Three Essays on Labor Market Mobility and Inequality

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Abstract

This dissertation consists of three essays on labor market mobility and inequality. The first chapter investigates the compensation outcomes of cross-industry mobility of S&P 1500 executives. In this study, we propose a theoretical framework by conceptualizing industries as social categories and identify industry affiliation with categorical membership. Besides finding an industry-changing discount in executives' external job mobility, we show that executives trade off part of their compensation premium to move to a higher-status (more prestigious) social category. The second dissertation chapter examines the effects of crisis-related wage cuts on employee turnover. Drawing on social comparison and relative deprivation theories, we hypothesize that, in periods of crisis, if organizations deviate from prevalent wage-cutting measures implemented by external referents, their employees are more likely to consider alternatives and leave the firms. We also theorize and document the differences in the quitting tendencies of high- vs. low-skilled workers and their destination firms, unveiling the effects of crisis-related wage cuts on individual careers and the inter-firm flow of human capital. In the last chapter, we examine the association between temporary employment and wage inequality. We argue that a disproportionate high concentration of temporary employment in the bottom of wage distribution leads to rent destruction in low-wage jobs, shifting rent allocation vertically from low to high earners and thus widening wage inequality. Together, these three studies (a) advance the literature on career mobility and the role of social evaluations in mobility and labor market inequalities, (b) provide empirical contributions by compiling large-scale datasets (e.g., a novel dataset on career and wage trajectories of a representative sample of the Spanish labor market, industry similarity matrices based on web crawling data and text parsing algorithms), and (c) contribute to broader questions such as how individuals weigh non-pecuniary rewards in career decisions and how labor market inequalities intensify in the aftermath of an economic crisis.

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1. Introduction

Work and employment relationships have transformed considerably in recent years, shaped by greater external job mobility (Bidwell & Briscoe, 2010), fewer permanent job relationships (Cappelli & Keller, 2013), and an uptick in temporary employment and flexible workforce practices (Bidwell, Briscoe, Fernandez-Mateo, & Sterling, 2013; Kalleberg, 2011). The coronavirus pandemic has only amplified this state of flux, increasing labor market uncertainties and accelerating the expansion of remote work. These shifts raise new theoretical and empirical questions about the role of human capital in creating competitive advantage, the evolution of labor markets and future of work, and the monumental challenge of pursuing inclusive and sustainable economic growth while providing productive employment and decent work for all.

This dissertation focuses on these important shifts and emerging patterns in work and employment, with an emphasis on the sociology of labor markets and interactions among organizations, workers and social structures like social networks and status hierarchies. Empirically, my approach relies on quantitative methods to leverage novel data on career and wage trajectories and conduct econometric analyses, primarily on large-scale linked employer-employee datasets from organizational contexts in the United States and Europe. Specifically, the essays in the dissertation feature two interconnected research streams: (1) social evaluations, career mobility, and strategic human capital, and (2) labor markets and inequality.

As part of the first stream examining the role of social evaluations in job-candidate matching processes and career mobility, **in the first chapter,** we examine cross-category movements in labor markets. We concur on the existence of the longstanding consensus among organizational scholars and economic sociologists that crossing categorical boundaries in labor markets results in lesser rewards than remaining in the same category (Ferguson & Hasan, 2013; Leung, 2014; Zuckerman, Kim, Ukanwa, & Von Rittmann, 2003). We set forth two unaddressed issues in this theoretical conundrum. First, if category-spanning discounts were universally salient and category spanners were consistently rewarded less than their equally qualified peers with consistent category affiliations, job markets should reach an equilibrium between recruiters and candidates that would eliminate cross-category movements over the long term. Paradoxically, we

observe persistent employee mobility across social categories, including across industries (Graham, Kim, & Kim, 2020). In short, previous accounts do not provide a compelling explanation as to why individuals continue to span labor market categories despite penalties.

Second, prior research largely assumes uniform returns and only partially explains category-spanning outcomes. While increasing evidence exists on audience- and candidate-side characteristics that determine the discount (Cudennec & Durand, 2022; Kacperczyk & Younkin, 2017; Merluzzi & Phillips, 2016; Pontikes, 2012), we lack an understanding of how the vertical hierarchy of categories themselves impact the categorial imperative.

We propose new theory on candidates' motivation to straddle categories despite the associated penalties and the unexplained variation observed in returns to spanning labor market categories. Specifically, we theorize on how category spanning influences social evaluations in relation to category status. To test our arguments, we identify industry affiliation with categorical membership of executives, focus on voluntary executive mobility across industries, and explore how crossing industry boundaries and industry status differences jointly affect compensation outcomes. Analyses with S&P 1500 executives' mobility and compensation patterns (1994-2019) -based on industry prestige rankings- provide consistent support for our theoretical predictions. Economic theories provide skill-, quality-, and performance-based explanations of executive compensation differentials, which we expand upon by considering the tradeoffs executives accept to work in particular types of businesses deemed more favorably by the public over low-status or stigmatized industries. This study makes unique contributions to social evaluation and categorization literatures, while enriching those on career mobility and executive compensation.

In the second chapter, the focus on employee mobility turns to analyzing the effects of crisis-related wage cuts on voluntary employee turnover. Drawing on pay-equity-perception insights derived from social comparison and relative deprivation theories, we hypothesize and find evidence that, in periods of crisis, workers determine the fairness of their wage cut by observing the responses in similar organizations presumably experiencing similar challenges. If employees observe their organization

deviates from prevalent cost-cutting measures, they are more likely to consider alternatives and leave their firms.

In parallel, we theorize on and document the differences in the quitting tendencies of high- vs. low-skilled workers and their destination firms (e.g., whether quitters remain in the same industry by joining a competitor or launching an entrepreneurial venture in the same industry), unveiling the effects of crisis-related wage cuts on individual careers and the inter-firm flow of human capital during and after post-crisis years. By examining how employees' exit decisions are affected by the wage changes of external referents, this chapter advances the literature at the nexus of social comparison theory and the unfolding model of turnover. Besides the theoretical contributions, this study provides practical insights for managers and policy makers in light of their recent actions to respond to the intricacies of Covid-19 crisis. While the parameters of the Covid-19 crisis vary significantly from those of the 2008 financial downturn, our research offers important and timely insights about the potential impact of organizations' wage-cutting decisions during the coronavirus pandemic.

The second research stream included in the dissertation, also centers on work and employment relationships, exploring how organizational processes (specifically employment practices) create and aggravate labor market inequalities. For this research, we use a large-scale administrative linked employer-employee dataset and explore the links between individuals and organizations, and among organizations and institutional contexts that lead to widening inequalities. As part of this stream, **in the third chapter,** we examine the association between temporary employment and wage inequality in Spain by analyzing the distribution of temporary employment across different wage layers. We argue that a disproportionately high concentration of temporary employment in the bottom of wage distribution leads to rent destruction in low-wage jobs, shifting rent allocation vertically from low to high earners and thus widening wage inequality.

In this way, we answer recent calls for examining how organizational practices can contribute to, institutionalize, and reproduce inequalities at the societal level beyond intra-organizational level (Amis, Mair, & Munir, 2020; Bapuji, Ertug, Patel, & Allen, 2020), as well as for multi-disciplinary examinations of the rise of non-standard employment practices and its implications on income inequality (Bidwell et al., 2013;

Cobb, 2016; Sørensen & Sorenson, 2007). To explore the effect of temporary employment rates on wage inequality and conduct a granular analysis of the hypothesized mechanisms, we take advantage of the uniquely rich information afforded by combining datasets from administrative Spanish Social Security and Tax Records, along with regional data from the Spanish Statistical Office (INE). Our results suggest that a high ratio of temporary employment filled by low-paying jobs is positively associated with within-industry wage inequality.

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2. Chapter 1: Spanning Labor Market Categories with Status Differences: The Joint Effects of Cross-industry Mobility and Industry Prestige on Executive Compensation

Abstract

Research cautions against crossing categorical boundaries in labor markets since consistent category affiliations obtain relatively higher rewards. Despite the expected discount, individuals continue to span labor-market categories, while their motivations and the factors behind the discounting variation remain unclear. Our study presents new theory to address these questions and suggests the discount depends on status differences between the spanned categories. We first specify the baseline mechanisms underlying the category-spanning discount: on the audience side, ambiguity and future commitment concerns, and on the candidate side, individuals' preference for obtaining experience in different domains. We further hypothesize that coming from a high-(low-) status category should weaken (strengthen) these demand- and supply-side mechanisms. To test our hypotheses, we identify industry affiliation with category membership and examine S&P 1500 executives' mobility and compensation patterns (1994-2019) based on industry prestige rankings. As predicted, we discover that industry-changing executives realize lower compensation gains than those who do not change industries, and greater discounts if they shift from a low- to high-status industry (status leap) than the reverse (status leak). Our study advances category and status theories by presenting an agentic view of category spanners' status-seeking motivation and category status as a determinant of the spanning discount.

2.1. Introduction

According to extant research on classification systems, audiences devalue offerings that span social categories in favor of typical offerings that hew tightly to categorical boundaries (Bowers, 2015; Durand & Paolella, 2013). For labor markets, abundant theoretical accounts and empirical evidence show the advantages of remaining within a single category rather than straddling categories (Ferguson & Hasan, 2013; Zuckerman, Kim, Ukanwa, & Von Rittmann, 2003). Conceptually, this categorical imperative is often predicated on audiences' confusion when evaluating category spanners or on candidates' inability to effectively target multiple audiences (Hsu,

Hannan, & Koçak, 2009). With regard to mechanisms driving the category-spanning discount, research shows that perceptual factors beget an evaluative discount regardless of quality differences between category spanners and others (Negro & Leung, 2013).

This disciplining perspective leaves two key issues unanswered. First, if category-spanning discounts were universally salient and category spanners were consistently rewarded less than equally qualified peers, job markets should reach an equilibrium between recruiters and candidates that would eliminate cross-category movements over the long term. Paradoxically, we observe persistent employee mobility across social categories, including across industries (Graham, Kim, & Kim, 2020). In this way, prior research does not offer a compelling explanation for individuals' motivation to span labor market categories despite penalties. Moreover, it largely assumes uniform returns and offers a partial justification for the variation in category-spanning outcomes. While there is growing evidence on how audience- and candidate-side characteristics determine the discount, less clear are the impacts of qualitative differences among categories. For these reasons, a comprehensive theory is needed to explain (a) candidates' motivation to change categories despite recognition that their actions will be discounted, (b) the unexplained variation in category-spanning penalization, and (c) the likely interrelatedness between these two considerations.

To address these issues, our framework extends category theory by examining spanners' motivations to cross categorical boundaries (i.e., the candidate side) and how status differences among categories moderate both candidate and audience's concerns underlying the evaluative discount. Before clarifying how this two-pronged status extension refines our theoretical understanding of category spanning in labor markets, we specify the audience-side mechanisms behind the evaluative discount and the supply-side considerations missing in previous accounts. On the demand side, we draw on ambiguity and future commitment concerns. Ambiguity refers to the complications in audiences' technical assessments regarding the suitability of a category spanner's attributes in a new context (Ruef & Patterson, 2009), while future commitment concerns refer to audiences' normative assessments vis-à-vis spanners' flight risks and willingness to contribute over time (Leung, 2014). On the supply side, we outline factors that might lead candidates to accept lesser pecuniary rewards. We draw on economic models (Gayle, Golan, & Miller, 2015) and experimental studies in the human capital literature (Antonovics & Golan,

2012) suggesting that candidates ascribe value to gaining experience in different work environments to expand their expertise and social ties, as well as studies on individuals' acceptance of lower salaries in return for non-pecuniary rewards (Fang & Tilcsik, 2022; Hu & Hirsh, 2017). Hence, we first hypothesize that straddling categories is accompanied with lesser immediate monetary returns.

Building on the status extension, we further hypothesize that the status of labor-market categories, defined as a superior rank in the social order (Prato & Ferraro, 2018), moderates both the demand- and supply-side mechanisms. For status leaks (when the candidate's origin has a higher status than the destination category), on the demand side we expect precedence from a high-status category to attenuate ambiguity, as high levels of categorical status positively affect judgments of deviant actions (Montauti & Wezel, 2016) based on spanners' greater discretion to cross categories (Kodeih, Bouchikhi, & Gauthier, 2019). Similarly, a high-status category affiliation could mitigate commitment concerns since audiences give these candidates the benefit of the doubt (Phillips, Turco, & Zuckerman, 2013; Sgourev & Althuizen, 2014), resulting in more favorable assessments of spanners' flight risk. On the supply side, since high status affiliations augment future potential rewards (Piazza & Castellucci, 2014), candidates should be less eager to renounce a high-status affiliation. Consequently, a status leak will decrease the expected utility value of a new experience if it comes with a lower-status affiliation.

Conversely, we expect status leap (a move from a low- to a high-status category) to reinforce the baseline mechanisms. On the demand side, audiences evaluate candidates in low-status positions more severely in spite of their professional competence (Kim & King, 2014) and associate low-status categories with lower levels of commitment (Campbell & Hahl, 2022; Ridgeway, 1981), suggesting a positive moderation of ambiguity and commitment concerns. On the supply side, we explain how status leap increases candidates' ascribed value of work experience in a new domain and in turn, amplifies the baseline supply-side mechanism. We ground our logic in high-status affiliations' ability to deliver emotional and moral satisfaction (Pollock, Lashley, Rindova, & Han, 2019) and in peer and market attention promising potential future rewards (Gould, 2002). In consequence, we hypothesize that categorical status moderates the category-spanning discount: if an individual moves to a higher-status category (status

leap), the discount should be greater than that experienced by someone moving to a lower-status labor market category (status *leak*).

To test our hypotheses, we identify executives' industry affiliation with categorical membership and their industry changing with category spanning. Following our agentic approach, we use information on voluntary executive mobility across S&P 1500 companies, executives' compensation patterns and data on industry prestige. Employing a matched-sample design, we find industry-changing executives realize lower compensation gains than closely matched ones who do not change industries, whether within-industry movers or comparable stayers who have never changed jobs. As expected, the compensation discount is large for executives who move to a higher-status industry (*leapers*), while minimal for those who move to lower-status industries (*leakers*). Supplementary analyses on the breakdown of compensation differences, individual-, firm- and board-level parameters, and potential confounding effects of industry relatedness (based on novel operationalizations of industry similarity) negate several alternative performance and skill-transferability explanations, while corroborating our proposed social-evaluation mechanisms.

This research offers important theoretical contributions to category and status theories, as well as empirical and theoretical advancements to executive mobility work. Counter to accounts portraying category spanners as passive recipients of an evaluative discount, we conceptualize spanners as strategically motivated agents seeking status advancement. We thus introduce category status as a novel moderator explaining why cross-category mobility could be more or less discounted based on the status of the category in which a candidate is embedded. Adding to prior research studying attributes of categories themselves (Cudennec & Durand, 2022; Kovács & Hannan, 2015; Pontikes, 2012), our theory recalibrates the excessive emphasis on a uniform disciplining effect of categories (Durand & Paolella, 2013), and conceptualizes categories as prestige-laden evaluative lenses impacting candidate- and- audience-side evaluations, rather than sieves used to screen out misfits (Delmestri & Greenwood, 2016; Sharkey, 2014). While providing a more nuanced theoretical understanding of the categorical imperative in labor markets, our research introduces high-status industry affiliation as a novel non-pecuniary reward, advancing work on status attainment (Bidwell, Won, Barbulescu, & Mollick, 2015; Rider & Tan, 2015).

2.2. Theoretical Background

2.2.1. Category Spanning and Status in the Labor Market

Defined as "interfaces of cognitive agreement" (Durand & Thornton, 2018: 632) regarding an object or social actor, categories help audiences parse and grasp market offerings (Hannan, 2010; Leahey, Beckman, & Stanko, 2017). Straddling these cognitive schemes results in an illegitimacy discount (Zuckerman, 1999). Organizational scholars who study labor markets present similar predictions about the disciplining effect of labor market classifications, which consistently confirm the benefits of signaling a single professional identity versus spanning labor market categories[1] (Leung, 2014; Zuckerman et al., 2003). Research has increasingly questioned these absolute categorical imperatives and introduced boundary conditions, which include career stage and position (Custódio, Ferreira, & Matos, 2013; Ferguson & Hasan, 2013), complexity of tasks (Leung, 2014), contextual factors such as salience of screening processes (Merluzzi & Phillips, 2016), and the audience's level of expertise (Cudennec & Durand, 2022) and orientation (Kacperczyk & Younkin, 2017; Pontikes, 2012). We concur that employers typically afford advantages to individuals who remain within specific labor market categories.

The literature connecting the sociological notion of labor market identity to categorical affiliation and the disadvantages of diluting this affiliation draws on Spence's (1973) signaling theory. Recruiters rely on signals, defined as the "observable characteristics providing information about unobservable attributes" (Bergh, Peruffo, Chiu, Connelly, & Hitt, 2020: 549) in order to reduce uncertainty and adverse selection risks inherent in employee selection and evaluation processes. To this end, a candidate's industry affiliation serves as a readily accessible signal, a cue that helps recruiters categorize and better understand the candidate beyond firm- or individual-level characteristics. Social evaluation research underscores the relevance of these signals, particularly for shaping audiences' perceptions of candidates' human capital (Leung & Sharkey, 2014) and commitment (Leung, 2014) regardless of their quality and performance differences.

[1] Industries are thought to serve as categories that provide cognitive boundaries around and between clusters of social actors, be they firms or individuals, that share a "family resemblance" (Rosch & Mervis, 1975). We follow the studies equating industry boundaries with such discrete categories; and thus use industry and category interchangeably or complementarily (e.g., Georgallis, Dowell, & Durand, 2019).

Status has a similar effect on assessments of both recruiters and candidates: although status and observable quality are loosely coupled (Simcoe & Waguespack, 2011), differences in status or prestige sway audiences' assessments (Piazza & Castellucci, 2014). As an example, Rider and Tan (2015) show that organizations prefer to hire individuals from higher-status over lower-status competitors. On the candidate side, employees typically value employer status as a non-pecuniary employment benefit (Bidwell et al., 2015). In terms of status effects on the category-spanning discount, social evaluation research has primarily focused on the candidate's or audience's status. Durand and Kremp (2016), for instance, show that although middle-status actors benefit from conforming to established categories, high- and low-status actors are relatively immune to the category-spanning discount. This line of research on individual and organizational status has not considered the status of the categories themselves nor how the status of labor market categories specifically affects category-spanning discounts.

2.2.2. Industry Categories and the Executive Mobility-Compensation Relationship

Executive markets differ from other labor markets due to "variations in job demands, discretion, liability, stability, and visibility" (Andrus, Withers, Courtright, & Boivie, 2019: 1153), yet both CEO and non-CEO hiring processes are subject to uncertainty and decision-making heuristics (Boivie, Graffin, Oliver, & Withers, 2016; Khurana, 2002). In fact, skill and quality assessments are especially challenging when selecting and compensating top-level executives (Graffin, Boivie, & Carpenter, 2013), which is why social psychological approaches to executive mobility and compensation have complemented economic perspectives by examining how recruiters use diverse signals to ascribe value to candidates. These signals include directorships in other organizations (Boivie et al., 2016), relative salary standings (Seo, Gamache, Devers, & Carpenter, 2015), compensation disparities among executives (Essman, Schepker, Nyberg, & Ray, 2021), board characteristics in executives' former firms (Zhu & Shen, 2016), and the status of firms and executives (Focke, Maug, & Niessen-Ruenzi, 2017).

Overall, theoretical accounts and empirical evidence abound on how heterogeneity in individual-, board- and firm-level characteristics can directly or indirectly inform variation in executive compensation (see a review by Cannella,

Hambrick, & Finkelstein, 2009). Despite the increased prevalence of executives moving across industries (Graham et al., 2020), neither economic theories nor social psychological approaches to mobility explicitly address the impact of crossing industry boundaries or the differences in industry prestige on executive compensation.

One exception is the vibrant research stream in the generalist-versus-specialist debate. In this realm, several studies assert the importance of the diversity of industry categories in executives' career history (Custódio et al., 2013; Hu & Liu, 2015), but the effects of industry change in compensation have not been clearly isolated. Theoretically, it is often implicitly or explicitly assumed that industry-changing top executives possess generalist skills, while within-industry movers are specialists (Brockman, Lee, & Salas, 2016). On a similar note, from an empirical viewpoint, multiple parameters (e.g., number of positions and firms, professional experience in conglomerates) have been used to operationalize the accumulation of generalist human capital, none of which allows distinguishing the net effect of a particular industry change. These limitations impede teasing out demand- and supply-side considerations when switching industry categories and the associated compensation-setting processes in the executive job market.

Besides not isolating the effect of a specific industry change, existing work ignores heterogeneity in characteristics of the industries involved in executives' category spanning. Research has explored the role of executive and firm status in executive compensation variation (Belliveau, O'Reilly, & Wade, 1996; Graffin, Wade, Porac, & McNamee, 2008). A key tenet of our framework is that industry categories also differ in status, an important desirability indicator especially in voluntary departures. Accordingly, both demand- and supply-side judgments should also reflect industry-status considerations, beyond individual and organizational factors.

2.3. Hypothesis Development

To better understand how audiences assess candidates' crossing of industry-category boundaries and why candidates might opt to straddle these labor market categories, we consider both supply- and demand-side evaluations related to changing industry category, combined with industry-status effects on such evaluations. Before examining status-related considerations, we first explain why we use executives' industry affiliation as a categorization lens and identify the specific mechanisms behind a

category-spanning penalty in executive job markets. We then proceed with the category-status extension, proposing that status leaks weaken these baseline mechanisms, whereas status leaps strengthen them.

2.3.1. Changing Industry-Category Affiliation in the Executive Labor Market

The reviewed literature on categories and executive mobility does not explicitly use industry affiliation as a categorization factor in executive labor markets, yet industry lines appear to cluster executives. By illustration, from a performative viewpoint, an executive who works for Shell Corporation is not only labeled as a "Shell executive" but categorized as an "oil and gas executive." As implied in this example, categories contribute to meaning creation in job-candidate matching processes, and social actors derive part of their social identity from membership in an industry category or a socially legitimated grouping of similar organizations (Sharkey, 2014).

These theoretical grounds conceptualize industry affiliation as a categorization lens, and also in practice, intermediaries in executive-selection processes generally rely upon industry boundaries. For instance, executive search firms–instrumental in executive selection and evaluation–(Bonet, Cappelli, & Hamori, 2013; Cappelli & Hamori, 2014) often organize their activity by industry. In some cases, headhunters focus on specific industries or sectors, such as education and energy. By the same token, consulting firms regularly conduct industry-level executive surveys (e.g., PwC's global executive surveys on talent flows in the oil and gas industry, Deloitte's "Talent" series, focused on talent priorities based on executives' industry affiliations). These practices suggest the existence of important cognitive boundaries among industries in executive labor markets, as well as the maintenance and reproduction of shared meaning and effectiveness of these boundaries by influential market actors such as headhunters, consulting firms and media outlets.

Cross-industry moving executives who span such cognitive boundaries can be perceived as category spanners because of their simultaneous memberships in two or more categories positioned on the same level of the classification hierarchy (Vergne & Wry, 2014). The following section sheds light on compensation outcomes for industry-

changing executives by analyzing the demand- and supply-side explanatory mechanisms motivating the category-spanning discount.

Demand-side (Audience) Considerations in Industry Changes

On the demand side, we anticipate two mechanisms through which changing industry category affects recruiters' evaluations: ambiguity and future commitment concerns. Ambiguity arises from cognitive difficulties in determining which characteristics of a category spanner apply in a particular context. In other words, if an executive can be mapped to more than one category, one of which cannot be understood without additional contextual information, the actor's identity is perceived as ambiguous, resulting in devaluation (Abbott, 1997; Ruef & Patterson, 2009).

It could be argued that atypical executives would be filtered out in early selection stages (Fernandez-Mateo & King, 2011) and that information on top executives is plentiful, hence, it is not reasonable to expect salient quality differences between cross-industry movers and comparable peers, all of whom underwent elaborate screening and evaluation processes. However, the audience-side argument suggests that the discount occurs because category spanners are perceived as more difficult to position, understand and evaluate (Hsu et al., 2009). Ambiguity does not only arise from differences in actual qualities: the offerings of category-spanning actors can still be ambiguous and subsequently discounted even if quality is controlled for (Leung & Sharkey, 2014).

Ambiguity issues relate to cognitive difficulties and limitations that undermine the identification and assessment of category spanners' presumably measurable attributes and their applicability in new domains (Boulongne & Durand, 2021). In addition to complications in technical assessments, recent research suggests erratic career movements also hinder normative evaluations regarding social actors' purpose and future commitment (Galperin, Hahl, Sterling, & Guo, 2020; Leung, 2014). From the demand side, industry-changing executives might be viewed as lacking future commitment, besides provoking perceptions of ambiguity.

Future commitment refers to perceptions of candidates' "willingness to apply their capabilities to benefit the firm over the long term" (Campbell & Hahl, 2022: 3) or a sufficiently long time period (Galperin et al., 2020). Hence, commitment addresses a

hire's future cumulative organizational contribution over the course of their employment. In any labor market, recruiters who intend to form long-term employment relationships with job candidates evaluate not only their qualities and performance, but their potential commitment and flight risks (Galperin et al., 2020). The cost of replacing an executive ranges from 90 to 200% of their departing salary (Andrus et al., 2019), so commitment-related concerns are particularly salient in executive labor markets.

Uncertainty and costly adverse selection threats emerge since employers a priori cannot easily foresee candidates' future commitment. For this reason, recruiters rely on various signals to infer this information (Spence, 1973), including the ordered choice of jobs an employee accumulates (Leung, 2014). Moreover, studies show that commitment perceptions are shaped by both employees' tenure in their organization as well as the *type* of business (Galperin et al., 2020; Leung, 2014). Consequently, while executives' industry tenure might assuage recruiters' commitment doubts, the decision to change industries might give rise to skepticism. In sum, beyond ambiguity questions, changing industry can raise commitment concerns, resulting in a discount on the evaluations of industry-changing executives.

Supply-side (Candidate) Considerations in Industry Changes

On the supply-side, we expect executives to ascribe value to an experience in a new domain, weighing potential future career benefits and non-pecuniary rewards when changing industries. Although we conceptually restrict our analysis to a particular category change and the first industry change, empirically, executives' career decisions typically include long-term horizons when changing firms and industries. Subsequently, an immediate compensation change is not the only determinant in their decision to change firms and industries. Executives may trade off part of their immediate compensation premium for future monetary gains and other non-pecuniary rewards. Executives probably expect a lower compensation premium when moving to another industry, yet might still value the opportunity to diversify their skill set and social ties and/or enjoy other non-financial rewards, such as increased prestige (Focke et al., 2017).

Indeed, research suggests accumulated experience in multiple firms and industries pays off, especially in later career stages. For instance, Hu and Liu (2015) find that CEOs accrue diverse social connections across employers and time, which mitigates

information asymmetry and boosts employing firms' odds of accessing external investment funds. These social-connection assets and the associated advantages for firms generate future compensation increases for executives, corroborating the generalist premium in later career stages (Custódio et al., 2013). Similarly, economic studies suggest that procuring experience in different work environments is statistically significant and quantitatively important, motivating younger executives in particular to work in different industries (Gayle et al., 2015). Experimental studies in the human capital literature confirm similar predictions (Antonovics & Golan, 2012).

Taken together, when moving across industries, we expect demand-side ambiguity concerns to complicate recruiters' cognitive identification and evaluation of executive competences. In addition, spanning across industries will activate recruiters' normative judgments about executives' commitment and exit risks, further limiting potential compensation gains. On the supply side, we expect executives to ascribe value to experience in different industries. Our baseline hypothesis proposes that compensation outcomes of industry-changing executives reflect the aforementioned considerations:

> ***Hypothesis 1.*** *Ceteris paribus, the average compensation gain for executives who move across industries is smaller than that of executives who do not change industries.*

2.3.2. The Moderating Effect of Industry Status

Our approach to understanding the evaluation of category-spanning executives essentially focuses on the choices made by executives on the supply side, their assessment on the demand side and how the combination thereof impacts compensation outcomes. Monetary rewards, i.e., rational socio-cognitive content, constitute the basis of these decisions. However, an important aspect of executives' choices and recruiters' evaluations is status, which encompasses both moral and emotional socio-cognitive matters and rational content (Pollock et al., 2019; Wowak, Gomez-Mejia, & Steinbach, 2017).

Status is defined as "socially constructed, intersubjectively agreed-upon and accepted ordering or ranking of individuals, groups, organizations, or activities in a social system" (Washington & Zajac, 2005: 284). The status of an individual, firm or industry

is "the prestige accorded because of the abstract positions rather than because of immediately observable behavior"[2] (Gould, 2002: 1147). Status systems are "directly tied to the pattern of relations and affiliations in which the actor does and does not choose to engage" (Podolny, 2005: 13), such that social actors seek high-status affiliations (Piazza & Castellucci, 2014). Relevant for our research questions, status has also been shown to impact category-spanning outcomes, i.e., high-status actors are relatively immune to the category-spanning discount, rendering conformity incentives less valuable for them (Sgourev & Althuizen, 2014).

Categorical status–i.e., the social standing of a particular type of business–should also affect category-spanning outcomes since it influences the assessment of social actors embedded in that category (Sharkey, 2014). Categorical status derives not from how well an organization performs but, fundamentally, from who or what an organization is. That is, some industries attract more social deference, i.e., they have higher status, because the type of economic activity is aligned with broadly held social values, whereas other activities violate these norms and have relatively lower status (Pollock et al., 2019). These differences in status or prestige influence the desire of other actors to affiliate with a concrete industry, as well as the assessment of actors affiliated with said industry (Han & Pollock, 2021).

If executives derive part of their social identity from their membership in an industry category and industry categories are laden with varying status levels that also impact category-spanning outcomes, the compensation outcomes of cross-industry executive mobility should also reflect these category (industry) status perceptions. On the demand side, given that category status affects audiences' judgments, cross-industry mobility can be interpreted as better or worse simply due to the status of the industry to which an executive belongs. On the supply side, when candidates make decisions about

[2] Note the important distinction between two different forms of social evaluation: industry status and reputation. Unlike the closely related economic concept of reputation, which is often predicated solely on past performance, status also relates to social deference and privilege, which are grounded in the value systems underlying status hierarchies (Piazza & Castellucci, 2014; Sorenson, 2014). Pollock et al. (2019) address this distinction by highlighting moral and emotional evaluations, which are relatively more salient in status perceptions. These moral and emotional considerations affect social actors' affiliation choices. For this reason, status is generally defined in terms of associations with prominent others (Sorenson, 2014), whereas reputation is defined as fulfilling observer expectations of performance, i.e., whether a firm or industry is "known for something" (Blagoeva, Kavusan, & Jansen, 2020; Lange, Lee, & Dai, 2011). We focus on status because our primary interest is the desired affiliation patterns in voluntary moves and how social deference underlying the status systems impact evaluations on the demand and supply sides.

changing industry category, they are aware of the value of affiliation with a high-status industry and adjust their reward expectations accordingly. Thus, industry status should affect the strength of our baseline mechanisms.

Status Leaks: Demand-side Considerations

On the demand side, we argue that concerns about ambiguity and commitment– the mechanisms underlying the industry changing discount in our baseline hypothesis– could be attenuated if an executive proceeds from a high-status industry. One important inference from the status literature is that status regulates audiences' perception such that high-status affiliations can generally help new entrants gain legitimacy[3] (Piazza, Phillips, & Castellucci, 2020). When there is ambiguity about a candidate's quality or appropriateness, observers fall back on previously held beliefs to reduce evaluative uncertainty and minimize adverse selection risks (McDonnell & King, 2018). Category status matters in such evaluations because it "immediately and automatically affects whether a social actor is viewed as worthy of deference" (Sharkey, 2014: 1387). Hence, the status of the category should mitigate the consequences of deviant actions and act as a "stabilizing force" so that actors embedded in high-status categories are ascribed less ambiguity and more discretion to cross categories (Kodeih et al., 2019; Phillips et al., 2013). In our case, we expect the ambiguity prompted by changing industry to be interpreted in a more favorable light if an executive comes from a high-status industry.

Regarding commitment issues, stakeholders are less critical of deviant actions from social actors who are embedded in high-status categories, giving them the "benefit of the doubt" (Phillips et al., 2013). These halo effects are well documented across multiple contexts for different social evaluations, including the status effect in the evaluation of accounting restatements (Sharkey, 2014) and employment discrimination (McDonnell & King, 2018), as well as reputation effects in investor reactions to M&A deals (Blagoeva, Kavusan, & Jansen, 2020) and earnings surprises (Pfarrer, Pollock, & Rindova, 2010). Extending this logic, we propose that affiliation with a high-status

[3] Under certain circumstances, having a high-status affiliation can also have a downside as it may shift audience attention from the focal actor to the higher status affiliate (e.g., Piazza et al., 2020; Reschke, Azoulay, & Stuart, 2018). In our case, the salience of diminished audience attention arguments is alleviated, because the status spillover derives not from a high-status affiliate with whom an executive competes for audience attention but from a prestigious industry affiliation. For a comprehensive account of negative effects of high-status alters, please see Piazza et al. (2020).

industry can also mitigate skepticism about cross-industry movers' commitment, favorably affecting audiences' judgments.

Status Leaks: Supply-side Considerations

Industry status could also affect candidates' social evaluation judgments, predisposing them to prefer jobs in higher-status industries. Although high status is often accompanied by financial rewards in the long term, the desire to retain a high-status affiliation is not solely motivated by immediate monetary gains. Future monetary gains, in addition to moral and emotional considerations, also have an impact on social actors' behaviors. The ranking of industries in a hierarchy reflects these moral considerations and manifests social values. In the context of executive mobility, we expect social actors to also assign value to prestigious *types* of work pursuant to whether an industry and type of economic activity "meet, exemplify, or violate a broadly held set of values or norms within a society or social group" (Pollock et al., 2019: 449). These moral perceptions of industries, as well as potential future monetary rewards brought by high-status affiliations, should enter the utility functions of executives and determine the equilibrium point in the compensation setting. Thus, the ascribed value to this new experience will not be high if an executive moves from a high- to a low-status industry.

We build our industry status argument and the moderating effect of affiliation with a higher status category on spanning discounts on three main premises. First, like organizations, executives can acquire status by virtue of their membership in salient status-ranked social categories (industries). Second, affiliation with a high-status category can mitigate both ambiguity and commitment concerns that underlie the industry-switching discount on the demand side. Third, because association with a higher-status industry might constitute a benefit in and of itself, the supply-side mechanism will be weaker for executives embedded in a high-status category, i.e., executives might be less eager to change industries when the move is from a high- to low-status industry. Thus, we expect status leaks to diminish the negative effect of industry switching on compensation.

> ***Hypothesis 2.*** *If an executive moves to a lower-status industry than his or her current one (status leak), the negative effect of industry switching on compensation gain is attenuated.*

Status Leaps: Demand-side Considerations

Conversely, we expect status leaps to strengthen both demand- and supply-side baseline mechanisms, i.e., the moderation effect will be reversed if an executive moves from a low- to a high-status category. On the demand-side, both ambiguity and commitment mechanisms hinge on the assumption that crossing labor market categories raises audiences' concerns on candidates' deviating from the "ideal worker" standard (Acker, 1990; Dumas & Sanchez-Burks, 2015), leading to an evaluative discount. Audiences evaluate candidates in low-status positions more strictly, even when they are sufficiently competent for a job (Kim & King, 2014). Specifically relevant to our industry category context, evidence suggests that social actors from low-status industries face greater skepticism from the audience, subject to lower evaluations and higher penalties regardless of their true underlying quality (Barlow, Verhaal, & Hoskins, 2018; Roulet, 2015). Consequently, we expect to observe greater stringency in audiences' technical assessments of a category spanner's qualities and greater ambiguity if an industry-spanning executive comes from a low-status industry.

With regard to commitment evaluations, studies of categorical conformity show category membership plays a role in shaping audiences' perceptions (Hsu et al., 2009). For instance, female candidates–particularly mothers–who are traditionally classified in low-status categories, are assumed to have lower levels of work commitment despite the lack of empirical evidence indicating as much (Campbell & Hahl, 2022; Correll, Benard, & Paik, 2007). Research suggests that categorical noncompliance is mistrusted, and a low position in the status hierarchy reinforces the view of deviance whereas a high-status position leads to path-blazing assessments (Goldberg, Srivastava, Manian, Monroe, & Potts, 2016). Extending these insights to our context of labor market category spanning, we expect low-status industry affiliation to strengthen baseline commitment concerns, since audiences' skepticism regarding spanners' commitment and intentions should be more salient. In consequence, we expect a more severe evaluative discount for candidates from low-status industries.

Status Leaps: Supply-side Considerations

In our baseline hypothesis, we suggested that a smaller compensation increase might be acceptable to executives because they ascribe value to gaining experience in

diverse domains in light of potential pecuniary and non-pecuniary rewards. We further propose that status leaping will strengthen this baseline mechanism: the ascribed value of a new industry experience will be higher when coupled with obtaining a higher-status affiliation. We ground this proposition on potentially higher-value of social ties and higher subsequent future rewards that might accompany high-status affiliations (Piazza & Castellucci, 2014), as well as the moral and emotional gains derived from engagement in a more prestigious economic activity (Pollock et al., 2019). In the supply-side baseline mechanism, we propose executives might accept a relatively smaller compensation gain in exchange for enhanced diversity of expertise and social connections given their potential to pay off in the long run. Status in a social system, by definition, implies centrality in a relationship network and makes social actors highly coveted (Gould, 2002). Thus, the expected utility of new social ties should be higher when category-spanning comes with a higher-status affiliation.

We also expect status leaps to increase the expected utility of non-pecuniary rewards, increasing the ascribed value of a novel experience in a different industry, and thus further accentuating our baseline supply-side mechanism. High-status industries are more attractive also because affiliation with a prestigious economic activity affords a deeper sense of value (Pollock et al., 2019), identity and enhanced well-being (Hu & Hirsh, 2017). Financial rewards such as salary, bonuses and stock options are the typical outcomes studied in executive compensation from an economic perspective. Beyond economic incentives, however, research increasingly emphasizes the social psychological dimensions of executive pay. Together with the financial sources of extrinsic motivation, executive behavior responds to the satisfaction of personal values and preferences. For our purposes, these explanations of executive compensation rely on the value of non-pecuniary rewards to elucidate executive behavior (Wowak et al., 2017), which plausibly include social status.

While the specific effect of industry status has not been studied, there is evidence that people accept lower salaries for more socially respectable work. For example, Fang and Tilcsik (2022: 14) state that "Studies estimate that nine out of ten employees are willing to accept somewhere between 23–32% lower salaries for personally meaningful work". Though meaning varies across individuals, evidence abounds on people's willingness to accept lower salaries for work more socially respectable or meaningful

which may bring emotional rewards, enhanced well-being and identity (Hu & Hirsh, 2017). Specifically for status, Focke and colleagues (2017: 313) show CEOs' "total compensation is on average 8% lower for firms listed in *Fortune*'s ranking of America's most admired companies" and conclude that "CEOs are willing to trade off status and career benefits from working for a publicly admired company against additional monetary compensation". This tradeoff is not exclusive to CEOs; similar evidence also exists for corporate managers, MBA graduates and university professors (Bidwell et al., 2015). Extending this logic, we suggest that top executives might renounce some compensation gain if it entails moving to publicly admired *types* of business as opposed to low-status or stigmatized industries.

Our status *leap* discussion suggests that affiliation with a low-status category can strengthen both demand- and supply-side mechanisms behind the industry-switching discount, echoing the contingency arguments underpinning Hypothesis 2. On the demand side, we suggest precedence from a low-status industry will strengthen both ambiguity and commitment concerns since recruiters will be more skeptical when assessing executives embedded in low-status categories. Regarding the supply-side mechanism, we conceptualize the ascribed value to a new category experience as a function of the status difference between the destination and origin categories. In other words, category status differences moderate the baseline mechanism, and candidate-side considerations behind executives' acceptance of lesser monetary rewards will be more pronounced in status leaps. This positive moderation of demand- and supply-side mechanisms in status leaps denotes a more severe discounting of category spanners.

> ***Hypothesis 3.*** *If an executive moves to a higher-status industry than his or her current one (Status Leap), the negative effect of industry switching on compensation gain is amplified.*

2.4. Methods

2.4.1. Data and Research Setting

To test our hypotheses, we use information on mobility and compensation patterns of S&P 1500 executives between 1999 and 2014[4] and cull data from multiple sources. We gather compensation and individual-level data from ExecuComp, and firm-level information from Compustat. We use Gallup's opinion measures as a proxy for industry status. To ensure we only work with executives who have moved voluntarily, we follow a multistep protocol integrating several datasets. To filter out job movements stemming from merger and acquisition (M&A) deals, we use the Thomson Reuters SDC database, along with the databases in Ewens, Peters & Wang (2018) and Phillips & Zhdanov (2013), which map M&A deals from the SDC database to Compustat GVKEYs. In addition, we use press releases in Factiva to check information on involuntary dismissals and exclude them from our analyses. For CEOs specifically, we also consult the Gentry, Harrison, Quigley, and Boivie (2021) dataset, which documents the reasons motivating CEO departures in S&P 1500 firms, and use Boardex to determine governance factors with board-level information. Finally, to test alternative explanations, we build an industry relatedness matrix with Text-Based Industry Classification datasets of Hoberg & Phillips (2010, 2016), which aggregates firm-pairwise similarity scores from the text analyses of 10-K product descriptions.

We begin our empirical investigation with ExecuComp by identifying all executives who changed organizations from 1999 to 2014 and find 3,290 changes in GVKEY. Since our aim is to distinguish the effect of changing industry from other confounding factors, we select only the first job change recorded in the dataset, eliminating other moves from the final panel. Given that 410 of the changes pertained to more than one GVKEY change by the same executive, there are 2,880 unique executives who changed GVKEYs. In all the analyses, we treat the first full years before and after the job change as the first pre-job-change and post-job-change years, respectively. To this end, we exclude 348 executives who did not have compensation information for at least one full year, both before and after the job change. We end up with 2,532 unique

[4] Although we measure job changes between 1999-2014, we include pre- and post-job-change observations. Hence, the overall data time frame is 1996-2017 for the 3-year window analyses. The range expands to 1994-2019 for 5-year window analyses in the robustness checks.

executives who moved to another firm, including dismissals, and had compensation information for both pre- and post-job change years.

We then identify and exclude involuntary dismissals, following a multistep protocol guided by the executive turnover literature. As proposed by Balsam, Kwack & Lee (2017), we first exclude 1,139 executives with gaps longer than two years in their work history, even those who reappear later in the database with the same executive ID.[5] We proceed by excluding job changes related to mergers and acquisitions. To identify whether M&A activity prompted an executive's move, we track and compare Thomson Reuters SDC M&A firm codes with the GVKEYs in ExecuComp and Compustat using the databases of Phillips and Zhdanov (2013) and Ewens et al. (2018). With this, we exclude 167 executives for whom the origin and destination firm dyad in the job change matched acquirer and target firm dyad in the M&A deals. Furthermore, the circumstances of remaining job changes were inspected by manually checking Factiva databases and contrasting press articles to reduce the likelihood of turnover being incorrectly classified as voluntary. Thus, we exclude 19 additional executives, for whom the reason to quit suggested involuntary dismissal, as outlined by Gentry et al. (2021).

After employing this multistage protocol, for the CEOs remaining in our sample, we also cross-check with Gentry et al.'s (2021) reason-for-leave database, which includes information solely on CEOs. Out of the remaining 123 job-changing CEOs after our prior exclusions, only nine were coded as involuntary departures in Gentry et al.'s (2021) dataset. After manually verifying the information on departure reasons for the nine misidentified CEOs, we excluded all of them. Note also that 92.6% of the CEOs, i.e., 114 out of 123, initially coded as voluntary departures were indeed not forced/involuntary departures in Gentry and colleagues' dataset. This almost complete overlap with our data gives us further confidence about the voluntary departures of non-CEO executives included in our sample, whose reason to quit we are unable to verify with other sources.

[5] ExecuComp only tracks movements among S&P 1500 firms: if executives go to a firm outside the S&P 1500, their compensation will be missing for the periods immediately following the job change, even if they return to an S&P firm later on. Similarly, if an executive transfers from a non-S&P 1500 firm to an S&P 1500 firm, pre- and post-job-change compensation data cannot be compared since our data is limited to post-change compensation. This gap-year exclusion addresses the probability that a departure is involuntary and prevents missing job spells, together with the experience and compensation accumulated during these spells, from contaminating our results.

After all exclusions, we have 1,198 executives who voluntarily moved to another organization between 1999 and 2014. Among them, 647 executives moved to firms in the same industry. After removing these 647 within-industry movers, our sample comprises 551 remaining executives who voluntarily moved to another industry. As explained in the matching protocol, we use 647 spared within-industry movers in the matched-sample design when we match the cross-industry moving executives with comparable executives who have not changed industry, whether they are within-industry moving executives or stayers who have not changed jobs.

Matched Sample Construction

To distinguish the effects of industry changing on compensation, we needed to discern how industry-changing executives' compensation would have evolved had they not changed industry (if they had instead moved within the same industry or stayed in their firms, i.e., comparison condition). That is to say, we needed a comparison group of executives who were unaffected by industry change but followed a compensation trend parallel to that expected of the treated executives had they not changed industry. Since it is infeasible to randomly assign executives to industry changing and comparison conditions, we try to find similar conditions in a quasi-experiment. Beyond the strict restrictions we apply on reason for departures, following prior studies, we have constructed a matched-sample design (e.g., Boivie et al., 2016). Specifically, we construct a quasi-control group comprising executives who moved to another firm in the same industry, hereafter *within-industry movers*, or stayed in their firms, hereafter *stayers*.

To construct the matched samples, we use one-on-one (k2k) coarsened exact matching (CEM) (Blackwell, Iacus, King, & Porro, 2009; Iacus, King, & Porro, 2012) and first match each cross-industry-moving executive to another executive who voluntarily moved to a same-industry firm, and another who stayed in their current firm. For this purpose, we use several individual- and firm-level parameters as matching covariates and controls. The executives' tenure in their origin firms, age and gender may influence the way our mechanisms operate, hence, we use executive *age, gender and tenure* as matching covariates. Since different actors are involved in the compensation-setting processes of CEOs versus non-CEOs and promotion to CEO position could represent another reward form (Boivie et al., 2016), we also use the *CEO indicator* in the

origin firm as a matching covariate. In addition, because pay at a new firm is a function of past compensation, we add the average value of the executive's *total compensation* for the three years prior to the job change and *pay rank* in their origin firm as matching covariates. Finally, to ensure that firm performance among origin firms of cross-industry movers versus comparison group does not differ, we use the average value of return on assets values *(ROA)* of origin firms for the three years prior to job change as an additional matching covariate.

CEM procedures eliminated 43 out of 551 industry changers for whom a proper match could not be found. Therefore, as mentioned earlier, we match each remaining 508 cross-industry moving executive with a within-industry moving executive and another executive who had not moved to another firm and possessed similar observable traits in the seven matching covariates. In the end, our final sample includes 1,524 executives (hereafter Panel-A): (a) 508 industry changers (including leapers and leakers) (b) 1,016 closely matched executives in the comparison group (including within-industry movers and stayers, i.e., counterfactual group of executives who did not change industry). We report all analyses based on this panel of matched samples, including 7,610 executive-year observations, for which we have information on all variables of interest.[6]

Comparison of Pre-Job-Change Covariates

In an ideal matched set, the comparison and treated samples should comply with the exchangeability assumption, meaning they should be in an identical position before the treatment and display no significant differences in observable pretreatment covariates (Heckman, Ichimura, & Todd, 1997; McDonnell & Cobb, 2020). Since we focus on two treatments here, i.e., industry changing versus not industry changing and status leap versus status leak, we assess each group's compliance with the exchangeability assumption by comparing individual-, firm-, and board-level determinants of compensation in the period just before the job change.

[6] The results of our analyses are similar if we use information on all voluntary movers and compare compensation patterns of industry changers only with within-industry movers, i.e., 1,198 job-changing executives identified before matching (hereafter Panel-B), or an alternative set of matched samples created with propensity score matching (using psmatch2 function in Stata 15 instead of the cem function used to build Panel-A).

The means of the individual- and firm-level characteristics for different treatment groups in the period just before the job change are presented in Table 2.1. T-tests indicate no significant difference in any of the control variables. As observed in Figure 2.1, the compensation distributions for different treatment groups are also substantively similar in the period before the job change. An *imbalance* check also reveals that the matching procedure helped reduce the distance between the control and treatment groups.

Insert Table 2.1 and Figure 2.1

2.4.2. Measures

Dependent Variable

We use *total compensation* (data item TDC1) as the dependent variable for all core regressions. Following recent studies on executive and CEO compensation (e.g., Gupta, Mortal, & Guo, 2018), we winsorize total compensation at the top and bottom 1%. The distribution is still skewed after winsorizing, i.e., the mean compensation is greater than the median, so we transform the compensation measures by using a natural logarithm.

Independent Variables on Industry Changing

We define *post-job change* as a dummy variable that equals 1 for the periods after a job change (hereafter treatment period). In most models, we report results for the -/+ 3-year window before and after each job change. We also employ alternative durations for pre- and post-job-change windows, ranging from -/+1-year to -/+ 5-years, which yields consistent results. We are interested in the differences in the interaction of the *post-job change* variable with *industry changer*. *Industry changer* is a dummy variable that equals 1 if the executive moved across industries. To classify firms according to industry, we use the North American Industry Classification System (NAICS), which organizes businesses by the *type* of economic activity. In our analyses, we use the first two NAICS digits, which designate the business sector, to operationalize industry change.

Independent Variables Related to Industry Status: Status Leap and Status Leak

We use Gallup's annual industry prestige rankings as a proxy for industry status. First published in 2001, Gallup's yearly opinion polls track Americans' overall impressions of 25 business sectors and have been previously used in organizational research (e.g., McDonnell & Werner, 2016). The results are based on telephone interviews with a random sample of Americans aged 18 and older, living in all 50 U.S. states and the District of Columbia. Samples derived from Survey Sampling International are weighted to match the national demographics of gender, age, race, Hispanic ethnicity, education, region, and population density for people 18 and older in the U.S. population (for more details, please see "Gallup Historical Trends: Business and Industry Sector Ratings" from 2001 to 2014). In total, Gallup interviewed over 7,000 people, with a yearly average of more than 500 respondents.

In these interviews, respondents are asked to rate business and industry sectors: *"On another subject, for each of the following business sectors in the United States, please say whether your overall view of it is very positive, somewhat positive, neutral, somewhat negative or very negative".* As per publicly available Gallup reports, we calculate industry prestige scores by subtracting total negative responses from total positive responses for this question, and end up with 14 annual industry prestige rankings from 2001 to 2014.

This dynamic measure has several advantages for our study. First, it is time-variant, so it captures temporal dynamics and the evolving effect of macroeconomic and sociological factors on industry status. Categories and their vertical status are not static; hierarchies evolve over time. By way of example, some industries' ratings vary from year to year depending on their prominence, such as the relative rating declines of the finance and insurance and real estate industries following the 2008 financial crisis. Second, aside from 1999 and 2000, the surveys were conducted from 2001 to 2014, years which overlap with our study period.[7] Third, the comprehensiveness and detail for the 25 industries in the Gallup rankings are conceptually close to the 23 two-digit NAICS industry codes used

[7] Note that we investigate the outcomes of job changes between 1999 and 2014. For executives who changed jobs in 1999 and 2000, we use 2001 Gallup rankings, which are the earliest available. For all other years, we use the Gallup ranking corresponding to each job-change year.

to operationalize industry changes.[8] Finally, Gallup's sampling technique is aligned with the sociological underpinnings of our industry status discussion as the surveys capture the prestige perceptions of the broader population–which should be embedded in both candidates' and recruiters' social evaluations. In fact, the industry rankings follow reasonable trends: industries such as information, professional and scientific services consistently rank high in prestige, while oil and gas and public administration rank low (see Figure 2.2).

After crafting annual industry prestige rankings using Gallup surveys, we obtain a dynamic industry prestige ranking for each job-change year in our time frame. Using these rankings, we code each observation relating to an *industry changer* as either a *status leaper* or a *status leaker*. *Status leaper* is a dummy variable that equals 1 for all observations associated with an executive who moved to a higher-status industry. *Status leaker* is a dummy variable that equals 1 if the industry-changing executive moved to a lower-status industry.

Insert Figure 2.2

Control Variables

We consider numerous factors that might influence executive compensation. At the individual (executive) level, we control for *gender, tenure,* and *age,* following the literature on executive compensation. Given that promotion to CEO positions is itself a reward, we also control for *change in CEO indicator,* which we operationalize with four binary variables: *non-CEO to non-CEO, non-CEO to CEO, CEO to non-CEO,* and *CEO to CEO,* representing the CEO indicator in the origin and destination firms, respectively.

[8] Similarly, in cases in which the 2-digit NAICS code and Gallup industry group do not exactly match, we average the ratings of the closest Gallup categories. As an example, there is no direct match for NAICS code 51, i.e., "information". Therefore, we calculate its prestige rating by taking the average of the two closest Gallup categories: "computer" and "internet". Moreover, two doctoral students conducted the matches as an additional check to ensure interrater reliability. Neither the matching nor the results yield qualitative differences that alter the results. However, to account for the possibility that our prestige ranking was overly precise, we employ different specifications to operationalize leaps and leaks. For details, please see status-tier discussion in "Effect Sizes and Additional Robustness Checks" section.

At the firm level, since the characteristics of focal and destination firms each affect compensation, we include both the origin and destination firms' *size, total debt, capital expenditures, sales, cash, profit margin, sales growth,* and *value of acquisitions*. Poor performance is not only associated with executive replacement and mobility but also impacts compensation (Hilger, Mankel, & Richter, 2013), an effect that we capture with *return on assets (ROA)*. We also account for industry differences in pay norms by including *average industry executive pay* for each industry-year dyad in our time frame, as differences among industry wage averages might drive the results (Andrus et al., 2019). To address the impact of pay level on pay change and control for possible omitted variables, we also include a lagged dependent variable in all models, i.e., *compensation (1-year lag*) (Zona, Gomez-Mejia, & Withers, 2018). To reduce omitted variable biases and capture industry-specific factors that might affect compensation, we also control for macroeconomic factors through time-invariant industry effects (*industry dummies*) and time effects (*year dummies*).

Board characteristics may affect the proposed theoretical mechanisms and boards specifically decide CEO compensation terms (Brockman et al., 2016), consequently, we also include several governance measures. First, we control for *board size,* as larger boards might have access to greater resources and information (Gentry et al., 2021). Second, we control for *CEO duality* since CEOs who also chair a board might influence the compensation process (Balsam et al., 2017). Third, we include *board independence*, measured as the percentage of independent directors on the board. Board members' affiliations outside the firm could determine how ambiguity is processed, in consequence, we control for *board directorship*, measured as the total number of board memberships held by directors in other firms and the average *board network size* of board members. Finally, evidence exists that top executives' relationship with their firm's board differs when executives serve on boards whose composition is more diverse than the focal one (Zhu & Shen, 2016). Thus, we control for *board gender* and *industry diversity*. *Board industry diversity* is measured with the Blau index based on individual members' dominant industry background, i.e., the industry in which each board member has served for the longest time, following Semadeni, Chin, & Krause (2021).

Variable definitions, operationalizations and information sources appear in Table 2.2.

Insert Table 2.2

2.4.3. Statistical Approach

Our estimation strategy aims to examine changes in compensation of industry-changing executives following a job change relative to the compensation changes experienced by the matched executives in the comparison group. Our main specifications take the following form:

$$Y_{its} = f\,[\tau_t + \psi_s + \boldsymbol{\beta_0}\,(\textit{Post-Job Change})_{it} + \boldsymbol{\beta_1}\,(\textit{Post-Job Change x Industry Changer})_{it} + \beta_{CV}CV_{its} + \epsilon].$$

where the dependent variable (Y_{its}) is logarithmic total compensation (TDC1) for executive "i" in year "t" working in industry "s", and τ_t and ψ_s representing year and industry fixed effects, respectively. CV is a parsimonious vector of control variables (coherent with previous literature) comprising all items explained in the control variables section. The coefficient of interest is $\boldsymbol{\beta_1}$, which allows us to capture the effect of the treatment, i.e., *industry change*, at the treatment period, i.e., *post-job change*. In the models used to measure the effect of status leap and status leak, we replace interaction of interest with "$\boldsymbol{\lambda_1}$ *(Status Leaper x Post-Job Change)*$_{it}$*"* and "$\boldsymbol{\lambda_2}$ *(Status Leaker x Post-Job Change)*$_{it}$*"* and compare the two. Standard errors are clustered at the executive level, considering the nonindependence of within-executive observations (Bertrand, Duflo, & Mullainathan, 2004).

2.4.4. Results

Table 2.3 presents the summary statistics and correlations for the matched sample (Panel-A) used for the analyses. In all models reported throughout the study, the variance inflation factor (VIF) for any control variable never exceeds 10, suggesting that multicollinearity is not a concern.

Insert Table 2.3

The core regression results are presented in Table 2.4. In Model 1, we first test the impact of industry changing on compensation, including all observations for pre- and post-job-change periods. Hypothesis 1 predicts (β_1) *post-job change x industry changer* will have a negative coefficient, indicating that compensation gain is smaller for executives who moved to another industry than comparable executives who did not change industry. This is the coefficient of interest since it measures the treatment effect in the treatment period (post-job change). In Model 1, the negative significant coefficient of *post-job-change x industry changer* ($ß_1 = -.11; p < 0.01$) means that the compensation gains of executives who moved to a different industry are smaller than those of executives who did not (comparison group). While Model 1 includes all observations, Model 2 shows that the effect remains when we narrow down the observations to a -/+ 3-year window before and after the job change ($ß_1 = -.10; p < 0.05$). Hence, the results in Models 1 and 2 support Hypothesis 1 with a strong, negatively diverging effect of industry changing on compensation.

Insert Table 2.4

We then examine the effects of the status of industries involved in the move, as proposed in Hypotheses 2 and 3, and present the results in Models 3 and 4. We estimate models similar to our core specification but replace the interaction of interest ($ß_1$) with *Status Leaper x Post-Job Change* (λ_1) and *Status Leaker x Post-Job Change* (λ_2). We expect *Status Leaper x Post-Job Change* (λ_1) to have a smaller negative coefficient than *Status Leaker x Post-Job Change* (λ_2). As expected, Model 3 shows that the former has a significant negative coefficient ($\lambda_1 = -.17; p < .01$), whereas the latter coefficient is not statistically significant. Model 4 shows that the effect remains when we narrow down the observations to a -/+ 3-year window before and after the job change. A Wald test comparing the coefficients of Status Leapers and Status Leakers ($p < 0.1$) reveals that the difference between the two is statistically significant. The compensation gains of executives who moved to a higher-status industry are smaller than those of executives who moved to a lower-status industry. These findings support the moderation effects in H2 and H3. Moreover, the nonsignificant interaction effects in Status Leak places a boundary condition on the effect of industry changing on compensation, suggesting that

industry changes reduce compensation gains only when executives move to industries with a higher status than their current one.

Dynamic Effects

Our methodological approach relies on the parallel-paths assumption. To verify the absence of divergent trends across different groups of executives prior to the job change and better assess how industry changes, status leaps and status leaks affect compensation over time, we construct a dynamic difference-in-differences model.

The coefficients reported in Models 1 to 3 of Table 2.5 indicate no significant differences across executive groups (industry changers, status leapers and status leakers, respectively) in the pre-job-change patterns of compensation (from t-3 to t-1), suggesting that our results are not driven by diverging pretreatment trends. For industry changers (Model 1 of Table 2.5), we observe negative and statistically significant coefficients starting from the very first year after the job change (t+1), highlighting the negative divergence of industry changers from the comparison group. Models 2 and 3 suggest that for status leapers, the negative divergence at post-job-change periods is not only statistically significant but also larger than that of executives who move to lower-status industries: that is, status leapers experience significantly lower compensation gains (note that, for status leakers, absolute values of post-job-change coefficients are positive and insignificant through all post-job-change periods).

Insert Table 2.5 and Figure 2.3

Figure 2.3 illustrates the interaction effects for industry changers. Blue bars representing 90% confidence intervals consistently contain the x-axis in pretreatment periods, indicating an absence of pre-job-change compensation differences among different groups. In contrast, as hypothesized, the compensation pattern of industry changers significantly and negatively diverges from that of the comparison group after the job change. These dynamic effects strengthen the internal validity of our methodological approach, reliant on the parallel trend assumption.

2.4.5. Supplementary Post Hoc Analyses

The results presented thus far show a negative divergence in compensation patterns for industry changers, which is moderated by industry status. Moreover, we rule out several alternative explanations through sample matching, examining individual-, firm-, and board-level parameters, and demonstrating dynamic effects. The findings confirm that the exchangeability and parallel pretreatment assumptions hold. However, our results do not directly demonstrate the proposed mechanisms of social evaluation, which we address in several post hoc analyses.

Breakdown of Differences: Change in Base Salary Versus Performance-related Pay

We argue that the post-job-change difference in compensation between industry changers (including leapers and leakers) and the comparison group is related to neither pre- nor post-job-change performance-related differences among these groups; rather, it stems from social evaluations while setting compensation. If this were the case, one would expect the difference to accrue from the predetermined portion of the compensation package (base pay), while the treatment should not divergently affect performance-based pay (bonus) for different groups.

To address this argument, when testing the effects of both industry change and industry status, we replace the dependent variable (total compensation) with separate variables for salary and bonus. Models 1 and 2 of Table 2.6 show that salary of industry changers negatively diverges from matched comparison group (for -/+ 3-year window: β_1 *= -.07**; SD = .03).* As verified by Wald tests, Models 3 and 4 show that the negative effect is stronger for status leapers than for status leakers, for whom the effect is not significant. However, Models 5 to 8 show no significant differences in bonuses after treatment for any of the groups. Similar to total compensation, we see no diverging pre-job-change patterns either in salary or bonus. The average salary of industry-changing executives starts to differ negatively from the comparison group after the job change, whereas for bonuses, no differences are observed among any of the treated groups before or after the change.

These results provide insight into our proposed mechanisms. The significant difference we observe in base pay lends credibility to our findings, as salary partly measures the value a company places on attracting an individual and hence approximates the predetermined worth of human capital (Murphy & Zabojnik, 2004). Non-diverging pretreatment trends for bonuses contradict a potential alternative explanation, i.e., that industry-changing executives might be relatively low performers. Moreover, similar post-treatment trends for bonuses support our argument that the treatment does not affect performance-related pay, with the lower compensation gain accompanying mobility likely stemming from mechanisms other than performance differences.

Insert Table 2.6

CEOs Versus Non-CEOs

We have described several particularities regarding the hiring practices of CEOs versus non-CEOs and the various motives in the selection and evaluation, yet have suggested that both CEOs and non-CEOs are subject to our compensation explanations. Empirically, we have attempted to exclude factors that would affect CEOs differently. For instance, we control for the change in the *CEO indicator* in all our empirical analyses, since promotion to CEO is itself a reward and determines a considerable portion of compensation variation. We have also carefully controlled for numerous board factors since replacing CEOs and determining compensation are among the key responsibilities of corporate boards.

To further substantiate whether our mechanisms apply to both CEOs and non-CEOs, we divide the sample and run the same regression for non-CEOs versus CEOs in Models 5 and 6 of Table 2.4, respectively (note that the total number of observations in Models 5 and 6 adds up to the N in Model 2). The results show that the compensation of industry-changing top executives negatively diverges from their comparison-group peers in both the non-CEO and CEO samples, corroborating that both are subject to our proposed mechanisms. Nevertheless, the industry changing discount is larger for the CEO sample (Model 6: $ß_1$ = *-.26)* than for non-CEOs (Model 5: $ß_1$ = *-.08).* The difference between coefficients is statistically significant according to Stata *suest* and *test*

functionalities (*p-value < 0.1*), suggesting that the industry-switching discount is greater for CEOs than for non-CEO executives. Although we did not formally hypothesize it, a higher penalization for CEOs resonates with our arguments, as issues related to future commitment and adverse selection risks might be particularly salient and relatively more costly for CEOs.

Industry Similarity

We suggest that the category-spanning discount mainly derives from ambiguity and commitment concerns on the demand side. Moreover, we argue that such concerns impact executive mobility outcomes even if executives' actual performance does not differ across groups. One can claim that when executives switch industries, they lose some of the industry-specific human and social capital, which could confound our proposed mechanisms. If this were the case, one would expect the penalization to disappear or to be smaller when executives move to related or relatively closer industries, where their industry-specific skills might be more relevant. Therefore, we conduct another set of analyses examining compensation of industry changers separately and consider the similarity or relatedness between industries involved in a job move to gain confidence that skill transferability does not drive our results.

When operationalizing industry similarity, we aim to capture temporal dynamics, i.e., whether some industry dyads have become more similar during our timeframe, since NAICS codes are not regularly updated. To this end, we build an *industry relatedness* matrix using text-based analysis of firms' 10-K filings, that are updated annually. Hoberg and Phillips (2010, 2016) employed a text-based analysis of firms' 10-K filings to capture product similarity between firms, identify competitive rivals and build a firm-pairwise similarity matrix, in which only GVKEY dyads with product similarity above a certain threshold appear, i.e., competing firms.

We first map GVKEYs in the Hoberg and Phillips databases to NAICS, assigning each GVKEY the corresponding NAICS code. We then operationalize *industry relatedness* as a function of the co-occurrence of NAICS codes in the Hoberg and Philips firm pairwise similarity matrix. We define the similarity from industry "i" to "j" as the number of times both industries "i" and "j" occur in the firm-pairwise matrix, summed and divided by the total number of appearances of industry "i" in the Hoberg and Phillips

(2016) matrix. To measure industry relatedness, we use all entries in the similarity matrix for the three years preceding a focal period. For instance, to calculate industry relatedness scores for 2005, we take into account co-occurrences in 2003, 2004 and 2005. Then, we use this 2005 industry relatedness score for executive cross-industry changes that occurred in 2005. This three-year time frame prior to the focal year captures the dynamic evolution of the relatedness measure, and considers market participants should have acquired an understanding of these similarities (Leung, 2014).

After crafting annually updating industry relatedness matrices, we obtain an *industry relatedness*$_{ij}$ score for each industry dyad in our cross-industry moves, updated annually as per the changing product descriptions in the three preceding years. The negative insignificant interaction coefficient *post-job-change * industry relatedness*$_{ij}$ in Model 1 of Table 2.7 negates the alternative explanation of skill transferability for industry changers: moving to a more related industry does not have a positive effect on compensation, when we operationalize industry similarity with web-crawling data and text-parsing algorithms trained on business descriptions.

Insert Table 2.7

Effect Sizes and Additional Robustness Checks

The difference in effect sizes of the coefficients for changing industry versus remaining within the same industry is economically important. To calculate the relative changes in compensation for various conditions, we follow Hubbard, Pollock, Pfarrer and Rindova (2018) and predict the effects for different combinations of not changing industry versus industry changing and pre- versus post-job-change (based on Model 1 of Table 2.4: *post-job-change = 0* and *post-job-change = 1*, respectively). To facilitate interpretation, we calculate the predicted effect on total compensation value for each condition, with all other variables held at their mean. The resulting effects, using the *margins* command in Stata 15, appear in Table 2.8. The Row Difference column indicates that, on average, going to another industry means a compensation gain that is $378,530 less than that of a comparison-group executive (more than 10% of the average compensation of an executive in our sample). The column difference between remaining

within industry and cross-industry moving for the "*post-job-change = 1*" condition is $388,870, while the difference in the pre-job-change column is only $10,340 ("*post-job-change = 0*" condition). These numbers also confirm the non-divergence among different treatment groups in the pre-job-change periods, as documented in the Dynamic Effects section.

Insert Table 2.8

Finally, we conduct several robustness tests. First, we rerun all estimations with the entire sample of voluntary departures (Panel-B) rather than the matched samples (Panel-A). The results still hold for both the baseline and moderation hypotheses. We also check that the results are robust to different specifications of the dependent variable (e.g., non-winsorized, not logged). We then estimate the models with different measures of industry change (e.g., 2-, 3-, and 4-digit NAICS codes). Although we cannot test the industry status hypotheses, we test the baseline hypothesis and confirm a statistically significant discount associated with changing industry when employing different industry specifications. The results also hold when the same analyses are performed for different time windows, such as +/- 2, 4 and 5 years before and after the job change, as well as a +/- 1-year window allowing an immediate comparison between the two periods before and after the job change. Regressions using change in compensation rather than total compensation as dependent variable yield very similar results.

Another viable concern is that we treat status differentials as categorical constructs measured by dummy variables for status leap and leak since we surmise categorical differences in industry status are most relevant. We employ an alternative approach as a robustness test. First, we divide industries into five status tiers, where the highest tier consists of the top 5 industries; the next tiers include industries ranked 6 to 10, 11 to 15, and 16 to 20; and the last tier contains industries ranked below 20. Industry changes between dyads in which the two industries belong to the same status tier (a small difference between industry prestige scores) are coded neither as a leap nor as a leak. This approach accounts for the possibility that our Gallup prestige-score coding is overly

precise. The results are again robust to this alternative way of operationalizing status leaps and leaks.

2.5. Discussion

To better understand candidates' motivation to straddle categories despite the associated penalties and the unexplained variation observed in returns to spanning labor market categories, we examine how category spanning influences social evaluations in relation to category status. To test our hypotheses, we identify industry affiliation with categorical membership of executives, focus on voluntary executive mobility across industries, and explore how crossing industry boundaries and industry status differences jointly affect compensation outcomes. In our baseline hypothesis, we propose recruiters devalue executives who move across industries due to ambiguity and future commitment concerns. On the supply side, scarcely considered to date, we argue and find a smaller compensation premium accrues to executives who straddle categories, perhaps because they weigh the potential long-term gains of accumulating experience in different sectors and place value on non-pecuniary rewards.

Industry status exerts a moderating influence on the mechanisms underlying the baseline effect, further confirming that industries function as an evaluative categorical lens. Status indicators seem to complement audiences' and candidates' social judgments of industry-spanning boundaries. In relation to industry status, on the demand side, we theorize that high-status affiliation can mitigate ambiguity and commitment concerns because audiences conduct more favorable assessments. However, we also argue that these tendencies contrast with executives' preference for high-status over low-status industries. Our findings consistently support our hypotheses and the proposed mechanisms explaining how the category spanning penalty is contingent upon the status differentials between spanned categories.

2.5.1. Contributions

While our conceptual development relies on the rich sociology of markets literature, our study contributes to research on categories and status, as well as the work at their intersection (Bowers & Prato, 2018; Delmestri & Greenwood, 2016; Durand & Kremp, 2016). In addition, we draw on and advance executive mobility research to test

our integrating framework, providing important theoretical and empirical advancements on the antecedents and consequences of executive mobility.

Theoretical Implications for Category Research

Although categorical imperative research is primarily focused on audiences' concerns which generate an evaluative discount, category straddling is a two-way process comprising both audience- and candidate-side judgments. In this regard, our findings reinforce the need to jointly examine both sides in category spanning. To date, it is unclear why market actors span categories in light of expected discounts. Addressing the overlooked motivations of candidates who span labor market categories, our status extension to category theory responds to this quandary with an agentic perspective. We consider the utility derived from high-status affiliations and argue that obtaining status or cashing out high status might be an antecedent of spanning categories.

Relatedly, we introduce categorical status as a new determinant of the category-spanning penalty. Prior studies have examined how heterogeneity in either audience or candidate characteristics moderate illegitimacy discounts (Phillips & Zuckerman, 2001; Syakhroza, Paolella, & Munir, 2019; Zhang, Wang, & Zhou, 2020). However, the outcomes of social actors' cross-category movements might also depend on whether some categories are considered more desirable than others. In other words, even if candidates and audiences do not differ along the established discount determinants (e.g., candidates being high-, mid-, or low-status; audience being market-taker or market-maker) (Durand & Kremp, 2016; Pontikes, 2012), the discount might still vary based on inter-category status differences. While social categories themselves are central to classification systems, prior studies have investigated separately how category straddling and status affect labor market outcomes. In fact, research on market classification has rarely operationalized differences, developed or measured the impact of categories' social hierarchy on category-spanning discounts. Failing to account for status differentials among categories and their impact on differences in supply- and demand-side assessments could also partially explain the mixed findings for category-spanning penalties.

While introducing a new contingency factor to the evaluation of category spanners, that is category status, we follow the recent turn in categorization research that

relaxes the emphasis on a uniform disciplining effect of categories (Durand & Paolella, 2013). Core studies of the categorical-imperative draw on institutional and ecological theories and hinge on a standard universal penalization for category spanners, which affixes audience expectations to a strict compliance with categorical prototypes. In contrast, recent research suggests that not all combinations of categories are similarly penalized, prompting a more nuanced case-by-case assessment of how audiences apprehend category spanners (Durand & Paolella, 2013). Besides this ontological turn in categories research (see also Kennedy & Fiss, 2013), we join studies that conceptualize industry categories not only as sieves filtering out atypical candidates but also as meaning- and status-laden evaluative lenses influencing social evaluations (Delmestri & Greenwood, 2016; Sharkey, 2014). This stream of research has explored the effect of important categorical attributes like contrast (Hannan, Pólos, & Carroll, 2007), leniency (Pontikes, 2012), distance (Kovács & Hannan, 2015) and inter-category congruence (Cudennec & Durand, 2022). Adding to this field, our study illuminates the understudied moderation effects of category status on the mechanisms underlying the category-spanning penalty.

Theoretical Implications for Status Research

This study advances research on status attainment reflecting recent calls to distinguish how social actors' behaviors respond to the status derived from their category (Prato, Kypraios, Ertug, & Lee, 2019; Sharkey, 2014) and the behavioral consequences of status pursuits (Raz, Behfar, Cowen, & Thomas-Hunt, 2021). Our view of candidate agency draws on a central insight from status attainment research: social actors actively bid (Hsu et al., 2009) and strategically invest in status (Rider & Tan, 2015). Bidwell et al. (2015) argue that relative to low-status competitors, high-status employers can hire more capable junior employees at equivalent cost because individuals expect future career opportunities to increase with employer status. In a similar vein, Rider and Tan (2015) conceptualize status as a reward form exchanged for monetary compensation in labor markets. Specifically, Rider and Tan (2015: 369) suggest studies "should consider more broadly the trade-offs that individuals accept to work for *particular employers.*" Our study expands this perspective by considering the tradeoffs executives accept to work in *particular types of business*, i.e., industries. To our knowledge, this is the first empirical

study to examine industry status as a determinant of compensation in external job mobility.

In addition to status-based models of market competition (Piazza & Castellucci, 2014; Podolny, 1993) and hiring (Rider & Tan, 2015), this prestige-sensitive perspective complements other research on the sociology of markets focused on the status of occupations (e.g., Zhou, 2005) and industries (e.g., Sharkey, 2014), as well as on categorical stigma (e.g., Greve, Palmer, & Pozner, 2010). The consideration of non-financial goals like prestige vis-à-vis compensation also resonates with psychological and behavioral economics research, showing that wages and job characteristics enter the utility function in a complex manner. An individual's utility of job outcomes is determined relative to the outcomes of relevant peer groups and his or her expectations, aspirations and values (Kahneman, Diener, & Schwarz, 1999). In addition, concerns about status permeate and profoundly alter a broad range of human behavior (Frank, 1985). Our results imply that affiliation with a high-status social category might compensate monetary rewards, reinforcing existing evidence that status affects compensation patterns.

Theoretical and Empirical Implications for Executive Mobility Research

This study also advances research on the individual consequences of executive mobility, by first examining how executives' industry affiliation impacts mobility outcomes above and beyond individual, firm, and board-level factors. While the antecedents and consequences of executive mobility have attracted enormous scholarly attention from corporate governance, finance, and labor economics scholars (Andrus et al., 2019; Cannella et al., 2009; Hilger et al., 2013), neither economic nor social psychological perspectives isolate the impact of industry changing on compensation. Our findings also show how status differences among industries impact mobility outcomes.

Second, by introducing industry status as a new determinant of executive compensation, we extend the work on the influence of non-financial rewards on executive decisions (e.g., Chin, Hambrick, & Treviño, 2013; Wiseman, Cuevas-Rodríguez, & Gomez-Mejia, 2012). Research predicated on economic theories generally views top executives as having individualized agency and an exclusive focus on financial self-interest. However, top executives also value non-pecuniary rewards, i.e., the "more

psychological sources of satisfaction that executives seek to achieve via their actions" (Wowak et al., 2017: 672). Hence, understanding the utility derived from accommodating psychological values, beliefs, and preferences otherwise unaccounted for in standard economic models sheds light on the antecedents of executive behavior.

In addition, we address calls to specifically study movements by non-CEOs beyond job markets for CEOs or top management teams (Boivie et al., 2016; Buyl, Boone, & Wade, 2015). Unlike the studies that "treat the cadre of top executives at the apex of the organization as a unitary construct" (Bermiss & Murmann, 2015: 1697), we study the consequences of non-CEO mobility (together with CEOs) and the conditions surrounding the job changes on an individual basis. Theoretically, this approach renders a finer analysis of both supply- and demand-side mechanisms. Empirically, we join recent calls to consider diverse antecedents of voluntary versus involuntary mobility (Andrus et al., 2019; Gentry et al., 2021), which enhances confidence in our findings. To this end, we replicate and verify Gentry and colleagues' coding for various forms of voluntary and involuntary departure of CEOs, as well as extend their procedure to non-CEO executives. The reason-for-leave protocols following and extending the state-of-art research in executive mobility, together with our new measures of industry status and industry relatedness, based on web-crawling data and text-parsing algorithms trained on business descriptions, will help future researchers explore related questions.

2.5.2. Limitations and Future Research

Executive mobility is a particularly appropriate setting to study category-spanning discount and status as the voluminous literature in this field helps us eliminate a wide array of confounds which predict compensation differentials. However, like any employee mobility, executive mobility is an endogenous process that lacks a shock reflecting exogenous variation, which would be ideal to test our hypotheses. Thus, the outcomes of executives' cross-industry mobility are not outcomes of random processes (Raffiee & Byun, 2020). To address this limitation, we implemented an extensive set of methodological tools such as coarsened exact matching and a detailed comparative analysis of the trends in the dependent variable and numerous individual-, firm-, and industry-level compensation determinants. We also examined the changes in different components of executives' compensation packages. As reported, we found no evidence

that the matched treatment and comparison groups differed in pre-job-change periods concerning any available individual-, firm-, or governance-level parameter. Despite these efforts, the endogeneity affecting mobility research generally advises caution about causal interpretation of results.

Second, regarding the effect of industry status, we suggest that both demand- and supply-side factors contribute to the observed compensation effects of inter-industry status differences. Specifically, from the demand side, we argue that affiliation with a high-status industry might mitigate ambiguity and commitment doubts. On the supply side, we expect executives might trade off a portion of the compensation premium to move to a higher-status industry, positively moderating candidates' ascribed value of an experience in a new domain. Our findings and insights from the relevant literature support these arguments overall, but we do not directly test what executives and recruiters think. We lay out solid conceptual premises that underlie our propositions for supply and demand supported by novel matching and reason-for-leave protocols following and extending state-of-art research in executive mobility. However, a caveat applies to our conclusions as our data allow us neither to explicitly apportion compensation changes to demand- versus supply-side explanations nor to know precise intentions of executives when they change industries.

Lastly, given the lack of diversity in our research setting, our study cannot parse out the degree of industry-status relevance as a desirability factor across different audiences. Thus, research extension could examine potentially varying effects of industry status among different audiences and candidates. One area for relevant future study would be the differential effects for women and ethnic minorities. Research has explored the impact of ascriptive characteristics such as race and gender on hiring. Gender and race may also have direct effects on our proposed mechanisms in that, although not directly related to workers' performance (Bidwell, 2011), they might affect recruiters' perceptions of ability and evaluations of deviant actions. Our dataset includes very few women and racial minorities among the top S&P 1500 executives, precluding further exploration. Empirical settings with greater diversity or appropriate experimental designs could better illuminate how our proposed mechanisms operate.

2.6. Conclusion

Questioning the conventional view, which treats category spanners as passive recipients of evaluative discounts, we have taken an agentic perspective that conceptualizes category-spanning candidates as actively seeking status advancement. Addressing the observed straddling of labor market categories and the variance in its outcomes, we build an integrated theory of categorical imperative and category status that extends recent work on the nexus of category systems and status hierarchies. Our conceptualization of industry status as a novel reward form, measured with novel empirical methods, and our findings encourage reconsideration of established assertions in this field and also contribute to broader conversations on how individuals weigh non-pecuniary rewards in career decisions.

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2.8. Chapter 1 Tables and Figures

Table 2.1. Pre-Treatment Covariate Comparisons at the Period before Job Change

Pre-Treatment Covariate	Panel-A: Matched Sample[a]			Industry Changers[b]		
	Mean (industry changers)	Mean (comparison group)	*t-test*	Mean (status leapers)	Mean (status leakers)	*t-test*
Executive-level						
Female	0.09	0.08	*0.69*	0.09	0.09	*0.95*
Tenure	4.26	4.33	*0.75*	4.07	4.44	*0.31*
Executive Age	48.41	49.07	*0.15*	48.20	48.60	*0.64*
Firm-level						
Return on Assets (ROA)	0.08	0.08	*0.90*	0.07	0.08	*0.44*
Total Debt	0.25	0.25	*0.92*	0.24	0.26	*0.48*
Capital Expenditures	0.05	0.05	*0.39*	0.05	0.05	*0.49*
Sales	7.70	7.55	*0.18*	7.67	7.73	*0.69*
Cash	0.16	0.15	*0.76*	0.17	0.15	*0.27*
Profit Margin	0.01	0.01	*0.96*	0.02	0.01	*0.64*
Sales Growth	0.09	0.57	*0.41*	0.08	0.10	*0.37*
Value of Acquisitions	181.98	166.64	*0.85*	207.27	159.59	*0.73*
Board-level						
Board Size	9.64	9.39	*0.20*	9.52	9.76	*0.45*
CEO Duality	0.57	0.52	*0.16*	0.60	0.54	*0.27*
Board Independence	0.64	0.66	*0.12*	0.64	0.64	*0.92*
Board Directorship	10.59	10.04	*0.12*	10.52	10.66	*0.81*
Board Network Size	1780.19	1693.92	*0.12*	1799.58	1761.11	*0.74*
Board Gender Diversity	0.22	0.21	*0.34*	0.23	0.22	*0.59*
Board Industry Diversity	0.61	0.58	*0.15*	0.62	0.61	*0.41*

[a] *n* (Panel-A: Matched Sample) = 1,524; *n* (Industry Changers) = 508; *n* (Comparison Group) = 1,016
[b] *n* (Industry Changers) = 508; *n* (Status Leapers) = 252; *n* (Status Leakers) = 256

Table 2.2. Variable Descriptions and Sources

Variable	Description and Source
Total Compensation (log)	Total annual pay in thousands of dollars (Source: ExecuComp data item TDC1)
Salary (log)	Base salary in thousands of dollars (Source: ExecuComp data item SALARY)
Bonus (log)	Bonus in thousands of dollars (Source: ExecuComp data item BONUS).
Industry Changer	Dummy variable that equals 1 if the job-changing executive goes to a firm with a different 2-digit NAICS code and zero otherwise (Source: Execucomp)
Post-job-change	Dummy variable that equals 1 for the periods after the job change (treatment period) (Source: ExecuComp)
Industry Status Score	Net positive prestige score calculated by the difference between total positive responses and total negative responses in the Gallup Survey (Source: Gallup)
Status Leaper	Dummy variable that equals 1 if the industry status score of the new firm is higher than that of the previous firm (Source: Gallup)
Status Leaker	Dummy variable that equals 1 if the industry status score of the new firm is lower than that of the previous firm (Source: Gallup)
Female	Dummy variable that equals 1 if the executive is a woman (Source: ExecuComp)
Tenure	Number of years as an executive in the current firm (ExecuComp)
Executive Age	Age of executive in years (ExecuComp)
CEO Indicator	Dummy variable that equals 1 if the executive is a CEO (Source: ExecuComp data item CEOANN)
Return on Assets (ROA)	Earnings before interest and taxes divided by total assets (Compustat EBIT / AT)
Total Debt	Total debt, defined as debt in current liabilities plus long-term debt, divided by total assets [Compustat (DLC+DLTT) / AT].
Capital Expenditures	Capital expenditures divided by total assets (Compustat CAPX / AT)
Sales (log)	Natural log of sales in thousands of dollars (Compustat data item SALE)
Cash	Cash and short-term investments divided by total assets (Compustat CHE / AT).
Profit Margin	Net income divided by sales (Compustat NI / SALE)
Sales Growth	Yearly sales growth rate (Compustat SALE)
Value of Acquisitions	Value of acquisitions (Compustat data item AQC)
Average Industry Executive Pay	Average compensation of executives working in each 2-digit NAICS code by year (Source: ExecuComp and Compustat)
Industry Relatedness	Industry similarity that is operationalized with firm-pairwise similarity scores from the text analyses of 10K product descriptions. For details, please see "Industry Relatedness" section under Supplementary Analyses (Source: Hoberg & Phillips (2010, 2016) Data Library)
Board Size	Total number of directors (Source: Boardex)
CEO Duality	Dummy variable that equals 1 if the CEO also holds chairman/chairwoman position and zero otherwise (Source: Boardex)
Board Independence	Percentage of independent directors in the board (Source: Boardex)
Board Directorship	Total number of board memberships that directors have in other firms (Source: Boardex)
Board Network Size	Average network size of the board members (Source: Boardex)
Board Gender Diversity	Blau index calculated as $1–\Sigma(P_i)^2$, where P_i is the proportion of directors on a given board in the *i*th category of a given dimension (Source: Boardex)
Board Industry Diversity	Measured as the Blau index after we identified each board member's dominant industry background, i.e., the industry in which a board member has served for the longest time (Source: Boardex)

Table 2.3. Descriptive Statistics and Correlation Matrix

	Variables	Mean	S.D.	Min	Max	1	2	3	4	5	6	7	8	9	10	11	12
1	Total Compensation (log)	7.64	1.01	4.64	10.09	1.00											
2	Salary (log)	6.20	0.61	2.03	7.46	0.73	1.00										
3	Bonus (log)	1.90	2.76	0.00	8.60	0.07	0.00	1.00									
4	Job Changer	0.71	0.45	0.00	1.00	0.24	0.22	0.00	1.00								
5	Industry Changer Dummy	0.34	0.47	0.00	1.00	0.07	0.06	-0.00	0.46	1.00							
6	Post-Job-Change Dummy	0.66	0.47	0.00	1.00	0.24	0.28	-0.22	0.30	0.14	1.00						
7	Industry Status Score	14.24	19.11	-62	51.00	0.08	0.08	0.01	0.05	0.04	-0.05	1.00					
8	Status Leaper Dummy	0.14	0.35	0.00	1.00	0.05	0.03	0.03	0.26	0.56	-0.03	0.02	1.00				
9	Status Leaker Dummy	0.20	0.40	0.00	1.00	0.04	0.04	-0.03	0.32	0.70	0.19	0.04	-0.20	1.00			
10	Female	0.08	0.27	0.00	1.00	-0.03	-0.00	-0.06	0.00	0.01	0.02	0.04	0.01	0.01	1.00		
11	Tenure	4.17	3.03	1.00	20.00	0.23	0.25	-0.15	0.18	0.06	0.11	-0.01	-0.04	0.11	0.01	1.00	
12	Executive Age	52.62	6.71	30	79.00	0.25	0.30	-0.10	0.09	0.03	0.43	-0.02	-0.04	0.07	-0.03	0.37	1.00
13	CEO Indicator	0.33	0.47	0.00	1.00	0.44	0.45	0.09	0.11	-0.02	0.18	0.02	-0.01	-0.02	-0.08	0.19	0.25
14	Return on Assets (ROA)	0.08	0.12	-3.08	0.91	0.13	0.08	0.03	0.03	0.06	-0.01	0.10	0.01	0.06	0.02	0.05	0.03
15	Total Debt	0.25	0.22	0.00	2.42	0.12	0.11	-0.05	0.02	-0.01	0.05	-0.04	-0.03	0.02	0.03	0.04	0.05
16	Capital Expenditures	0.04	0.05	0.00	0.70	-0.05	-0.06	0.05	-0.04	0.01	-0.08	-0.22	-0.04	0.05	0.01	-0.02	-0.02
17	Sales	7.61	1.57	-0.89	12.91	0.55	0.49	-0.03	0.19	0.15	0.15	0.05	0.07	0.12	0.04	0.13	0.19
18	Cash	0.15	0.16	-0.00	0.91	-0.09	-0.16	0.02	-0.00	0.02	-0.01	0.14	0.04	-0.01	-0.01	-0.03	-0.10
19	Profit Margin	-0.00	1.57	-103	8.43	0.05	0.03	0.01	0.02	0.01	0.01	-0.01	0.00	0.01	0.00	0.02	0.02
20	Sales Growth	0.20	5.25	-1.00	378.91	-0.00	-0.00	0.03	-0.01	0.00	-0.01	-0.02	0.02	-0.01	-0.01	-0.02	-0.00
21	Value of Acquisitions	221.59	1415	-3698	43123	0.14	0.09	0.02	0.03	0.02	0.04	0.01	-0.01	0.03	-0.02	0.03	0.02
22	Average Industry Pay	7.85	0.30	6.19	8.90	0.16	0.12	-0.19	-0.05	-0.07	0.24	0.27	-0.04	-0.04	0.07	0.17	0.15
23	Board Size	9.63	2.41	1.00	26.00	0.24	0.25	0.03	0.07	0.03	0.01	0.02	0.00	0.04	0.02	0.08	0.10
24	CEO Duality	0.50	0.50	0.00	1.00	0.07	0.06	0.13	0.02	0.02	-0.06	-0.01	-0.01	0.04	-0.05	0.02	0.06
25	Board Independence	0.68	0.18	0.00	1.00	-0.08	-0.05	-0.21	-0.06	-0.05	0.10	-0.07	-0.02	-0.03	0.05	0.08	0.08
26	Board Directorship	10.04	3.98	2.27	32.44	0.12	0.08	0.04	0.06	0.03	0.00	0.01	0.03	0.02	0.00	-0.03	0.02
27	Board Network Size	1689	935	27.7	7135.5	0.32	0.20	0.00	0.11	0.12	0.06	0.08	0.09	0.06	0.02	0.03	0.02
28	Board Gender Diversity	0.23	0.14	0.00	0.50	0.14	0.15	-0.05	-0.01	0.07	0.06	0.04	0.05	0.04	0.17	0.09	0.10
29	Board Industry Diversity	0.56	0.23	0.00	0.92	0.21	0.18	-0.02	0.16	0.20	0.07	0.02	0.11	0.14	0.01	-0.02	0.08

		13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28
14	Return on Assets (ROA)	0.02	1.00														
15	Total Debt	-0.01	-0.07	1.00													
16	Capital Expenditures	0.03	-0.01	0.05	1.00												
17	Sales	0.02	0.21	0.18	-0.01	1.00											
18	Cash	-0.01	0.01	-0.32	-0.10	-0.26	1.00										
19	Profit Margin	0.00	0.20	-0.01	-0.01	0.12	-0.02	1.00									
20	Sales Growth	-0.01	-0.01	0.00	-0.01	-0.04	0.00	-0.00	1.00								
21	Value of Acquisitions	-0.01	0.00	0.05	-0.04	0.18	-0.05	0.01	-0.00	1.00							
22	Average Industry Pay	-0.05	-0.04	0.06	-0.05	0.11	0.04	0.03	0.00	0.05	1.00						
23	Board Size	-0.03	0.02	0.12	-0.04	0.45	-0.23	0.02	-0.01	0.09	0.01	1.00					
24	CEO Duality	0.01	0.04	0.07	0.06	0.13	-0.07	0.01	-0.00	0.03	-0.08	0.09	1.00				
25	Board Independence	-0.05	-0.07	0.01	-0.01	-0.15	-0.08	0.01	-0.00	-0.03	0.08	0.07	-0.02	1.00			
26	Board Directorship	0.00	-0.09	0.11	0.00	0.13	-0.01	-0.01	0.02	0.03	0.04	0.17	-0.03	0.18	1.00		
27	Board Network Size	-0.01	0.00	0.09	-0.04	0.41	0.08	0.01	-0.01	0.15	0.11	0.34	0.03	0.06	0.36	1.00	
28	Board Gender Diversity	-0.01	0.09	0.09	0.02	0.27	-0.15	0.03	-0.02	0.05	0.08	0.30	0.12	0.21	0.10	0.27	1.00
29	Board Industry Diversity	0.01	0.04	0.13	-0.00	0.35	-0.10	-0.00	0.01	0.05	-0.01	0.29	0.06	0.13	0.31	0.39	0.26

Notes: Correlation coefficients with an absolute value greater than 0.02 are significant at the .05 level.

Table 2.4. Core Regression Results (Dependent Variable: Log Total Compensation)

	Industry Changing		Industry Status		Non-CEO vs. CEO	
VARIABLE	**Model 1** (all observations)	**Model 2** (+/- 3 years)	**Model 3** (all observations)	**Model 4** (+/- 3 years)	**Model 5** Only non-CEO (+/- 3 years)	**Model 6** Only CEO (+/- 3 years)
(A) Industry Changer × Post-Job-Change (β_1)	-0.11***	-0.10**			-0.08*	-0.26*
	(0.03)	(0.04)			(0.04)	(0.15)
(B) Status Leaper × Post-Job-Change (λ_1)			-0.17***	-0.16***		
			(0.05)	(0.05)		
(C) Status Leaker × Post-Job-Change (λ_2)			-0.03	-0.02		
			(0.05)	(0.06)		
Female	-0.02	-0.05*	-0.00	-0.03	-0.03	-0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.23)
Tenure	0.01***	0.01*	0.01***	0.01	0.01	-0.01
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)
Executive Age	-0.00	-0.00	-0.00	-0.00	-0.00*	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Return on Assets (ROA)	0.26***	0.38***	0.27***	0.48***	0.47***	0.05
	(0.07)	(0.09)	(0.09)	(0.11)	(0.12)	(0.36)
Total Debt	0.02	0.03	0.04	0.05	0.05	0.09
	(0.02)	(0.05)	(0.05)	(0.06)	(0.07)	(0.16)
Capital Expenditures	-0.19	-0.43*	-0.30	-0.44*	-0.26	-1.00
	(0.19)	(0.24)	(0.22)	(0.26)	(0.26)	(0.75)
Sales	0.14***	0.16***	0.14***	0.17***	0.17***	0.21***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)
Cash	0.13**	0.15**	0.07	0.13	0.17**	0.01
	(0.05)	(0.07)	(0.07)	(0.08)	(0.09)	(0.25)
Profit Margin	-0.01	-0.01	-0.01*	-0.01***	-0.01***	0.06*
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.04)
Sales Growth	0.00	0.00	0.00	0.00	0.00	-0.03
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.03)
Values of Acquisitions	0.00**	0.00	0.00*	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Average Industry Pay	0.30***	0.26***	0.30***	0.26***	0.32***	0.18
	(0.05)	(0.07)	(0.06)	(0.08)	(0.09)	(0.18)
Compensation (1-year lag)	0.55***	0.51***	0.57***	0.50***	0.48***	0.45***
	(0.01)	(0.02)	(0.03)	(0.03)	(0.03)	(0.05)
Board Size	-0.00	0.00	-0.00	-0.00	-0.00	0.01
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)
CEO Duality	-0.01	-0.01	0.01	0.02	0.01	0.11*
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.06)
Board Independence	-0.09*	-0.09*	-0.08	-0.07	-0.09	-0.05
	(0.05)	(0.05)	(0.05)	(0.06)	(0.07)	(0.14)
Board Directorship	0.00	0.00	0.00	0.01*	0.00	0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Board Network Size	0.00***	0.00**	0.00*	0.00*	0.00**	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Board Gender Diversity	-0.00	-0.05	-0.04	-0.06	-0.03	-0.41*
	(0.06)	(0.07)	(0.06)	(0.08)	(0.08)	(0.23)
Board Industry Diversity	-0.04	-0.01	-0.04	-0.01	-0.02	-0.03
	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)	(0.17)
Non-CEO to non-CEO	-0.08*	-0.06	0.01	0.03	0.05	
	(0.05)	(0.05)	(0.07)	(0.07)	(0.06)	
Non-CEO to CEO	0.27***	0.34***	0.34***	0.45***	0.47***	
	(0.05)	(0.06)	(0.07)	(0.08)	(0.07)	

CEO to non-CEO	0.19***	0.28***	0.35***	0.45***		0.03
	(0.07)	(0.07)	(0.08)	(0.10)		(0.49)
CEO to CEO	0.29***	0.38***	0.32***	0.44***		0.05
	(0.05)	(0.05)	(0.08)	(0.08)		(0.47)
Post-Job-Change	0.15***	0.13***	0.15***	0.13***	0.17***	-0.14**
	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.07)
Constant	-0.60	-0.05	-0.59	-0.06	-0.16	0.23
	(0.43)	(0.58)	(0.43)	(0.57)	(0.23)	(1.36)
Year Dummies	YES	YES	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES	YES	YES
Observations	7,610	4,368	7,610	4,368	3,615	753
R-squared	0.67	0.64	0.67	0.64	0.63	0.63
Wald Test: Coefficient on (B) = (C) ?			4.86**	3.48*		
Comparing Industry Changing CEOs and non-CEOs in Models 5 and 6						T -statistic = 3.15*

Robust standard errors clustered at the executive level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.5. Dynamic Effects

VARIABLE	**Model 1** Industry Change	**Model 2** Status Leap	**Model 3** Status Leak
t-3	-0.00	0.01	-0.05
	(0.09)	(0.11)	(0.13)
t-2	0.02	-0.10	0.09
	(0.08)	(0.08)	(0.15)
t-1	-0.02	-0.02	-0.01
	(0.08)	(0.09)	(0.14)
t+1	-0.13**	-0.24***	0.05
	(0.07)	(0.09)	(0.11)
t+2	-0.11*	-0.18**	0.03
	(0.06)	(0.07)	(0.09)
t+3	-0.06	-0.14*	0.04
	(0.06)	(0.08)	(0.09)
All Control Variables	YES	YES	YES
Industry Fixed Effects	YES	YES	YES
Year Fixed Effects	YES	YES	YES
Observations	4,368	4,368	4,368
R-squared	0.64	0.64	0.64

Notes: The dependent variable for all models is the natural log of total compensation. All models include all controls and fixed effects used in the core regressions. Robust standard errors clustered at the executive level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.6. Separate Core Regressions by Dependent Variable: Base Salary and Bonus

	Dependent Variable: Salary				Dependent Variable: Bonus			
	Industry Changing		Industry Status		Industry Changing		Industry Status	
VARIABLE	Model 1	Model 2 (+/- 3 years)	Model 3	Model 4 (+/- 3 years)	Model 5	Model 6 (+/- 3 years)	Model 7	Model 8 (+/- 3 years)
(A) Industry Changer × Post-Job Change	-0.07**	-0.06*			0.13	0.13		
	(0.03)	(0.03)			(0.18)	(0.19)		
(B) Status Leaper × Post-Job Change			-0.12**	-0.11**			0.00	0.02
			(0.04)	(0.04)			(0.23)	(0.24)
(C) Status Leaker × Post-Job Change			0.00	0.00			0.05	0.03
			(0.04)	(0.04)			(0.25)	(0.29)
Constant	2.83***	2.94***	2.84***	2.95***	7.82***	7.34***	7.88***	7.29***
	(0.42)	(0.40)	(0.42)	(0.40)	(2.26)	(2.39)	(2.32)	(1.99)
Post-Job Change	0.12***	0.13***	0.12***	0.13***	-0.34***	-0.43***	-0.34***	-0.43***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.11)	(0.12)	(0.11)	(0.12)
All Control Variables	YES	YES	YES	YES	YES	YES	YES	YES
Year Dummies	YES	YES	YES	YES	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES	YES	YES	YES	YES
Observations	7,610	4,368	7,610	4,368	7,610	4,368	7,610	4,368
R-squared	0.54	0.54	0.54	0.54	0.40	0.40	0.40	0.40
Wald Test: Coefficient on (B) = (C) ?			2.34*	2.27*			0.37	0.24

Notes: The dependent variable for Models 1 to 4 is the base salary (log). For Models 5 to 8, the dependent variable is bonus (log). All models include all controls and fixed effects used in the core regressions. Models 1, 3, 5, and 7 include all observations in the matched sample, whereas Models 2, 4, 6, and 8 use observations that are within the 3-year windows before and after the job change.

Robust standard errors clustered at the executive level are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2.7. Effect of Industry Relatedness (Dependent Variable: Log of Total Compensation)

VARIABLE	**Model 1** Industry Changers only -/+ 3-years
***Industry relatedness*$_{ij}$** × Post-Job-Change	**-0.11**
(Industry similarity as per 10-K product descriptions)	**(0.22)**
Post-Job-Change	0.07
	(0.07)
Constant	0.27
	(1.11)
All Control Variables	YES
Year Dummies	YES
Industry Dummies	YES
Observations	1,402
R-squared	0.56

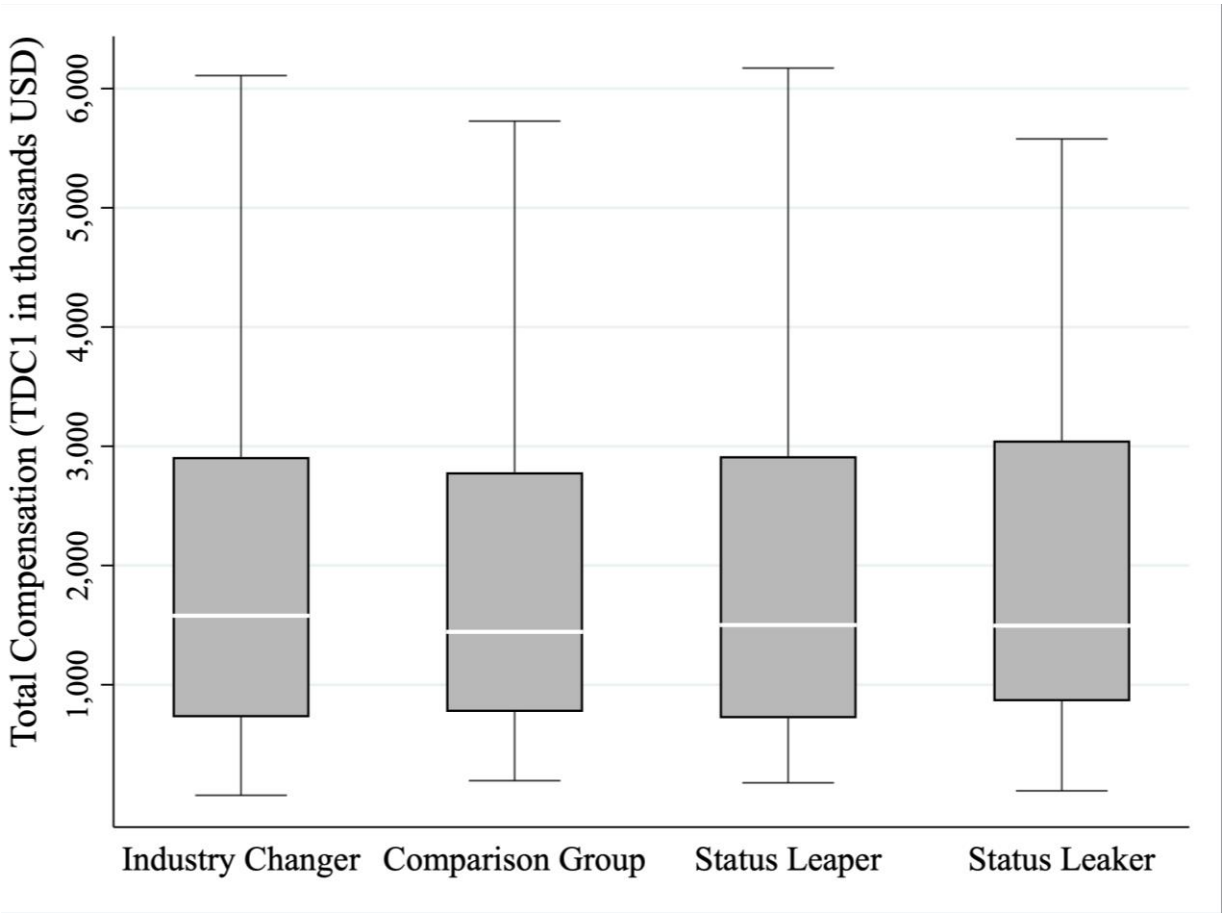
Notes: Model 1 includes observations for only industry changers. The negative insignificant interaction coefficient shows that moving to a related industry does not increase executive compensation, i.e., the industry changing discount does not weaken when industry changers move to similar/related industries.

Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.8. Effect Sizes on Compensation of Industry Changers vs. Comparison Group
(in thousands USD)

Variable	**Pre-job-change**	**Post-job-change**	***Row Difference***
Industry Changer	3089.39	3323.34	***233.95***
Comparison Group	3099.73	3712.21	***612.48****
Column Difference	***-10.34***	***-388.87****	***-378.53****

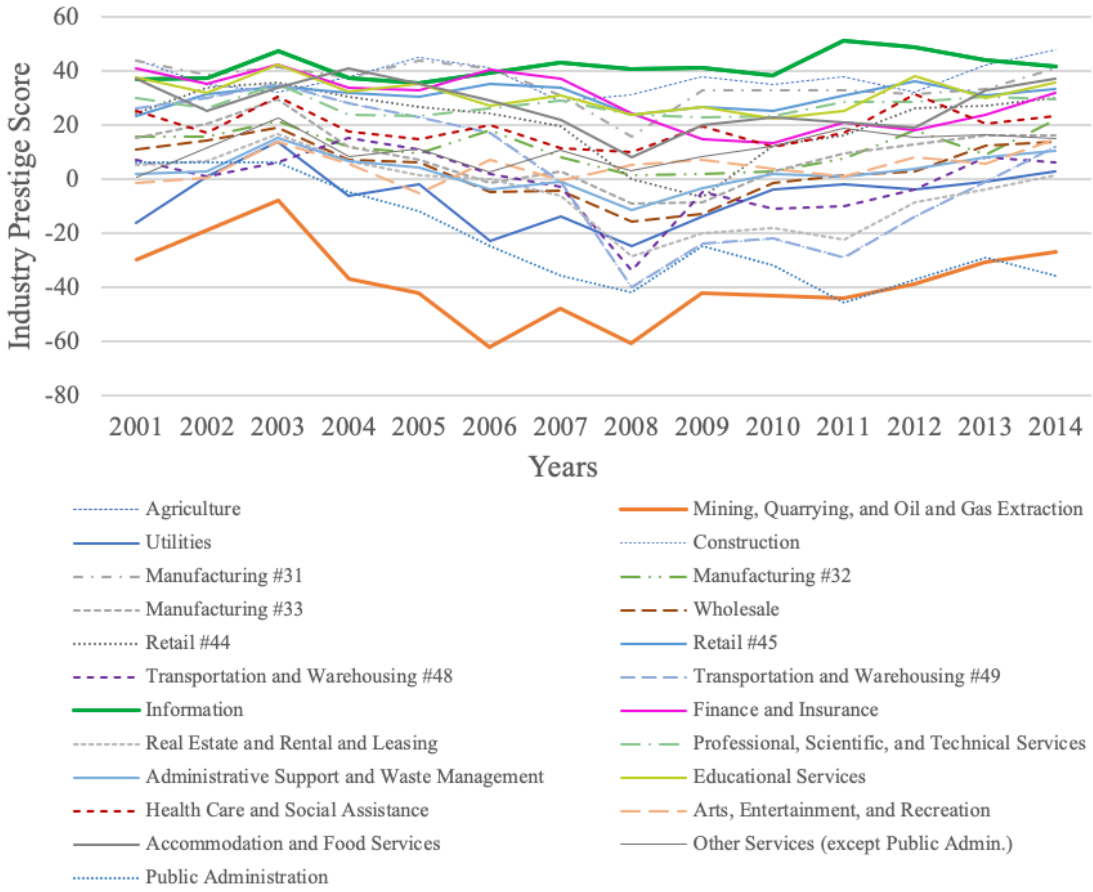
Figure 2.1. Compensation Distributions of Different Executive Groups in the Period before Job Change



Notes: The lower side of each box shows the 25th percentile of distribution, the upper side shows the 75th percentile, and the white line inside shows the median executive compensation for each group. Whiskers indicate minimum and maximum values (extreme outliers were excluded).

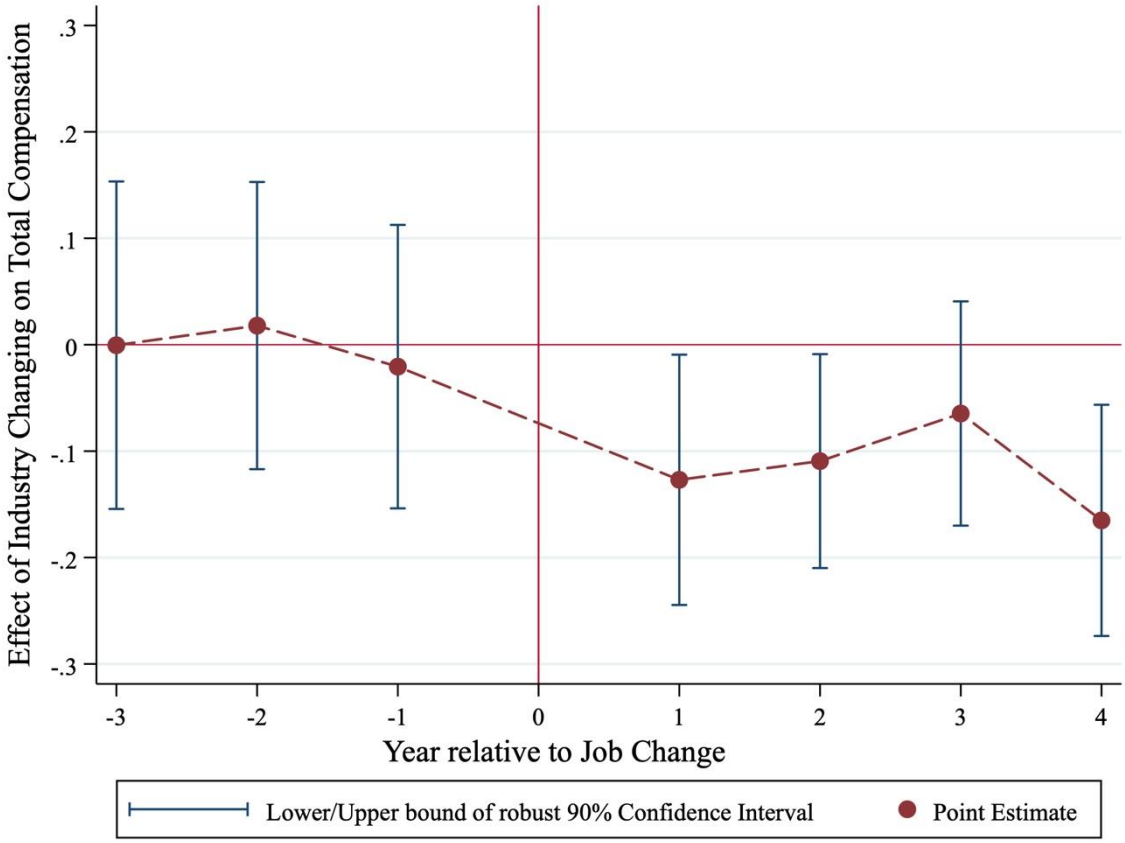
This graph shows that in the period just before the job change, the median compensation for different treatment groups is substantively similar. In addition, treatment and comparison groups exhibit very similar distributional characteristics, supporting the exchangeability assumption.

Figure 2.2. Industry Prestige Scores by Year



Notes: Industry prestige ratings are derived from Gallup's annual business and industry sector ratings. We calculate net industry prestige scores by subtracting total negative responses from total positive responses for the Gallup survey question addressing the perception of industry prestige. Because Gallup started to conduct surveys in 2001, for executives who changed jobs in 1999 and 2000, we use the 2001 ranking, which is the earliest available Gallup ranking.

Figure 2.3. Dynamic Effects of Industry Changing on Total Compensation



Notes: Estimates of industry changing effects on executive compensation in each year before and after the job change are plotted as connected red dots; 90% confidence intervals corresponding to robust standard errors (clustered at the executive level) appear as blue bars. The models include all controls and fixed effects used in the core regressions.

Blue bars consistently include the x-axis in pretreatment periods, indicating the absence of differences in compensation between the comparison and treatment groups in pre-job-change periods. In post-job change periods, the compensation pattern of industry changers significantly diverges in a negative way from that of the comparison group.

3. Chapter 2: Crisis-related Wage Cuts and Employee Turnover: Who Leaves? Why? Where to?

Abstract

This study investigates the effects of wage cuts -used as a crisis-response strategy- on voluntary employee turnover. Drawing on social comparison theory and the unfolding model of turnover, we suggest that crisis-related wage cuts operate as a push shock that increases employees' tendency to evaluate other employment alternatives. Specifically, during crisis times, we expect employees to update their future pay prospects according to the wage changes of external horizontal referents, i.e., similar others holding similar occupations in the same industry, instead of within-firm referents. Thus, we hypothesize that this shift in anchoring benchmarks will result in a higher likelihood of turnover when an employee's wage cut is higher than those of the external referents (relatively higher wage cut). Considering the differences in availability of information on wages and in anchoring bias of high- vs. low-skilled employees, we then hypothesize that the effect of relatively higher wage cut on turnover is stronger for high-skilled employees; and that, conditional on turnover, high-skilled employees are more likely to stay in the same industry, either by going to a competitor or establishing a spinoff firm. We test our hypotheses with continuous working histories of a representative sample of Spanish labor force, a panel dataset built with social security and tax records (N=2,183,478). Findings from compensation changes and subsequent career movements around the financial crisis (2007-2011) provide support for our hypotheses. By shedding light on employees' updated anchoring beliefs about outside options in times of crisis, this study advances the literature on the nexus of social comparison theory and turnover as well as provides practical insights to organizations facing the challenge of surviving a crisis while retaining firm-specific human capital.

3.1. Introduction

Because human capital is a crucial component of value creation (Bidwell & Keller, 2014; Campbell, Coff, & Kryscynski, 2012), and replacing an employee equipped with firm-specific skills is a costly endeavor (Berry, Lelchook, & Clark, 2012), voluntary turnover is generally an undesirable outcome for organizations (Wynen & Op De Beeck, 2014). Yet, the unfolding model of turnover suggests that an organization's employment system is in a steady-state and thus employees tend to stay in their firms unless there is a

push or pull shock that triggers employee movement (Lee, Mitchell, Holtom, McDaniel, & Hill, 1999; Lee, Mitchell, Wise, & Fireman, 1996). Together with other individual factors and organization-level changes, reducing compensation has been identified as one of the main push shocks that could propel employees to quit. Individual and organizational changes constituting such shocks, including wage changes, as well as their effects on individuals' career trajectories and reconfiguration of human capital across firms, have attracted scholarly attention (Pitts, Marvel, & Fernandez, 2011; Wynen & Op De Beeck, 2014). However, research on the impact of global shocks on employees' individual-level career decisions is relatively scarce.

One such external shock is economic downturns. In times of crisis, organizations may be required to cut labor costs, even though wage cuts lead to dissatisfaction, the desire to retaliate, and voluntary turnover (Larkin, Pierce, & Gino, 2012). Macrolevel analyses, that could inform decision makers about the consequences of wage cuts on employee turnover, have examined average levels of wage deceleration, freezes or cuts and their implications on general levels of unemployment during and after the crises (e.g., Adkins, Werbel, & Farh, 2001). However, global crises and their associated wage cuts could potentially have consequences on individuals' careers, which are hard to observe at the macro level. For instance, during the 2008 financial crisis, disproportionately more low-wage jobs were cut. This asymmetric layoff probability, and the fact that fired workers were mostly low wage, implied a growth on average wages, although there was not an effective increase in individuals' wages (Eurofund, 2013). Thus, the effects of crisis-related wage adjustments on individuals' career trajectories and firm compositions following the compensation change are indistinguishable unless individual job spells and compensation changes are analyzed synchronically with organization-, occupation-, and industry-level parameters. Overall, there is growing evidence that financial and economic crisis is positively related to turnover intention (Adkins et al., 2001). However, it is unclear whether and how wage cuts explain employees' voluntary turnover, and which employees are more likely to actually quit after wage cuts.

This study addresses these issues by drawing on social comparison theory, examining the relationship between wage cuts and turnover decisions, and exploring who quit their jobs after wage cuts. A central argument in the research on social perceptions of wage cuts (Fehr, Goette, & Zehnder, 2009) and social comparisons (Larkin et al., 2012;

Obloj & Zenger, 2017) is that employees' exit tendencies are determined by their comparative evaluations of the earnings they thought they deserved with what others received (Kacperczyk & Balachandran, 2018). Specifically, social comparison costs from pay inequality occur when employees benchmark their pay against that of their "salient referents" (Festinger, 1954). Unlike local shocks such as a decline in revenues of a particular firm, global crises presumably affect all firms in the same industry in similar ways. Since external referents comprise the market "benchmark" for employees and the pay of these market benchmarks are also likely to go through a wage adjustment during the crisis, these peers who perform similar roles in peer firms become a relevant comparison set against which to benchmark pay. Thus, we expect a focal employee to be cognizant of the changing market benchmark and use this reference point when comparing the utility inside the firm vs. his or her available utility outside the firm, even more so during global external shocks. To this end, we conceptualize "relatively higher wage cut" as a categorical status that indicates a higher percentage wage cut than the average experienced by external horizontal referents in a given year; "external horizontal referents" being defined as those working in (a) similar occupations and (b) in different firms operating in the same industry. We first hypothesize that if a focal employee experiences a relatively higher wage cut, she is more likely to conclude that her organization deviates from what ought to be done with regards to wage cutting measure, and thus more likely to consider alternatives and leave her firm.

This baseline proposition depends on the assumption that employees are informed about and have accurate beliefs of the changes in peer salaries. However, research shows that especially low-wage workers tend to wrongly anchor their beliefs and to underestimate their outside options (Jäger, Roth, Roussille, & Schoefer, 2022). In other words, high-skilled workers are more likely than low-wage ones to identify a higher relative wage cut when they experience one. Moreover, research shows that information pertaining to high-level roles is more likely to circulate (Meindl & Ehrlich, 1987). Because of the discrepancy in anchoring bias and in the availability of information for low- vs. high-level roles, we hypothesize that the baseline positive relationship between a relatively higher wage cut and turnover will be stronger for high-skilled employees than low-skilled ones. Relatedly, we also expect destination addresses to reflect these differences in availability of information for different-level roles. In other words, perception of opportunity on which quitters intend to capitalize should reflect these

differences. Given that there is more available information about higher-level roles and their wage alterations, conditional on turnover, quitting high-skilled employees' likelihood to stay in their industry will be higher than that of low-skilled employees.

We test our hypotheses with career and compensation evolution data of more than 600,000 individuals in Spain. One important challenge in testing these arguments is that observable and unobservable factors may jointly drive the relationship between a focal firm's action of a relatively higher wage cut and quitting decision of an individual working for the focal firm. For instance, firms have been shown to recalibrate wages during and after M&A deals (Arnold et al., 2021), and voluntary turnover could be especially high in such a situation. Alternately, wage cuts could be a function of a focal firm's decreasing performance, a particularity of the region where a firm is located, or an industry-level disruption related to the type of economic activity a focal firm is engaged with. Accordingly, any of these confounds, rather than a relatively higher wage cut, could drive individuals' quitting decisions.

We tackle this empirical challenge in two main ways. First, we control for an extensive set of firm-, industry- and region-level characteristics that prior studies have shown to predict employee turnover. Second, we do not rely on a comparison with the wage cuts of within-firm referents, which should reflect idiosyncratic firm characteristics and could be affected by social comparison (Gartenberg & Wulf, 2017). Instead, we exploit average changes in industry-region-occupation cells to capture the market-imposed (rather than firm-specific) change in benchmark pay during and after the crisis. To do so, we construct "synthetic" measures of wage cut differences based on the comparison between the wage change of a focal employee and average pay-change levels of external referents in the same industry, region, and occupation. The variation in synthetic measure of a relatively higher wage cut arises from regional and industry pay shocks, which are more likely to be a function of labor or demand-side conditions associated with the crisis than of the specific characteristics of the firms in which quitting employees are employed. We exploit this variation for our analysis by testing the relationship between a relatively higher wage cut for a focal employee and the likelihood of quitting. Consequently, we are able to measure the relationship between the changes in pay levels of external referents and the likelihood of quitting, while downplaying the confounding effects of firm-specific factors.

3.2. Theoretical Background

Theoretical accounts in labor economics, human resources and social psychology fields have identified individual, organizational (work-related), and external environmental factors that determine employee turnover (see Mawdsley & Somaya, 2016 for a review). A relevant theory for studying disruptions to employment systems and the factors that prompt turnover is the unfolding model of turnover (Lee et al., 1999, 1996). In essence, the model suggests that the employment system is in a steady state unless it is disrupted by 'shocks to the system'. These shocks alter the steady state of how employees think about their jobs and firms, make employees reevaluate their positions, and quit their jobs (Bonet, Elvira, & Visintin, 2022).

Research has studied individual- and organizational-level shocks that trigger employee movement, such as a sudden increase in relational capital of an individual (Byun, Raffiee, & Ganco, 2019) or performance fluctuations of a firm. Although less widely studied, several external factors are thought to influence turnover, such as unemployment rate, accession rate, and unionization rate (Cotton & Tuttle, 1986). Relevant to this study, the financial and economic crisis has many implications for career decisions of employees. With the market-imposed pressure of "doing more with less," organizations focus on reducing costs by adjusting employees' direct pay or other monetary rewards (Davidson, Nemec, & Worrell, 2001; Yoon, 2022). At the individual level, this can lead to feelings of job insecurity and a subsequent decrease in organizational commitment and a stronger intent to quit (Adkins et al., 2001; Greenhalgh & Rosenblatt, 1984).

When making such assessments and evaluating their jobs, individuals are inclined to engage in social comparisons, particularly to compare their wage changes with those of peers and salient referents (Levine, 1993). Labor economics literature documents two main underlying reasons as to why peer salaries affect the utility of employees gained from their own salaries. First, according to relative income model (Clark & Oswald, 1996), when an individual's earnings fall relative to the comparison level, an employee feels relatively deprived and unhappy. Second, from a rational updating viewpoint, employees rationally use the info on peer salaries to update their own future pay prospects-even if they do not feel deprived over peer salaries. Because peer wages provide

a signal about their own future wages, if an employee learns that his or her own salary is lower (higher) than peers' salaries, he or she will update expected future compensation upward (downward) (Sandvik, Saouma, Seegert, & Stanton, 2021).

Along similar lines, behavioral and social psychological perspectives in human resources literature examine social perceptions of pay, highlighting the importance of "fairness" concerns (Fehr et al., 2009) and social comparisons (Larkin et al., 2012; Obloj & Zenger, 2017). Fueled by theories of justice and fairness, this stream of literature has extended traditional agentic models with the view that humans make sense of their worth and standing through comparisons with others, comparisons that often include pay as a benchmark (e.g., Bloom & Michel, 2002; Kacperczyk & Balachandran, 2018; Pfeffer & Langton, 1993). Hence, the main argument of these theories is that employees' attitudes and behaviors are determined by their comparative evaluations of what they deserved compared to their judgments and information about what others received (Weick, 1966).

However, there are theoretical and empirical concerns about making inferences from this extant literature concerning the effect of wage cuts on employee turnover at crisis periods. Most importantly, since the seminal work of Festinger (1954), researchers drawing on social comparison theory have provided different answers to the central question in social comparison, that is "Who is the salient referent?". Moreover, we do not know much about whether referent groups shift at times of crisis (e.g., from vertical to horizontal, from within-firm to external referents). Another complication is that this kind of an investigation requires granular longitudinal data which provide information on individuals, firms, occupations, and industries concurrently—which are rarely available to researchers.

3.3. Hypothesis Development

3.3.1. The Effect of Wage Cut on Individuals' Exit Intentions

Overall, financial and economic crises have been associated with increased levels of turnover. Thus, consistent with the unfolding model of turnover, an economic crisis might constitute a shock that leads employees to consider potential alternative work arrangements. However, it is not clear "who" among the employees in a firm reacts to wage cuts with a stronger intention to quit. Moreover, because the unfolding model of

turnover is relatively silent with regards to the next destinations of the exiting employees (Byun et al., 2019), studies using this framework generally do not examine where employees who quit go. Knowing the characteristics of the quitting employees and their destinations, i.e., identifying whether quitters are high-skilled and if they join a firm's competitors, is important for organizations in order to assess the consequences of the wage cut decisions on competitive dynamics post-crisis.

To better understand how employee quit behavior is affected by the wage cuts at crisis times and to explore the heterogeneities in the destinations of the quitting employees, we draw on social comparison perspectives considering the potential changes in the benchmark pays as a consequence to crisis. Social comparison costs occur when there is a gap between one's own salary and that of peers who may be located either within or outside the firm, i.e., when employees benchmark their pay against that of salient within-firm or external referents (Festinger, 1954; Greenberg, Ashton-James, & Ashkanasy, 2007). These costs include but are not limited to reduced productivity (Obloj & Zenger, 2017), reduced teamwork (Gino & Pierce, 2009; Shaw, Gupta, & Delery, 2002), and increased turnover (Kacperczyk & Balachandran, 2018).

We argue that whereas local shocks (that typically affect only a limited group of individuals and/or organizations) trigger comparisons with within-firm referents, a global shock such as a financial crisis should elicit a comparison perspective that is relatively more concerned about external referents, i.e., market benchmarks (Maertz & Campion, 2004). In other words, we expect employees to be cognizant of wage changes of similar others who are outside their organizations and are supposed to suffer similar consequences of the crisis. Consequently, we suggest that an employee may still perceive a wage cut as unfair and evaluate outside options even if the cut does not negatively diverge from those of within-firm referents, i.e., wage cuts do not alter pay hierarchies within the firm.

This interpretation is also aligned with the theoretical accounts on relative deprivation. Perception of pay equity involves a ratio of outcomes to inputs as perceived by the person (Weick, 1966). As the reviewed literature suggests, questions of equity arise when a person compares their ratio with that of coworkers, supervisors, or peers outside the firm. Deprivation occurs when people compare themselves to others who are better

off and conclude that their disadvantage is undeserved. In other words, relative deprivation occurs when people compare their situation with alternative options using the principle of what "ought to be" (Smith, Pettigrew, Pippin, & Bialosiewicz, 2012). Previous research shows that egoistic relative deprivation, defined as people's feelings of deprivation due to their dissatisfaction with their position as an individual, predicts employee turnover intention. Hence, it is rather not the size of the "inequity gap" that triggers turnover as much as how people make sense of why the gap exists and whether anything could be done about it (Cho, Lee, & Kim, 2014).

We therefore define "external horizontal referents" as those working in (a) similar occupations and (b) in different firms operating in the same industry. We conceptualize "relatively higher wage cut" as a categorical status that indicates a higher percentage wage cut than the average experienced by external horizontal referents in a given year. Because external horizontal referents are supposed to suffer similar consequences of the crisis, employees may still experience the fairness-related implications of a wage cut if their wage cuts negatively diverge from those of similar others that work in similar occupations, in different firms in the same industry. We suggest that, at crisis periods, people determine whether their wage cut was fair and whether options exist by looking at similar organizations presumably suffering the same crisis' consequences. Looking at similar organizations' responses, if employees judge that their own organization deviates from what ought to be done with regards to wage cutting measure, they are more likely to consider alternatives and leave their firms.

Our baseline hypothesis follows:

> ***Hypothesis 1:*** *A higher wage cut than that experienced by similar others holding the same occupation in different firms in the same industry (i.e., relatively higher wage cut) is positively related to an employee's likelihood of quitting.*

3.3.2. Differing Effects for High- vs- Low-skilled Employees

Thus far, we have argued that higher wage cuts compared with external horizontal referents will spur employee exit. Proposed theoretical arguments draw upon social comparison and relative deprivation theories. These arguments rely on the implicit

assumption that the information about the compensation reductions of similar others are available to individuals who engage in social comparisons when rationally updating their beliefs about potential outside opportunities. However, the availability of such information varies across organizational levels and occupations. In addition, when there is noise in the available information on peer salaries, precision of the anchoring beliefs about outside benchmarks differs for low- vs- high-skilled workers.

Specifically, managerial roles are important because of the symbolic role they play in interpreting and socially constructing reality (Pfeffer, 1981). In fact, Meindl and Ehrlich (1987) suggest that organizational constituencies make sense of outcomes due to the highly circulating nature of information pertaining to high-level roles. Moreover, there is evidence that high unemployment rates associated with crisis periods have little impact on the turnover of high-performing employees or those with in-demand skill sets (Trevor, 2001). Overall, due to the heterogeneities on the availability of information regarding the referent group and the persistent demand for high-skilled employees even during crises, we expect the effects of proposed comparative mechanisms on turnover to be stronger for employees in higher level occupations (held by presumably high-skilled employees).

In addition, research shows that workers wrongly anchor their beliefs about outside options on their current wage (Jäger et al., 2022). In particular, low-paid workers underestimate wages elsewhere. Jäger and colleagues, for example, with a representative survey of the German workforce, found that 10% of low-wage jobs would not be viable at current wages if low-wage workers had a more accurate grasp of the outside opportunities and compensation levels of benchmark outside opportunities. Because low-skill workers underestimate the wage evolution of external referents, their inaccurate beliefs about outside options make low-skill workers less likely to experience relative deprivation. Consequently, it is relatively easier for employers to lower wages of low-skill employees who underestimate the wages at other firms.

In sum, we propose a moderation effect of skill level based on three main premises. First, on the supply-side, because information pertaining to higher-level roles is more accessible, and high-skill employees have a better-informed idea about the external pay benchmark, social comparison mechanism underlying the baseline

relationship will be stronger for high-skilled employees. Second, unlike low-skill workers, high-skilled employees do not underestimate wages outside their firms. Hence, they are more likely to spot and experience a higher relative wage cut, and the subsequently leave. Third, on the demand side, those equipped with in-demand skill sets, i.e., high-skilled employees, are more likely to find an outside opportunity as the demand for such workers is less affected by financial downturns, on average. This positive moderation of the mechanisms underlying the baseline hypothesis denotes a stronger association between relatively higher wage cut and likelihood of quitting for high-skilled workers. In other words:

> ***Hypothesis 2:*** *The relationship between a relatively higher wage cut and higher likelihood of quitting is stronger for high-skilled than for low-skilled employees.*

In the first two hypotheses, we have treated employee exit as a "binary outcome of staying or leaving" following previous studies centered on the unfolding model of turnover (Lee et al. 1996: 7). However, there are heterogeneities in the destinations of the exiting employees, i.e., where leaving employees go. We suggest that destination addresses should reflect heterogeneities in the availability of information on outside opportunities. Just like the perceptions of justice and fairness are shaped by differences at referent groups, perception of opportunity on which quitters intend to capitalize should reflect these differences. In other words, due to the differences of availability of information to highly skilled vs. low skilled workers, the saliency of the relatively higher wage cut is higher for certain groups. Because they presumably have more information about the referent groups/opportunities in the same industry, high-skilled employees are more likely to stay in the same industry (by either going to an existing competitor or establishing a spin-off) than low skilled employees. Thus, our final hypothesis follows:

> ***Hypothesis 3:*** *Conditional on quitting following a relatively higher wage cut, the probability of remaining in the same industry (either by going to a competitor or establishing a spinoff) is higher for high-skilled employees than for low-skilled ones.*

3.4. Methods

3.4.1. Data and Research Setting

To test our hypotheses, we first need data on individuals' percentage changes in wages and turnover behavior. Additionally, because the main independent variable needs to capture whether the wage cut of a given individual was higher than that of the external horizontal referent group, we also need information on the entire wage distribution: the average wage change for each occupation-industry-year combination.

To combine all this information in the analyses, we culled data from multiple sources. The main data come from the Continuous Sample of Working Histories (CSWH) (Muestra Continua de Vidas Laborales con Datos Fiscales) from Spain's Social Security Office. These datasets contain matched anonymized social security, income tax, and census records for a four percent, non-stratified random sample of the population that had any relationship with Spain's Social Security in that year. These records provide information on individuals' complete labor market histories from 1980 (or the year the individual registers with Social Security) to the year of data collection.

With social security datasets, we track individuals, their demographic and educational backgrounds, occupations, and wages. However, earnings information from Social Security records is censored at both the top and the bottom. Given our inquiry focusing on wage differentials, upper capped earning records might have polluted the analyses. To overcome this challenge, we merge social security records with the tax records which have info on non-capped earnings from 2006 onwards, for all the individuals that could be tracked with social security records. Thus, although we collected data on individual-level wage determinants going back to 1980, our analyses target wages and turnovers in the 2007-2011 period around the financial downturn, for which we can extract noncapped individual earning records and firm-level information from the tax datasets. Finally, we use autonomous community-level datasets from the Spanish National Statistics Institute in order to integrate region-level covariates.

In sum, to test our hypotheses we build a person-year panel dataset (N=2,183,478), with which we track compensation changes of individuals and wage patterns of referent groups around the financial crisis for the period from 2007- to 2011.

3.4.2. Key Variables

Dependent Variable

In testing Hypotheses 1 and 2, the binary dependent variable is *employee exit*, coded "1" if an employee's primary employer had changed since the previous period and "0" otherwise. In the tests for Hypotheses 3, we focus on the destination of the leaving employees distinguishing within-industry mobility and cross-industry mobility. Accordingly, the dependent dummy variable is within-industry mobility that equals 1 if job changing employee moves within the same industry either by going to an existing firm or launching a spin-off. We use 2-digit CNAE93 code to operationalize industry, which is the national classification of economic activity used by the National Statistics Institute of Spain *(Instituto Nacional de Estadística – INE).*

Main Independent and Moderator Variables: Relatively Higher Wage Cut and Skill Level

The rationale behind operationalizing *relatively higher wage cut* is to compare the wage cut experienced by each employee who stayed in the same organization for 2 consecutive years, with the average compensation change of similar employees who work in the same industry holding similar occupations in a given year. After calculating yearly wage change for each individual in the dataset, we calculate the average compensation change for 3 occupation groups in 59 industries in each of the 4 years of data.[9] In order to make a careful comparison taking into account the varying number of days that employees worked in a year, for comparison, we use inflation-adjusted hourly wages calculated with total earnings and total number of days and hours worked in the corresponding year. In sum, we calculate 708 (=59 industries * 3 occupation groups * 5 years) average percentages which proxy for the average changes of the relevant external horizontal referent groups. If an employee's wage delta is lower than the corresponding average (for the same occupation group in a given year), *relatively higher wage cut* is

[9] Note that the panel includes observations for 5 years (2007-2011). However, we do not calculate relative wage cuts and average changes for the first year, that is 2007. We start to observe relative wage cuts by the year 2008, when the financial crisis started to dramatically affect the labor market.

coded 1, and 0 otherwise. For the moderation hypothesis, *high-skill* takes value 1 if a leaving employee has a college or above degree, and 0 otherwise.

Control Variables

Individual, firm, industry, and regional level characteristics have been shown to influence employee turnover (Agarwal, Gambardella, & Olson, 2016). That's why we include an extensive set of control variables and fixed effects in our analyses. Firm-specific human capital may reduce turnover (Jovanovic, 1979). Therefore, we include *tenure* in firm (along with its quadratic form) calculated as the number of years an employee worked in the current firm. Voluntary turnover is also impacted by age (Topel & Ward, 1992) and varies by gender (Loprest, 1992), migration status (Raphael & Riker, 1999), and education (Buchinsky & Leslie, 2010). Hence, we include *age* (and its quadratic form), *gender* (a dummy variable that equals 1 for women), *born in Spain* dummy (1 if the employee is born in Spain), and dummies for 4 different levels of education. In the models that include individual fixed effects, education, gender, and born in Spain dummies are omitted as they do not vary within employees. However, the hypothesized interaction effects with the main independent variable (relatively higher wage cut) can still be tested.

As each job spell is tied to a firm identifier in the tax data, we also incorporate several firm-level control variables to all models. We control for *firm age* and *firm size* as measured by the total number of employees. Because *part-time* and *fixed-term* contracts may affect employee behavior, we include dummies for part-time work and fixed-term contracts.

Following a Hausman test, we include individual employee fixed effects. To reduce potential omitted variable biases and to capture industry-specific issues that might affect turnover, we also control for macroeconomic factors in the forms of time-invariant industry effects (industry dummies for 16 industry groups), time effects (5 year dummies), and occupation dummies (3 categories). We also account for time-invariant factors associated with contextual conditions by using a region dummy for each Spanish autonomous community included in the analyses (17 autonomous communities excluding Ceuta and Melilla, that are on the African soil).

3.4.3. Empirical Strategy: Model

We test the hypotheses by estimating a series of linear regressions. For Hypotheses 1 and 2, we estimate variations of the following model to test the main effect of relatively higher wage cut on employee exit and the moderating effect of skill level, respectively.

We use following estimation formula:

$$exit_{it+1} = f\,[\varphi_i + \psi_t + \tau_s + \zeta_o + \eta_r + \beta_0 + \beta_1\, RelativelyHigherWageCut_{it} + \beta_2\, HighSkilled_i\, x\, RelativelyHigherWageCut_{it} + \beta_3\, ControlVariables_{it} + \varepsilon_{itsor}]$$

Where the dependent variable $exit_{it+1}$ is the likelihood of turnover for employee 'i' at annual period 't+1'; φ_i, ψ_t, τ_s, ζ_o, and η_r stand for individual, year, industry, occupation-group, and autonomous-community fixed effects, respectively. In this equation, we test Hypotheses 1 and 2 through the estimation of β_1 and β_2, respectively. We expect β_1 and β_2 to be positive. Next, we examine the likelihood of going to a competitor conditional on employee exit. We estimate the following equation:

$$Within\_Industry\_Movement \mid Exit_{it+1} = [\delta_0 + \delta_1\, RelativelyHigherWageCut_{it} + \delta_2\, HighSkilled_i\, x\, RelativelyHigherWageCut_{it} + \delta_3\, ControlVariables_{it} + \eta_{itsor}]$$

We expect δ_2 to be positive indicating: conditional on mobility following a relatively higher wage cut, the probability of remaining in the same industry is higher for high-skilled employees than the rest.

3.4.4. Results

Table 3.1 presents the summary statistics and correlations for the sample used for the analyses.

Insert Table 3.1

Table 3.2 presents the main results. We test Hypothesis 1 through the estimation of β_1, expecting *the coefficient for relatively higher wage cut* to be positive. In Model 1, we find a positive association between *relatively higher wage cut* and *employee exit* ($ß_1$= 0.055; p-value<0.001). Model 2 shows that this association is robust to the inclusion of controls and fixed effects detailed in the preceding sections. Specifically, the coefficient indicates that the probability of employee exit at annual period t+1 increases by 3.8% as relatively higher wage cut increases by one standard deviation at annual period t. Model 3 provides support for Hypothesis 2 showing that the likelihood of high-skilled employees (who experienced relatively higher wage cuts) to leave their organizations is higher than that of low-skilled employees ($ß_2$= 0.008; SD= 0.001). We then examine the destination firms of quitting employees in Model 5. The positive interaction coefficient in Model 5 (δ_2= 0.019; SD= 0.006) indicates that quitting high-skilled employees are more likely to stay within the same industry (H3). In other words, a given firm is more likely to lose a high-skilled employee to a competitor if it cuts wages at a higher rate than do its competitors.

Insert Table 3.2

3.4.5. Robustness Checks

Finally, we conduct a panel robustness tests. Because very small enterprises might have ad hoc employment practices, we rerun all estimations by excluding the data that come from companies with fewer than 10 employees (Bonet et al., 2022). The results still hold for both the baseline and moderation hypotheses. Regarding the hypothesis that examines destinations of the quitting employees (H3), we estimate the models with different measures of industry change (e.g., 3- and 4-digit NACE codes) and confirm a statistically significant support for Hypothesis 3 when employing different industry specifications.

Another possible concern is that we treat *relatively higher wage cut* as a categorical construct measured by a dummy variable because the existence of a wage-change gap between a focal employee and horizontal external referents is most relevant. We employ an alternative approach as a robustness test using a continuous variable measuring the gap. The association between a wage cut gap and quitting becomes more stronger when we operationalize wage gup as a continuous variable, providing further confidence in our findings.

3.5. Discussion

In this study, we have analyzed how crisis-related wage cuts may prompt social comparisons with external referents to influence employee's mobility decisions. In addition, we theorize on and document the differences in the turnover of high- vs. low-skilled workers and their destination firms (e.g., whether workers who quit join a competitor in the same industry), unveiling the effects of crisis-related wage cuts on individual careers and the inter-firm flow of human capital.

In testing our arguments, we take advantage of numerous empirical benefits of population-level longitudinal data extracted from Spanish Social Security and Tax records. Thanks to the information on wage evolution of a representative sample of the Spanish workforce, together with information at firm, industry, and occupation levels, we identify all mobility instances, how the wage-cut rates of mobile employees compare to the referent groups, and where the quitting employees go. The main analyses provide consistent support for our hypotheses. Post hoc analyses further confirm that social comparison may be driving the relationship between relatively higher wage cut and employees' exit decisions.

Our findings are significant not only because they provide empirical support for our core prediction that as social comparison costs increase, employees are increasingly likely to exit their firms, but also because they link the literatures on social comparisons and turnover, in the specific timeframe surrounding the financial crisis. In so doing, our paper makes significant theoretical contributions to each of these bodies of research.

Our findings also invite future research that might examine the effects of similar external shocks (e.g., Covid-19 outbreak) and differences in responses based on individual differences. As we noted, relative deprivation is a subjective process.

Consequently, as it does for all subjective processes, the antecedents and precedents of the process vary as per heterogeneities at priorities and sense making mechanisms across individuals. The decision to exit should also potentially vary with individuals' differing access to relevant information on benchmark pay and differing tendencies to engage in social comparisons. In this regard, previous research suggests that gender might play a role in such differences. Belliveau (2005), for example, find that women have social networks that are of less utility in acquiring information about pay levels of external referents. Our supplementary analyses provide some support to this view by showing that the association between relatively higher wage cut and turnover is slightly weaker for women than men. However, our large-scale datasets do not allow us to adequately examine the particularities. Future qualitative or experimental work could help understand how our proposed mechanisms might differ for women vs. men.

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3.7. Chapter 2 Tables and Figures

Table 3.1. Descriptive Statistics and Correlation Matrix

VARIABLE	Mean	S.D.	Min	Max	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1. Employee Exit	0.18	0.39	0.00	1.00														
2. Within Sector Mobility	0.08	0.27	0.00	1.00	0.74													
3. Relative Wage Cut (Dummy)	0.39	0.49	0.00	1.00	0.01	-0.13												
4. Female (Dummy)	0.45	0.5	0.00	1.00	-0.08	-0.07	-0.04											
5. Age	37.92	10.83	17.08	83.00	0.30	0.45	-0.37	0.00										
6. Age Squared	1555.64	876.33	291.84	6889	0.12	0.10	0.06	0.01	0.00									
7. Tenure	5.19	5.91	0.00	32.02	-0.01	-0.11	-0.04	0.58	-0.03	0.01								
8. Tenure Squared	61.9	137.32	0.00	1025	-0.01	0.02	-0.01	0.60	0.03	0.00	-0.31							
9. Experience	12.36	9.45	0.00	46.03	-0.01	0.01	-0.06	0.01	0.01	0.04	0.03	-0.02						
10. College Education or Higher	0.2	0.4	0.00	1.00	0.44	0.49	-0.12	-0.07	0.31	0.04	-0.09	0.01	-0.06					
11. Days Worked in Year	309.37	100.38	1.00	366	0.27	0.35	-0.23	-0.04	0.16	-0.03	-0.04	-0.01	0.01	0.28				
12. Firm Size (# Workers)	527.3	1710.11	0.00	206393	0.23	0.33	-0.17	0.01	0.48	0.00	-0.01	0.03	-0.01	0.27	0.37			
13. Hourly Wage	8.56	7.39	1.89	85.27	0.52	0.46	0.07	-0.13	0.13	0.05	-0.11	-0.04	-0.02	-0.00	0.13	0.17		
14. Part Time Contract (Dummy)	0.19	0.39	0.00	1.00	0.06	0.03	0.07	0.04	-0.05	0.07	0.00	0.04	0.01	-0.02	0.02	-0.03	-0.05	
15. Fixed Term Contract (Dummy)	0.36	0.48	0.00	1.00	0.33	0.31	-0.18	-0.03	0.34	0.27	0.01	-0.04	0.08	0.12	0.26	0.18	0.22	-0.05

Notes: Correlation coefficients with an absolute value greater than 0.02 are significant at the .05 level.

Table 3.2. Main Results

	EXIT			WITHIN-INDUSTRY | EXIT	
VARIABLES	Model 1	Model 2	Model 3	Model 4	Model 5
Relatively Higher Wage Cut	**0.055*** (0.001)**	**0.038*** (0.001)**	**0.036*** (0.001)**	**-0.001 (0.003)**	**0.002 (0.003)**
Relatively Higher Wage Cut * High-skilled			**0.008*** (0.001)**		**0.019*** (0.006)**
Age		0.363*** (0.007)	0.364*** (0.007)	0.008 (0.015)	0.008 (0.015)
Age Squared		0.000*** (0.000)	0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
Tenure		0.063*** (0.000)	0.063*** (0.000)	-0.005*** (0.001)	-0.005*** (0.001)
Tenure Squared		-0.001*** (0.000)	-0.001*** (0.000)	0.000 (0.000)	0.000 (0.000)
Experience		0.007*** (0.001)	0.007*** (0.001)	0.017*** (0.003)	0.017*** (0.003)
Days Worked in Year		-0.002*** (0.000)	-0.002*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
Firm Size (# Workers)		-0.000*** (0.000)	-0.000*** (0.000)	0.000 (0.000)	0.000 (0.000)
Hourly Wage		0.002*** (0.000)	0.002*** (0.000)	0.000 (0.000)	0.000 (0.000)
Part Time Contract		-0.004*** (0.001)	-0.003*** (0.001)	-0.007** (0.003)	-0.007** (0.003)
Fixed Term Contract		0.132*** (0.001)	0.132*** (0.001)	0.014*** (0.003)	0.014*** (0.003)
Constant	-14.45*** (0.225)	-12.684*** (0.247)	-12.69*** (0.247)	-0.090 (0.455)	-0.095 (0.455)
Employee Fixed Effects	NO	YES	YES	YES	YES
Occupation Group Fixed Effects	NO	YES	YES	YES	YES
Industry Fixed Effects	NO	YES	YES	YES	YES
Year Fixed Effects (5 years)	NO	YES	YES	YES	YES
Region Fixed Effects (17 autonomous regions)	NO	YES	YES	YES	YES
N	2,183,478	2,183,478	2,183,478	463,244	463,244
R-squared	0.256	0.526	0.526	0.769	0.769

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors are in parantheses.

4. Chapter 3: Temporary Employment, Precarity Trap in Low-wage Jobs, and Wage Inequality

Abstract

Focusing on the distribution of temporary employment across different wage layers, this study examines the association between temporary employment incidence and wage inequality. We argue that a high share of low-wage jobs in total temporary employment (i.e., disproportionate high concentration of temporary employment in the bottom of wage distribution) leads to rent destruction in low-wage jobs, shifting rent allocation vertically from low to high earners, thus increasing wage inequality. We also hypothesize that the rent-shifting process is subject to some moderating factors. Specifically, we expect the effect to be amplified in industries with greater human-capital intensity and a smaller probability of temporary workers' transition to permanent jobs, while being less pronounced in industries with greater concentration of large firms. We test our predictions with data on wages of a representative sample of the Spanish labor force, extracted from administrative linked employer-employee datasets, which include 784,206 individuals from 2006 to 2018 (total N=4,967,236 person-year observations). Employing heteroscedastic models that simultaneously examine the effects of both micro and macro-level covariates on within-industry wage variance, we find the expected positive association between the ratio of total temporary employment that is occupied by low-wage jobs and wage inequality at the industry-region-year level. The results also support our moderation hypotheses. Building on and advancing market-based and rent-destruction accounts of evolving employment practices, we introduce the high concentration of temporary employment in the bottom of the wage distribution as a structural source of wage inequality.

4.1. Introduction

In recent decades, the number of workers employed in positions that deviate from regular full-time and permanent employment has increased dramatically in many OECD countries (Cappelli & Keller, 2013; Carnahan, Agarwal, & Campbell, 2010; Fernandez-Mateo & King, 2011; Kalleberg, 2000, 2008; Pedulla, 2016). Temporary work is one important manifestation of evolving employment systems and the increase in open employment relationships over closed internal labor market practices (Bidwell, 2013;

Cobb, 2016; Dencker & Fang, 2016; Hollister, 2011). In dual labor markets where workers are assigned varying degrees of job protection, those who do not work under permanent full-time employment arrangements (hereafter *temporaries)* have lower job security, while permanent workers are entitled to multiple protective benefits including severance pay in case of a dismissal[10] (Barbieri, 2009; Cappelli & Keller, 2013). Consequently, there is a legal gap between permanent and temporary contracts in terms of job security (Cazes & De Laiglesia, 2015). This gap encompasses disparities among workers regarding the probability of dismissal and job insecurity (Bidwell, 2013; Cobb, 2016), in particular during economic downturns and adverse shocks. Overall, temporary employment is less stable, and temporaries are more prone to being fired (Shao & Silos, 2013).

Beyond regulated protection gaps, growth of temporary contracts also has an impact on the way rents are allocated across workers in the labor market, impacting income inequality (Gebel & Giesecke, 2011; Pedulla, 2016). However, findings on the effects of temporary employment on income inequality are rather mixed. According to segmentation models, permanent workers are not only better protected in the case of dismissals but also enjoy a wage premium compared with temporaries (Polavieja, 2003). In contrast, other theoretical arguments suggest that temporaries may actually receive higher pay than they would otherwise - in return for the job security they cannot obtain, common in permanent contracts (Cazes & De Laiglesia, 2015). This is especially true in managerial positions (Anderson & Bidwell, 2019).

Regardless of the contrasting findings, organizational and economic sociology research has often focused on documenting the gap in wages of temporaries vs. permanent workers performing similar tasks, as well as on the general level of temporary employment in total employment and its effects on allocation of rents between temporaries and permanent workers. Thus, previous accounts examine the horizontal gap in wages between temporaries and permanent workers, but lack a compelling explanation for how growing temporary employment alters the vertical differentiation of wages at large, i.e., whether this shift causes rents to be transferred from low to high earners, be

[10] We use Kalleberg's (2000) classification and define temporary work as any form of employment that deviates from permanent full-time regular employment; thus, *temporaries* refers to workers with fixed-term contracts, which is the most widespread form of temporary employment. Fixed-term contracts come with a definite end-date and usually with a limited number of renewal times.

they temporaries or permanent workers. Moreover, while evidence exists on the effects of a general workforce rise of temporary employment on wages, less clear is the impact of the distribution of temporary employment across different wage layers on wage variance.

This study investigates how temporary employment, and specifically its differing concentration across income layers, affects wage variance. To examine the association between temporary employment and wages, we draw on and advance rent-destruction accounts of employment practices. Rent-destruction explanations of inequality (Dencker & Fang, 2016; Sorensen, 2000; Sørensen, 1996) suggest that rents, i.e., "incomes above what would be realized in a perfectly competitive market" (Tomaskovic-Devey & Lin, 2011:541) or wage premia, especially those received by low-wage employees, have dramatically declined. This process results from a shift toward a greater reliance on market forces to govern the employment relationship (Jensen, 1993; Fernandez, 2001), and a transition to "open" employment relationships instead of closed internal labor markets. We suggest that a disproportionately high concentration of temporary employment in low-wage jobs destructs both monopoly rents (rooted in bargaining power), and composite rents (rooted in firm-specific skills) for low-wage jobs.

Our framework refines previous accounts on rent destruction both theoretically and empirically, facilitating a better understanding of the temporary employment-wage inequality association. From a theoretical viewpoint, we (a) explain how firms have absorbed uncertainties by allocating a large proportion of unskilled workers to temporary jobs in response to structural changes such as international competition, globalization, and skill-biased technological change, and (b) elaborate on how this affects wage variance. To this end, we treat increasing high concentration of temporary employment in low-wage jobs as an indicator of expansion of market-oriented open employment practices that not only impacts workers' wage levels but also does so in varying degrees across workers who are in different positions at the organizational hierarchy and the wage distribution.

Empirically, we focus on the distribution of temporaries across different wage clusters, specifically temporary contracts' high concentration in the bottom of the wage distribution, in addition to the general share of temporary employment in total

employment. Moreover, we examine the effects of a high share of low-paying jobs in total temporary employment on the entire wage structure, instead of comparing the wages of temporaries vs. permanent workers. Thus, we aim to capture the effect of positive skewness of the distribution of temporary employment the over wage distribution (i.e., its high concentration in low-paying jobs) on income inequality.

In summary, we suggest that a higher ratio of total temporary employment that is occupied by low-paying jobs affects wage structure not only through creating a pay gap between temporaries and permanent workers but also through triggering job insecurity for low-paid workers at large, subsequent destruction of rents, and a change in the overall wage structure to low-paid workers' disadvantage (including permanent ones). Because industries differ in the degree of market forces' impact on employment practices (Tomaskovic-Devey & Lin, 2011; Wilmers, 2017), we expect the intensity of the proposed rent destruction mechanisms to depend on industry-level factors. Therefore, we also theorize on industry-level characteristics that moderate the association between the share of low-wage jobs in total temporary employment and wage inequality. Specifically, we expect the effects to depend on cross-industry differences in the probability of temporaries to transition to permanent jobs (which we name *precarity trap)*, on industry human capital intensity, and on the concentration of large firms in each industry-region in a given year.

Studying the association between temporary employment and wage structure requires multilevel datasets that permit a joint analysis of the effects of individual, organizational-, occupational-, industry-, and location-level attributes of the employment relations. To test our hypotheses, we construct a new dataset using administrative linked employer-employee datasets from social security and tax records of Spain. We also integrate region and industry-level determinants of wages from Spanish National Statistics Institute databases. To eliminate confounding factors that might stem from regional and time-variant determinants of wage variance, we also group workers under autonomous communities of Spain (hereafter *region)* and base our analyses on the wage variances in industry-region-year cells (Wilmers, 2017). As discussed in detail in the methods section, Spain is a particularly suitable setting for such an analysis for several reasons. To start with, Spain has one of the highest percentages of temporary employment (26.7%) among OECD countries (these range between 1.58% and 28.8%). In addition,

the Spanish labor market varies substantially across wage clusters and industries. Last but not least, thanks to the reliable administrative datasets that include information on all relevant levels, we can directly link the share of low-wage jobs in total temporary employment to income inequality in the workforce controlling for a wide array of micro- and macro-level covariates.

Aligned with our interest in the vertical allocation of rents across the entire wage structure, we employ a two-stage model and variance function regressions to understand the variation in wages (Western & Bloome, 2009). Specifically, we first model workers' average logged wage as a function of share of low-paying jobs in total temporary employment in each industry in a given region and year. In the second stage, we use generalized linear models targeting the residual variance derived from the first stage. This two-stage heteroscedastic framework enables us to test the dependence of residual variance on a wide array of covariates while simultaneously modeling the mean and the variance of log wages as a function of these covariates.

Our results confirm a positive association between a high share of low-paying jobs in total temporary employment and wage inequality, at the industry-region-year level. Supplementary analyses using different measures of inequality (e.g., Gini and Theil indexes for each industry-region-year unit) obtain similar results. We also employ quantile regressions to understand how our main independent variable affects wages in different clusters of income distribution and show that higher shares of low-wage jobs in an industry-region are associated with larger increases for wages of top-earners than those of low-earning workers. Finally, we conduct complementary analyses on the starting wages and show that an increase in the share of low-paying jobs in total temporary employment compresses the starting wages of low-paid workers but not those of high-paid workers, further corroborating rent-destruction arguments.

The remainder of the chapter is organized as follows. We first overview the literature on the temporary employment-wage inequality relationship and provide definitions of key constructs. We then develop hypotheses drawing on the differing concentration of temporary employment in low-wage jobs in different industry-region-year combinations, and its implications on destruction of rents and variation of wages. We then present the empirical results from the heteroscedastic panel-data analyses on

Spanish linked employer-employee datasets. After reporting several robustness tests and supplementary analyses that account for alternative explanations of the association, we conclude with a discussion of the findings as well as their theoretical and practical implications.

4.2. Temporary Employment and Wage Inequality

4.2.1. Definitions and Background

Because long-term full-time work arrangements were the norm in many industrial nations for much of the twentieth century, such regular permanent employment relationships also constitute the basis of the paradigm in which labor, organizational, economic, and public policy research rest (Cappelli & Keller, 2013; Kalleberg, 2000). However, structural changes related to international competition, globalization, and skill-biased technological change, especially in the last quarter of the century, have led to a fundamental restructuring of the employment relationships (Althauser & Kalleberg, 1981; Kalleberg, 2012). Large firms, that typically relied on internal labor markets and supplied stable jobs, premium wages and benefits, have started to increasingly use market-based practices of employment models replacing permanent contracts with more flexible employment arrangements (Bidwell, 2013; Cappelli, 1999; Osterman, 2011).

As highlighted by recent reviews on both topics, a careful analysis of non-standard forms of employment and income inequality requires clarity about constructs and levels of analysis (Amis, Mair, & Munir, 2020; Bapuji, Ertug, & Shaw, 2020; Bidwell, Briscoe, Fernandez-Mateo, & Sterling, 2013; Cobb, 2016). While non-standard (non-permanent) employment relationships become increasingly prominent to organize work, a number of conceptually related alternative definitions have emerged, albeit without clear distinctions. In organizational and sociological research, nonstandard employment relations have been associated with part-time work, temporary help, contract company employment, short-term and contingent work, and independent contracting.

Given our focus on job insecurity, subsequent rent destruction and changes in the wage structure, the distinction between temporary vs. permanent arrangement is essential. A common theme in the various studies listed above is the identification of alternative employment as differing from standard work arrangements because it was generally

assumed that work was done full-time, for an indefinite period of time (lifelong), and performed at the employer's place of business under the employer's direction. We use Kalleberg's (2000) classification and define temporary work as any form of employment that deviates from permanent full-time regular employment. Thus, *temporaries* refer to workers under such temporary arrangements and temporary arrangements refer to fixed-term contracts, which is the most widespread form of temporary employment. Fixed-term contracts have a specific duration, which come with an end-date and usually with a limited number of renewal times.

Unlike in the United States, where the above-mentioned macroeconomic shifts have led to rising skill-biased wage inequality, in Europe, relatively more rigid labor market institutions have prevented such rapid wage adjustments. Instead, European labor markets have absorbed uncertainties by allocating an increasingly large proportion of unskilled workers to temporary jobs (Gebel & Giesecke, 2011). Perhaps for this reason, temporary employment is usually equated with fixed-term contract employment in Europe, as in our study (De Grip, Hoevenberg, & Willems, 1997; Reilly, 1998). The percentage of fixed-term contracts dramatically rose in the last quarter of the twentieth century, particularly in countries where employers cannot easily terminate permanent contracts. For example, between 1985 and 1996, in Spain and France, countries with strong restrictions on dismissal of permanent workers, the proportion of fixed-term contracts more than doubled, increasing from 15.6% to 33.6% and from 4.7% to 12.6%, respectively. High levels of temporary employment persist today (See Table 4.1 for percentages in OECD countries in 2018).

Insert Table 4.1

4.2.2. Concentration of Temporary Employment in Low-wage Jobs and Wage Variance

Rising temporary employment in OECD countries, especially in low-paying jobs, is a consequence of notable structural labor markets changes. In addition to skill-biased technological changes (Acemoglu, 2002) and the demise of internal labor markets; international competition, globalization, and subsequent migration trends have increased

employers' tendency to replace low-skilled workers with workers from poorer economies, usually offering lower wages and little or no job security. While some high-skilled temporary workers benefit from such arrangements (Kunda, Barley, & Evans, 2002), the vast majority of fixed-term contracts are in low-wage jobs.

Spain stands out as having one of the highest rates of temporary employment arrangements among OECD countries and high employment protection legislation for permanent workers. In order to reduce unemployment, a 1984 labor law reform introduced very low termination costs for temporary employment, accelerating utilization of temporary contracts in Spain. Although Spanish authorities have introduced law amendments to decrease the ratio of temporary employment (e.g., 2012 labor market reform), the percentage of fixed-term contracts has remained higher than the OECD average (26.8% in Spain while the OECD total was 11.9% in 2018). An important feature of high levels of temporary employment in Spain, directly related to this article's motivation, is that the prevalence of temporary contracts varies greatly across different (a) earning groups and (b) industries. Take Figure 4.1 which shows that in 2018 in Spain, for each temporary contract in the top two deciles of wage distribution, there were almost four temporary contracts in the bottom twenty percent of the wage distribution. Although this trend has been an important topic of economic policy debate in Spain, empirical analyses of the distributional impact of temporary employment have been limited so far.

Insert Figure 4.1

We argue that increasing proportions of temporary employment occupied by low-paying jobs contribute to rent destruction in such low-wage jobs. Rent-destruction explanations hold that owners benefit when firms eliminate rent generating properties of employment relationships (Dencker & Fang, 2016; Sorensen, 2000; Sørensen, 1996), because it is more profitable for firms to set wages according to external labor forces than to internal labor market practices. Specifically, this perspective suggests that workers' disadvantages accrue from the destruction of two types of rents. *Monopoly rents* emerge when jobs and employment are closed to outsiders; for example, when employment arrangements are subject to collective bargaining contracts, as was the case for many

production workers until the end of last century. *Composite rents* appear when workers have obtained specific on-the-job training and therefore are more productive in one firm than in others (Becker, 1962; Dencker & Fang, 2016). Hence, composite rents refer to asset specificity, such as through joint investments of a firm and its employees in workers' firm-specific human capital.

We propose that a greater reliance on market forces to govern the employment relationship, and the subsequent replacement of permanent employment with temporary arrangements could reduce both types of rents. An increasing prevalence of temporary employment in low-wage jobs indicates that with more market-based employment contracts, the bargaining power of production workers and unions has waned, and their ability to secure above-market wages has also declined, diminishing monopoly rents (Dencker & Fang, 2016). As for the composite rents, if firms in a particular industry can easily replace low-skilled workers with cheaper ones from the external labor market, composite rents that stem from firm-specific human capital should decrease for workers in that industry. These effects should be more pronounced for non-core workers who are less valuable for firms' core competence, i.e., low-wage workers.

The incidence of rent destruction processes varies across different wage groups and industries. Employers seek greater flexibility by utilizing different forms of employment arrangements according to the perceived importance of different workforce groups. In some industries, firms are more likely to invest in their workers via relational employment contracts, developing highly skilled workers, and enhancing employees' functional flexibility (Wilmers, 2020). In other industries, firms obtain flexibility by reducing labor costs and hiring low-paid workers on temporary contracts. Most organizations have adopted both of these strategies for different groups of workers. Specifically, they use temporary contracts to obtain flexibility and buffer their most valuable, core workers from fluctuations in labor supply and demand.

Overall, external market-based compensation systems destroy rents for many low- and mid-wage workers as they reduce workers' ability to earn a wage premium for their labor (Dencker & Fang, 2016; Sorensen, 2000). In contrast, evidence exists that firms are increasingly using performance-based compensation systems that reward higher-skilled, higher-ranking workers and increase their earnings (Bell & Van Reenen, 2014; Lemieux,

MacLeod, & Parent, 2009; Schweiker & Groß, 2017). Research in economic sociology lends support to such a structural interpretation, suggesting that given pressures to transfer revenue to shareholders and to absorb adverse shock and market uncertainties, lower-level workers have lost bargaining power and therefore are more vulnerable to wage reduction (Avent-Holt & Tomaskovic-Devey, 2014; Hanley, 2014).

These studies therefore suggest that once a job is separated from a firm's internal labor market practices, its wages will be set close to the going market rate (Cappelli, 1999). Because market-based flexible employment arrangements are more common for low-wage jobs, rent-destruction consequences should be more severe for low-wage jobs. Replacing low-wage workers with temporaries, while keeping most of high-paid core workers under permanent contracts, also allows firms to disperse wages without triggering perceptions of inequity among high- and low-wage workers (Rawley & Simcoe, 2010).

Firms' preference for temporary contracts for jobs that are peripheral to their core competence has increased the ratio of temporary employment held by low-paying jobs, especially in industries that host uncertain product market environments (Cappelli & Keller, 2013; Matusik & Hill, 1998). Moreover, the threat of being replaced with temporaries may also depress wages for permanent low-wage workers partly at least by reducing their bargaining power (Bernhardt, Batt, Houseman, & Appelbaum, 2017). The implication, then, is that lower-wage workers will be most negatively affected by the increasing share of low paying jobs in total temporary employment.

Thus, we build the association between a high share of low-paying jobs in total temporary employment and within-industry wage inequality on three main premises. First, open employment relationships currently prevail over internal labor market practices, increasing the salience of flexible contractual arrangements and requiring individuals to shoulder a large share of market uncertainty once born by corporate employers (Kalleberg, 2011). Second, such contractual risks are not distributed evenly among different groups of workers. Specifically, as part of firms' preference or need to retain high-skilled top earners who are critical core competences assets, low-wage workers are more likely to work under temporary contracts. Third, a higher concentration of temporary employment arrangements in low-wage jobs in a given industry may

decrease the bargaining power and wages of permanent low-wage workers also, due to the increased threat of being replaced with temporaries. Consequently, we propose that the higher the proportion of temporary jobs held by low-paying workers in an industry in a given region and year the greater the variance of wages in this industry-region-year.

> ***Hypothesis 1:*** *The share of low-wage jobs in total temporary employment is positively associated with within-industry wage inequality.*

4.2.3. Precarity Trap: The Low Probability of Transitioning from a Temporary to a Permanent Job

In our baseline hypothesis, we propose that low-wage jobs' share in total temporary employment is directly linked with changing job insecurity, destruction of rents for low-paid workers, and thus wage inequality. Structural factors pertaining to the industry should impact the power of the association between distribution of temporary employment to different income clusters and wage variation.

To start with, some industries exhibit more precarity than others which should aggravate the rent destruction processes. Bourdieu (1963) used the term *précarité* to differentiate between workers with permanent jobs and those with casual ones in Algeria. Following this initial conceptualization, precarious work is defined as the absence of aspects of the standard employment relationship that support the decommodification of labor (Rubery, Grimshaw, Keizer, & Johnson, 2018). In other words, precarity is not necessarily related to the nature of tasks that fall under the definition of a particular job. Precarity rather addresses the lack of protective measures against a pure market-based employment regime, provided either by the employers or the state through labor rights and social protection. Thus, although precarious workers often fill permanent job needs, they are denied permanent employee rights and given instead unstable employment, lower wages, and lower legal protection.

In industries where transitioning to long-term contracts is relatively difficult, temporaries switch among temporary jobs. This self-reinforcing process leads to institutionalization of bad jobs, that are marked by precarity and insecurity, at the bottom of wage distribution (Kalleberg, 2011; Standing, 2011), and to conditions in which low-

wage roles are filled by people already in poverty, the so-called working poor (Dorling, 2014; Leana, Mittal, & Stiehl, 2012), generally under temporary work arrangements. Additionally, when the working poor continuously hop on and off among different temporary jobs and unemployment spells, they can rarely access resources that would improve their employment profile and increase their chances to secure better jobs.

Therefore, the prevalence of precarity in an industry that is associated with low pay, short-term contracts, limited social protection rights, and job insecurity, will have a direct effect on the rent destruction processes associated with the high concentration of temporary employment in low-paying jobs. If temporary employment is a steppingstone into long term work, the rent destruction processes will be milder as workers could still benefit from some composite rents due to their accumulated skills. In contrast, if temporary employment does not secure a transition to a permanent job (which we name *precarity trap*), low-wage employees will not only be stuck in low-paying and insecure jobs but also be challenged to improve their potential to access better jobs. In short, this will amplify the wage-depressing and rent-destructive effects of share of low-wage jobs in total temporary employment. Our first moderation hypothesis follows:

> ***Hypothesis 2:*** *In industries in which temporary workers transition less frequently into permanent jobs (precarity trap), the positive association between the share of low-wage jobs in total temporary employment and wage inequality is amplified.*

4.2.4. Human Capital Intensity of Industries

Another premise underlying the baseline hypothesis is that open employment relationships -as opposed to closed internal labor market practices- are commonplace, yet the penetration of market-based employment practices has been more pronounced for workers who are not critical to firms' core competences, i.e., for low-wage workers. In other words, firms use temporary workers in low-paying jobs as a mechanism to buffer the most valuable core workers from adverse shocks or fluctuations in supply and demand.

In this regard, skill-biased technological changes have had significant effects on employment relationships and on how firms determine who core workers are. Essentially,

the technological changes experienced in recent decades have increased layoffs (Bidwell et al., 2013) vertical disintegration (Brynjolfsson, Malone, Gurbaxani, & Kambil, 1994; Hitt & Bierman, 2001) and use of contingent workers (Sahaym, Steensma, & Schilling, 2007; Schilling & Steensma, 2001). Inequality research shows the disruptive effects of technological change on employment, including the differential returns to skills (Autor, Katz, & Krueger, 1998). These technological changes have heterogenous effects across different industries depending on the degree of technological replacement and-or the level of reliance on highly skilled workers to cope with the intricacies of and master technological change.

Given heterogeneous effects of skill-biased technological change across industries, we expect high levels of human capital intensity in a given industry to magnify the proposed rent-destruction effects for low-paying jobs. Firms in such industries will be particularly eager to retain their highly skilled workers. Hence, the negative effects of the high share of low-wage jobs in total temporary employment that destroy rents will be more severe.

> ***Hypothesis 3:*** *In industries that are more reliant on high-skilled workers, the positive association between the share of low-wage jobs in total temporary employment and wage inequality is amplified.*

4.2.5. Concentration of Large Firms in Different Industries

In internal labor markets, wages are set according to internal procedures and job evaluation systems beyond external market parameters (Doeringer & Piore, 1971). In other words, jobs are assigned wages based on their value to the firm and in relation to other jobs within the organization, not just external valuations in the labor market. Although jobs that require greater levels of competence and that are more highly valued by the firm receive greater pay, these systems were also developed to create a sense of internal pay equity and avoid the undesirable consequences of negative social comparisons (Dulebohn & Werling, 2007; Pfeffer & Davis-Blake, 1992). In contrast, market-based mechanisms assign wages to a job relative to similar jobs, without necessarily considering pay equity and social comparison concerns.

Historically, larger firms have been more likely to adopt internal wage setting practices, which set compensation through job evaluation, and served in part to reduce wage inequality inside firms (Gomez-Mejia, Berrone, & Franco-Santos, 2014). Though not exempt from the processes that gave rise to market-based employment practices in wage-setting, large firms are required to consider internal organizational concerns (e.g., pay equity, adverse effects of social comparisons) more than their smaller counterparts (Gartenberg & Wulf, 2020). Given the increasing prevalence of temporary employment, we suggest that the association between low-wage jobs' shares in temporary employment and wage inequality will be weaker in industries that have a higher concentration of large firms (i.e., where there are fewer actors as opposed to a competition among a large number of small firms). Therefore, our final moderation hypothesis follows:

> ***Hypothesis 4:*** *In industries with a higher concentration of large firms, the positive association between the share of low-wage jobs in total temporary employment and wage inequality is attenuated.*

4.3. Methods

4.3.1. Data and Research Setting

To test our hypotheses on the association between temporary employment and inequality, we first need information on individual-level determinants of wages. Additionally, because our main independent variable needs to capture the ratio of temporary employment that is occupied by low-wage jobs, we also need information on the entire wage distribution, types of contracts in each industry and in different wage layers. To combine all this information in our analyses, we culled data from multiple sources to include worker wages along with individual-, firm-, occupation-, industry-, and region-level covariates.

Our main data come from the Continuous Sample of Working Histories (CSWH) *(Muestra Continua de Vidas Laborales con Datos Fiscales)* from Spain's Social Security Office. These datasets contain matched anonymized social security, income tax, and census records for a four percent, non-stratified random sample of the population that had any relationship with Spain's Social Security (whether via employment, self-employment, unemployment, or retirement) in that year. The work data provide

information on individuals' complete labor market histories from 1980 (or the year the individual registers with Social Security) to the year of data collection. First, with social security person-month panel datasets, we track individuals, their demographic and educational backgrounds, occupations, and nature of contractual relationships with their firms dating back to 1980.

However, earnings information from the Social Security records is censored at both the top and bottom. Given that our inquiry focuses on wage variance, upper capped earning records might have polluted the analyses. To overcome this challenge, we merged social security records with the tax records which have info on non-capped earnings from 2006 onwards, for all the individuals that could be tracked with social security records. Thus, although we collected data on individual-level wage determinants going back to 1980, our analyses target wage variance in the 2006-2018 period, for which we can extract noncapped individual earning records and firm-level information from the tax datasets. Finally, we use autonomous community-level datasets from the Spanish National Statistics Institute in order to integrate region-level covariates (e.g., population growth, GDP growth of the region).

To create the final sample of individuals, we made some exclusions. For individuals who work at multiple establishments in a year, we only consider the main job, which is either the job with the longest spell within the same firm or the job with the highest earnings across firms. To ensure that jobs with unreasonably low earnings are not included in the analyses, we first dropped any observation with earnings below the mean wage of the bottom decile for each industry in a given region. Second, we integrate information on the minimum wage for each year within our time frame, and for full time workers we dropped observations earning less than half the minimum wage; for part-time workers, we dropped observations earning less than 25% of the minimum wage for any specific year (in Spain the minimum wage for part-time workers is half of the regular minimum wage). Aligned with recent studies on within-industry wage variance, we only include full-time workers in the main analyses reported here.

Therefore, we first build a person-month panel dataset that includes more than 40 million observations. We then aggregate the records at the yearly level, and build a yearly panel that covers employment spells, with a start/end date and tied to a firm identifier.

Each spell also includes information on individuals (e.g., age, gender, whether the contract is temporary or permanent), establishments, occupations, sectors, place of the workplace, and macroeconomic parameters of the corresponding region. After performing these exclusions and converting the records to annual datasets (person-year units), our sample includes 4,967,323 person-year observations between 2006 and 2018 that belong to 784,206 workers.

Per our theoretical propositions, we group these workers in industry-region-year cells. Industry, the main economic activity of each establishment, is captured by one of 59 two-digit sector codes in the National Classification of Economic Activities (CNAE-93). Because the CNAE was modified in 2009, sector codes (CNAE 2009) from 2009 and later have been matched with CNAE-93 equivalents. We operationalize *region* by using 17 autonomous communities of Spain (two other city-regions on African soil, Ceuta and Melilla, are excluded from the analyses). Finally, *year* can take one of the 13 values from 2006 to 2018.

Spain is an appropriate setting for our investigation for several reasons. First, Spain has one of the highest percentages of temporary employment (26.7%) among OECD countries (Table 4.1). In addition to being a relevant phenomenon to justify the setting for our research question, temporary employment in Spain is highly concentrated in low-paying jobs, and varies substantially across industries. Autonomous communities of Spain, and the provinces which are the communities' building blocks date back to 1830s. Due to several historical and sociopolitical reasons, inter-regional labor mobility is low in Spain (Tanova & Holtom, 2008). Consequently, second advantage of our research setting is that low inter-regional mobility downplays confounding factors and prepare a very good setting for an industry-region analysis. Finally, thanks to the unusually complete and reliable administrative datasets (vs self-reported earnings), we can directly link the share of low-wage jobs in total temporary employment to income inequality controlling for a wide array of both micro- and macro-level covariates.

4.3.2. Key Variables

Dependent Variable: Wage Inequality

The key outcome modelled (see model below) is the variance of workers' wages within industry-region-years. For wages, we use logged real hourly wages from tax records. We calculate hourly wages using information on total earnings, the number of days worked, and the percent employment time (e.g., eight hours per day for a full-time worker, four hours per day for a half-time worker, two hours per day for a quarter-time worker).

Main Independent Variable: Share of Low-wage Jobs in Total Temporary Employment

We define low-wage jobs as those earning less than the 20th percentile of the wage distribution in each industry in a given region and year (our results are robust to defining low-wage jobs with 10^{th} and 25^{th} percentile). Our main independent variable is the ratio of fixed term contracts in low-wage jobs to all fixed term contracts in each industry-region-year cell. Let us explain the operationalization of this variable that aims to capture the positive or right skewness of the distribution of temporary contracts over wage distribution (i.e., its high concentration in low-wage jobs). Assume that there are 1000 workers in an industry-region-year combination (e.g., working in construction industry in Madrid in 2008), and that the proportion of temporary employment in this cell is 25%: 250 of the individuals are temporaries while 750 are permanent workers. Also assume that 100 of 250 temporaries are in the bottom two deciles of the wage distribution, i.e., in low-wage jobs. Our independent variable is calculated as 100/250 = 0.4 (*share of low-wage jobs in total temporary employment).* Note that if we looked at the general level of temporary employment, like previous studies often do, it would be calculated as 250/1000 = 0.25. Assume temporary employment increases by 20%, from 250 to 300, in a given year while total employment remained constant at 1000. If the increase in temporary employment happens at the same rate for low-wage jobs (i.e., temporary employed low-paid workers also increase by 20 percent from 100 to 120), our independent variable would still take the value 120/300=0.4. Hence, this independent variable is not necessarily affected by a general rise in temporary employment (in fact, in this example, the general share of temporary employment in total employment went from

0.25 to 0.3). In other words, the counterfactual in this study is not the inexistence of temporary contracts, but the even distribution of temporary contracts across income clusters.

Moderator #1: Precarity Trap

Our first moderator aims to capture the probability of being stuck in a temporary work arrangement for a given worker in a particular industry in a given region (i.e., whether the temporary job is a steppingstone to transition to a permanent job or not). We measure it by the probability of continuing in a temporary position or unemployed in the year following the focal period, i.e., not securing a permanent job in the following year conditional on being a temporary worker, that is mathematically denoted as [1-(Probability of landing a permanent job)].

Moderator #2: Human Capital Intensity of Industries

To measure industries' *human capital intensity*, we follow prior research which relied on schooling intensity (Ciccone & Papaioannou, 2009) or the prevalence of educational credentials to proxy for human capital within the industry (Kriechel & Pfann, 2005; Teixeira & Tavares-Lehmann, 2014). Hence, we compute *human capital intensity* as the industry-level share of workers with higher education credentials (i.e., at least a bachelor's degree) in a given region and year.

Moderator #3: Concentration of Large Firms

To measure the concentration of large firms, we calculate the *Herfindahl Hirschman Index (HHI)* (Bertrand, Duflo, & Mullainathan, 2004) based on the number of workers in the establishments in each industry in a given region and year, that is available in the tax records. *HHI* can take a value between 0 and 1 where a larger *HHI* indicates that the market is controlled by fewer actors (i.e., higher concentration of large firms) rather than competition among a large number of small firms.

Fixed Effects and Controls

In addition to the above key variables of interest, described also in Table 4.2; to focus the analysis on the within-industry wage inequality, we include *industry, region,*

and year fixed effects while estimating the average wages. Specifically, we use dummy variables for 59 industries, 17 autonomous communities of Spain, and 13 years (2006-2018). At the individual worker level, we also include *dummies for four education levels and three occupation categories* in addition to *age* and *gender*. At the industry-level, we include dummy variables for the four broader sectors: manufacturing, services, construction, and agriculture. These variables aim to control for confounding industry characteristics that might contribute to both the degree of industry wage inequality and the extent to which industries seek out and select for a high share of low-wage jobs in total temporary employment. At the region level, we include controls for *GDP per capita and annual population growth rate* of the region. Finally, to distinguish the effect of the distribution of temporary employment (share of low-wage jobs in total temporary employment) from that of a rise in temporary employment generally, we include a control for the share of temporary employment within total employment in each industry-region-year cell. These controls are added in both the mean and variance equations to address competing explanations for the association between the share of low-wage jobs in temporary employment and wage inequality (for example, whether observed differences in worker characteristics explain the wage inequality across industry-region-year cells).

Insert Table 4.2

4.3.3. Statistical Approach and Estimation Strategy

To model within-industry wage variation, we use a two-stage variance function regression framework, as proposed by Western & Bloome (2009) and applied in recent inequality research (e.g., Wilmers, 2017; Zheng, Yang, & Land, 2011). Our estimation strategy diverges from regression-based studies of income inequality that model only between-group differences. OLS regressions provide a convenient framework for exploring between-group differences (e.g., wages of temporaries vs. permanent workers, male vs. female), where the regression coefficients describe differences in group means. However, in this study, we focus on the variance of wages, not the mean: for a given industry, a shift in the ratio of temporary employment that is occupied by low-paying jobs predicts a shift in the distribution of rents. Specifically, an increase in the share of low-

wage jobs in total temporary employment should be positively associated with wage inequality.

Our analysis has two main parts: a regression for an outcome variable (logged hourly wage), and an estimation for the logarithm of the residual variances (Western & Bloome, 2009). In the first stage, we fit the mean equation with individual-, industry-, and region-level covariates predicting average log wage of a worker in a particular industry-region-year with below estimation formula:

$$ln(y_{irpt}) = \beta_1 s_{rpt} + \beta_2 x'_{rpt} + \beta_3 x'_{irpt} + \alpha_{1r} + \alpha_{2t} + \alpha_{3p} + \sigma_{irpt} \quad (1)$$

where the outcome is logged hourly wages for individual i, who is located in region r and industry p, in year t. These wages are a function of the share of low-wage jobs in temporary employment in the worker's industry (s_{rpt}), in addition to other industry-region-year-level covariates (x'_{rpt}), as well as individual worker-level covariates x'_{irpt}. Industry-level fixed effects (α_{3p}) are also included, to restrict the second-stage variance model to within-industry wage inequality. The models also include region (α_{1r}) and year (α_{2t}) fixed effects.

The conditional variance of wages is modeled as:

$$Log\ (\sigma^2_{irpt}) = \lambda_1 s_{rpt} + \lambda_2 x'_{rpt} + \lambda_3 x'_{irpt} + \zeta_{1r} + \zeta_{2t} \quad (2)$$

where the outcome is measured as the estimated residual variance in wages remaining after fitting the mean equation (note that the dependent variable in Equation 2 is the logged squared residual variance, which we extracted from Equation 1). Whereas the conditional mean formula (1) models average log wages for an individual with given characteristics, the conditional variance formula (2) models the variability of residual wages (the squared residuals) for that industry-year-region combination. Because the outcome in the variance model is the residual from the conditional mean model, not wages proper, wage variability within an industry, year, and region that is due to the mix of workers of different educational attainments and other individual-level traits has already been controlled for. Hence, the variance model explores why, net of these composition effects, some industry-year-region combinations have more variable wages than others (Wilmers, 2017). In this regard, our coefficient of interest for the baseline hypothesis is

λ_1. If lambda one is positive, it supports Hypothesis 1 that the ratio of temporary employment occupied by low-wage jobs is positively associated with within-industry wage inequality.

In terms of operationalization, we first use OLS to estimate the mean regression (1). Second, the variance regression is estimated from the squared residuals of the OLS regression, using a gamma regression with a log link function. The gamma regression is a generalized linear model appropriate for right skewed dependent variables (like the squared residuals used here to measure variance). Finally, to obtain the correct standard errors for these estimates, we saved the fitted values from the application of the two steps to be used in weighted regressions. The OLS model is rerun, weighted with the inverse of the exponentiated predicted values from the previous gamma regression. The gamma regression is then rerun with the new residuals, and these steps are iterated until reaching convergence. This iterated weighting procedure provides corrected standard errors for the coefficients that take into account uncertainty in the mean regressions (Western & Bloome, 2009; Wilmers, 2017). The final output of this procedure provides estimates of the association of covariates with both average wages and the variance of wages. We only report the estimates from the variance equations (lambda coefficient), that are of primary interest for the inquiry on wage variation.

4.3.4. Main Results

Figure 4.2 plots the association between our independent and dependent variables. On the x-axis, there is "Share of Low-wage Jobs in Total Temporary Employment" (main independent variable). On the y-axis, we place within-industry variance of logged wages. Circles represent industries and their sizes. This figure shows that industries differ substantially in their distribution of temporary employment across different wage layers. The red prediction line also indicates a positive relationship between the ratio of temporary employment that is occupied by low-wage jobs and within-industry wage inequality. Although it is informative on the hypothesized baseline relationship, the graph does not yet take into account any control variables.

Insert Figure 4.2

Table 4.3 presents the main analytic results. Consistent with Figure 4.2, Model 1 of Table 4.3 shows a positive association between wage variance within industry-region-years and the share of low-wage jobs in temporary employment *(λ_1= 0.56; p < .001)*. Model 2 tests whether this association is robust to the inclusion of individual-level controls for age, gender, four education categories, and three occupation categories, along with regional controls (dummy variables for 17 regions, GDP per capita, population growth).

Insert Table 4.3

Although the numerous controls explain away approximately 30% of the variation (i.e., λ_1 decreases from 0.56 to 0.39), Model 2 still shows a robust link between the ratio of temporary employment that is occupied by low-wage jobs and within-industry wage inequality *(λ_1= 0.39; p < .001)*. This result supports Hypothesis 1, that the share of low-wage jobs in temporary employment is positively associated with wage inequality. Exponentiating the main results suggests that a one percentage point increase in the main independent variable is associated with a 0.56% increase in wage inequality.

In Model 3, we test the hypothesized moderation effect of *precarity trap.* The positive interaction coefficient of *Precarity Trap * Share of Low-wage Jobs in Total Temp. (β= 0.40; p < .001)* shows that the baseline association is stronger in industries where temporary workers transition less frequently to permanent jobs (i.e., there is a more severe precarity trap), supporting Hypothesis 2. Similarly, in Model 4, the positive coefficient of *Human-Capital Intensity * Share of Low-wage Jobs in Total Temp. (β= 0.32; p < .001)* indicates that the baseline association is stronger in industries that are more reliant on highly skilled workers, supporting Hypothesis 3. Finally, in Model 5, the negative significant interaction coefficient of *Herfindahl Hirschman Index * Share of Low-paying Jobs in Total Temp (β= - 1.53; p < .001)* supports Hypothesis 4: when there is a higher concentration of large firms in an industry, the association between share of low-wage jobs in total temporary employment and wage inequality is weaker.

4.3.5. Supplementary Analyses

Conditional Quantile Regressions

The results of the variance function regressions show a robust association between the share of low-wage jobs in total temporary employment and wage inequality. Moreover, we find support for our moderation hypotheses, and also distinguish the effects of concentration of temporary employment in the bottom of the wage distribution from several alternative explanations through a wide array of fixed effects and controls, such as the general percentage of temporary employment. However, although the two-stage variance function framework efficiently captures the wage variance in the entire wage structure, it does not indicate the precise effect of our independent variable on wages themselves, at different points of distribution.

Our argument is that a high share of low-wage jobs in total temporary employment contributes to job insecurity, rent-destruction for low-wage jobs, and a vertical shift of rents from low- to- high-wage workers. If this were the case, one would expect our independent variable to have a smaller/negative effect on raw wages of low-wage employees than on those of higher-wage workers. In order to test the effect of share of low-wage jobs in total temporary employment on raw wages at different points across the wage distribution, we conduct a conditional quantile regression analysis, which estimates the association of a covariate at different percentiles of the conditional distribution of wages (Killewald & Bearak, 2014). To this end, we specifically examine the 10^{th}, 25^{th}, 50^{th}, 75^{th}, and 90^{th} percentiles of the wage distribution within each region-year.

The results shown in Table 4.4 indicate that out of the five points tested in the wage distribution, only the bottom 10th percentile is negatively predicted by the share of low-wage jobs in total temporary employment *(β= -0.03; $p < .001$)*. Models 1 to 5 show a clear pattern: in industries with a higher share of low-wage jobs in total temporary employment, low-income workers earn lower wages than workers in industries in which the distribution of temporary employment over wage layers is less skewed. As for high-income workers, the positive coefficients for the 50^{th}, 75^{th}, and 90^{th} percentile analyses show that they earn higher wages in industries where a greater ratio of temporary employment is occupied by low-wage jobs. These results corroborate our argument that a higher share of low-wage jobs in an industry-region-year unit contributes to rent

destruction for low-wage jobs, while it does not for high-wage jobs. Thus, opposite-signed wage patterns at different sides of the distribution support to our proposition that a higher-share of low-wage jobs in total temporary employment shifts rents from low- to high-level workers.

Insert Table 4.4

Variance Function Regressions with Sample of Permanent Workers Only

Conditional quantile regressions are not only in line with our theoretical propositions but also provide insights as to who enjoy wage premia. To this end, one of our other theoretical propositions is that a higher concentration of temporary employment arrangements in low-wage jobs in a given industry might decrease the bargaining power and wages of permanent low-wage workers, also, due to the increased threat of being replaced with temporaries. Thus, to understand how our main independent variable affects wage structure among permanent workers also, we run the variance function regressions with the sample of permanent workers only. The positive coefficient in Model 6 of Table 4.3 *($\lambda_1 = 0.17;\ p < .001$)* shows that a high share of low-wage jobs in total temporary employment is positively associated with within-industry inequality also among the permanent employees sample. These results imply that rents are not simply transferred from temporary to permanent workers but are rather transferred from temporaries to already advantaged, high-earning permanent workers.

4.3.6. Robustness Checks

To enhance confidence in our findings, we conduct additional analyses with different specifications of the sample, main independent variable, and the dependent variable (wage inequality), along with alternative estimation strategies. First, we rerun all estimations by defining low-wage jobs as those lower than the 10^{th} and 25^{th} percentile. Coefficients are quantitatively similar to those reported in the main analyses, that define low-wage jobs as those lower than the 20^{th} percentile. We also rerun the analyses by including previously excluded observations with unreasonably low earnings. Analyses

from this sample yield an even stronger effect of the share of low-wage jobs in total temporary employment on wage inequality, i.e., a larger lambda.

In addition, we employed a different estimation strategy. One of the main reasons we relied on variance function regressions is that by including individual characteristics (e.g., education, occupation, gender) in the first-stage mean estimation, we try to distinguish the structural rent destruction mechanism from the occupation and human capital factors found in previous research (Mazzolari & Ragusa, 2013). As an alternative, a more straightforward approach, we first calculated wage variance at each industry-region-year cell, then regressed wage variance on our independent variable and the same covariates used in the variance function regressions, but this time also using weights according to the number of workers in each cell. Finally, we also conduct a similar analysis with Gini and Theil indexes for each industry-region-year cell as the dependent variable. Results still hold and are statistically significant with these alternative estimation methods using either industry-region-year wage variance, Gini, or Theil Index as a dependent variable.

4.4. Discussion

Given the rising relevance of temporary employment across the globe, this study explores its effects on wage inequality, refining and extending rent-destruction accounts of market-based employment relationships. In understanding the association between temporary employment and wage inequality, we build on organization theory insights regarding the high concentration of temporary employment among non-core workers, i.e., low-wage workers, and combine them with a structural perspective from economic sociology. We develop several theoretical arguments and boundary conditions about how the disproportionately high concentration of temporary employment in low-wage jobs contributes to rent-destruction processes in such jobs, shifting of rents from low- to high-level workers, and consequently affecting wage inequality. In this way, we answer recent calls for examining how organizational practices can contribute to, institutionalize, and reproduce inequalities at the societal level beyond intra-organizational inequality (Amis et al., 2020; Bapuji et al., 2020), as well as for multi-disciplinary examinations of the rise of non-standard employment practices and its implications on income inequality (Bidwell et al., 2013; Cobb, 2016; Sørensen & Sorenson, 2007).

To explore the effect of temporary employment rates on wage inequality and conduct a granular analysis of the hypothesized mechanisms, we take advantage of the uniquely rich information afforded by datasets from administrative Spanish Social Security and Tax Records, along with regional data from the Spanish Statistical Office (INE). Our results suggest that a high ratio of temporary employment filled by low-paying jobs is positively associated with within-industry wage inequality. Although we trace the implications at the industry-level, our mechanisms and the moderator variables also shed light on how firm-level employment practices contribute to inequality.

In addition to advancing research on employment relationships and wage inequality, this study sheds light on contemporary social dilemmas. Thanks to the detailed information on both micro- and macro-level determinants of wages, we show that observed differences in worker characteristics does not suffice to explain inequality. Specifically, our findings indicate that even if there is not a salient pay gap between temporary vs. permanent workers with similar characteristics, the uneven distribution of temporary employment across income clusters can increase wage inequality. Thus, the novel structural source of wage inequality we propose, i.e., the share of low-wage jobs in total temporary employment, casts doubts on the efficacy of meritocratic pay practices in reducing inequalities (Castilla & Benard, 2010; Sandel, 2020).

Finally, our theoretical propositions centered on the precarity trap in bad jobs draws attention to another social dilemma: the problem of the working poor, which has become even more pressing due to the pandemic. Brady, Fullerton, & Cross (2010) affirm that in the United States in 2001, there were more than four times more people in working poor than unemployed poor households. As the world struggles to overcome the COVID-19 pandemic and its devastating effects on the working poor, evidence-based inequality research is paramount. Given that flexible temporary employment practices have been often used to address unemployment issues, creating good jobs (Kalleberg, 2011; Standing, 2011) and simultaneously resolving working poor problems should be also prominent in policy decisions. Our findings suggest that well-intentioned efforts to diminish unemployment might have had unintended consequences such as increasing inequalities.

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4.6. Chapter 3 Tables and Figures

Table 4.1. Percentage of Temporary Employment in OECD Countries in 2018

Lithuania	1.58	Germany	12.55
Latvia	2.68	Turkey	12.58
Estonia	3.47	Switzerland	13.11
United States	3.95	Canada	13.31
United Kingdom	5.55	Slovenia	15.89
Hungary	7.31	Finland	16.45
New Zealand	7.89	France	16.71
Slovak Republic	8.29	Sweden	16.82
Norway	8.43	Italy	17.02
Czech Republic	8.93	Croatia	20.01
Austria	9.07	Korea	21.16
Iceland	9.23	Netherlands	21.48
Luxembourg	9.86	Portugal	21.97
Ireland	10.03	Poland	24.38
Denmark	10.70	Spain	26.81
Belgium	10.76	Chile	27.58
Greece	11.29	Colombia	28.80

Notes: Temporary employment includes wage and salary workers whose job has a pre-determined termination date. **Source:** OECD Employment Outlook (2020)

Table 4.2. Definitions of Key Variables

Variable	Definition
Share of Low-wage Jobs in Total Temporary Employment	Ratio of fixed term contracts in low-wage jobs to all fixed term contracts in each industry-region-year cell
Wage Inequality	Residual variance of logged real hourly wages
Precarity Trap	Probability of continuing in a temporary position or unemployed in the year following the focal period, i.e., not securing a permanent job in the following year conditional on being a temporary worker, mathematically denoted as [1- (Probability of landing a permanent job)]
Human Capital Intensity	Industry-level share of workers with higher education credentials (i.e., at least a bachelor's degree) in a given region and year
Concentration of Large Firms	Herfindahl Hirschman Index (HHI) (Bertrand, Duflo, & Mullainathan, 2004) based on the number of workers in the establishments located in each industry in a given region and year

Table 4.3. Results

	MAIN RESULTS (All full-time workers)					SUPPLEMENTARY (Only permanent)
VARIABLES	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
FIXED EFFECTS						
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	No	Yes	Yes	Yes	Yes	Yes
Education fixed effects	No	Yes	Yes	Yes	Yes	Yes
Industry group fixed effects	No	Yes	Yes	Yes	Yes	Yes
Region controls (GDP per capita, population growth)	No	Yes	Yes	Yes	Yes	Yes
OTHER CONTROLS						
Industry's dependence on temporary employment	-0.24***	-0.10***	-0.09***	-0.05***	-0.08***	-0.64***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Age		0.01***	0.01***	0.01***	0.01***	0.02***
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Female		-0.10***	-0.10***	-0.10***	-0.10***	-0.04***
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
MAIN EFFECT						
Share of Low-wage Jobs in Total Temporary Employment	**0.56*** **	**0.39*** **	**0.10** **	**0.32*** **	**0.42*** **	**0.17*** **
	(0.01)	**(0.01)**	**(0.01)**	**(0.01)**	**(0.01)**	**(0.01)**
MODERATORS						
(PT) Precarity Trap [1- Pr(Landing a Perm. Job)]			-0.12***			
			(0.02)			
PT X Share of Low-paying Jobs in Total Temp. (Hyp. 2)			0.40***			
			(0.05)			
Human Capital Intensity (HC)				-0.27***		
				(0.02)		
HC X Share of Low-paying Jobs in Total Temp. (Hyp. 3)				0.32***		
				(0.05)		
Herfindahl Hirschman Index (HHI)(Large Firms)					1.11***	
					(0.12)	
HHI X Share of Low-paying Jobs in Total Temp. (Hyp. 4)					-1.53***	
					(0.19)	
N	4,967,236	4,967,236	4,967,236	4,967,236	4,967,236	3,256,495

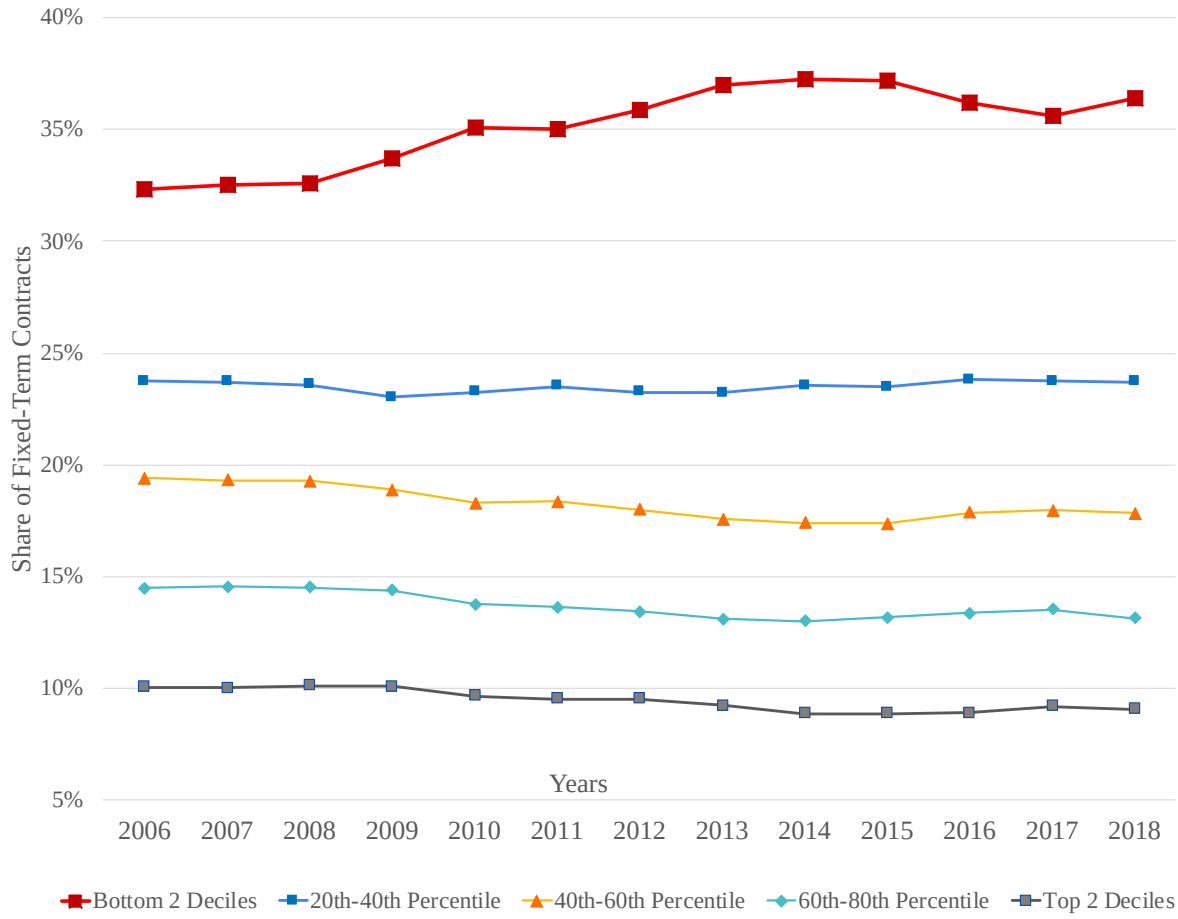
Notes: All models report the results of the variance function regression, i.e., λ_1 coefficient from the equation (2). Corrected standard errors (in parentheses) were calculated using the iterated weighting procedure, as described in the "Statistical Approach" section. *** p<0.001, ** p<0.01, * p<0.05.

Table 4.4. Quantile Regressions (Dependent Variable: Log Hourly Wage)

VARIABLES	Model 1 10th	Model 2 25th	Model 3 50th	Model 4 75th	Model 5 90th
MAIN EFFECT					
Share of Low-wage Jobs in Total Temporary Employment	-0.03***	0.08***	0.13***	0.16***	0.17***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Industry's dependence on temporary employment	-0.05***	-0.05***	-0.04***	-0.04***	-0.03***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
INDIVIDUAL-LEVEL CONTROLS					
(No education / primary education)					
Secondary Education	0.07***	0.08***	0.09***	0.12***	0.14***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Bachelor Degree / College	0.12***	0.13***	0.14***	0.18***	0.21***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Masters or Above Education	0.16***	0.18***	0.22***	0.32***	0.41***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Age	0.01***	0.01***	0.01***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Female	-0.14***	-0.13***	-0.14***	-0.17***	-0.20***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
FIXED EFFECTS					
Region fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Mean industry fixed effects	Yes	Yes	Yes	Yes	Yes
Occupation fixed effects	Yes	Yes	Yes	Yes	Yes
Education fixed effects	Yes	Yes	Yes	Yes	Yes
Industry group fixed effects	Yes	Yes	Yes	Yes	Yes
Region controls (GDP per capita, population growth)	Yes	Yes	Yes	Yes	Yes
N	4,967,236	4,967,236	4,967,236	4,967,236	4,967,236
R-squared	0.42	0.43	0.44	0.44	0.43

Notes: The quantile regressions estimate the effect of the share of low-wage jobs in total temporary employment in each industry-region-year unit on log hourly wages at different percentiles of the conditional distribution of wages. Models 1 to 5 show the effect of the main independent variable on the 10th, 25th, 50th, 75th, and 90th percentiles of the wage distribution, respectively. The dependent variable in all models is logged hourly wage (Note that it is different from the variance function regressions, which analyze residual variance of logged hourly wages). Standard errors are in parentheses. *** p<0.001, ** p<0.01, * p<0.05.

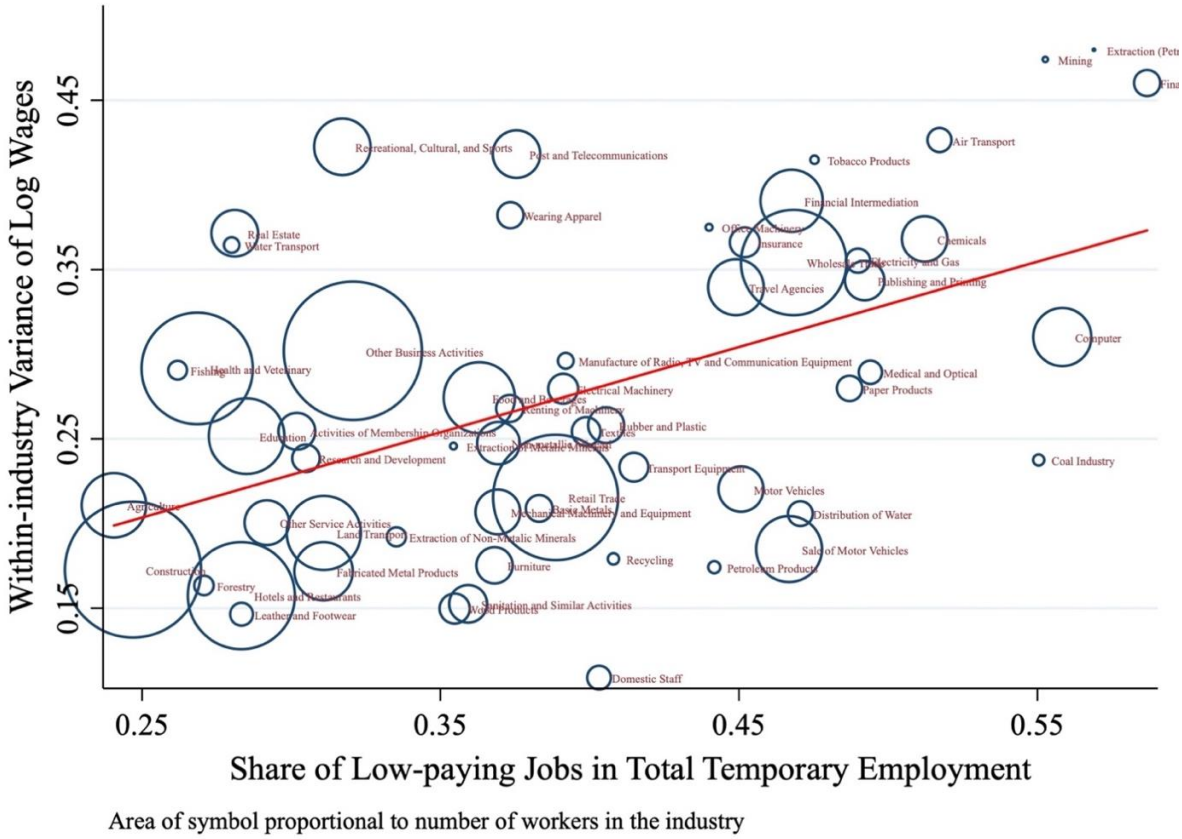
Figure 4.1. Ratio of Temporary Employment Occupied by Different Income Clusters



Notes: This graph plots the ratio of temporary employment, that is occupied by different income layers in Spain (2006-2018). The red line represents the proportion of temporaries in the bottom two deciles, while the gray line represents the share at the top 20 percent of the wage distribution. If temporary employment were evenly distributed to different income layers, we would expect these lines to overlap around 20 percent. However, low-wage jobs consistently occupy the highest percentage of temporary employment. In 2018, for each temporary contract in the top of the distribution (i.e., in top two deciles), there are four temporary contracts in the bottom two deciles (i.e., in 2018 low-wage jobs occupy 36% of total temporary employment, while high-wage jobs occupy only 9%).

Source: Spanish Social Security records: MCVL - Muestra Continua de Vidas Laborales con Datos Fiscales (Continuous Working Histories

Figure 4.2. Share of Low-Paying Jobs in Total Temporary Employment and Within-industry Variance of Wages



Notes: This graph shows the association between the share of low-wage jobs in total temporary employment (main independent variable) and within-industry wage variance. Circles represent industries and their sizes. The figure shows that industries differ from each other in terms of the distribution of temporary employment in different wage layers. The red prediction line's positive slope suggests a simple positive association between the ratio of temporary employment that is occupied by low-wage jobs and within-industry wage inequality.

5. Conclusion and Final Remarks

This dissertation centers on changing work and employment practices. At the individual-level, we examine the effects of these changes on individual's career trajectories. At the firm-level, we look at the implications of these changes on firms' quest to retain their valuable employees, i.e., firm-specific human capital, and creating competitive advantage through human capital. At a macro-level, we inquire how these changes in mobility patterns and contracting practices contribute to social inequality.

While our conceptual development relies on the rich sociology of labor markets literature, our study provides unique contributions specifically to the literatures on social evaluation, career mobility, and inequality. The first chapter advances research on categories and status, as well as the vibrant stream of research at their intersection (Bowers & Prato, 2018; Delmestri & Greenwood, 2016; Durand & Kremp, 2016). In this chapter, we also draw on and advance executive mobility research to test our integrative framework, providing important theoretical and empirical advancements on the antecedents and consequences of executive mobility (e.g., Boivie, Graffin, Oliver, & Withers, 2016; DiPrete, Eirich, & Pittinsky, 2010). In the second chapter, we again focus on individual mobility decisions, in light of the specific conditions surrounding an economic crisis. In the third chapter, we examine how the increasing replacement of permanent contracts with more flexible employment arrangements, and specifically the high concentration of these flexible arrangements in the bottom of the wage distribution, impacts wage inequality. In so doing, we refine and extend rent-destruction accounts of market-based employment relationships (e.g., Dencker & Fang, 2016).

In addition to the theoretical contributions, these essays provide important empirical advancements that will facilitate future research. Throughout the dissertation, we have compiled several datasets, as well as replicated and verified some important findings from prior research. In the first chapter, derived from U.S.-based organizational contexts, we use datasets including but not limited to ExecuComp, Compustat, BoardEx, Thomson Reuters SDC database on M&A deals, and industry similarity matrices based on web crawling data and text parsing algorithms. Also as part of the first chapter analyses, we replicate and verify Gentry, Harrison, Quigley, & Boivie (2021) coding for various forms of voluntary and involuntary CEO departures, as well as extend their

procedure to non-CEO executives. Our reason-for-leave protocols following and extending state-of-art research in executive mobility, together with our new measures of industry status and industry relatedness, based on web-crawling data and text-parsing algorithms trained on business descriptions, will help future researchers explore related questions.

In Chapters 2 and 3, we have worked with a novel dataset on career and wage trajectories of a representative sample of the Spanish labor market. While organizational research increasingly seeks and integrates administrative linked employer-employee datasets, few studies exist of Spanish records. This research scarcity is mainly due to the operational difficulties inherent in building panel datasets from the Spanish workforce, a process that requires significant time, effort, and computing power. Using our novel dataset, we can track individuals and their wage and mobility trajectories, as well as gather details about their firms dating back to 1980, to explore the critical questions which can only be addressed via this kind of big longitudinal data (e.g., composition of the workforce in organizations and its role in explaining business dynamics, the flow of workers across new and established firms and its performance implications).

These longitudinal datasets and our protocols for building them will pave the way for future research. We are particularly excited about future research that will be conducted, once these datasets are extended with 2020 and 2021 data, corresponding to Covid-19 outbreak years. Despite the numerous challenges unleashed, from the research standpoint, the pandemic constitutes an impactful exogenous shock that could allow causal identification of organizational problems and shed light on how organizations can enhance their resilience to future disruptions. Taking into consideration Spain's dual labor market institutional setting and pandemic-related shifts, we expect these novel datasets and our protocols to make these disruptions researchable to offer the foundation for several new critical explorations concerning other shocks.

Finally, in addition to their theoretical and empirical implications, all three chapters shed light on relevant contemporary debates on work and employment. Given the rising relevance of individuals' changing and quitting jobs *en masse*, evidence-based research on understanding preferable jobs is paramount (e.g., Fang & Tilcsik, 2022; Wilmers & Zhang, 2022). In this regard, we provide insights as to what kind of

nonpecuniary rewards might motivate individuals to work (Chapter 1) and what kind of considerations might make individuals leave their firms (Chapter 2). Our findings from the Chapter 3 sheds light on two other important topics of policy debate: i. temporary employment and ii. wage inequality. These two topics are particularly important for Spain, as Spain stands out not only by having one of the highest rates of temporary employment arrangements among OECD countries but also by being one of the most unequal countries in the EU, ranking fourth after Romania, Bulgaria, and Greece (Vaughan-Whitehead, 2016). By shedding light on the impact of temporary work arrangements on increasing wage inequality, our findings provide valuable insights to the intensified public debate on inequality, especially in the aftermath of the economic crisis.

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