Beyond Blame: Evaluative Stigma, Attribution, and Employee Careers after Employer Failure*

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Abstract

Employment continuity facilitates access to career opportunities, professional growth, and financial security, making involuntary disruptions—such as employer failures—potentially consequential for individuals' career trajectories. Although prior research has explored how organizational leaders' careers fare following employer failure, implications for the modal employee remain theoretically unclear. Integrating theories of evaluative stigma, evaluation, and careers, we develop a theoretical framework to clarify how employer failure relates to subsequent career outcomes for employees. Using confidential, anonymized data from the US Census complemented by detailed, identifiable data from the Automatic Speech Recognition industry, we find that employer failure is negatively related to organizational leaders' wage growth, consistent with internal attribution and evaluative stigma. Conversely, modal employees experience wage (and industry retention) outcomes comparable to their unaffected peers, consistent with an affiliation with a failed employer not stigmatizing this set of employees. However, modal employees are not always unscathed: Career penalties emerge when failure involves a scandal, when industry labor market conditions become more competitive, and when employees belong to marginalized demographic groups (gender, race, immigration). By clarifying conditions of evaluative stigma, we extend organizational research on careers and evaluations, highlighting the conditions under which affiliations with failed firms are related to subsequent career outcomes.

Keywords: careers, evaluations, failure, labor markets, human capital, scandal, stigma

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Employment provides individuals with financial stability, professional development, and personal security, making job continuity a central concern for most employees. Although certain occupations—such as project-based or freelance roles—involve frequent career transitions (e.g., Barley, Bechky, and Milliken 2017), the majority of individuals strongly prefer stable, long-term employment within formal organizations (Kalleberg 2009; Kalleberg, Reskin, and Hudson 2000). This preference for stability and fit can lead employees to remain in their current roles (Meyer and Herscovitch 2001; Mitchell and Dacin 1996; Mitchell et al. 2001; Mobley 1977; Sørensen 2000), even when better alternatives are available outside their employer (Jäger et al. 2021; Mincer 1986). Consequently, individuals may undervalue external labor market opportunities and underestimate their earning potential. For example, a survey by Pew Research showed that while 80% of employees were not planning to look for a job in the next six months, those who did realized wage gains (Igielnik 2022).

An acute, yet understudied, threat to job continuity and stability is employer failure—when organizations cease operations, forcing employees to look for new jobs. Such failures can prompt individuals to reassess their professional trajectories, but the involuntary nature of these disruptions introduces significant uncertainty about both immediate earnings and longer-term career prospects. Despite the substantial disruption that employer failure poses—and the extensive literature on related employment shocks, such as layoffs and other forms of "precarious work" (Kalleberg 2009)—theoretical and empirical research on how failure shapes employee careers remains limited.

Organizational research has provided suggestive evidence for organizational leaders. Those at or near the top of the firm hierarchy often experience adverse career outcomes following their employer's failure (Rider and Negro 2015; Semadeni et al. 2008; Sutton and Callahan 1987),[1] Because leaders' play an influential and central role in shaping organizational outcomes (Boeker 1992; Park, Chung, and Rajagopalan 2021; Park and Westphal 2013; Salancik and Meindl 1984), hiring firms are likely to make internal attributions (Heider 1958; Ross and Fletcher 1985), interpreting failure as reflective of their competence. This attribution results in an evaluative stigma for leaders from failed firms (Semadeni et al. 2008; Sutton and Callahan

1. An exception is Carnahan (2017), who examines the failure of solo-practitioner law firms; however, that study investigates the founding of new firms rather than the subsequent career trajectories of practitioners.

1987).

When we consider whether evaluative stigma extends downward to the modal employee, a theoretical tension emerges. On the one hand, affiliation with a failed firm could stigmatize all employees by signaling lower average quality, skills, or potential to prospective employers—a perspective that has been suggested but not theorized or tested (see Figure 1 in Wiesenfeld, Wurthmann, and Hambrick 2008). On the other hand, employers may treat affiliation with a failed firm as situational and therefore not informative about the quality of the modal employee. In this scenario, displaced workers should experience outcomes comparable to peers from ongoing firms (i.e., firms that have not failed). Moreover, because failure forces individuals into job searches that they might not otherwise have completed, they may realize career outcomes similar to those of peers who move voluntarily between ongoing firms. We address this tension by developing a framework that integrates insights from research on evaluations, stigma, attribution, and careers, asking: *What are the career consequences of employer failure for the average employee?*

Our framework centers on the conditions under which evaluative stigma (Goffman 1963) is likely to arise after employer failure. Hiring under uncertainty often leads firms to rely heavily on observable—even if imperfect—signals to infer candidates' quality, potential, and fit (e.g., Chatman 1991; Correll, Benard, and Paik 2007; Corritore, Goldberg, and Srivastava 2018; Rivera 2012). One particularly salient signal is prior employer affiliation: Candidates from stable, reputable firms are typically perceived as higher quality (Bidwell et al. 2014; Phillips 2001). Reversing this logic suggests that affiliation with unstable or failed firms may convey a negative signal. Although employers are unlikely to explicitly blame the average employee for their firm's failure, evaluative stigma may still extend downward through "courtesy stigma" (Goffman 1963; Jensen 2006; Wiesenfeld, Wurthmann, and Hambrick 2008). Even absent direct responsibility or blame, evaluators might infer that firms that fail employ workers of lower average quality, leading to internal attributions that resemble those directed at leaders. In this way, the modal employee's affiliation with a failed firm can become discrediting, negatively shaping subsequent career outcomes.

If evaluative stigma does not extend to the modal employee, whether through direct stigma

or courtesy stigma, then employer failure produces a theoretically important divergence: Outcomes differ systematically by hierarchical position. Whereas leaders are likely subject to internal attributions and thus evaluative stigma, modal employees may be insulated because they have little influence over strategic decisions or firm-level outcomes. For these employees, affiliation with a failed firm may be treated as situational—consistent with external attribution (Repenning and Sterman 2002; Ross 1977; Ryan and Connell 1989; Schilit and Locke 1982; Shaver 2012)—or as simply uninformative for assessing quality. Moreover, most firm failures are relatively routine. Although high-profile collapses such as Enron, WorldCom, and Lehman Brothers dominate public perceptions, such dramatic cases are rare. Between 1993 and 2019, roughly 800,000 firms failed annually in the United States, displacing about 3.85 million employees each year, 3.82 million of whom worked at firms with fewer than 1,000 employees (Figure 1). These common, lower-profile failures are unlikely to provoke strong evaluative stigma for non-leaders. Together, this perspective underscores a central contribution of our framework: Employer failure does not stigmatize employees uniformly; rather, attribution processes generate differentiated evaluative stigma between organizational leaders and modal employees.

[Figure 1]

After establishing the general theoretical relationship between employer failure and employee careers, we extend our framework by considering additional sources of heterogeneity that may affect the average employee's career outcomes after employer failure. First, we examine how characteristics of the failure itself—in particular, routine failures versus those involving scandal—might influence the likelihood of evaluative stigma across the organizational hierarchy. Second, we theorize that broad labor-market conditions, including industry growth or contraction and the local concentration of competitors, shape the career opportunities available after employer failure. Finally, we explore how individual-level characteristics relevant to hiring decisions moderate career outcomes after employer failure. Specifically, we consider specialized human capital, given its close link to firm performance (Becker 1964; Jara-Figueroa et al. 2018; Shrader and Siegel 2007), and we examine demographic factors, namely gender, race, and immigration status, which have been shown to influence evaluative biases during hiring (Castilla 2008; Correll and Benard 2006; Pager, Bonikowski, and Western 2009).

A primary reason for the limited theoretical and empirical understanding of employer failure and employee careers is the difficulty of acquiring comprehensive data on employee career trajectories after firm failure. Prior organizational research has largely concentrated on organizational leaders, often examining a small number (or even just one) of high-profile failures, and has usually focused on subsequent career outcomes within the same industry. Although this work has generated valuable insights into outcomes, such as title changes and employer prestige, it has overlooked the potential career impact of employer failure on the modal employee and has not considered a fundamental career outcome: wages.

To address these challenges and test our more generalizable theory, we collected two complementary employee-employer matched datasets: One from the United States Census (Census) and another from the Automatic Speech Recognition (ASR) industry. We obtained access to 25 years of employee-employer matched data from the Census and the Internal Revenue Service, allowing us to track employment histories and earnings for nearly every taxpayer in the US and providing definitive information about whether an employment change was precipitated by employer failure. However, Census data are de-identified and lack detailed information about employee roles (e.g., titles) or specific firm characteristics that may be of interest. To address this limitation, we supplemented our analysis with data from the ASR industry, an innovation-driven sector characterized by frequent employer failure (Botelho and Chang 2022; Hall and Woodward 2010; Puri and Zarutskie 2012). The ASR data does not include wages but it provides detailed employee information, including job titles and specific firm contexts. Additionally, the ASR data contain two instances of employer failure associated with major scandals, enabling a direct test of our arguments related to the role of attribution and evaluative stigma. To estimate the demand-side effects of employer failure and subsequent career outcomes, we used coarsened exact matching (Blackwell et al. 2009; Iacus, King, and Porro 2012), comparing ex-employees from failed firms to similar employees who left firms that continued operating (ongoing firms).

Using the Census data, we find a negative relationship between employer failure and subsequent wage growth. However, this is limited to organizational leaders: Leaders from failed firms experience lower wage growth relative to similar leaders leaving ongoing firms, a result

consistent with prior research and suggestive of evaluative stigma via internal attribution. By contrast, and consistent with a lack of internal attribution for non-leaders, we find no evidence of a wage growth penalty for the modal employee from a failed firm. Employees who leave ongoing firms experience wage growth of about 10.8 percentage points in their next job and employees from failed firms experience slightly higher wage growth (an additional 1.7 percentage points). Thus, after firm failure, the modal employee's subsequent career is similar to peers from ongoing firms. We also find that employees from failed firms are not forced out of the labor market nor do they endure prolonged unemployment.

Results from the ASR dataset confirm this finding by using a complementary career outcome—whether the employee remains in the ASR industry. However, the ASR results also suggest a boundary condition related to evaluative stigma: Employees associated with scandal-related failures experience uniformly negative career outcomes, suggesting that scandals provoke outcomes consistent with internal attribution and evaluative stigma throughout levels of the organizational hierarchy. Finally, we discuss the role of labor market (e.g., shrinking industries, competition density) and employee characteristics (e.g., human capital, race/ethnicity, gender, immigration status), finding that certain employees suffer greater consequences than others from having worked at a failed firm.

EMPLOYMENT DISRUPTION AND THE CHALLENGE OF EVALUATING JOB CANDIDATES

Our theoretical focus is on one specific form of involuntary career disruption: Employer failure, namely the cessation of an organization's operations. Despite the frequency of employer failures and the substantial number of employees affected annually (Figure 1), our theoretical understanding of how an affiliation with a failed employer affects employees' subsequent careers remains surprisingly underdeveloped (see Botelho and Chang (2022) for a discussion). Unlike investors or suppliers, who can diversify their risks across multiple organizations, employees generally lack the same ability. Moreover, predicting firm failure ex ante remains inherently challenging, even for informed stakeholders (Haveman and Cohen 1994; Phillips 2001;

Puri and Zarutskie 2012). Given these considerations, developing a theoretical understanding of the conditions under which employer failure shapes employees' subsequent career outcomes is particularly important.

Layoffs represent a seemingly related, yet distinct, form of involuntary employment disruption, where employers selectively terminate a subset of employees. Research on layoffs—and especially "mass layoffs"—offers preliminary insight into involuntary career disruptions. Employees displaced by layoffs typically face wage penalties compared to unaffected employees (e.g., Couch and Placzek 2010; Jacobson, LaLonde, and Sullivan 1993; Lachowska, Mas, and Woodbury 2020). The primary explanation is that hiring firms interpret layoffs as negative indicators of employee quality, given that layoffs generally involve selective terminations where lower-performing employees are most likely to be dismissed (Gibbons and Katz 1991; Jacobson, LaLonde, and Sullivan 1993). Indeed, laid-off employees are shown to have lower wages even before displacement (Jacobson, LaLonde, and Sullivan 1993), reinforcing prospective employers' negative perceptions in subsequent labor market outcomes.

One might extrapolate that an affiliation with a failed employer similarly disadvantages affected employees. Prospective employers face uncertainty when assessing job candidates, lacking complete information about candidate quality and productivity. It is thus common for hiring firms to rely heavily on observable affiliations, proxies, and/or signals to reduce uncertainty and inform hiring evaluations (Spence 1973). Research has shown that employers frequently depend on various affiliations and observable signals—even those with tenuous connections to underlying quality—to assess competence, capability, and organizational fit (Chatman 1991; Correll, Benard, and Paik 2007), including one's prior employer (Bidwell et al. 2014; Botelho and Chang 2022; Phillips 2001). However, employer failure substantively differs from layoffs in ways that complicate generalization. Layoffs typically send a much clearer signal about individual employee performance; for example, among a large set of employees, the firm decided to only lay off a given subset. Employer failures, meanwhile, simultaneously affect all employees, which may introduce ambiguity regarding a given employee's competence, skills, and quality.

This theoretical ambiguity sets the stage for our framework, in which we consider the intersection of employer failure, evaluations, and employee careers. Next, we draw from research

on organizational leaders affected by employer failure and integrate insights from attribution theory and evaluative stigma to theorize the conditions under which employees may (or may not) face an evaluative stigma in their subsequent career after employer failure. We then extend our theoretical framework by briefly considering additional sources of heterogeneity—including whether the employer failure included a scandal, broader labor-market conditions, and individual-level characteristics—all of which may moderate the presence of an evaluative stigma associated with affiliation to a failed employer.

EMPLOYER FAILURE, EVALUATIVE STIGMA, AND SUBSEQUENT CAREER OUTCOMES

Organizational Leaders and Attribution

Evaluative stigma occurs when an individual possesses an attribute, has an affiliation, or engages in behaviors that external audiences perceive as discrediting (Goffman 1963). For organizational leaders—those at or near the top of a firm's hierarchy—one particularly salient characteristic shaping external audiences' evaluations is the performance of their firm (Salancik and Meindl 1984). Leaders typically reap significant benefits during periods of strong organizational performance but suffer considerable negative consequences when performance deteriorates. During periods of poor firm performance, organizational leaders bear the brunt of external criticism, often losing power, experiencing job termination, and facing difficulties securing comparable employment elsewhere (Gilson 1989; Hambrick and Mason 1984; Hermalin and Weisbach 1998; Park, Chung, and Rajagopalan 2021; Park and Westphal 2013). Leaders are aware of the evaluative stigma related to firm outcomes and may proactively engage in impression management or even identify scapegoats within the organization to deflect blame and protect their professional reputations (Boeker 1992).

Evaluative stigma emerges in contexts of poor firm performance because external evaluators must engage in sensemaking to understand negative organizational outcomes and attribute blame (Wiesenfeld, Wurthmann, and Hambrick 2008). Organizational leaders, who are inher-

ently responsible for their firms' strategic direction, become natural focal points for such attributions. Although negative evaluations stemming from employer failure may occur from a variety of external audiences, including investors, media, and suppliers, prospective employers are particularly important evaluators. Employer failure forces leaders (and all other employees) into the labor market, making prospective employers' assessments especially consequential for their subsequent career outcomes.

Organizational research provides limited insights into the connection between employer failure and subsequent career outcomes, primarily through single-industry methodologies documenting negative career effects for leaders (Rider and Negro 2015; Semadeni et al. 2008; Sutton and Callahan 1987). For example, leaders from failed banks faced a greater likelihood of demotion in their subsequent employment compared to similarly positioned leaders who exited the same banks prior to failure (Semadeni et al. 2008). Sutton and Callahan's (1987) analysis of four computer firm bankruptcies links these career consequences to evaluative stigma, emphasizing that firm failure "spoils" the reputation of organizational leaders.

Central to these theoretical expectations of an evaluative stigma is the assumption of internal attribution: Prospective employers hold organizational leaders directly responsible for the failure of the firms they lead. Attribution theory provides a useful theoretical structure to this reasoning, distinguishing between internal and external attributions (Heider 1958; Ross and Fletcher 1985). Internal attributions occur when evaluators perceive an affiliation with an event or outcome as informative of an individual's characteristics; external attributions, by contrast, reflect outcomes perceived as driven by situational or environmental factors beyond the individual's control and are thus uninformative about an individual's characteristics (Fiske and Taylor 1991; Ross 1977).[2] Given organizational leaders' clear responsibility for strategic direction and decision-making (Boeker 1992; Park, Chung, and Rajagopalan 2021; Park and Westphal 2013; Salancik and Meindl 1984), prospective employers should naturally default to internal attribution when evaluating these leaders, resulting in an evaluative stigma. Consistent with this point,

2. Conceptually, external attribution is distinct from a lack of internal attribution. Absent direct evidence of evaluator beliefs, however, the two are difficult to separate empirically and, in our setting, yield the same prediction: No evaluative stigma for modal employees. Because one's prior employer is a salient labor market signal, we treat the absence of internal attribution as consistent with external attribution, while acknowledging that we cannot definitively rule out true non-attribution.

evidence suggests that leaders who explicitly deny responsibility for their firm's failure are perceived even more negatively by evaluators, reinforcing the dominance of internal attribution in evaluating organizational leaders (Sutton and Callahan 1987).

In sum, although organizational research connecting employer failure and the subsequent careers of organizational leaders remains limited, existing evidence suggests adverse career outcomes for leaders associated with failed firms. The primary mechanism underlying these outcomes is evaluative stigma: Prospective employers interpret an affiliation with a failed firm as discrediting, negatively shaping career opportunities for organizational leaders.

The Modal Employee and Internal Attribution

Organizational leaders constitute only a small subset of a firm's workforce. Although existing organizational research has documented negative career outcomes for leaders affiliated with failed firms, it remains theoretically unclear whether—and under what conditions—these negative consequences extend downward to lower-level (i.e., modal) employees.

Internal attribution and blame vs. courtesy stigma. At first glance, applying internal attribution to the average employee seems logically questionable. Organizational leaders clearly bear direct responsibility for strategic decision-making whereas lower-level employees do not have such direct control over firm outcomes. For example, when a bank fails, its leadership is blamed for poor strategic decisions (Semadeni et al. 2008), but it would seem unreasonable to blame the bank tellers, human resources personnel, or the IT department at that bank for the failure. Nevertheless, internal attribution *does not necessarily require explicit blame*. Ross (1977: 176) discusses that observers (evaluators) can use an affiliation between an actor and an event to make inferences about the "internal dispositions of the actor (e.g., abilities, traits, or motives)." Thus, prospective employers may interpret affiliation with a failed employer as a noisy but credible signal regarding employee quality, skills, or competence, generating evaluative stigma through a process of "courtesy stigma" (Goffman 1963). Although Wiesenfeld et al. (2008: 233, their Figure 1) theorize about organizational leaders, they acknowledge the possibility that stigma arising from employer failure may extend categorically, affecting "a group or perhaps everyone associated with the firm." Thus, even without direct blame, mere affiliation

with a failed firm may become sufficiently discrediting to stigmatize the modal employee, producing negative career outcomes that closely mirror those resulting from the internal attribution faced by leaders.

Further supporting this expectation is the fact that hiring firms face significant uncertainty when assessing job candidates, prompting them to rely heavily on observable signals and proxies—even if noisy—to infer candidate quality (Spence 1973). Organizational research identifies prior employer affiliations as one influential signal utilized by prospective employers (Bidwell et al. 2014; Phillips 2001). Firms regard employees as strategic assets (Fombrun and Shanley 1990; Molloy and Barney 2015), and reputable, high-performing firms typically attract and retain employees possessing strong human and social capital (Gatewood, Gowan, and Lautenschlager 1993; Rivera 2012). Although existing literature primarily emphasizes the positive career implications of affiliation with successful firms, the inverse logic should also operate: An affiliation with a poorly performing (failed) firm could be interpreted negatively for the modal employee.

Even absent plausible culpability of non-leaders, one might infer that firms that eventually fail tend to employ workers of lower average quality relative to their more successful counterparts. Weaker firms could struggle to attract top talent, and the employees they do manage to hire may receive inferior training, develop overly specialized or obsolete skills, or lack human capital relevant to contemporary labor market demands (Groysberg, Lee, and Nanda 2008). It is also possible that these employees are in roles that they are not suited to hold. Phillips (2001) discusses this point directly, theorizing, that new and smaller firms—which are at greatest risk for failure—can use title promotions as a mechanism to keep employees at the firm. He finds that firms with characteristics consistent with a higher risk of failure are more likely to promote their employees rapidly. If a subset of these firms fails, failed firms will have a greater proportion of "overpromoted" employees than non-failed firms. This suggests that employees from failed firms may occupy positions above their actual capabilities, which could hinder their ability to secure comparable employment after failure.

Independent of whether these perceptions accurately reflect employee quality, affiliation with a failed firm can also affect the job search process more broadly, such as reducing dis-

placed employees' ability to leverage professional networks. Prospective employers may hesitate to hire or refer individuals from stigmatized firms through referral networks essential in the labor market (Fernandez and Fernandez-Mateo 2006; Granovetter 1973; Smith, Menon, and Thompson 2012). In sum, if employer failure indeed generates evaluative stigma beyond organizational leaders due to courtesy stigma and related uncertainty about an employee's competence, skills, or quality, we would expect the modal employee affiliated with a failed firm to systematically experience worse career outcomes than otherwise comparable employees from ongoing firms.

A lack of evaluative stigma and employer failure as a potential career opportunity. Although evaluative stigma may broadly affect employees through a courtesy stigma, prospective employers may alternatively make external attributions when assessing modal employees affiliated with failed firms. Unlike organizational leaders who shape strategic decisions and are clearly accountable for firm outcomes, as previously discussed, modal employees primarily execute tasks determined by higher-level managers. From an attribution theory perspective, prospective employers evaluating these lower-level employees may perceive their job loss as resulting from external circumstances beyond their control rather than reflecting negatively on their individual quality or competence, even tangentially through a courtesy stigma. Returning to our example of a bank's failure, employers evaluating bank tellers, human resources personnel, or IT staff might reasonably interpret affiliation with the failed bank as incidental and therefore uninformative regarding individual employee skills, quality, or potential. In such cases, the affiliation with a failed employer does not serve as a credible negative signal, shielding the modal employee from evaluative stigma and the associated adverse career outcomes.

It is also possible that prospective employers make no attribution, which would yield expectations similar to external attribution but is difficult to empirically detect without direct evidence of evaluator beliefs. Although distinguishing empirically between external attribution and non-attribution is challenging, two considerations make the absence of internal attribution more consistent with external attribution. In labor markets, prior employer affiliations and experience are salient, routinely used signals that predict subsequent career outcomes (Bidwell et al. 2014; Botelho and Chang 2022; Phillips 2001). Thus, when evaluators do not make inter-

nal attributions, namely stigmatized inferences about modal employees from failed firms, the more plausible interpretation is that they treat the affiliation as situational, in line with external attribution.

Supporting a lack of attribution for modal employees is research demonstrating that evaluative audiences systematically vary their attributions based on an individual's position within a given hierarchy. Audiences consistently hold higher-status individuals to higher standards of accountability due to their presumed influence over organizational outcomes (Graffin et al. 2013; Rhee and Haunschild 2006). Graffin et al. (2013) showed that higher-status members of the British Parliament faced harsher scrutiny for ethical violations than lower-status members who committed similar infractions. Extending this logic to employer failure, we may expect prospective employers to direct internal attributions—and thus evaluative stigma—primarily toward those near the top of the hierarchy (i.e., organizational leaders), while not making the same attributions to modal employees.

If the modal employee avoids significant evaluative stigma, employer failure may paradoxically create opportunities for career improvement, similar to peers who search out new career opportunities from ongoing firms. Employees typically exhibit substantial inertia, remaining with current employers due to person-organization fit (Bermiss and McDonald 2018; Chatman 1991) and organizational commitment (Meyer and Herscovitch 2001; Mitchell et al. 2001; Mobley 1977). Thus, employees rarely assess their external labor market value (Igielnik 2022; Jäger et al. 2021; Mincer 1986), instead developing strong attachments and deriving significant meaning from their current roles (Wrzesniewski and Dutton 2001). Even when employees occasionally explore external opportunities, actual turnover remains uncommon (Griffeth, Hom, and Gaertner 2000). Over time, employees thus become effectively "locked in," reluctant or unable to seek external opportunities that may better align with their true market value and capabilities (Bidwell 2011; Sørensen 2000). Employer failure disrupts this inertia, forcing employees to engage with the external labor market. Under conditions where prospective employers primarily attribute failure externally, displaced employees may uncover career opportunities comparable—or even superior—to their previous positions.

If modal employees from failed firms do not receive an evaluative stigma, they should expe-

rience career outcomes comparable to similar employees from ongoing firms. In this scenario, employer failure may even serve as a career "reset," allowing affected employees to move their careers closer to their true market value (Jäger et al. 2021).

Employer Failure Type, Industry, and Employee Heterogeneity

A primary theoretical contribution of our framework lies in highlighting that the likelihood of evaluative stigma relates to an employee's position in the organizational hierarchy, resulting in broadly negative outcomes for all employees or, for non-leaders, outcomes comparable to those of unaffected employees. We now extend our theorizing by considering additional sources of heterogeneity that may shape outcomes for the modal employee following employer failure. Specifically, we consider how characteristics of the employer failure itself (e.g., scandals versus routine failures), broader industry conditions (e.g., shrinking industries, competitor proximity), and individual-level factors (e.g., human capital, demographics) might further moderate employees' subsequent career trajectories.

Employer Failure Type: Routine Failure vs. Scandal

We have thus far assumed that firm failures primarily arise from general strategic mismanagement or poor performance—situations typically attributed to organizational leaders (Park, Chung, and Rajagopalan 2021; Park and Westphal 2013; Salancik and Meindl 1984). We have argued that prospective employers generally make internal attributions for organizational leaders but that it is less clear whether an evaluative stigma will occur for lower-level (modal) employees. However, if modal employees do not receive internal attribution, it should be the case that certain employer failures may include deeply discrediting events, such as scandals or fraud, which may create a blanket evaluative stigma for the average employee.

Unlike routine failures, scandal-related failures involve moral and ethical breaches, eliciting stronger responses from evaluators and attracting extensive media attention that heightens public awareness and scrutiny (Jensen 2006; Wiesenfeld, Wurthmann, and Hambrick 2008). Such

conditions make it difficult for prospective employers to differentiate accountability based on organizational hierarchy, potentially extending internal attributions—and an evaluative stigma—to modal employees. Real-world examples underscore this possibility. Modal employees from high-profile scandals, such as Enron and Theranos, have faced considerable hiring skepticism and difficulty securing subsequent employment, despite lacking direct involvement in misconduct.[3]

Broader organizational research reinforces this perspective, highlighting that scandals uniquely erode evaluators' distinctions between levels of accountability, thereby widening suspicion (Aven 2015; Jensen 2006). Additionally, research indicates that employees often internalize or conform to the ethical norms within their organizations, raising prospective employers' concerns that even lower-level employees from scandal-ridden firms might have indirectly internalized certain compromised norms or behaviors (Dimmock, Gerken, and Graham 2018; Pierce and Snyder 2008).

Therefore, if internal attribution is a main driver of career outcomes for organizational leaders versus modal employees, employer failures with an associated scandal should diminish or eliminate any protective buffer, triggering evaluative stigma even for the average employee. We should thus expect employees from scandal-related failures to experience worse career outcomes than those displaced by routine firm failures.

Post-Failure Outcomes and Demand for Skills

Beyond characteristics of the failure itself, broader industry and local labor-market conditions may moderate the relationship between employer failure and the modal employee's career outcomes. Our theoretical framework suggests that a lack of internal attribution may insulate modal employees from the evaluative stigma associated with employer failure, potentially enabling comparable career outcomes to unaffected peers. If so, post-failure outcomes for affected employees should be contingent on the availability of suitable employment opportunities.

Employees' career outcomes depend on the demand for their skills in the labor market. Consider a scenario where a firm's employees possess skills that are highly specialized—whether

3. https://www.nytimes.com/2002/03/03/us/big-burden-for-ex-workers-of-enron.html; https://www.cnn.com/2019/03/14/tech/theranos-employees/index.html

firm-specific (Becker 1964) or task-specific (Byun and Raffiee 2023; Gibbons and Waldman 2004)—and thus not widely valued by other employers. In situations where fewer firms value or seek these skills, displaced employees will encounter limited job opportunities. Extending this logic, employees from failed firms operating in shrinking (or stagnant) industries may face particularly challenging career outcomes. In such competitive labor markets, prospective employers are more discerning and attentive to potential negative signals, increasing the likelihood that affiliation with a failed firm is viewed unfavorably. Thus, industry contraction likely magnifies the likelihood of adverse career implications after employer failure.

Moreover, beyond the existence of other ongoing firms in the industry, the geographic location of these firms may significantly shape the career consequences of employer failure. Employees, much like entrepreneurs (Dahl and Sorenson 2012), generally prefer to find employment near their current location due to the personal and financial costs of relocating. Therefore, a higher local concentration of competitor firms creates conditions in which employees from a failed firm may have greater access to alternative employment opportunities. Indeed, industry wages tend to be higher in geographically concentrated industry clusters (Wheaton and Lewis 2002). Firms within similar industries often strategically co-locate to tap into shared pools of specialized human capital (Krugman 1991; Saxenian 1996; Sorenson and Audia 2000). Relatedly, geographic proximity should also allow employees to leverage their networks when looking for a new job. Thus, when a firm fails, nearby competitors might proactively recruit affected employees, viewing failure as an opportunity to acquire valuable talent without the legal risks of violating non-compete clauses (Marx, Strumsky, and Fleming 2009). As a result, modal employees displaced by employer failure in densely concentrated labor markets may actually experience improved career outcomes.

Individual Characteristics: Specialized Human Capital and Demographics

Beyond one's position in the organizational hierarchy, other employee characteristics likely moderate the relationship between employer failure and subsequent career outcomes for the modal employee. Organizational researchers' primary focus on firm leaders limits our understanding of individual-level heterogeneity. That said, even among senior partners at a failed law

firm, educational status was found to moderate the adverse career outcomes of failure (Rider and Negro 2015). Building upon this logic, we consider two sets of employee characteristics—specialized human capital and demographic factors—that have been shown to shape employers' evaluations of job candidates. These characteristics may influence whether prospective employers make assessments consistent with internal attribution after employer failure, thereby affecting the subsequent career outcomes of the modal employee.

Specialized human capital refers to skills, knowledge, or expertise uniquely tailored to specific roles or industries, often highly valued but in short supply (Becker 1964). Skills have been linked to firm performance and competitiveness, especially in innovation-driven industries (Jara-Figueroa et al. 2018; Shrader and Siegel 2007). Reflecting the strategic value of specialized human capital, firms actively engage in talent recruitment from successful rivals (Combes and Duranton 2006; Isaac 2015). Proximity to talent pools even incentivizes co-location despite competition risks (Rotemberg and Saloner 1990). Thus, employees possessing specialized human capital may experience relatively favorable career outcomes after firm failure compared to those without these skills.

Demographic characteristics, including gender, race, and immigration status, are similarly important due to their established relationship with evaluative biases in hiring. Status characteristics theory highlights that evaluators consistently raise greater competency doubts about minority group members relative to majority counterparts adversely affecting evaluative outcomes (Abraham, Botelho, and Lamont-Dobbin 2024; Berger 1977; Correll, Benard, and Paik 2007; Correll and Ridgeway 2003; Moss-Racusin et al. 2012). Gender differences are found even when evaluators have access to objective performance information (Foschi 1996) and racial differences occur in evaluations due to small changes to the scale evaluators use (Botelho et al. 2025).

If prospective employers make internal attributions for organizational leaders but not modal employees, this differentiation may be less likely for individuals who frequently face bias in the hiring process, disproportionately disadvantaging individuals from underrepresented backgrounds. Thus, even if the modal employee is typically shielded from internal attribution, common stereotypes about competence tied to demographic characteristics may lead to minority

employees from failed firms to face evaluative stigma and worse subsequent career outcomes than similar employees from ongoing firms.

EMPIRICAL APPROACH

We aim to understand the relationship between employer failure and subsequent employee career outcomes. To do this, we need detailed, longitudinal data, across various firms, comparing career outcomes of employees from failed firms against those from ongoing firms—firms that have not failed. The ideal approach entails randomly triggering firm failures and tracking employees' career outcomes compared to unaffected peers. However, such a field experiment is neither practical nor ethical. Instead, we approximate this ideal using complementary longitudinal datasets that match employers with their employees.

When comparing career outcomes, we considered two possible counterfactual groups: (i) employees who exited ongoing firms and (ii) employees who remained with ongoing firms. Because our theoretical interest centers on how prospective employers evaluate employees from failed firms, our primary analyses compare employees from failed firms to observationally similar employees from ongoing firms (i). We discuss the robustness of our results using the second counterfactual group (ii) later in our analyses.

Our aim is to test a more comprehensive and generalizable theory than prior organizational research, which has typically used case studies focusing exclusively on organizational leaders. Thus, we collected detailed employee-level data from the US Census, offering generalizable insight into the US workforce and providing an objective measure of a key outcome of interest in one's career trajectory: wages. A limitation of the Census data is a lack of detail about employees (e.g., positions, titles) and firms (e.g., firm name). Therefore, we supplement the Census data with detailed employee-level data from the Automatic Speech Recognition (ASR) industry. Although the ASR data are less complete and lack generalizability, they are non-anonymized and therefore offer important additional detail to test our theoretical framework.

US CENSUS

The Census maintains two comprehensive databases on employers and employees in the US: the Longitudinal Business Database (LBD) and the Longitudinal Employer Household Dynamics (LEHD) database. The LBD contains founding and dissolution dates for all US-based employers from 1976-2014, while the LEHD includes quarterly wages from IRS tax records for employees at those employers. Employer-level data are aggregated by state-level identifiers, tracked across states, with unique identifiers for individual employees.

The LBD and LEHD represent a unique resource for studying the US labor force given the completeness of their coverage, especially compared to databases that rely on self-reported or imputed data (e.g., LinkedIn, surveys). However, researchers must apply for Special Sworn Status to access these data within secure Census facilities and adhere to strict confidentiality requirements. Further, all analyses must be reviewed and checked by Census officers via a standardized submission process, which frequently includes feedback and revision requests. For example, researchers are restricted from disclosing certain subsample analyses that may identify a subset of observations deemed to be too small. Thus, all disclosed information for a project is compared to previously disclosed information to check for potential subsample issues. Moreover, not all data collected by Census are available for all research projects. For this project, we examined LEHD data for 25 states and the District of Columbia (DC).[4]

Because our analysis examines employees' career outcomes, and individuals may move across states from which we do not have data (e.g., from WA to MA), or individuals may work during a time where we have data on one state (e.g., CO in 1999) but not another (e.g., AR in 1999), we prioritize collecting full career histories. Thus, consistent with other research using Census data, we opted for employee-level completeness by including only individuals we could follow for their entire career history. That is, we removed from our sample any individual who held a job outside the state-years for which we have LEHD data.[5] Moreover, because our

4. LEHD states in this project that were made available to us were CO, IL, IN, LA, MD, MO, and WA since 1990, CA, PA, and OR since 1991, GA since 1994, NM, RI, and TX since 1995, HI and ME since 1996, DE, IA, NV, SC, and TN since 1998; UT since 1999, OK and VT since 2000, DC since 2002, and AR since 2003.

5. This is possible to detect because the LEHD contains a file that notes whether every worker is employed *somewhere* in the US during each year; therefore, if the worker is employed somewhere in the US, but not in any of the states we have access to, we conclude that the worker is not fully observable in our set of state-years and is therefore dropped from our analysis.

outcome variable (discussed below) is the difference in wages between a worker's current and former employer, we require that the worker spend at least three quarters at each employer to have a reasonable basis upon which to compare wages. Finally, we exclude North American Industry Classification System (NAICS) two-digit codes 91 and 61 (governmental and religious organizations, respectively) in order to focus strictly on commercial firms.

US Census Measures

Dependent variable. Wages represent an equalizing metric for comparing career outcomes across firms and industries since individuals are interested in keeping or increasing their wages over time. Therefore, we examine changes in employee wages to assess the relationship between employer failure and subsequent career outcomes. Our dependent variable, *Wage Differential*, measures the wage difference between an employee's job at time t_0 and their (previous) job at time t_{-1}. Wages are reported quarterly at the employee-employer level; thus, we calculate annualized salary as four times the reported wages in the penultimate quarter at their previous job (t_{-1}), and four times the reported wages in the second quarter of their subsequent or current job (t_0). This method ensures that a complete quarter's wages are consistently captured. For example, regardless of whether an employee exits in the first or last week of a quarter, the wage from that full quarter is reported. To maintain consistent comparisons, all wages are inflation-adjusted to constant 1982 dollars.

A positive (negative) *Wage Differential* indicates that the individual earned higher (lower) wages at their current job relative to their previous job. To mitigate the influence of extreme outliers, we winsorize the wage values at the 99th percentile.[6] Finally, we measure wage differences as the difference in logged wages between the two jobs. For example, an employee whose earnings increases from $39,000 (10.57 logged) to $40,000 (10.60 logged) would have a wage differential of approximately +$1,000 (or +0.03 in log terms).

Independent variables. Our primary explanatory variable indicates whether an employee was employed by a firm at the time of its failure. We rely on the LEHD dataset to determine firm failure timing. Specifically, the quarterly granularity of the LEHD allows us to pinpoint the

6. Census disclosure-avoidance policies do not permit us to reveal the 99th percentile wage.

exact quarter of failure by identifying the final quarter during which an employer paid wages to employees and whether a given employee was at the firm in the quarter that it failed.[7] *Failure*, takes the value of 1 if an employee was affiliated with the firm during its final quarter and 0 if the employee worked at an ongoing firm during the same period.

The Census data do not contain information regarding an employee's position, function, title, or seniority level. Therefore, we used the wage distribution within each firm to approximate where an employee likely sits on the organizational hierarchy. To approximate the likelihood that an employee is an organizational leader, we used their relative wages within each firm. *High Earner* takes the value of 1 for employees whose earnings place them in the top 5% of their employer's wage distribution, and 0 otherwise. We selected this threshold to maximize the inclusion of organizational leaders, preferring to potentially incorporate some non-leaders rather than risk excluding leaders. It is important to note certain ambiguities inherent in this measure. For example, some CEOs have famously opted for low wages and instead receive lucrative stock options (not captured by these Census data).[8] Additionally, some highly compensated employees might be individual contributors rather than organizational leaders. Despite these limitations, we expect that the top 5% of wage earners will include the vast majority of organizational leaders. We will also discuss the robustness of our results to other cutoffs.

Control variables. We include several employee- and firm-level control variables drawn from the LEHD to account for characteristics that may influence wage outcomes. At the employee level, we control for available demographic information. *Female* takes the value of 1 if the Census indicates the sex of the employee as female and 0 if male. We calculate an employee's *Age* using an employee's birth year. *Immigrant*, takes the value of 1 if the employee was born outside of the US, and 0 if they were born in the US. To account for race and ethnicity, we construct the variable *Minority*, which takes the value of 1 for all employees who report a race/ethnicity other than non-Hispanic white.[9]

7. Although the LBD has such an indicator, Chow et al. 2021 note that this indicator is not always reliable and suggest using the LEHD in order to more accurately determine the death of a firm.

8. https://www.wsj.com/articles/tesla-ceo-elon-musks-37-584-salary-reflects-california-minimum-wage-1460759161

9. The Census also provides coarse education categories for the highest level of education achieved: college graduate, some college, high school graduate, or some high school; however, this value is primarily imputed from the American Community Survey, which is based on a random sample. *College* takes the value of 1 if the employee has graduated from college and 0 if they have not. Because this value is heavily imputed, we do not use education

At the firm level, because a firm's size may influence wage outcomes, we control for *Firm Size*, measured as the logged number of employees at the firm during the focal year. To account for industry and geographic variation, we include fixed effects for the firm's six-digit NAICS industry and the state where the employee works. Additionally, state-level unemployment could influence wages. We control for *Unemployment*, defined as the state-level unemployment rate during the year and in the state where the worker *seeks* employment, which may differ from the state where the worker was previously employed, and thus does not necessarily drop out of the model given state fixed effects.

Lastly, we control for the elapsed time between leaving the previous employer and joining the subsequent employer. *Move Lag (ln)* is the natural log of the count of quarters between the two jobs. This variable captures any differences in job-search dynamics related to prior employer status.

US Census Matching

As noted above, our lack of an experimental design limits causal inference and makes it challenging to fully account for all differences between employees at failed firms and those at ongoing firms. To strengthen our empirical comparisons, we use a matching estimation. Our goal is to ensure a more balanced comparison between employees from failed firms and similar employees from ongoing firms. Although we do not claim that matching provides a causal estimation, it allows us to provide more generalizable and robust evidence of the relationship between employer failure and employee careers within our context. Again, our data consist of employee job transitions, primarily from ongoing firms, with a smaller number from firms that failed. Each transition includes both employee- and firm-level characteristics.

We use Coarsened Exact Matching (CEM), a non-parametric technique designed to achieve sample balance by grouping observations based on a specific "treatment" (Blackwell et al. 2009; Iacus, King, and Porro 2012). The extensive size of our Census data allows for one-to-one matching, pairing each employee from a failed firm with a similar employee from an ongoing firm. Although one-to-one matching risks creating small analytical samples, in our large dataset

in our primary models, but are results are robust to its inclusion.

we avoid this tradeoff between reducing bias and maintaining statistical power. After matching, our analytical sample consists of approximately 2,200,000 observations.[10]

At the employee level, we match exactly on gender and coarsely on age, using five distributional bins. At the employer level, we match exactly on location (state), industry (four-digit NAICS), and the firm's founding year. We also match on firm size (i.e., number of employees) using five distributional bins.[11]

US Census Estimation

We use ordinary least squares (OLS) regression to estimate the relationship between *Wage Differential* and *Failure*. Our unit of analysis is a job transition event, specifically, an employee moving from one employer to another employer. For employees from failed firms, this event represents a transition from their failed firm to a subsequent employer; for all other employees, it represents a transition from one ongoing firm to another. We specify our main regression model as follows:

$$W_{i,t} = \alpha + \beta F_{i,t} + \gamma U_{i,t} + \psi B_t + \delta_{i,t} + \rho_{i,t} + \tau_t + \epsilon_{i,t}, \tag{1}$$

where *W* is the wage difference for employee *i* between their employer at time t_0 and their prior employer at time t_{-1}. *F* is an indicator variable taking the value 1 if employee *i* worked at a firm that experienced *Failure* at time *t*, and 0 otherwise (i.e., at an ongoing firm). We are also interested in the interaction between *F* and whether the employee is an organizational leader (*High Earner*; *H*), *U*, and *B* are vectors of time-variant and invariant control variables at the employee (*U*) and firm (*B*) level (see subsection Control Variables). δ includes industry fixed effects, ρ includes state fixed effects, τ includes year fixed effects, and ϵ is the error term. We cluster standard errors at the firm level.

10. As required by Census disclosure-avoidance policy, all statistics, including this count, are rounded to four significant digits.

11. Note that we do not match on every available employee and firm characteristic, such as *Immigrant* and *Minority*, and because education is mostly imputed it is not a desirable matching criterion. This is for theoretical and practical reasons: In theory, it is not advisable to match on too many variables as it may result in an unrepresentative sample (Blackwell et al. 2009; Iacus, King, and Porro 2012). In practice, the enormous size of the LEHD data for 26 states exhausted available memory capacity on Census computers when adding additional variables.

AUTOMATIC SPEECH RECOGNITION INDUSTRY

While the Census data provide generalizable estimates, this comes at the expense of detail regarding specific firms and employees. To address this limitation, we assembled a complementary employer-employee dataset focused on a single innovation-driven industry: Automatic Speech Recognition (ASR). ASR is a form of artificial intelligence that translates spoken language into text by analyzing large datasets of recorded human speech. Although the technology only recently reached human-level accuracy (Bishop 2017), ASR firms have claimed near-perfect performance since the early 1980s (Creitz 1982). In the intervening years, unable to attain such vaunted performance expectations, many ASR firms failed.

We compiled detailed data on ASR firms' entry and exit events between 1952 and 2013 by manually reviewing more than 15,000 pages from two industry newsletters, *ASRNews* and *Speech Recognition Update*. Employee-level data were obtained from several sources, including trade journals, proceedings from several annual ASR conferences, patent records, CapitalIQ, and internet sources, such as Bloomberg BusinessWeek, the ASR historical repository 'elsnet,' and ZoomInfo. Our sample focuses on employees who made at least one career move within the ASR industry between 1980 and 2010. The resulting dataset contains 6,296 observations of employees from 374 unique ASR firms, of which 88 failed during our study period.

Our analytic approach to the ASR data is similar in many respects to our description of the Census data. Below, we highlight only the aspects in which the ASR data differ from the previously described Census data.

ASR Measures

Dependent variable. Comprehensive wage data is difficult to collect outside of Census or comparable registry sources. Therefore, for the ASR employee sample, we focus on a different, yet important outcome variable: whether an employee remains employed in the ASR industry. Changing industries can have formidable implications for employees. Individuals develop substantial and valuable industry-specific human and social capital over their careers (Byun and Raffiee 2023; Gibbons and Waldman 2004; Phillips 2002), especially in innovation-driven in-

dustries (Jara-Figueroa et al. 2018; Lazear 2004). Thus, the inability to secure a subsequent job in the ASR industry may diminish these accumulated investments. *Staying in Industry* takes the value of 1 if an employee's subsequent job is within the ASR industry and 0 otherwise.[12]

Independent variables. The primary independent variable, *Failure*, mirrors our Census measure with one difference: Due to data constraints, we can only determine whether an employee worked at the firm during the year of its failure, rather than the specific quarter.

A notable strength of the ASR data relative to the Census is the availability of detailed position titles, allowing us to directly identify organizational leaders. We classified employees as organizational leaders based on their job titles at the time of firm failure. This includes employees holding a C-level position (e.g., CEO, CTO, founder) or another executive position (e.g., executive vice president). *Executive* takes the value of 1 for employees in these organizational leadership positions and 0 otherwise.

Control variables. To account for specialized human capital, we use job titles: *Engineer*, takes the value of 1 if the employee held an engineering or science-related role (e.g., software engineer, computational linguist) and 0 otherwise. We approximate employee *Age* as 21 years plus the number of years elapsed since the individual's first appearance in our dataset, when an individual likely joined the workforce upon graduating from college. We infer gender from given names, with the variable *Female* equal to 1 if the employee's name is more commonly perceived as a woman's name than a man's name, using a name-gender classifier algorithm. Given that firms in innovation-driven industries sometimes co-locate for access to a labor supply (Saxenian 1990), *High ASR Density* takes the value of 1 if the geographic area has an above-median concentration of ASR firms and 0 if not.

12. In Appendix A (US Census: Staying in Industry), we use a similar outcome variable using the Census data that captures whether an employee stays in the same six-digit NAICS industry when they switch employers. Appendix Table AA1 shows that our main ASR results are consistent in the Census, providing a connection between the two datasets. Furthermore, Appendix Table AA2 demonstrates that staying in industry is associated with higher wage growth in the short-term (Models 1, 3, 4) and in the long-term (Model 2) providing a link between the wage differential outcome in the Census data and the staying in the industry outcome in the ASR data.

ASR Matching and Estimation

We use a matching strategy similar to the one described for the Census analyses. Due to the smaller ASR dataset, however, we use one-to-many matching, assigning weights to balance variance. Within each stratum, observations receive weights based on the relative proportion of employees from failed ASR firms to employees from ongoing firms. At the employee level, we matched exactly on *Engineer* and *Female*, and we matched coarsely on *Age*, using four distributional bins. At the firm level, we matched exactly on location (state in the US; country for international observations) and bucketed firm founding year by decade. Finally, we matched exactly on the observation year. The CEM procedure yielded a matched sample of 2,466 observations at 275 unique ASR firms, 87 of which failed.

Our estimation equation follows the specification detailed previously in the Census analysis.

RESULTS

We first discuss the main empirical relationships of interest, starting with the Census data and then the ASR data. Subsequently, we explore additional analyses that unpack and clarify our primary findings.

US Census Main Results

Table 1 provides descriptive statistics for the variables included in our Census analysis. Table 2 provides correlations.[13] The average employee in our matched sample is approximately 45 years old, 28% are female, 14% are immigrants, and 13% identify as a racial/ethnic minority.

After an employee exits their employer (whether a failed or ongoing firm), their subsequent wages are similar to their wages at their former employer (*Wage Differential*). These unconditional means should be interpreted cautiously as many factors are related to an individual's wages, such as their firm's industry, their experience, and state-level variation.

[Table 1]

[Table 2]

13. Census disclosure-avoidance policies prevent revealing minimum or maximum values.

Table 3 Model 1 provides the results for the initial relationship between *Wage Differential* and *Failure*, and Figure 2a provides the coefficient plot (*Failure* line). All analyses are conditional on a mobility event (i.e., an employee exiting a failed firm or an ongoing firm and securing a new job). Consistent with the idea that employees may not earn a market wage at their current employer (e.g., Jäger et al. 2021; Mincer 1986), employees who exit ongoing firms earn a positive wage differential of about 11 percentage points. Thus, job mobility is associated with positive wage growth. The positive coefficient on *Failure* suggests that, on average, employees from failed firms experience about 1.4 percentage points higher wage growth than employees who leave ongoing firms. Although statistically significant, this 1.4 percentage point difference is modest in practical terms. Using the wages in our sample, which are inflation-adjusted 1982 dollars, it represents a difference of about $250. Because the modal employee in our sample is not an organizational leader, these initial results do not support the existence of an evaluative stigma for the average employee affiliated with a failed employer.

It may be the case that employer failure does not affect subsequent career outcomes for *any* employee, irrespective of hierarchical position. Model 2 tests this directly by interacting *High Earners* with *Failure*; Figure 2b provides a coefficient plot from this regression. For employees from ongoing firms (constant), there is a positive relationship between job mobility and wage growth. The positive coefficient on *High Earners* indicates that organizational leaders from ongoing firms experience even more wage growth when they move to a new employer relative to the modal employee from ongoing firms.

Focusing on the relationship of interest, the main effect of *Failure* suggests that the average employee from a failed firm experiences slightly larger wage growth than similar employees from ongoing firms (1.7 percentage points or about $290). However, organizational leaders from failed firms do not experience the same level of wage growth after working at a failed firm. Instead, the interaction between *Failure* and *High Earner* suggests that organizational leaders from failed firms experience approximately one-third lower wage growth (0.0352/0.1083 = 0.33) compared to organizational leaders exiting ongoing firms. While wage growth for organizational leaders *is* attenuated after firm failure, we do not find evidence that this penalty "trickles down" to the modal employee. Instead, we find evidence that employees from failed

firms are just as well off as similar employees who leave ongoing firms.

These findings are consistent with our theoretical expectations regarding attribution following employer failure. Prospective employers appear to make internal attributions—and thus an evaluative stigma applies—to employees at the top of the organizational hierarchy, whereas the modal employee does not seem to experience similar evaluative stigma. This evidence does not support the notion that average employees face a courtesy stigma merely through their affiliation with a failed employer (cf. Wiesenfeld, Wurthmann, and Hambrick 2008). Furthermore, while prior organizational research discusses the signaling advantages derived from working at a reputable firm (Bidwell et al. 2014), we do not find support for the inverse logic: That affiliation with a failed firm signals that the modal employee is evaluated as lower quality to subsequent employers. In addition to disentangling between competing theoretical predictions, our results replicate and extend prior organizational research on employer failure by using a representative, matched sample of over two million US employees.

Overall, these results suggest that prospective employers are not using firm failure to make negative internal attributions—such as expectations of lower quality, potential, skills or competence—about modal employees affiliated with failed firms. However, the same is not the case for organizational leaders.

[Table 3]

[Figure 2]

Addressing alternative explanations: Failed firm differences and short-lived wage changes. Before unpacking this main result and focusing on the ASR industry, two alternatives are worth considering. First, despite our matching approach, it may be the case that failed firms might still systematically differ from ongoing firms, potentially driving observed wage differences. Second, it may be the case that the small wage growth observed for the average employee following employer failure might be short-lived.

If firms that fail differ sharply from those that do not, the positive wage differential may be a product of these differences, rather than a lack of internal attribution for modal employees. We used the detailed timing available in the Census data to test this possibility. Our main employer failure measure, *Failure*, takes the value of 1 if an employee worked at the firm in the quarter

that the firm failed. We constructed a placebo measure *Failure (placebo)* that takes the value of 1 if an employee leaves the firm during the four quarters (one year) before the firm failed.[14] If the results in Table 3 Models 1 and 2 are driven by underlying firm differences between failed and ongoing firms, we would expect *Failure (placebo)* to produce similar effects. However, in Table 3 Model 3 (coefficient plot in Figure 2a, *Failure (placebo)* line), the estimated coefficient on *Failure (placebo)* is no longer substantively or statistically significant. This analysis provides reassurance that our main findings are unlikely driven by unobserved firm-level differences and instead likely reflect employee affiliation with firm failure itself.

Next, it is possible that the observed wage increase for modal employees is temporary. Although even a short-term bump aligns with our attribution logic, it is informative to check whether this positive wage differential persists. To test this relationship, we collected a longer-term wage outcome: *Wage Differential (5-Year)*, comparing employees' logged wage five years after leaving their employer (failed or ongoing) to their logged wages at their former employer. Table 3 Model 4 (Figure 2a provides the coefficient plot, *Failure - 5 year* line) shows that the positive wage differential after firm failure (Model 1) persists.

Census Robustness

We conducted several robustness checks related to our main findings. First, we address the possibility that our results may reflect wage reductions implemented by firms prior to failure (i.e., a potential cost saving technique). We explore this issue in two ways. Initially, we examine whether average firm-level wages systematically decline in the year of failure by comparing them to the preceding two years (all in inflation-adjusted 1982 dollars). Average wages at failed firms in the year before failure were approximately 1.8% lower than in the year of failure, while wages two years before failure were roughly 2.2% lower than in the year of failure. Therefore, wages increased year-over-year, and it does not appear that firms on the verge of failure implemented pre-failure wage reductions.[15] Although firm-level averages might obscure

14. To be exact, if the firm failed in Q2 of 2005, this placebo defines *Failed* as employees who left the firm in Q1 2004, Q4 2004, Q3 2004, or Q2 2004.

15. Wages for the failure year are adjusted based on available quarterly data. Specifically, if a firm failed in the second quarter, we doubled the observed wage data to approximate a full year's wages. Note, this may still underestimate wages if failure occurred early in a quarter. Average firm wages are calculated from *all* employees whose careers remain fully within the 26 states covered by our data, not only the CEM-matched subset.

individual-level wage changes, this analysis suggests that systematic wage reductions prior to failure are unlikely.

To further address potential individual-level wage reductions, we re-estimate our main specification (Model 1 in Table 4), using wage data from the quarter occurring one full year before firm failure (instead of the quarter immediately before firm failure). This measure incorporates any wage decreases experienced by individuals in the year of failure. The results closely replicate our initial estimates (Table 3, Model 1), confirming robustness to timing adjustments.

A second potential concern involves overestimation due to sample selection bias, given our analysis includes only employees who secure subsequent employment after leaving their prior firm. If a substantial proportion of displaced employees do not find new jobs (and thus have post-failure wages of zero), our results might overstate the true wage effects of employer failure. To examine this possibility, we need to examine *all* employees of those failed firms and not just those from our CEM matched sample. Doing so reveals that 97.6% of employees of failed firms subsequently find employment at another firm (as captured by the LEHD). Of those, 90% find another job by the following quarter after their former firm failed, and 95% find that next job within two quarters following failure. (As a reminder, our analyses control for the lag between employment spells.)

For the remaining 2.4% of employees not observed in the LEHD after employer failure, we link those individuals to the Census Integrated Longitudinal Business Database (ILBD), which captures self-employment income not included in the LEHD. Approximately one-fifth of these individuals (less than 0.5% of the total displaced employees) appear in the ILBD within one year, suggesting that fewer than 2% of employees become truly unemployed (or retire) after employer failure. This indicates that our primary estimates are unlikely to be substantially biased by attrition.

Next, given that our definition of organizational leaders (*High Earners*) is based on being in the top 5% wage cutoff, we test the sensitivity of this cutoff. Models 2 and 3 in Table 4 replicate our main analyses using alternative thresholds: top 10% (90th percentile, Model 2) and top 1% (99th percentile, Model 3). The consistency of results across these models indicates that our findings regarding internal attributions for organizational leaders are not driven by the specific

cutoff choice.

Above, we acknowledged the limitations of the *College* variable given that it is largely imputed. It is omitted from all other models, but in Model 4 we include it, with consistent results. Finally, given the correlation between *Minority* and *Immigrant* (0.356), we re-estimate our main results without the *Immigrant* and *Minority* variables in Models 5 and 6, respectively. The estimated coefficient on *Failure* is similar across these results.

[Table 4]

ASR Main Results

Our main outcome of interest in the ASR sample is whether the employee remained employed within the ASR industry (*Staying in Industry*). The ASR data allow us to clearly identify direct competitors developing the same types of products, in contrast to the Census data, where similarity among firms is limited to coarse NAICS industry classifications, thus offering a more precise comparison.

Table 5 presents unweighted descriptive statistics, and Table 6 provides correlations for our main variables. In our matched sample, about 22% of employees moved from ASR firms that failed during the observation period, and among employees who left their firms, roughly 30% subsequently moved to another ASR company. Executives constitute approximately 11% of the sample, and engineers 28%. Given that our data collection relied on industry newsletters, patent records, conference proceedings, and other publicly available sources, executives and engineers were more readily identified compared to other employees, such as back-office staff. Women comprise approximately 10% of our sample, reflecting historical underrepresentation of women in innovation-driven industries, particularly in leadership and technical roles.

[Table 5]

[Table 6]

To test the relationship between employer failure and the likelihood of staying in the ASR industry (*Staying in Industry*), we use our CEM-matched sample in Table 7. All analyses condition on employees exiting either a failed or an ongoing firm. Model 1 includes an interaction

between *Failure* and *Executive* (with the coefficient plot in Figure 3a), testing differences between organizational leaders and the modal employee.

The coefficient on *Failure* is positive and marginally significant (p = 0.09), suggesting that the modal employee from a failed ASR firm is at least as likely, and maybe slightly more likely, to remain within the ASR industry compared to similar employees from ongoing firms. This result implies that, for the modal employee, association with a failed firm does not appear to produce an evaluative stigma in terms of industry retention. Indeed, if prospective employers were making negative internal attributions about non-executive employees affiliated with failed firms, we would expect these employees to be less likely to remain within the ASR industry.

In contrast, the coefficient on *Executive* is positive, indicating that executives from ongoing firms are significantly more likely than non-executives to remain in the ASR industry following job transitions from ongoing firms. This finding aligns with the general perspective that employees benefit from substantial industry-specific human capital (Becker 1964). However, the negative and significant interaction between *Failure* and *Executive* reveals that executives from failed firms are less likely to remain in the industry relative to executives from ongoing firms.

These findings parallel those from our Census analyses. Prospective employers appear to make negative internal attributions producing an evaluative stigma for executives. However, the modal employee does not seem to receive a similar internal attribution, suggesting that prospective employers evaluate non-executives from failed firms similarly to their counterparts from ongoing firms.

[Table 7]

[Figure 3]

Unpacking the Lack of Evaluative Stigma After Firm Failure for Modal Employees

Across both samples, we find consistent evidence that affiliation with a failed firm is negatively related to career outcomes but that this is limited to organizational leaders. Although our Census

analysis approximated seniority through relative wages (top 5%, as well as sensitivity at the 1% and 10% level), the ASR dataset directly identified executives, helping confirm that leaders bear the brunt of evaluative stigma. Leveraging the unique strengths of each dataset, we now investigate distinct sources of heterogeneity to inform further plausible mechanisms that may support this differential evaluative stigma: individual demographic characteristics, competitive conditions, and the type of employer failure.

Demographics. If a lack of internal attribution is a plausible mechanism explaining why the modal employee avoids an evaluative stigma in the subsequent labor market, then the likelihood that this occurs may be connected to other employee characteristics that are frequently linked to career outcomes: race/ethnicity, immigration status, and gender (Abraham, Botelho, and Lamont-Dobbin 2024; Berger 1977; Correll and Ridgeway 2003; Friedberg 2000; Moss-Racusin et al. 2012).

A benefit of the Census data is that gender, race/ethnicity, and immigration data are collected. In our main results (Table 3), we found that individuals from underrepresented groups experienced larger wage growth than individuals from majority groups. However, it is important to note that this does not mean that individuals from underrepresented groups earn higher absolute wages. Table 8 interacts *Failure* with *Minority* (Model 1), *Immigrant* (Model 2), and *Female* (Model 3), and Figures 4a, 4b, and 4c provide the coefficient plots respectively.

We find that racial/ethnic minorities and immigrants from failed firms subsequently experience lower wage growth than white or US-born employees, respectively. For example, minority employees from failed firms experience less wage growth (about 1 percentage point) compared to minorities from ongoing firms whereas white employees from failed firms experience more wage growth (about 2 percentage points) than white employees from ongoing firms. Conversely, immigrant employees from failed firms experience less wage growth (2 percentage points) than immigrants from ongoing firms. Related to status characteristics theory (Berger 1977; Botelho et al. 2025; Correll and Ridgeway 2003), which shows that gendered and racial stereotypes are more likely to emerge when uncertainty increases, this finding suggests underrepresented employees may be more susceptible to an evaluative stigma after employer failure than white employees. By contrast, we find no significant interaction between *Failure* and gen-

der in the Census; this may be because we match on gender, whereas we do not match on race/ethnicity or immigration or because gender differences are often industry and role specific (Eagly and Karau 2002).

In the ASR sample, we cannot measure immigration status or race/ethnicity but inferred gender from an employee's given name using GenderChecker.com, which we matched on for our analyses. From Table 7 Model 2 (with Figure 3b providing the coefficient plot), we find evidence that women from failed firms are more likely to stay in the ASR industry than those from ongoing firms. Thus, across both samples, gender does not seem related to a higher likelihood of internal attributions for the average employee.

Human capital. We theorized that human capital, and in particular specialized human capital, is related to career outcomes. If prospective employers are unlikely to make internal attributions for the modal employee from failed firms, it should be the case that employees with higher levels of human capital are especially likely to avoid internal attributions.

[Table 8]

[Figure 4]

A limitation of the Census data is the absence of a direct measure of specialized human capital. Although education is available, it is not only overly broad for assessing specialized skills but is also largely imputed, as discussed. The ASR dataset, however, offers a more precise measure, allowing us to identify employees with specialized human capital in an innovation-driven industry that is heavily dependent on engineering talent. Using detailed job titles, we coded employees who held engineering or related scientific roles. Table 7 Model 3 (Figure 3c provides the coefficient plot) interacts *Failure* and *Engineer* to test whether specialized human capital moderates the likelihood of staying in the ASR industry following firm failure. Although we previously found that modal employees from failed firms are generally unaffected by employer failure, here we find that this is especially true for those with specialized human capital: Engineers from failed firms are significantly *more likely* to remain in the ASR industry than their counterparts from ongoing firms.

This result highlights another potential mechanism, related to a lack of internal attribution, explaining why firm failure does not create an evaluative stigma for certain employees. Specifi-

cally, when employees leave an ongoing firm voluntarily, their availability to competitors is not widely known. By contrast, when a firm fails, competitors become immediately aware that valuable human capital is available. Consequently, engineers from failed firms—possessing highly sought-after skills—become particularly attractive targets for hiring. This targeted recruitment by competitors further reinforces the lack of internal attribution that prospective employers likely apply to modal employees from failed firms.

Local employment conditions and opportunities. The local availability of skilled labor is one reason why firms accept the heightened competitive risks associated with co-locating near rivals (Krugman 1991; Saxenian 1996). Therefore, if prospective employers do not make internal attributions to the modal employee following a competitor's failure, we should expect proximity to competitors to increase displaced employees' likelihood of more favorable outcomes, namely remaining in the industry where they have built specific human capital as well as social capital. We examine this possibility in both datasets, leveraging their complementary strengths: The Census provides broad industry coverage with limited geographic granularity (state-level), while the ASR dataset offers detailed geographic resolution but within a single industry.

In Model 4 of Table 8, we examine whether industry-specific labor-market conditions moderate our Census findings. Specifically, we identified industries (defined at the one-digit NAICS level) whose share of overall state-level employment had declined at least 10% or more during the prior five years (*Shrink*).[16] The interaction of *Shrink* and *Failure* is negative (Figure 4d provides the coefficient plot), suggesting that the increase in post-failure wages may be ameliorated when jobs in that industry are less plentiful. This analysis suggests that differential evaluative stigma may be more likely when labor market dynamics favor job candidates, and that when labor market dynamics tighten—favoring employers—employees from failed firms may be more likely to receive an evaluative stigma.

The detail in the ASR data allows us to identify the exact geographic locations of employees and firms using latitude and longitude coordinates, which are unavailable in the Census. Table 7, Model 4 examines how proximity to competitor firms (*High ASR Density*) relates to stay-

16. For observations from year-states where we do not have data five years prior, this is replaced with the prior four, three, two, or one year. For the first year a state is observed, this variable is set to 0.

ing within the ASR industry (with Figure 3d providing the coefficient plot). Consistent with agglomeration research, the marginally significant positive effect of *High ASR Density* indicates that employees are generally more likely to remain in the ASR industry when employed within a geographic cluster. More notably, the significant interaction between *Failure* and *High ASR Density* demonstrates that employees affiliated with failed firms located in such clusters are especially likely to remain in the industry. This result reinforces the general finding that prospective employers do not interpret affiliation with a failed firm as a negative signal regarding the quality of modal employees. Moreover, this evidence is also consistent with ongoing firms perceiving a competitor's failure as an advantageous opportunity to hire available talent.

Forced into the labor market. Although the wage growth difference between the modal employee from a failed firm versus from an ongoing firm is modest (1.7 percentage points), it is worth considering why employees from failed firms may experience a slight career benefit. It is important to note that this benefit is also heterogeneous with regard to employee characteristics, with racial/ethnic minorities and immigrants experiencing less wage growth after employer failure, as well as local labor market conditions, where employees from shrinking industries realize less wage growth. Moreover, in the ASR data, our evidence suggests that specialized human capital and co-location may also result in a greater benefit.

A more general explanation may be that because employees usually experience a wage increase when they find a new job (Jäger et al. 2021; Mincer 1986), firm failure *forces* individuals into the labor market. Thus, because prospective employers are unlikely to apply an evaluative stigma to the average employee from a failed firm, firm failure forces this mobility event without concerns related to whether a job applicant is credibly looking for a job. In other words, employees from failed firms *need* another job whereas employees from ongoing firms may be testing the market or trying to gain negotiation leverage over their current employer. Our theorizing and empirical approach thus far is similar to related organizational research: We are primarily interested in the demand-side evaluation of employees who came from failed firms. This demand-side focus is driven by the assumption that when individuals look for a new job, they are interested in at least maintaining their current wage.

To examine whether the forced mobility of failure is related to a wage increase, it is helpful

to consider the counterfactual of employees staying at their firm—that is, employees from failed firms who may have likely stayed at that firm had it not failed. Of course, many outcomes are not possible with a "stayer" counterfactual, such as whether an employee leaves the ASR industry. However, an advantage of Census data is that we can compare wage differentials between leavers and stayers. Specifically, we match employees from failed firms to employees who stayed at ongoing firms, using the same matching strategy described above, and we analyzed the relationship between *Wage Differential* and *Failure*.

Although we cannot directly report the details from these results due to Census disclosure-avoidance regulations, we are authorized to describe the results from an analysis that used the same specification as Table 3 Model 1 but with a matched counterfactual of stayers. Based on Census review of the analysis, we are authorized to make the following statement: "Among all workers whose entire careers occurred in the 25 states available to us and DC, we find that the estimated coefficient on *Failure* was both positive and statistically significant."

The Limits of Avoiding an Evaluative Stigma: Failure Type

We theorized that evaluative stigma in the context of firm failure would be generated by evaluators believing that an employee's association with a failed firm is a credible proxy of their quality, skills, and competence (internal attribution) and not unrelated to these characteristics. We further theorized that if prospective employers apply an evaluative stigma selectivity across employees, it is likely the case that affiliations with certain failures should stigmatize all associated employees. Specifically, researchers have discussed that evaluative stigma may be especially strong when the reasons for failure move beyond "honest incompetence" (Hendry 2002) and include more nefarious events, such as scandal, which may call all associated employees' trustworthiness and integrity into question (Wiesenfeld, Wurthmann, and Hambrick 2008).

The anonymized nature of the Census data does not allow us to identify the causes of firm failure, and particularly whether the failure involved a scandal. However, in the ASR data we were able to review the set of failures and identify two firms whose failures were associated with a widely publicized scandal: Kurzweil Applied Intelligence (1994; KAI) and Lernout & Hauspie (2000; LH). Appendix B (ASR Scandals) provides more background on these scan-

dals. Like many other ASR firms, both KAI and LH had claimed a high level of accuracy for their speech recognition software yet struggled to deliver on those claims, resulting in disappointing sales. Unlike other ASR firms, LH and KAI fabricated sales to conceal their poor performance. In both cases, executives served jail time. Moreover, similar to Enron, WorldCom, and Theranos, these failures were highly publicized in the media.

Given that the nature of failure was different than the routine failures in the rest of our sample, we removed employees who worked at these two firms—at the time of their failure—from our main sample. For these analyses, we match (again using CEM) employees who worked at LH or KAI to employees from firms that failed for reasons not involving a scandal. (Note that employees of ongoing firms are not included in this analysis.) Our matching criteria are the same as the main sample, but we do not match on geography. Appendix Table BA3 provides the descriptive statistics for this sample.

Table 9 presents the results from this analysis, and Figure 5 provides the coefficient plots from these regressions. In Model 1, employees from scandal-affiliated failures are significantly more likely to leave the ASR industry than their counterparts from routine failures. Model 2 further demonstrates that this adverse outcome is consistent regardless of employee seniority, meaning that both executives and non-executives alike experience evaluative stigma after scandal-driven firm failure. In Model 3, the main effect of *Engineer* again shows that employees with specialized human capital from failed firms are the most likely to stay in the ASR industry, however, the interaction between *Engineer* and *Scandal* suggests that this increased likelihood is erased when engineers come from a failed firm with an associated scandal.

[Table 9]

[Figure 5]

These analyses reinforce our theoretical expectation that scandals constitute a critical boundary condition: When failure signals compromised integrity or ethics, the evaluative stigma extends beyond organizational leaders, negatively impacting modal employees.

DISCUSSION

We develop a theoretical framework specifying when evaluative stigma occurs after employer failure and whether expectations for organizational leaders differ from those for modal employees. Consistent with prior research, we expect leaders from failed firms to face evaluative stigma because they bear direct responsibility for firm performance. However, it is less clear whether stigma extends to the modal employee. Given the salience of prior employer affiliation as a labor-market signal, affiliation with failed firms may convey a negative signal via courtesy stigma, resembling the internal attributions directed at leaders. At the same time, because the average employee has little influence over firm-level outcomes, affiliation may not trigger an evaluative stigma for the modal employee. Thus, the theoretical framework predicts that evaluative stigma may differ by hierarchical position—internal attribution and stigma for leaders and insulation for modal employees. We then extend our framework by considering heterogeneity in three domains: failure type (routine vs. scandal), which affects whether stigma generalizes across the hierarchy; labor-market conditions (industry contraction and geographic concentration), which shape post-failure opportunities; and individual characteristics, namely specialized human capital and demographics, which are commonly associated with evaluative bias.

We test our theory using two complementary datasets—a large, representative sample from the US Census and detailed hand-collected data from the innovation-driven Automatic Speech Recognition (ASR) industry—we found empirical support that evaluative stigma is based on employees' hierarchical position. Organizational leaders consistently experienced adverse career outcomes following employer failure, whereas modal employees did not. In the Census data, leaders from failed firms experienced substantially lower subsequent wage growth compared to matched peers from ongoing firms, whereas modal employees from failed firms had marginally higher wage growth (1.7 percentage points) compared to similar peers from ongoing firms. The ASR data reinforced this pattern: Executives from failed firms were significantly less likely to remain in the industry, while modal employees had outcomes comparable to their counterparts from ongoing firms.

Our results then help delineate the conditions under which evaluative stigma broadly applies. Specifically, the ASR data revealed that when firm failure involves highly suspect cir-

cumstances, such as scandals, evaluative stigma extends beyond leaders to negatively affect all employees. Employees from scandal-associated failures were uniformly less likely to remain in the ASR industry compared to peers from routine failures, providing strong evidence of an evaluative stigma. Our analyses further highlight heterogeneity in how employer failure is related to subsequent career outcomes. Employees with specialized human capital experienced more favorable outcomes after firm failure. The local availability of employment opportunities also proved important. In the ASR data, employees located in industry clusters were more likely to remain in the industry after failure. Similarly, Census results demonstrated that wage benefits from firm failure diminished considerably when employment opportunities within an industry shrank significantly. Thus, displaced employees' outcomes are more beneficial when employment alternatives are more abundant. Demographic factors also moderated the relationship between employer failure and career outcomes. Although we found no gender-based differences, racial minorities and immigrants in the Census sample experienced smaller wage gains following employer failure than did white, native-born peers.

Contributions

A main contribution of our paper is the expansion of organizational research on employer failure and employee careers. By considering *all* employees—not merely those within the "upper echelons" of organizations—we were able to integrate insights across relevant research on careers, evaluations, stigma, and attribution to provide a more comprehensive theoretical framework for understanding whether and how an affiliation with a failed firm is related to subsequent career outcomes. Prior focus on leaders has made it difficult to disentangle whether observed career outcomes, and thus underlying mechanisms, stem from an employee's hierarchical position, the nature of the failure event itself, or a combination of these factors. Furthermore, reliance on small-sample case studies may obscure the role of broader contextual factors, including economic downturns, industry trends, shifting competitive landscapes, or individual characteristics. By expanding our theoretical lens beyond organizational leaders to include modal employees from numerous organizations, we establish a more robust framework to better understand the implications of employer failure and employee careers.

Our approach highlights the presence of conditional internal attribution as a key driver of evaluative stigma following employer failure. Prior research implicitly assumes that evaluators predominantly make internal attributions, viewing firm outcomes as directly reflective of organizational leaders' competence and part of their responsibility (Boeker 1992; Park, Chung, and Rajagopalan 2021; Park and Westphal 2013; Salancik and Meindl 1984). Although organizational leaders' career outcomes are consistently linked to firm performance, it remains a theoretical puzzle whether such internal attributions extend downward to the modal employee. Although prospective employers are unlikely to explicitly blame the average employee for their employer's failure, affiliation with a failed firm could nonetheless evoke an evaluative stigma via internal attribution. Specifically, prospective employers might infer that firms that eventually fail tend to employ workers of lower average quality relative to their more successful counterparts, leading to a "courtesy stigma" (Goffman 1963; Wiesenfeld, Wurthmann, and Hambrick 2008). This risk is especially heightened in a hiring context given the significant uncertainty hiring firms face, which leads prospective employers to rely on observable affiliations and signals, such as one's prior employer.

This perspective also contributes to broader organizational research on careers, particularly regarding the signaling value of employer affiliations. Prior research has highlighted how affiliation with reputable employers serves as a positive signal, thereby enhancing employees' and prospective employers' perceptions of candidate quality (Bidwell et al. 2014; Graffin et al. 2008; Phillips 2002). However, apart from research on employer failure, there has been less of a focus on the potential negative aspects of having certain employer affiliations. Our theoretical framework discusses that there are conditions under which an affiliation with a failed firm may generate negative signals in the labor market. We show that prospective employers' interpretations vary systematically, influencing career outcomes differently for organizational leaders compared to modal employees. An important direction for future research would be to explore potential heterogeneity in positive signaling contexts, examining whether benefits derived from affiliations with reputable or high-status firms similarly vary according to employee characteristics, such as department, demographics, or role.

We also contribute to research on employee mobility and external job searching. Prior stud-

ies have shown that individuals exhibit considerable inertia, preferring to stay with their current employers (Meyer and Herscovitch 2001; Mitchell and Dacin 1996; Mitchell et al. 2001; Mobley 1977; Sørensen 2000) and not frequently engaging in active job searches (Igielnik 2022). As a result, many employees underestimate their external market value and potential wage gains (Jäger et al. 2021). By showing that modal employees displaced by employer failure experience wage growth comparable to—and even marginally exceeding (by about 1.7 percentage points)—matched peers from ongoing firms (who experience about 10.8 percentage points of wage growth), we provide connections across this broader literature. Our results suggest employer failure involuntarily disrupts employee inertia, compelling affected workers to explore external employment opportunities and enabling them to realize wage gains similar to unaffected job seekers. Moreover, we find no evidence that employer failure leads to prolonged or permanent unemployment. Despite these potential benefits, we highlight that it is unlikely that employees would hope for such events, given the general preference for employee stability.

To research on evaluative stigma, we clarify the boundary conditions of internal attribution and thus stigma across the organizational hierarchy after employer failure. Although the modal employee who experiences a routine firm failure generally goes on to attain career outcomes comparable to peers from ongoing firms, we find that this is not the case when their employer's failure involves a scandal. Empirically, we illustrate this point using two ASR firms whose failures were prominently associated with significant ethical breaches. In these two cases, the vast majority of employees were substantially more likely to exit the industry than matched employees from routine failures. These findings directly contribute to organizational research on stigma, evaluations, and scandal, which emphasizes how severe, negative events can erode evaluators' distinctions about individual responsibility and extend stigma categorically across individuals (e.g., Dimmock, Gerken, and Graham 2018; Jonsson, Greve, and Fujiwara-Greve 2009; Paruchuri and Misangyi 2015; Pierce and Snyder 2008; Pontikes, Negro, and Rao 2010; Yenkey 2018). Thus, our findings highlight that mere affiliation alone may be insufficient to trigger courtesy stigma; rather, the nature and public perception of the failure event significantly influence whether categorical stigmatization occurs. This insight helps to further explain why some associations or actions become stigmatizing while others do not (Adut 2005; Aven 2015).

Our consideration of employee-level and labor market-level heterogeneity enriches our theoretical framework and provides further detail. Regarding the individual employee, we find in the Census data that racial minorities and immigrants experience less wage growth following employer failure compared to US-born, non-minority employees. Interestingly, we do not find evidence of gender-based disadvantages in either dataset. Given that extensive prior research has demonstrated that demographic factors often shape evaluative biases and career outcomes (Abraham, Botelho, and Lamont-Dobbin 2024; Berger 1977; Botelho and Abraham 2017; Correll and Ridgeway 2003; Moss-Racusin et al. 2012), our results extend this literature by suggesting that the likelihood of internal attribution following employer failure may also differ depending on employee demographics, disadvantaging certain groups.

Additionally, the ASR data allow us to examine heterogeneity in specialized human capital. We find consistent evidence that employees possessing specialized skills, such as engineers in the ASR industry, are most likely to remain employed within their industry after firm failure. This result underscores that a lack of internal attribution in the wake of employer failure may particularly benefit employees with highly sought-after skill sets (Gibbons and Waldman 2004; Jara-Figueroa et al. 2018; Lazear 2004; Phillips 2002). Thus, employer failure can paradoxically create opportunities, especially for employees who are highly valued by competing firms.

Regarding labor-market factors, we considered industry dynamics and geographic proximity of competitor firms as conditions that may moderate career outcomes following employer failure. In the Census data, we demonstrated that wage growth for displaced employees is notably attenuated when employer failure occurs within a shrinking industry. This finding further supports our broader theoretical argument regarding internal attribution and evaluative stigma: Under tight labor-market conditions, prospective employers may be more likely to rely on an affiliation with a failed firm as a signal of employee quality, weakening the protective buffer that modal employees experience. Complementarily, our detailed location data from the ASR industry show that displaced employees are more likely to remain within their industry when their employer fails in geographic areas with a high concentration of proximate ASR firms. This finding suggests that employer failure can present strategic hiring opportunities for nearby

firms seeking to recruit valuable human capital (Saxenian 1990). Taken together, these results highlight how labor-market dynamics shape the career implications of employer failure, offering implications for strategic human capital management.

Finally, we provide an empirical contribution to organizational research by replicating and generalizing findings previously established through narrowly focused case studies. In developing our broader theoretical framework, we first confirmed the negative relationship between employer failure and subsequent career outcomes for organizational leaders. Using millions of employees from thousands of firms across multiple industries and spanning a quarter-century, we robustly replicate these prior case-study findings, further strengthening their external validity. Additionally, our use of matched organizational leaders from ongoing firms as a comparison set helps isolate the specific impact of employer failure. Although replication is widely recognized as critical to scientific inquiry, it remains relatively uncommon within organizational research. Further, by focusing on wages, we utilize a more generalizable and broadly comparable outcome than has typically been used in prior studies, allowing us to track individuals who change employers across industries where other outcome variables such as titles or prestige may not be directly comparable.

Alternative Explanations, Limitations, and Future Directions

Although we provide consistent evidence for our main results, we acknowledge that it is important to consider alternative explanations. First, our results might reflect unobserved differences between firms that fail and those that continue operating. To address this possibility, we conducted a placebo analysis comparing employees who left firms in the year *before* failure with employees who remained until the firm actually failed. The absence of similar effects in this placebo group strengthens our confidence that observed outcomes are related specifically to experiencing the firm's failure rather than pre-existing differences. Second, one might argue that employees of failed firms would have remained at their jobs had their firms not failed, implying a different counterfactual scenario. While our primary theoretical interest was understanding demand-side evaluations by prospective employers, we conducted additional analyses comparing employees from failed firms with matched employees who remained at ongoing firms. This

complementary supply-side perspective yielded results consistent with our primary findings, reinforcing our main conclusions. Third, one might argue that the observed wage growth among employees from failed firms could be driven by these firms cutting wages immediately prior to their failure, artificially inflating the observed subsequent wage growth. However, we did not observe that this systematically occurred.

The results from our additional analyses provide greater confidence in the relationships we identified, however, additional mechanisms not considered here could also play a role in our findings. One possibility is that employees from failed firms actively engage in impression management strategies to counteract or preempt potential evaluative stigma, thereby improving their subsequent career outcomes relative to other job seekers. Understanding employees' responses to potential stigma following employer failure represents a significant opportunity for future research. Additionally, it is possible that third parties, such as recruiters, former colleagues, or industry contacts, might intervene in ways that influence affected employees' career outcomes. These interventions could be beneficial, such as providing referrals and facilitating job opportunities, but they might also have unintended consequences, such as inadvertently publicizing an individual's affiliation with a potentially stigmatized firm.

We triangulated our findings using two complementary datasets, however, our ability to draw causal inferences is limited for a few reasons. First, although the Census data provide detailed, quarter-by-quarter career histories, they do not specify the exact reasons employees leave ongoing firms. For employees from failed firms, separations are clearly involuntary, but for the matched set of employees from ongoing firms, we cannot determine whether separations are voluntary or involuntary. However, our robustness analysis—matching employees from failed firms with similar employees who remained at ongoing firms—helps alleviate this concern. Second, our data do not capture critical stages in the hiring process, such as job postings, interviews, or detailed recruitment interactions. If available, such data could enhance our understanding of how employers respond strategically to employees from failed firms, a dynamic suggested by our industry shrinking analysis in the Census and local competition analysis in the ASR industry. Furthermore, detailed recruitment data might also illuminate why certain demographic groups, notably racial minorities and immigrants, experience comparatively worse

outcomes following employer failure.

We view our study as an initial step toward systematically investigating a key career phenomenon that has largely eluded organizational researchers, in part due to a lack of detailed, generalizable data. This gap in data availability has, in turn, constrained theoretical development regarding the broad impact of employer failure on employee careers. Our findings suggest several fruitful avenues for future research.

Our results indicate that, on average, the modal employee is not subject to internal attributions following employer failure. This pattern is consistent with hiring firms using external attribution when assessing these candidates, given the salience of employer signals in the labor market. That said, distinguishing external attribution from non-attribution would require direct evidence of evaluator beliefs, which we do not possess. Future research should examine hiring managers' and recruiters' assessments along with their belief to disentangle between the two.

Another promising direction involves examining more precisely how the underlying causes of firm failure influence subsequent employee outcomes. Although firm failure is quite common, research lags behind the role of failure and careers (Botelho and Chang 2022; Botelho, Gulati, and Sorenson 2024). Identifying clear causes of firm failure is challenging: The anonymity of Census data precludes analysis based on named firms, and even within the ASR sample, where firms were identified, exact reasons for failure remained elusive in most cases. Typically, publicly available accounts of firm failure lack detail, given the absence of mandated reporting on the causes of failure. Future researchers who are able to collect contemporaneous data on why firms fail—such as through interviews or proprietary reports—would greatly advance our understanding. Additionally, our results highlight the importance of exploring how other employee-level and labor market-level characteristics interact with employer failure. We provided initial evidence that certain individual characteristics and labor market factors moderate observed outcomes. Future research could more thoroughly theorize and empirically investigate these factors, given our findings.

Practical Implications

Our study offers practical insights for employees, though caution is warranted against overgeneralizing from these findings. On the one hand, modal employees may find reassurance that joining firms with uncertain prospects need not necessarily harm their long-term career trajectories, given that firm failure does not typically translate into negative evaluations for them via lower wage growth or likelihood to stay in the industry. However, this assurance does not extend equally to *all* employees. Individuals accepting leadership positions at high-risk or struggling firms should carefully evaluate the firm's performance and outlook, given the strong potential for evaluative stigma to negatively shape their subsequent career outcomes. Additionally, modal employees from certain backgrounds or who lack certain skills may experience worse career outcomes after employer failure, highlighting the need for increased vigilance. Moreover, because post-failure career benefits tend to accrue most in densely concentrated labor markets, employees considering roles in geographically isolated firms or declining industries should recognize the heightened risks associated with fewer local employment alternatives.

Overall, our results should alleviate concerns among average employees about potential negative career implications of employer failure. Nevertheless, the presence of significant evaluative stigma following firm failures involving scandals underscores the necessity of careful due diligence. Prospective employees, particularly those joining new or lesser-known organizations, should not only evaluate business plans and industry risks but also closely examine the character and integrity of organizational leaders to mitigate the risks of becoming affiliated with scandal-related failures.

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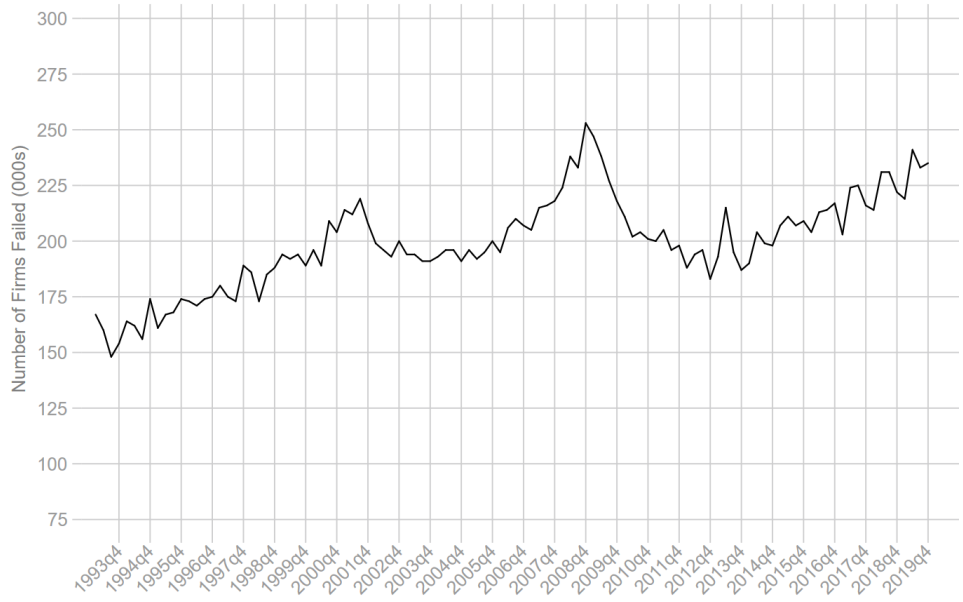
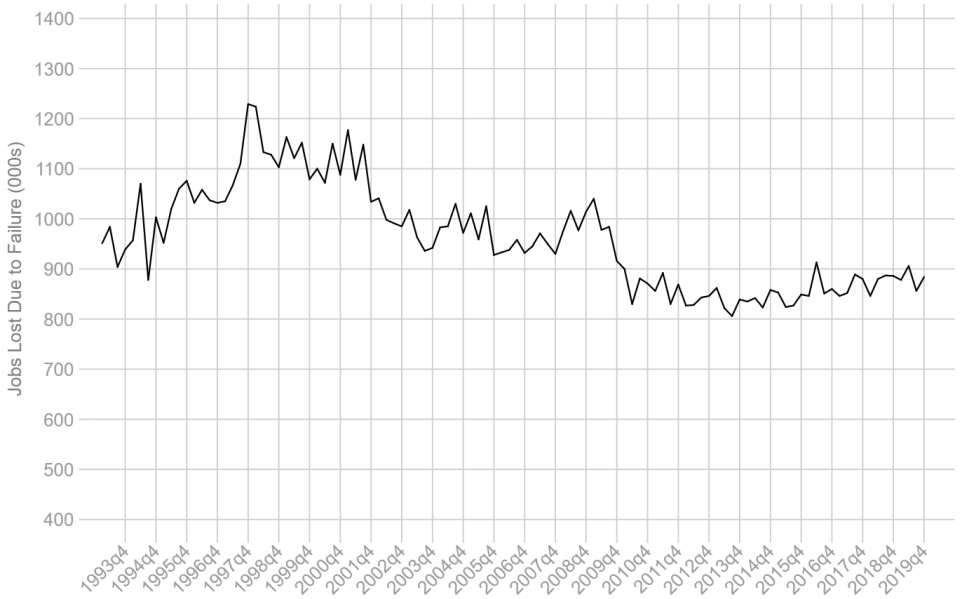
FIGURES AND TABLES

Figure 1: Firm Failure and Job Loss in US: 1993 to 2019

(a) Number of Firms Failed Per Quarter



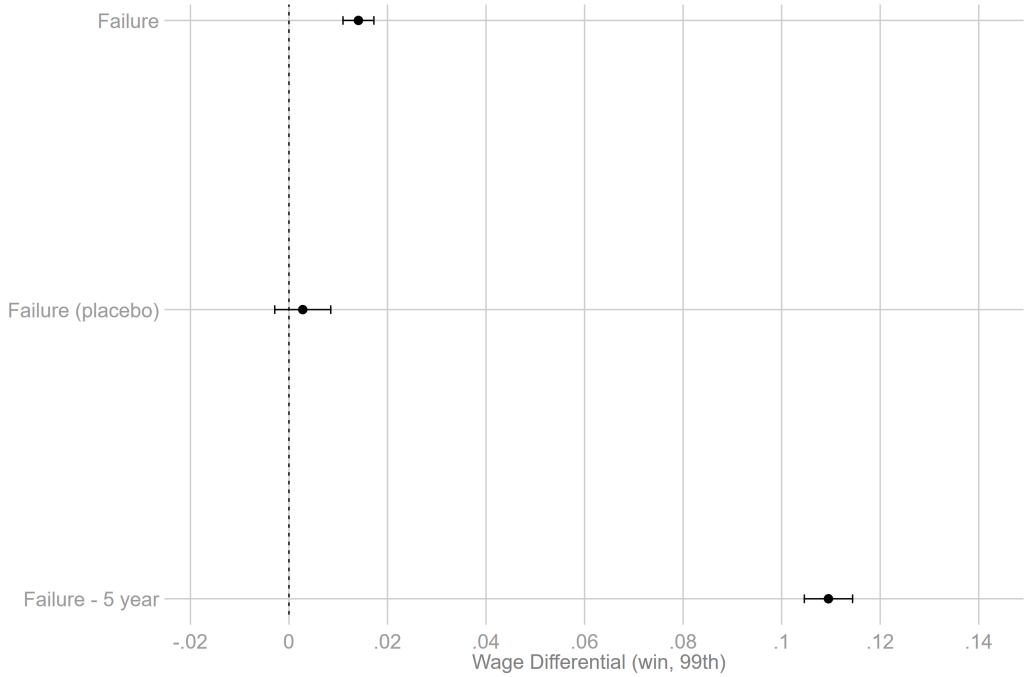
(b) Job Loss Due to Firm Failure



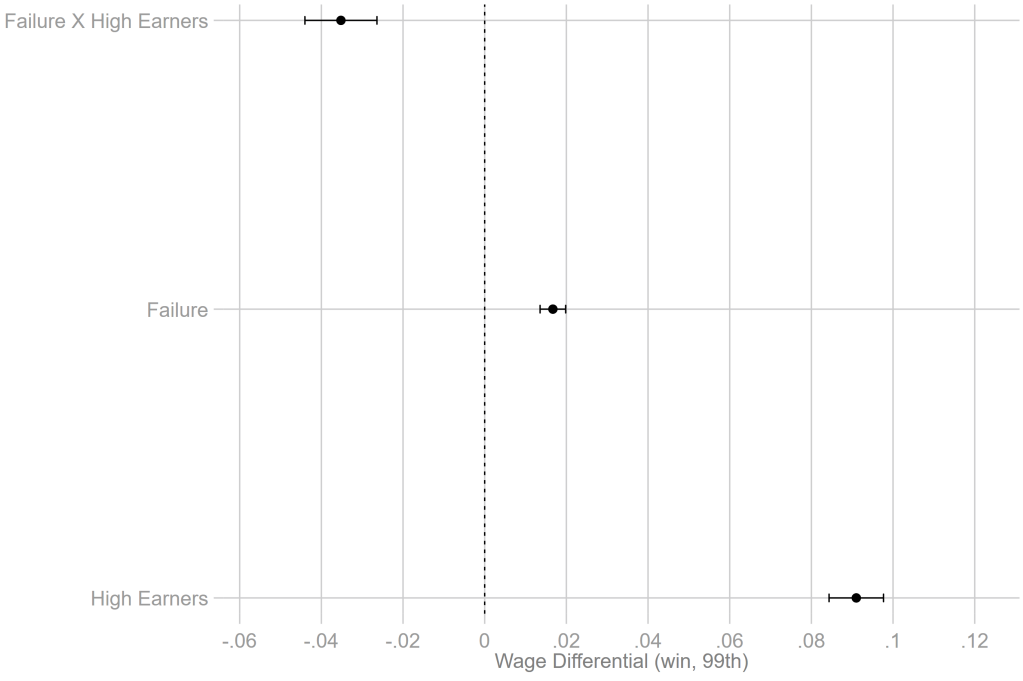
Note: From publicly available BLS data. Firm failure statistics are from `https://www.bls.gov/web/cewbd/table9_1.txt` and job loss is the sum of job loss from each firm size class (`https://www.bls.gov/web/cewbd.supp.toc.htm#sizeclass`).

Figure 2: Comparison of Wage Differentials by Firm Failure in Census Matched Sample

(a) All Movers

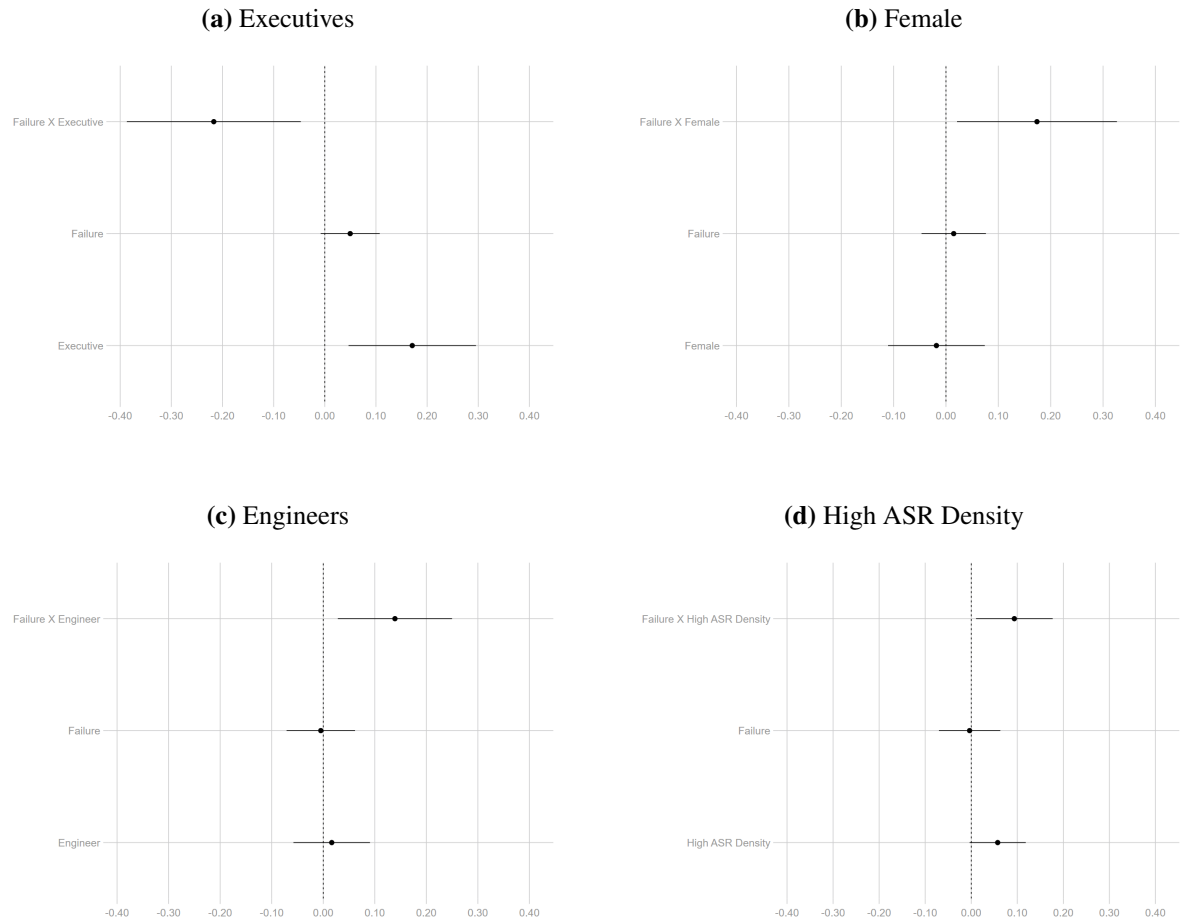


(b) High Earner



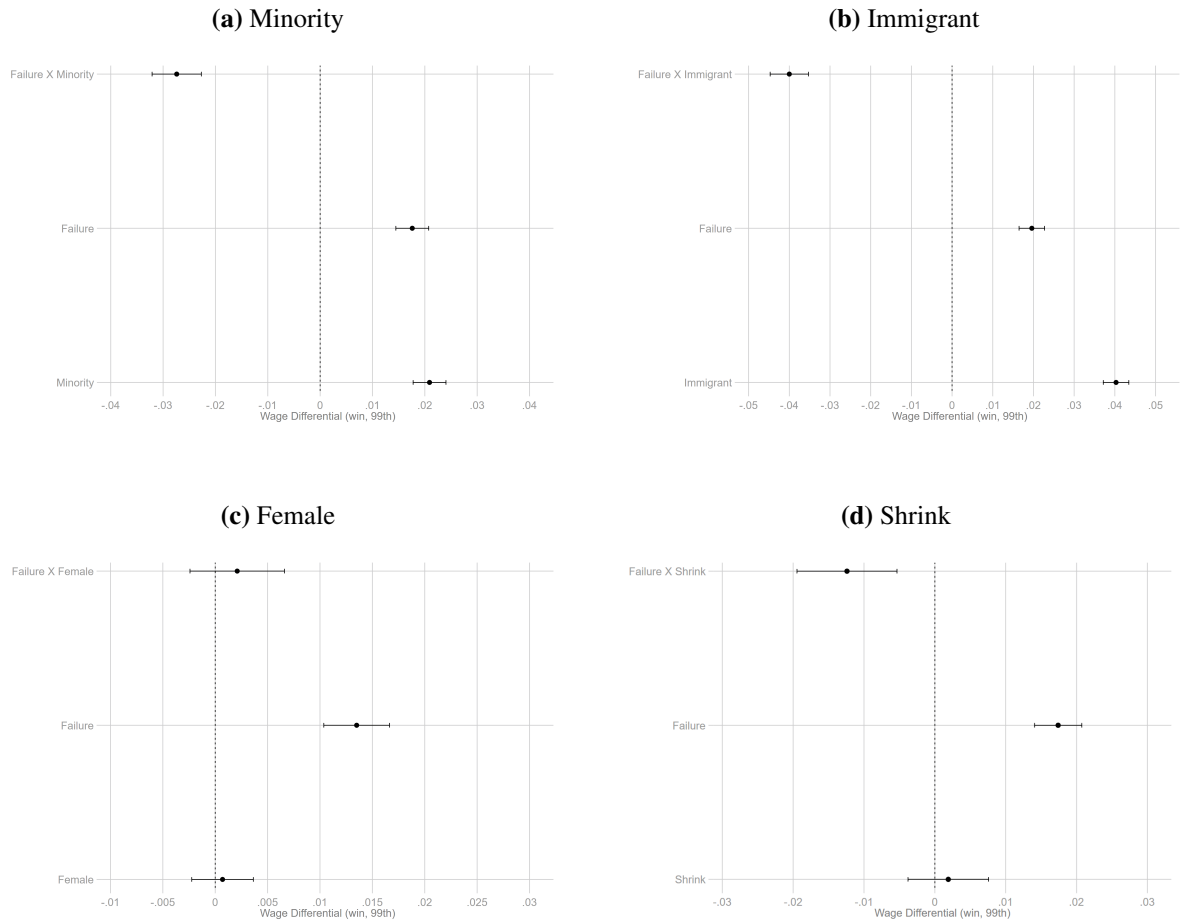
Note: Comparison of *Wage Differential* by *Failure*, showing the coefficient plots from Table 3. Bars represent 95% confidence intervals. (a) the *Failure* coefficient is from Model 1, the *Failure (placebo)* coefficient is from Model 3, and the 5-year *Failure* coefficient is from Model 4. (b) is from Table 3 Model 2.

Figure 3: ASR Failure and Staying in the Industry



Note: Comparison of *Staying in Industry* by *Failure* and worker characteristics, showing coefficient plots from Table 7. Bars represent 95% confidence intervals. (a) is from Model 1, (b) is from Model 2, (c) is from Model 3, and (d) is from Model 4.

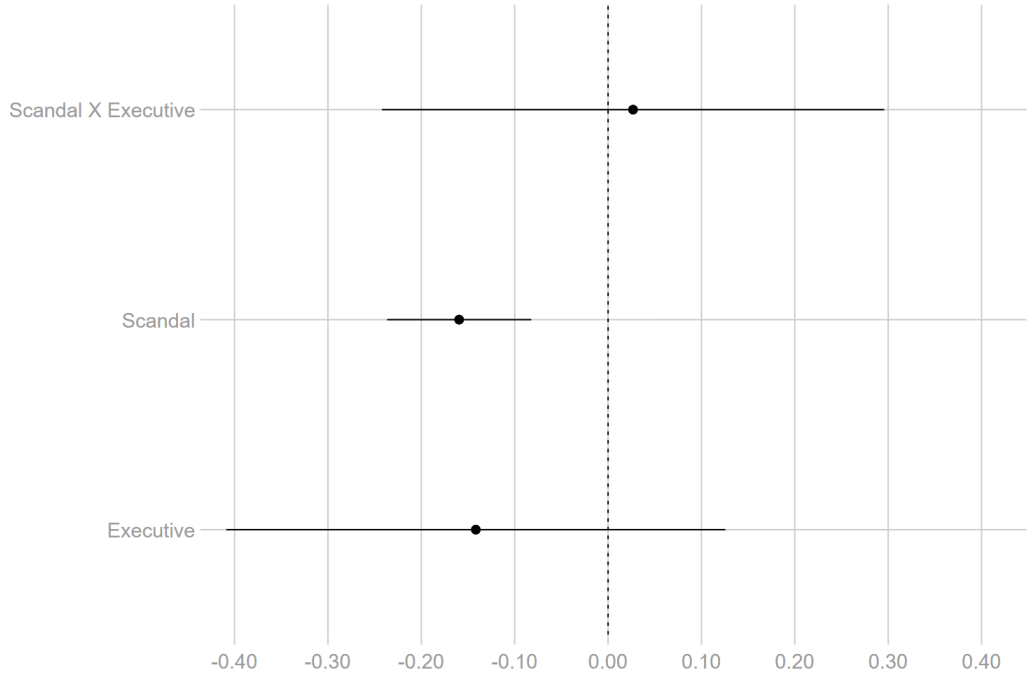
Figure 4: Interaction effects for Census wage differentials



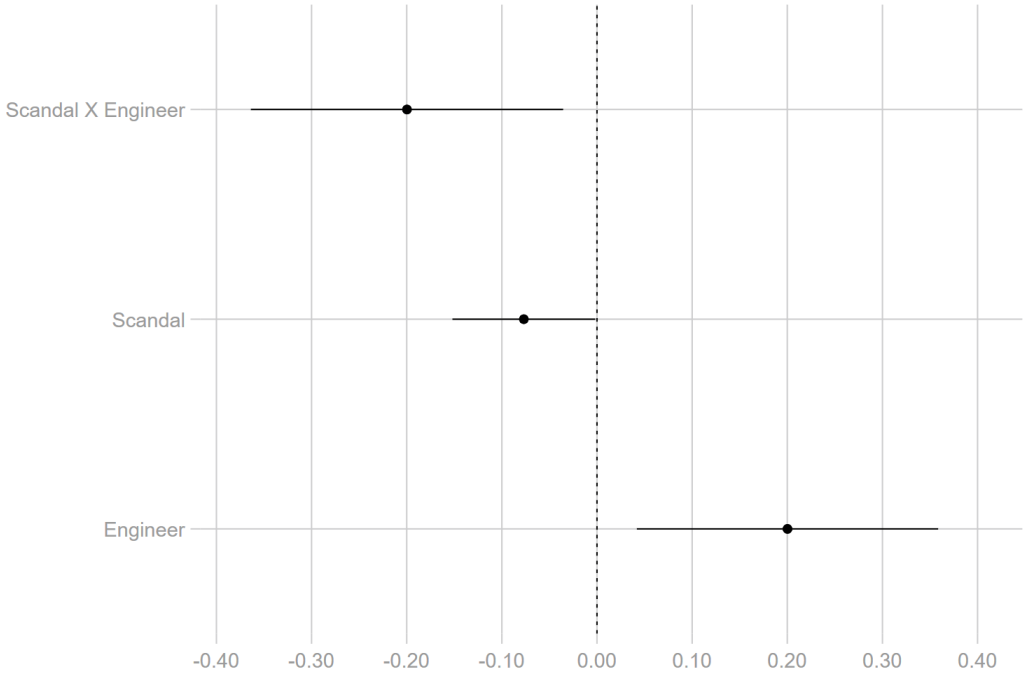
Note: Comparison of *Wage Differential* by *Failure* and worker/industry characteristics, showing coefficient plots from Table 8. Bars represent 95% confidence intervals. (a) is from Model 1, (b) from Model 2, (c) Model 3, and (d) Model 4. Minority, Immigrant, and Female are from Census. *Shrink* are industries whose share of employment decreased by at least 10% in the prior five years.

Figure 5: ASR Scandal and Staying in the Industry

(a) Executives



(b) Engineers



Note: Comparison of *Staying in Industry* by *Scandal* and worker characteristics, showing coefficient plots from Table 9. Bars represent 95% confidence intervals. (a) is from Model 2, and (b) is from Model 3.

Table 1: Descriptive Statistics: Census Matched Sample. *N*=2,200,000.

Variables	Mean	SD
Wage Differential (ln)	-0.0067	0.3979
Wage Differential, previous year (ln)	0.0016	0.4189
Wage Differential, 5 years (ln)	-0.0049	0.3939
Failure	0.5002	0.5
Failure (placebo)	0.046	0.2098
Age	45.44	8.666
Female	0.2836	0.4507
Immigrant	0.1372	0.3441
Minority	0.1282	0.3343
College	0.4078	0.4914
Firm Size (ln)[a]	6.794	3.054
Unemployment	6.079	1.834
Manufacturing	0.1053	0.3069
High Earner	0.086	0.2804
High Earner (90th percentile)	0.186	0.3891
High Earner (99th percentile)	0.0363	0.1871
Move Lag (ln)	0.2593	0.5842
Shrink	0.2669	0.4423
Move Lag $= 0$	0.7876	0.4089
Move Lag ≤ 2	0.9305	0.2542

Note: Observations are worker moves, collected for all workers whose entire careers occurred in the following 25 states and DC: AR, CA, CO, DC, DE, GA, HI, IL, IN, IA, LA, ME, MD, MO, NV, OK, OR, PA, RI, SC, TN, TX, UT, VT, and WA. Workers who did not move firms are not included. Census disclosure rules mandate rounding of observations to the nearest 10,000 and the truncation of reported statistics to four significant figures; therefore, decimal points do not always appear. Minimum values, maximum values, and true medians cannot be disclosed. [a] is the log of the number of employees at the firm at a given year.

Table 2: Correlations: Census Matched Sample

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1) Wage Differential (ln)										
(2) Wage Diff. prev. year (ln)	–0.0417									
(3) Wage Diff. 5 years (ln)	0.9735	–0.0418								
(4) Failure	0.0246	0.0845	0.0209							
(5) Failure (placebo)	–0.0004	–0.0057	–0.0010	–0.1877						
(6) Age	–0.0645	–0.1242	–0.0686	0.0106	0.0040					
(7) Female	–0.0005	0.0204	–0.0002	–0.0004	0.0062	0.0237				
(8) Immigrant	0.0239	0.0192	0.0244	0.0072	–0.0022	–0.0835	–0.0102			
(9) Minority	0.0161	0.0148	0.0166	–0.0177	0.0026	–0.0537	0.0712	0.3561		
(10) College	0.0163	0.0180	0.0165	–0.0168	0.0013	–0.0309	0.1081	0.0671	0.0475	
(11) Firm Size (ln)[a]	0.0273	0.0118	0.0268	0.0005	0.0115	0.0186	0.0563	0.0040	0.0848	0.0167
(12) Unemployment	–0.0157	0.0498	–0.0157	–0.0029	–0.0210	0.0709	0.0037	0.0885	0.0540	0.0195
(13) Manufacturing	–0.0034	–0.0151	–0.0034	0.0030	–0.0005	0.0217	–0.0939	–0.0176	–0.0019	–0.1057
(14) High Earner	0.0439	–0.0573	0.0434	0.1016	–0.0113	0.0807	–0.0691	–0.0124	–0.0415	0.0933
(15) High Earner (90th perc.)	0.0680	–0.1053	0.0674	0.0874	–0.0087	0.0928	–0.0917	–0.0091	–0.0492	0.1307
(16) High Earner (99th perc.)	0.0347	–0.0946	0.0348	0.0503	–0.0048	0.0484	–0.0457	–0.0049	–0.0253	0.0540
(17) Move Lag (ln)	–0.0350	0.2122	–0.0369	–0.1724	0.0249	0.0247	–0.0136	0.0020	–0.0070	0.0129
(18) Shrink	0.0005	–0.0022	–0.0011	0.0081	–0.0057	0.0618	–0.1419	0.0312	0.0150	–0.1224
(19) Move Lag = 0	0.0448	–0.2101	0.0469	0.1744	–0.0246	–0.0121	0.0092	0.0057	0.0109	0.0070
(20) Move Lag ≤ 2	0.0345	–0.1583	0.0358	0.1208	–0.0223	–0.0243	0.0132	–0.0088	–0.0009	–0.0250

Variables	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	
(12) Unemployment	-0.0406									
(13) Manufacturing	0.0980	-0.0184								
(14) High Earner	-0.0403	0.0088	-0.0205							
(15) High Earner (90th perc.)	-0.0176	0.0084	-0.0201	0.6116						
(16) High Earner (99th perc.)	-0.0939	0.0058	-0.0228	0.4865	0.3761					
(17) Move Lag (ln)	-0.1416	0.0295	-0.0254	-0.0189	-0.0098	0.0297				
(18) Shrink	0.0981	0.0248	0.4479	-0.0232	-0.0266	-0.0289	-0.0345			
(19) Move Lag = 0	0.1480	-0.0314	0.0192	0.0228	0.0237	-0.0197	-0.8635	0.0270		
(20) Move Lag ≤ 2	0.0893	-0.0227	0.0212	0.0152	0.0001	-0.0267	-0.7818	0.0298	0.4986	

Note: Observations are worker moves, collected for all workers whose entire careers occurred in the following 25 states and DC: AR, CA, CO, DC, DE, GA, HI, IL, IN, IA, LA, ME, MD, MO, NV, OK, OR, PA, RI, SC, TN, TX, UT, VT, and WA. Workers who did not move firms are not included.

Table 3: Matched Census Sample OLS Regressions of Wage Differential on Firm Failure (vs. Employees from Ongoing Firms)

	Wage differential			Wage differential (5 years)
	Model 1	Model 2	Model 3	Model 4
Failure	0.0141***	0.0167***		0.1095***
	(0.0016)	(0.0016)		(0.0025)
High Earner	0.0678***	0.0910***		-0.0858***
	(0.0023)	(0.0034)		(0.0034)
Failure × High Earner		-0.0352***		
		(0.0045)		
Failure (placebo)			0.0028	
			(0.0029)	
Minority	0.0078***	0.0078***	0.0074***	0.0009**
	(0.0011)	(0.0011)	(0.0011)	(0.0014)
Immigrant	0.0201***	0.0200***	0.0203***	0.0049***
	(0.0011)	(0.0011)	(0.0011)	(0.0014)
Female	0.0017+	0.0017	-0.0018+	-0.0132***
	(0.0010)	(0.0010)	(0.0010)	(0.0013)
Age	-0.0033***	-0.0033***	-0.0033***	-0.0076***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Firm Size (ln)	0.0044***	0.0044***	0.0044***	0.0051***
	(0.0004)	(0.004)	(0.004)	(0.0007)
Unemployment	-0.0015*	-0.0015*	-0.0015*	0.0020**
	(0.0006)	(0.0006)	(0.0006)	(0.0007)
Move Lag (ln)	-0.0231***	-0.0232***	-0.0252***	-0.1914***
	(0.0010)	(0.010)	(0.011)	(0.0012)
Constant	0.1094***	0.1083***	0.1162***	0.1867***
	(0.0053)	(0.0053)	(0.0052)	(0.0063)
Year FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y
Observations	2,200,000	2,200,000	2,200,000	1,580,000

Note: Observations are worker moves, collected for all workers whose entire careers occurred in the following 25 states and DC: AR, CA, CO, DC, DE, GA, HI, IL, IN, IA, LA, ME, MD, MO, NV, OK, OR, PA, RI, SC, TN, TX, UT, VT, and WA. Workers who did not move firms are not included. Census disclosure rules mandate rounding of observations to the nearest 10,000 and the truncation of reported statistics to four significant figures. *Wage Differential* is measured as an employee's salary at their current job minus their salary at their previous job (logged). *Failure* takes the value of 1 if the employee is moving from a failed firm in that year, and ongoing is the reference category, which represents an employee moving from a firm still in operation. *Failure (placebo)* takes the value of 1 if the employee is moving from a failed firm during the year before the firm failed and 0 otherwise. Models include fixed effects for year, state of the failed employer, and the failed employer's six-digit NAICS industry code. Robust standard errors clustered at the firm level are in parentheses. Significance levels: $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Observations = 2,200,000.

Table 4: Matched Census Sample OLS Regressions of Wage Differential on Firm Failure (vs. Employees from Ongoing Firms): Robustness

	Wage differential (previous year)	Wage differential				
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Failure	0.0113***	0.0208***	0.0173***	0.0140***	0.0142***	0.0140***
	(0.0016)	(0.0016)	(0.0016)	(0.0016)	(0.0016)	(0.0016)
High Earner	0.0674***			0.0685***	0.0677***	0.0675***
	(0.0023)			(0.0023)	(0.0023)	(0.0023)
High Earner 90		0.1028***				
		(0.0022)				
Failure × HE 90		-0.0444***				
		(0.0030)				
High Earner 99			0.1098***			
			(0.0049)			
Failure × HE 99			-0.0386***			
			(0.0063)			
College				-0.0041***		
				(0.007)		
Minority	0.0077***	0.0099***	0.0066***	0.0078***		0.0143***
	(0.0011)	(0.0011)	(0.0011)	(0.0011)		(0.0011)
Immigrant	0.0206***	0.0197***	0.0197***	0.0203***	0.0226***	
	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)	
Female	0.0021*	0.0060***	-0.0001	0.0018+	0.0011	0.0019+
	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0010)
Age	-0.0033***	-0.0033***	-0.0032***	-0.0033***	-0.0034***	-0.0033***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Firm Size (ln)	0.0043***	0.0043***	0.0047***	0.0044***	0.0044***	0.0045***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Unemployment	-0.0014*	-0.0015*	-0.0016**	0.0015*	0.0015*	0.0015*
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Move Lag (ln)	-0.0234***	-0.0237***	-0.0244***	-0.0230***	-0.0230***	-0.0231***
	(0.0010)	(0.010)	(0.010)	(0.0010)	(0.0010)	(0.0010)
Constant	0.1170***	0.1012***	0.1078***	0.1111***	0.1161***	0.1103***
	(0.0053)	(0.0053)	(0.0053)	(0.0053)	(0.0053)	(0.0053)
Year FE	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y
Observations	2,200,000	2,200,000	2,200,000	2,200,000	2,200,000	2,200,000

Note: Observations are worker moves, collected for all workers whose entire careers occurred in the following 25 states and DC: AR, CA, CO, DC, DE, GA, HI, IL, IN, IA, LA, ME, MD, MO, NV, OK, OR, PA, RI, SC, TN, TX, UT, VT, and WA. Workers who did not move firms are not included. Census disclosure rules mandate rounding of observations to the nearest 10,000 and the truncation of reported statistics to four significant figures. *Wage Differential (1-year)* is measured as an employee's salary at their current job minus their salary at their previous job in the year prior to their departure (logged). *Failure* takes the value of 1 if the employee is moving from a failed firm in that year, and ongoing is the reference category, which represents an employee moving from a firm still in operation. *High Earner* denotes employees in the top 5% of earners for the firm. *High Earner (90th percentile)* denotes employees in the top decile of earners for the firm. *High Earner (99th percentile)* denotes employees in the top 1% of earners for the firm. Models include fixed effects for year, state of the failed employer, and the failed employer's six-digit NAICS industry code. Robust standard errors clustered at the firm level are in parentheses. Significance levels: $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Observations = 2,200,000.

Table 5: Descriptive Statistics: ASR Matched Sample

Variables	N	Mean	SD	Min	Max
Staying in Industry	2,466	0.300	0.458	0.000	1.000
Failure	2,466	0.217	0.412	0.000	1.000
Executive	2,466	0.110	0.313	0.000	1.000
Engineer	2,466	0.281	0.449	0.000	1.000
High ASR Density	2,466	0.340	0.474	0.000	1.000
Female	2,466	0.102	0.302	0.000	1.000
Employee Age	2,466	28.679	7.485	21.000	70.000
Year	2,466	1999.255	4.540	1981	2010

Note: Unit of analysis is an employee's move to a new firm. *Staying in Industry* takes the value 1 if the employee moves to an ASR firm and 0 if the employee takes a job with a firm outside of the ASR industry. *Failure* takes the value of 1 if the employee is moving from a failed firm in that year, and ongoing is the reference category, which represents an employee moving from a firm still in operation in that year. *Engineer* takes the value of 1 if the employee's title indicates that they held an engineering or science-related role (e.g., software engineer, computational linguist) and 0 otherwise. *High ASR Density* takes the value of 1 if the number of ASR firms within a 100-mile radius of the firm that the employee exited is above the sample median.

Table 6: Correlations: ASR Matched Sample

Variables	(1)	(2)	(3)	(4)	(5)	(6)
(1) Staying in Industry						
(2) Failure	-0.215					
(3) Executive	-0.069	0.031				
(4) Engineer	-0.0004	0.356	-0.099			
(5) High ASR Density	0.176	-0.257	0.018	0.055		
(6) Female	-0.009	0.214	-0.078	0.053	-0.0003	
(7) Employee Age	-0.041	0.020	0.294	-0.093	0.060	-0.071

Note: Unit of analysis is an employee's move to a new firm. *Staying in Industry* takes the value 1 if the employee moves to an ASR firm and 0 if the employee takes a job with a firm outside of the ASR industry. *Failure* takes the value of 1 if the employee is moving from a failed firm in that year, and ongoing is the reference category, which represents an employee moving from a firm still in operation in that year. *Engineer* takes the value of 1 if the employee's title indicates that they held an engineering or science-related role (e.g., software engineer, computational linguist) and 0 otherwise. *High ASR Density* takes the value of 1 if the number of ASR firms within a 100-mile radius of the firm that the employee exited is above the sample median.

Table 7: Matched OLS Regressions of Employee Staying in ASR Industry on Firm Failure (vs. Employees from Ongoing firms)

	Model 1	Model 2	Model 3	Model 4
Failure	0.050+	0.015	-0.005	-0.004
	(0.029)	(0.031)	(0.034)	(0.034)
Executive	0.171**	0.147*		
	(0.063)	(0.057)		
Failure × Executive	-0.217*			
	(0.086)			
Female	0.020	-0.018	0.014	0.014
	(0.038)	(0.047)	(0.040)	(0.040)
Failure × Female		0.174*		
		(0.078)		
Engineer		0.054	0.016	
		(0.033)	(0.038)	
Failure × Engineer			0.139*	
			(0.056)	
High ASR Density	0.080**	0.076**	0.069*	0.057+
	(0.028)	(0.027)	(0.027)	(0.031)
Failure × High ASR Density				0.094*
				(0.042)
Employee Age	-0.007***	-0.007***	-0.005**	-0.005**
	(0.002)	(0.002)	(0.002)	(0.002)
Constant	0.576**	0.586**	0.556**	0.566**
	(0.195)	(0.192)	(0.194)	(0.196)
Year FE	Y	Y	Y	Y
Observations	2,466	2,466	2,466	2,466
R-Squared Adj.	0.045	0.047	0.039	0.037

Note: Unit of analysis is an employee's move to a new firm. *Staying in Industry* takes the value 1 if the employee moves to an ASR firm and 0 if the employee takes a job with a firm outside of the ASR industry. *Failure* takes the value of 1 if the employee is moving from a failed firm in that year, and ongoing is the reference category, which represents an employee moving from a firm still in operation in that year. *Engineer* takes the value of 1 if the employee's title indicates that they held an engineering or science-related role (e.g., software engineer, computational linguist) and 0 otherwise. *High ASR Density* takes the value of 1 if the number of ASR firms within a 100-mile radius of the firm that the employee exited is above the sample median. Year fixed effects are included in all models. Robust standard errors clustered at the firm level are in parentheses. Significance levels: $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 8: Matched Census Sample OLS Regressions of Wage Differential on Firm Failure (vs. Employees from Ongoing Firms): Heterogeneity

	Model 1	Model 2	Model 3	Model 4
Failure	0.0176***	0.0196***	0.0135***	0.0174***
	(0.0016)	(0.0016)	(0.0016)	(0.0017)
Minority	0.0209***	0.0072***	0.0078***	0.0078***
	(0.0016)	(0.0011)	(0.0011)	(0.0011)
Failure × Minority	-0.0274***			
	(0.0024)			
Immigrant	0.0200***	0.0403***	0.0201***	0.0201***
	(0.0011)	(0.0016)	(0.0011)	(0.0011)
Failure × Immigrant		-0.0400***		
		(0.0024)		
Female	0.0017	0.0017	0.0007	0.0018+
	(0.0010)	(0.0010)	(0.0015)	(0.0010)
Failure × Female			0.0021	
			(0.0023)	
Shrink				0.0019
				(0.0029)
Failure × Shrink				-0.0124***
				(0.0036)
High Earner	0.0676***	0.0677***	0.0679***	0.0677***
	(0.0023)	(0.0023)	(0.0023)	(0.0023)
Age	-0.0033***	-0.0033***	-0.0033***	-0.0033***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Firm Size (ln)	0.0044***	0.0044***	0.0044***	0.0044***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Unemployment	-0.0015*	-0.0015*	-0.0015**	0.0015*
	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Move Lag (ln)	-0.0231***	-0.0232***	-0.0231***	-0.0231***
	(0.0010)	(0.010)	(0.010)	(0.0010)
Constant	0.1075***	0.1066***	0.1096***	0.1088***
	(0.0053)	(0.0053)	(0.0053)	(0.0054)
Year FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y
Observations	2,200,000	2,200,000	2,200,000	2,200,000

Note: Observations are worker moves, collected for all workers whose entire careers occurred in the following 25 states and DC: AR, CA, CO, DC, DE, GA, HI, IL, IN, IA, LA, ME, MD, MO, NV, OK, OR, PA, RI, SC, TN, TX, UT, VT, and WA. Workers who did not move firms are not included. Census disclosure rules mandate rounding of observations to the nearest 10,000 and the truncation of reported statistics to four significant figures; therefore, decimal points do not always appear. *Wage Differential* is measured as an employee's salary at their current job minus their salary at their previous job (logged). *Failure* takes the value of 1 if the employee is moving from a failed firm in that year, and ongoing is the reference category, which represents an employee moving from a firm still in operation. *Manufacturing* denotes NAICS classifications starting with the digit 1. *Shrink* denotes industries that had at least a 10% smaller share of state-level employment vs. five years prior. Models include fixed effects for year, state of the failed employer, and the failed employer's six-digit NAICS industry code. Robust standard errors clustered at the firm level are in parentheses. Significance levels: $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Observations = 2,200,000.

Table 9: Matched OLS Regressions of Employee Staying in ASR Industry on Scandal (vs. Employees from Firm Failure without Scandal)

	Model 1	Model 2	Model 3
Scandal	-0.158***	-0.159***	-0.077*
	(0.037)	(0.038)	(0.037)
Executive	-0.118***	-0.142	-0.122***
	(0.020)	(0.133)	(0.020)
Scandal × Executive		0.027	
		(0.133)	
Engineer	0.047	0.047	0.200*
	(0.039)	(0.039)	(0.079)
Scandal × Engineer			-0.200*
			(0.081)
Female	-0.004	-0.004	-0.019
	(0.024)	(0.024)	(0.017)
Employee Age	0.004	0.004+	0.003
	(0.002)	(0.002)	(0.002)
Constant	0.274+	0.273+	0.243
	(0.157)	(0.157)	(0.166)
Year FE	Y	Y	Y
Observations	633	633	633
R-Squared Adj.	0.044	0.043	0.052

Note: Unit of analysis is an employee's move to a new firm. *Staying in Industry* takes the value 1 if the employee moves to an ASR firm and 0 if the employee takes a job with a firm outside of the ASR industry. *Scandal* takes the value of 1 if the employee is moving from a failed firm with an associated scandal in that year, where there are two instances of ASR firms failing due to scandal. (*Failure* without an associated scandal is the reference category.) *Engineer* takes the value of 1 if the employee's title indicates that they held an engineering or science-related role (e.g., software engineer, computational linguist) and 0 otherwise. Year fixed effects are included in all models. Robust standard errors clustered at the firm level are in parentheses. Significance levels: $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Beyond Blame: Evaluative Stigma, Attribution, and Employee Careers after Employer Failure

Online Appendix

A US CENSUS: STAYING IN INDUSTRY

We also constructed the *Staying in Industry* variable using the Census data and replicated the findings from the ASR sample. *Staying in Industry* takes the value 1 if the employee stays in the same six-digit NAICS industry when they move to a new firm and 0 if the employee takes a job with a firm outside of this industry.

Table AA1: Matched Census Sample OLS Regressions of Staying in Industry on Firm Failure

	Model 1	Model 2
Failure	0.3693***	0.3738***
	(0.0045)	(0.0046)
High Earner	0.1039***	0.1425***
	(0.0024)	(0.0033)
Failure × High Earner		-0.0588***
		(0.0045)
Female	0.0245***	0.0244***
	(0.0027)	(0.0027)
Age	0.0037***	0.0037***
	(0.0001)	(0.0001)
College	-0.0145***	-0.0144***
	(0.0011)	(0.0011)
Firm Size (ln)	0.0136***	0.0136***
	(0.0017)	(0.0017)
Constant	0.1241***	0.1221***
	(0.0119)	(0.0119)
Year FE	Y	Y
Prior Employer State FE	Y	Y
Prior Employer Industry FE	Y	Y

Note: Observations are worker moves, collected for all workers whose entire careers occurred in the following 25 states and DC: AR, CA, CO, DC, DE, GA, HI, IL, IN, IA, LA, ME, MD, MO, NV, OK, OR, PA, RI, SC, TN, TX, UT, VT, and WA. Workers who did not move firms are not included. Census disclosure rules mandate rounding of observations to the nearest 10,000 and the truncation of reported statistics to four significant figures. *Staying in Industry* takes the value 1 if the employee stays in the same six-digit NAICS industry when they move to a new firm and 0 if the employee takes a job with a firm outside of this industry. *Failure* takes the value of 1 if the employee is moving from a failed firm in that year, and ongoing is the reference category, which represents an employee moving from a firm still in operation. *High Earner* takes the value of 1 if the employee is in the top 5% of their firm's wage distribution and 0 otherwise. Models include fixed effects for year, employer state, and employer industry. Robust standard errors clustered at the firm level are in parentheses. Significance levels: $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Observations = 2,200,000.

Table AA2: Matched Census Sample OLS Regressions of Wages and Staying in Industry

	Wage diff. Model 1	Wage diff (5yr) Model 2	Wage diff. Model 3	Wage diff. Model 4
Staying in Industry	0.0114***	0.1160***	0.0147***	0.0150***
	(0.0015)	(0.0022)	(0.0017)	(0.0019)
Shrink			0.0031	
			(0.0027)	
Staying in Industry × Shrink			-0.0126***	
			(0.0034)	
Quarters in Industry				-0.0018**
				(0.0007)
Staying in Industry × Quarters in Industry				0.0016*
				(0.0007)
Female	-0.0026*	0.0165***	-0.0027**	-0.0026*
	(0.0010)	(0.0012)	(0.0010)	(0.0010)
Minority	0.0053***	0.0007	0.0054***	0.0052***
	(0.0011)	(0.0014)	(0.0011)	(0.0011)
Immigrant	0.0201***	0.0076***	0.0201***	0.0198***
	(0.0011)	(0.0014)	(0.0011)	(0.0011)
Age	-0.0032***	-0.0082***	-0.0032***	-0.0031***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Firm Size (ln)	0.0041***	0.0041***	0.0041***	0.0041***
	(0.0004)	(0.0006)	(0.0004)	(0.0004)
Unemployment	-0.0016**	0.0015*	-0.0016**	-0.0016**
	(0.0006)	(0.0007)	(0.0006)	(0.0006)
Move Lag (ln)	-0.0244***	0.1949***	-0.0244***	-0.0246***
	(0.0010)	(0.0013)	(0.0010)	(0.0010)
Constant	0.1163***	0.2020***	0.1153***	0.1158***
	(0.0053)	(0.0059)	(0.0054)	(0.0053)
Year FE	Y	Y	Y	Y
Prior Employer State FE	Y	Y	Y	Y
Prior Employer Industry FE	Y	Y	Y	Y
Observations	2,200,000	1,580,000	2,200,000	2,200,000

Note: Observations are worker moves, collected for all workers whose entire careers occurred in the following 25 states and DC: AR, CA, CO, DC, DE, GA, HI, IL, IN, IA, LA, ME, MD, MO, NV, OK, OR, PA, RI, SC, TN, TX, UT, VT, and WA. Workers who did not move firms are not included. Census disclosure rules mandate rounding of observations to the nearest 10,000 and the truncation of reported statistics to four significant figures. *Staying in Industry* takes the value 1 if the employee stays in the same six-digit NAICS industry when they move to a new firm and 0 if the employee takes a job with a firm outside of this industry. *Shrink* denotes industries that had at least a 10% smaller share of state-level employment vs. five years prior. *Quarters in Industry* is the number of quarters the employee has spent in the six-digit NAICS industry when they move to a new firm. Models include fixed effects for year, employer state, and employer industry. Robust standard errors clustered at the firm level are in parentheses. Significance levels: $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

B ASR SCANDALS

This section provides additional detail on the two ASR firms that failed due to scandal: Kurzweil Applied Intelligence (KAI) and Lernout and Hauspie Speech Products (LH), as well as descriptions of the sample used to analyze employees from failed firms to employees from these two firms. Both firms engaged in financial fraud when their speech recognition technology failed to perform as advertised, leading the companies to fall far short of sales forecasts.

KAI was founded in 1982 by Raymond Kurzweil, a prolific technologist and serial entrepreneur whose ventures had attracted capital from Harvard University's endowment and the venture arm of Xerox (Maremont 1996). Among the lofty goals of the Waltham, Massachusetts-based company was to create a voice-activated word processor for medical professionals that did not require users to pause after speaking each word, as was common in earlier dictation systems (Kurzweil 2000). However, the technology did not work as well as promised, which hurt sales and threatened the company's chances of completing an IPO.

CEO Bernard Bradstreet had joined KAI as CFO, then became co-CEO with Kurzweil, and ultimately assumed responsibility for company operations as sole CEO. At first, Bradstreet and other executives made seemingly minor adjustments to sales reports, for instance by booking as revenue some contracts that were in the final stages of negotiation but not yet signed. By the time of its August 1993 IPO, however, the company had booked millions of dollars in non-existent sales in order to be able to report itself as profitable (Maremont 1996). Documents were forged to fool auditors, and software supposedly sold to customers was shipped to a local warehouse. Bradstreet was convicted in May 1996 of masterminding the fraud and was sentenced to jail. KAI soon failed.

Jo Lernout and Pol Hauspie founded LH in 1987. By the 1990s the Belgian company had become the largest firm in the ASR industry, reporting revenue growth remarkable enough to draw scrutiny from investigative journalists. However, the firm's financials were not available to the SEC until LH acquired the US-based firm Dictaphone in 2000. The newly-transparent financials revealed that sales in Singapore and South Korea had skyrocketed to $143.2 million from less than $300,000 in a single year. Moreover, the bulk of sales were to 30 companies, many of which shared the same office address. Wall Street Journal reporters found that several of those companies claimed never to have done business with LH (Maremont, Eisinger, and Song 2000).

LH CEO Gastion Bastiens stepped down shortly after the investigative article was published, and the SEC launched an audit of LH. Following the audit, LH restated earnings since 1998, and the founders stepped down as co-chairmen. Trading of its stock was suspended, and by the end of 2000 the company failed. Founders Hauspie and Lernout were eventually sentenced to five years imprisonment for securities fraud.

Appendix Table BA3 contains descriptive statistics for the sample composed of LH, KAI, and a set of failed ASR firms that were matched to LH and KAI using Coarsened Exact Matching.

Table BA3: Descriptive Statistics for ASR Firms with Scandal

Variables	N	Mean	SD	Min	Max
Staying in Industry	633	0.186	0.390	0.000	1.000
Scandal	633	0.608	0.489	0.000	1.000
Executive	633	0.047	0.213	0.000	1.000
Engineer	633	0.476	0.500	0.000	1.000
Female	633	0.182	0.386	0.000	1.000
Employee Age	633	27.367	6.567	21.000	60.000

Note: Unit of analysis is an employee's move to a new firm. *Staying in Industry* takes the value 1 if the employee moves to an ASR firm and 0 if the employee takes a job with a firm outside of the ASR industry. *Scandal* takes the value of 1 if the employee is moving from a failed firm with an associated scandal, where there are two instances of firms failing due to scandal, in that year. *Failure* without an associated scandal is the reference category. *Engineer* takes the value of 1 if the employee's title indicates that they held an engineering or science-related role (e.g., software engineer, computational linguist) and 0 otherwise.