretti, Enrico, 2011. Local labor markets. Handb. Labor Econ. 4, 1237–1313.
Noguchi, Yuki, 2002. WorldCom lays off 17,000 workers. The Washington Post June 29.
Ouimet, Paige, Zarutskie, Rebecca, 2014. Who works for startups? The relation between firm age, employee age, and growth. J. Financ. Econ. 112 (3), 386–407.
Raghunandan, Aneesh, 2021. Financial misconduct and employee mistreatment: evidence from wage theft. Rev. Account. Stud. 26 (3), 867–905.
Roberts, Michael R., Whited, Toni M., 2013. Chapter 7: endogeneity in empirical corporate finance. Handbook of the Economics of Finance 2.A 493–572.
Sadka, Gil, 2006. The economic consequences of accounting fraud in product markets: theory and a case from the US telecommunications industry (WorldCom). Am. Law Econ. Rev. 8 (3), 439–475.
Schrand, Catherine M., Zechman, Sarah L.C., 2012. Executive overconfidence and the slippery slope to financial misreporting. J. Account. Econ. 53 (1–2), 311–329.
Securities and Exchange Commission (SEC), 2004. "SEC Charges Kenneth L. Lay, Enron's Former Chairman and Chief Executive Officer, with Fraud and Insider Trading." AAER No. 2051 July 8. Available at: https://www.sec.gov/litigation/litreleases/lr18776.htm.
Srinivasan, Suraj, 2005. Consequences of financial reporting failure for outside directors: evidence from accounting restatements and audit committee members. J. Account. Res. 43 (2), 291–334.
Sullivan, Daniel, von Wachter, Till, 2009. Job displacement and mortality: an analysis using administrative data. Q. J. Econ. 124 (3), 1265–1306.

Tate, Geoffrey, Liu, Yang, 2015. The bright side of corporate diversification: evidence from internal labor markets. Rev. Financ. Stud. 28 (8), 2203–2249.
Tate, Geoffrey A., Liu, Yang, 2016. The human factor in acquisitions: cross-industry labor mobility and corporate diversification. Working Paper.
Topel, Robert, 1991. Specific capital, mobility, and wages: wages rise with job seniority. J. Polit. Econ. 99 (1), 145–176.
US Bureau of Labor Statistics (BLS), 2016. Quarterly Census of Employment and Wages: Handbook of Methods. https://www.bls.gov/cew.
Walker, W. Reed, 2013. The transitional costs of sectoral reallocation: evidence from the clean Air act and the workforce. Q. J. Econ. 128 (4), 1787–1835.
Whitaker, Richard B., 1999. The early stages of financial distress. J. Econ. Finance 23 (2), 123–132.
Zakolyukina, Anastasia A., 2018. How common are intentional GAAP violations? Estimates from a dynamic model. J. Account. Res. 56 (1), 5–44.
Zhou, Yuqing, Makridis, Christos, 2019. Financial Misconduct and Changes in Employee Satisfaction. Working Paper. Available at SSRN 3467787.