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Creating a More Quit-Friendly National Workforce? Individual Layoff  
History and Voluntary Turnover

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### Abstract

While Bureau of Labor Statistics data reveal that U.S. employers laid off over 33 million employees since 1994, virtually no research has addressed the behavior of layoff victims upon reemployment. In a first step, we investigate how layoffs shape voluntary turnover behavior in subsequent jobs. Utilizing a recently developed fixed effects specification of survival analysis, we find that a layoff history is positively associated with quit behavior. This effect is partially mediated by underemployment and job satisfaction in the post-layoff job. The remaining indirect effect is consistent with the notion that layoffs produce a psychological spillover to post-layoff employment, which then manifests in quit behavior. We also find that layoff effects on turnover attenuate as an individual's layoffs accumulate and vary in magnitude according to the turnover "path" followed by the leaver.

## Creating a More Quit-Friendly National Workforce? Individual Layoff

### History and Voluntary Turnover

One of the standard mechanisms through which today's companies attempt to cope with competitive pressures is through downsizing (Cascio, 1993). The extent to and manner in which downsizing contributes to subsequent organizational performance is a complex issue, with scholars offering explanations for both positive (e.g., Love & Nohria, 2005) and negative downsizing effects (e.g., Trevor & Nyberg, 2008). What there is no dispute about, however, is the prevalence of downsizing in the U.S.—the Bureau of Labor Statistics reports over 30 million layoffs between 1994 and 2010, a figure that does not include small-scale layoffs (i.e., those displacing fewer than 50 employees). While layoffs are more numerous when firms are struggling economically (Cascio, 1993), company financial health, employee tenure, job performance, and industry all fail to protect American employees from the layoff axe (Cascio, 2002).

Often lost in the analysis of layoffs and their effects on the companies that initiate them is that a majority of layoff victims ultimately find new jobs. In the aggregate, these millions of victims therefore comprise an increasingly noteworthy proportion of this nation's workforce. Research documents the trauma and disruption that layoffs can bring to individuals, including damaging effects to income (e.g., Kletzer & Fairlie, 2003; Seninger, 1997), physical health (e.g., Kivimaki, Vahtera, Elovainio, Pentti, & Virtanen, 2003), mental health (e.g., Kets de Vries & Balazs, 1997), and general attitudes about work (e.g., Brockner et al., 1994; Konovsky & Folger, 1991; Wanberg, Bunce, & Gavin, 1999). What remains unclear, however, is whether these or other layoff outcomes ultimately manifest in worker behaviors in post-layoff employment (Datta, Guthrie, Basuil, & Pandey). If layoffs do affect the behaviors of their victims upon

reemployment, layoffs become relevant not merely for the organizations that engage in them, but for all organizations. Indeed, behavioral changes after a layoff amount to material human capital changes within the external talent pool upon which virtually all firms rely. Hence, our aim here is to begin to determine how employee behavior is evolving as a function of the downsizing strategies that are now standard business practice (Cascio, 1993).

One of the most crucial of these behaviors is voluntary turnover, which is notoriously expensive (e.g., Cascio, 2000) and, in the aggregate, is a critical predictor of a variety of organizational performance outcomes (Hausknecht & Trevor, 2011). We argue that a layoff history should be associated with greater voluntary turnover likelihood in post-layoff employment, a prediction that is supported through the development of a broad theoretical rationale that integrates complementary arguments from several otherwise distinct literatures. While these arguments explore different mechanisms for a layoff effect, each is consistent with the expectation that layoffs produce psychological effects that spill over into subsequent employment relationships and loosen the ties between employee and employer. Measures of job satisfaction and underemployment in the post-layoff job, as well as data on the reason for quitting, allow us to better infer whether any layoff-quit associations are consistent with our broad rationale. Thus, our work lays a theoretical foundation for the wider study of layoff victims' behaviors in post-layoff employment. In exploring this framework, we provide the first direct evidence on whether important employee behaviors are a function of a prior employer's severing of the employment relationship. Given that the broad and generally indiscriminant reach of layoffs affects tens of millions of American workers, the study also speaks to the potentially highly consequential effects of institutionalized downsizing on the subsequent stability of the modern American workforce.

### Construct Definitions and Theory

*Layoffs* can be understood as one avenue for downsizing, which is the planned reduction in workforce (Cascio, 1993). While natural attrition also constitutes downsizing activity (Cascio, 2010), we focus on layoffs, defined here as the termination of non-temporary employment for business reasons (e.g., cost reduction). Although layoff victims may ultimately be selected from the feasible set according to, for example, their position in the performance distribution, a layoff requires that the initial decision to reduce headcount be motivated by business-level concerns. The post-layoff behavior of interest here is *voluntary turnover*, defined as any employee-initiated separation (i.e., a quit). We also explore two aspects of the post-layoff job as potential mediators of the proposed relationship: *underemployment*, which is present when the new job is inferior or of lower quality than the prior job (Feldman, 1996); and *job satisfaction*, the degree to which one is content with his or her job.

Our theoretical approach to the issue of whether being laid off makes one more likely to quit in subsequent employment is built upon two rather distinct conceptual platforms. One of these is quite straightforward, primarily involving connecting bivariate relationships that are established within the literature. The other, however, while more intriguing, is also more difficult to study, requiring inferences about unmeasured mediating constructs to link the two readily observable events of interest here. In short, we envision that a layoff history results in greater likelihood of quitting behavior for two distinct reasons: (a) psychological spillover, in which experiencing a layoff leads to an untethering from, or looser tie to, subsequent employment, as suggested by the literatures addressing psychological contracts, trust, job insecurity, and the unfolding model of turnover's shock construct; and (b) occupational underemployment, in which layoffs result in lower-quality post-layoff employment (see Figure 1). The former explanation

can both allow greater variety in layoff conceptualization (e.g., layoffs anytime in one's employment history) and provide more compelling implications for theory and practice. Thus, we first describe the application of the various literatures that converge in support of the layoff experience itself spilling over into perceptions of looser ties to post-layoff employment. We then explore the occupation-based explanation (underemployment in post-layoff employment) for the layoff-turnover relationship. Finally, we further examine the validity of our conceptual framework by investigating whether the pattern of layoff effects on turnover across different quit reasons (i.e., turnover paths) is consistent with our theory.

[Insert Figure 1 about here]

#### *Layoffs, Psychological Spillover, and Voluntary Turnover in Post-Layoff Jobs*

Broadly, we suggest that the psychological ties that bind individuals to organizations are weakened by the experience of a layoff. This expectation of psychological spillover, in which a past layoff untethers the victim from subsequent employers, is evident in the lay business press's frequent characterization of layoffs as precipitating a free agent mentality, leaving the workplace replete with employees with low levels of commitment and loyalty to the employer (O'Reilly, 1994; Hirsch, 1987; Munk, 2000; Pink, 2001). Yet, specific theorizing is lacking to date. Fortunately, however, several conceptual frameworks suggest how this spillover can occur. While we are limited here in that we cannot measure the mediating mechanisms that we describe, the manner in which these literatures converge to yield the prediction of a positive layoff-turnover association provides a strong, albeit indirect, conceptual basis for our hypotheses (see Figure 1).

*The psychological contract in post-layoff jobs.* The psychological contract describes the reciprocal exchange agreement between employee and employing organization, as perceived by

the worker (Rousseau, 1989). While these perceptions of what is owed to, and from, the employer are influenced by the employee's interactions with organizational representatives, complete information regarding employer intentions is unlikely, especially at early stages of the employment relationship (Rousseau, 2001). As a consequence, new employees rely on generalized employment schemas—mental organizing frameworks that frame the employment experience—to help guide the establishment of the terms of the psychological contract (Rousseau, 2001). We argue that layoffs affect these schemas and thereby influence post-layoff expectations of both employer and employee obligations surrounding long-term employment.

Schemas, including those specific to employment, resist change. Nevertheless, they are affected by experience (Crocker, Fiske, & Taylor, 1984), with perceptions of substantial differences between schemas and experience bringing about adjustment (Rumelhart & Norman, 1978). Being laid off would appear to provide such discrepant information. Indeed, research suggests that layoffs often engender, among victims, the negative affective experience of psychological contract violation (Brockner et al., 1994; M. S. Kim & Choi, 2010; McLean Parks & Kidder, 1994; Morrison & Robinson, 1997; Pugh, Skarlicki, & Passell, 2003; Rousseau, 1989) and the underlying perception that the organization has breached its obligations under the contract (Rust, McKinley, & Edwards, 2005). That layoffs yield perceptions of contract breach and negative violation reactions implies both that pre-layoff schemas are deficient in their weighting of the layoff threat (given that employment schemas guide expectations surrounding employer obligations) and that the victim's attention will be focused on this deficiency. Accordingly, we argue that layoffs compel their victims to adjust employment schemas to allow for an increased likelihood of future layoffs. Given the reciprocal nature of psychological contracts, a greater salience of the layoff threat, because it indicates reduced employer

commitment to long-term employment, similarly frees the layoff victim from any such obligation in future employment relationships. Reduced felt obligation to remain with an employer, more formally characterized as low “normative commitment,” is associated with greater voluntary turnover in meta-analytic studies (e.g., Meyer, Stanley, Herscovitch, & Topolnytsky, 2002). Thus, while the separation of employee and employer after a layoff ends the employment relationship, we expect that employment schema change spills over to subsequent employment relationships, positively affecting victims’ voluntary turnover likelihood.

*Trust in post-layoff jobs.* A second conceptual framework that predicts a layoff effect on voluntary turnover in reemployment is based on the trust literature. “Trust is a psychological state comprising the intention to accept vulnerability based on positive expectations of the intentions or behavior of another” (Rousseau, Sitkin, Burt, & Camerer, 1998, p. 395). We suggest that a layoff from one job will reduce victims’ trust of subsequent employers. According to McEvily, Perrone, and Zaheer (2003, p. 94), “rather than being based on direct experience with the object of trust, initial trust impressions can be based on trust in a source other than [this object], such as another individual or collectivity.” Stewart (2003) found empirical support for consumers exhibiting such “trust transfer” across organizational settings. In the context of a layoff, this trust transfer construct suggests that a victim’s initial trust in a post-layoff employer could be derived from his or her trust in the employer that previously imposed the layoff. In addition, trust is difficult to repair, requiring both the re-establishment of positive expectations and the overcoming of negative expectations (P. H. Kim, Ferrin, Cooper, & Dirks, 2004). Hence, reduced trust in a layoff employer that is transferred to a subsequent job will tend to remain low. Empirical studies of layoff-driven trust effects support such conceptualizing. Pugh et al., (2003) and Kim and Choi (2010) each found that laid-off individuals held lower trust in the post-layoff

employer than in the pre-layoff employer, with Pugh et al., (2003) additionally finding increased cynicism and worry about mistreatment.

Morrison and Robinson (1997) posited that low trust promotes employee monitoring of what the organization provides to the employee. An increase in such employee vigilance can, in turn, result in a greater likelihood of perceived discrepancies between the employee's experienced outcomes and his or her expectations of what the organization is obligated to provide. This suggests, consistent with March and Simon's (1958) stipulation that quitting will be more likely when perceptions of employee contributions exceed the perceived value of employer inducements, that reductions in trust will subsequently lead to more turnover. Relevant research is consistent with these positions, with meta-analytic support for the negative relationship between trust in the employer and turnover intentions (Dirks & Ferrin, 2002). Consequently, given the expectation that loss of trust spills over from the layoff-job to post-layoff employment, we anticipate that layoffs will predict voluntary turnover in subsequent jobs.

*Shock impacts in post-layoff jobs.* Similar to the case with the psychological contracts literature, analysis of the unfolding model of turnover (T. W. Lee & Mitchell, 1994; T. W. Lee, Mitchell, Wise, & Fireman, 1996) suggests that a layoff in one job can predispose an employee to be more likely to quit a later job. The basis for this extension across job boundaries is the model's focus on incidents referred to as shocks. Shocks are jarring events that lead employees to make judgments about their jobs. Research on the unfolding model of turnover has shown that a layoff is a shock to the system that launches psychological decision processes associated with quit behaviors among layoff survivors (T. W. Lee et al., 1996; Trevor & Nyberg, 2008). While the implications of layoffs-as-shocks have yet to be addressed among layoff victims, the literature surrounding the unfolding model suggests that victims' evaluations of the post-layoff

work context should be affected. Specifically, Lee and Mitchell (1994) describe shocks as shaking employees from natural inert tendencies regarding the external job market. While attention to the daily routines of work and family life often divert attention away from alternative job opportunities, shocks force people into reappraisal of job-relevant data: “shocks to the system constitute the jarring event that forces people to notice readily available opportunities” (Lee & Mitchell, 1994, p. 71). Although shock effects across jobs have not been addressed in the unfolding model literature, a shock’s impact on the awareness of outside opportunities should be quite salient to those forced to suffer the psychological, social, and financial ramifications of a layoff. Indeed, Lee and Mitchell (1994) note that, more generally, shocks heighten a person’s need to understand the organizational environment. For the layoff victim, the relevant organizational environment is associated with the post-layoff job, rather than the job from which he or she was displaced.

Hence, we posit that individuals reemployed following a layoff shock will be more likely to attend to external job opportunities. With greater awareness of alternatives, it is increasingly likely that a prospective employer offering utility that exceeds that associated with the current employer will be discovered. This imbalance then results in a greater tendency to quit (March & Simon, 1958; Mobley, Griffeth, Hand, & Meglino, 1979; Trevor, 2001).

*Job insecurity in post-layoff jobs.* The lay business press has long proclaimed the existence of the layoff-based psychological spillover and untethering that we adopt as our general conceptual frame. Specifically, these sources have argued that the prevalence of layoffs in the U.S. economy has created a free-agent mentality in which worker perceptions of job security, and subsequently their loyalty, have greatly diminished (O’Reilly, 1994; Hirsch, 1987; Munk, 2000; Pink, 2001). Perceptions of job insecurity, defined as concerns about continuity in a

job situation (Davy, Kinicki, & Scheck, 1997), and reduced loyalty should be especially prevalent among layoff victims, who, having been previously targeted for layoff, are less likely to underestimate the threat of layoff. The job insecurity research, however, does not explicitly address the key spillover assumption underlying the lay business free-agent argument—i.e., that a layoff event induces job insecurity upon the victim's reemployment. A recent qualitative study of 77 unemployed layoff victims, however, does find that layoffs yielded the adoption of a free-agent mentality in which there was no expectation of loyalty from either employer or employee (Mendenhall, Kalil, Spindel, & Hart, 2008). To the extent that these no-loyalty expectations continue as the unemployed are hired into post-layoff jobs, this study supports the psychological spillover across jobs that we espouse.

Should such spillover of job insecurity perceptions to post-layoff employment exist, the extant research on job insecurity effects suggests that layoffs will, via greater job insecurity perceptions, lead to quit behavior in post-layoff jobs. This inference is consistent with the meta-analytic finding of a negative association between job insecurity perceptions and organizational commitment (Cheng & Chan, 2008; Sverke, Hellgren, & Näswall, 2002), which is negatively related to turnover (e.g., see Griffeth, Hom, & Gaertner (2000) for meta-analytic support), as well as a positive association between job insecurity and emotional exhaustion (Boswell, Olson-Buchanan, & Harris, 2014), which is positively tied to turnover behavior (e.g., Lapointe, Vandenberghe, & Panaccio, 2011). Moreover, meta-analyses report strong evidence that perceived job insecurity and turnover intent are positively related (Cheng & Chan, 2008; Sverke et al., 2002), further suggesting that a layoff history makes one more likely to quit.

*Layoff effects on voluntary turnover.* The literatures on psychological contracts, trust, employment shocks, and job insecurity all suggest that the psychological impact of a layoff spills

over into subsequent employment, yielding an untethering from the job, as characterized by adapted employment schema and reduced felt obligation, enhanced awareness of external job opportunities, lower trust, and heightened perceptions of job insecurity. Thus, given that these factors serve to increase the ease in and attractiveness of quitting, we expect a greater probability of voluntary turnover in jobs that follow a layoff.

*Hypothesis 1: A layoff history increases the probability of voluntary turnover.*

*Attenuation of layoff effects (curvilinearity).* As layoffs accumulate, however, it is likely that the layoff impact on the explanatory mechanisms eventually weakens. With each layoff, ensuing adjustments to general employment schema and perceived obligations of employers leave subsequent layoffs less likely to be inconsistent with the psychological contract. Similarly, with sufficient layoffs, employee trust in the employer's personnel decisions and employee expectations of job security are likely to be almost completely eroded, at which point additional layoffs should not yield further untethering from post-layoff jobs. Moreover, the degree to which a layoff constitutes a shock should degrade over repeated experience with being laid off, thereby reducing increases in the deliberate appraisal of alternative opportunities; at the same time, with accumulated layoff shocks, the attention available to be focused on alternative employment opportunities is likely to approach a ceiling. In sum, when understood as a likely outcome, an additional layoff eventually should have diminished influence, once reemployed, on felt obligation, trust, job insecurity, and assessment of job alternatives.

Although no previous research has examined the influence of multiple layoffs on individual attitudes or behaviors, this expectation of curvilinear layoff effects is also supported by Pierce and Aguinis' (2013) recent argument that, within the management literature, "all seemingly positive monotonic causal relations (i.e.,  $X \rightarrow Y$ ) reach a context-specific inflection

point,  $I$ , after which they cease to be positive, resulting in an overall pattern of curvilinearity” (p. 317) and by an established precedent for non-linearity in voluntary turnover likelihood (e.g., Nyberg, 2010; Salamin & Hom, 2005; Trevor, Gerhart, & Boudreau, 1997). We therefore anticipate smaller effects on voluntary turnover behavior as layoffs accumulate.

*Hypothesis 2: The positive relationship between a layoff history and voluntary turnover attenuates with multiple layoffs.*

#### *Layoffs, Underemployment, and Voluntary Turnover in Post-Layoff Jobs*

Arguments for psychological spillover and weakened ties to post-layoff employers obscure the fact that a layoff may also reduce the quality of future employment (Karren & Sherman, 2012). Feldman (1996), for example, argued that, after a layoff, individuals are likely to experience underemployment—defined as inferior or lower quality employment, as compared to some standard (e.g. a past job). Underemployment is, in turn, negatively related to job satisfaction, a classic turnover antecedent, and positively related to job search and the intention to quit (e.g., Feldman, Leana, & Bolino, 2002; D. C. Maynard, Joseph, & Maynard, 2006; McKee-Ryan & Harvey, 2011), consistent with Mobley’s (1977) influential characterization of voluntary turnover as a function of the relative levels of current employer utility and expected alternative employer utility. Thus, independent of psychological spillover, layoffs may contribute to turnover simply because the victims are reemployed in inferior jobs (see Figure 1).

Post-layoff jobs may involve lower pay, which is how underemployment is commonly described and operationalized (e.g., Feldman, 1996; Feldman et al., 2002). In addition to wage underemployment, hours underemployment, sometimes referred to as involuntary part-time status (i.e., employment that offers fewer hours than the employee would prefer) appears in the literature as an alternative operationalization (e.g., Maynard, et al., 2006). While we investigate

wage and hours underemployment here, we note that inferior jobs, of course, also can entail additional drivers of job dissatisfaction, such as lower quality benefits, promotion opportunities, working conditions, co-workers, and supervisors. To account for such sources of dissatisfaction that arise from lower quality jobs, and given job satisfaction's well established status as a turnover antecedent (e.g., see Griffeth, Hom, and Gaertner (2000) for meta-analytic support), we investigate both job satisfaction and underemployment as evidence that lower quality jobs mediate layoff effects. Hence, based on the assumption that layoff victims should be more likely to quit when they find themselves reemployed in inferior jobs, we predict that underemployment and job satisfaction partially mediate the positive effect of layoffs on victims' subsequent voluntary turnover. Further, given the causal ordering described above, we anticipate that layoff-driven underemployment operates on turnover through dissatisfaction, as low pay or inadequate hours are likely viewed by employees as undesirable characteristics of the post-layoff job.<sup>1</sup>

*Hypothesis 3: The positive relationship between a layoff history and voluntary turnover is partially mediated by underemployment.*

*Hypothesis 4: The positive relationship between a layoff history and voluntary turnover is partially mediated by job satisfaction.*

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<sup>1</sup> Notably, the proportion of the layoff effect not mediated by underemployment and job satisfaction (i.e., the direct effect of layoffs on voluntary turnover) represents the effect available for explanation by the four processes described earlier in the psychological spillover section. We emphasize, however, that the presence of a direct effect is evidence only that underemployment does not fully mediate a layoff-turnover relationship, and does not, in and of itself, support the presence of any of these psychological spillover explanations.

*Hypothesis 5: The indirect effect of a layoff history on voluntary turnover that is partially mediated by underemployment is further mediated by job satisfaction.*

#### *Layoffs and Voluntary Turnover Paths*

We have contended that layoff victims are more likely to leave reemployment for two distinct reasons: an untethering from post-layoff jobs that spilled over from the layoff event and reemployment in lower quality jobs that results in underemployment and lower job satisfaction. The validity of the former of these two explanations can be further examined by exploring relative layoff effects along different voluntary turnover “paths” that leavers follow. Recently, T. H. Lee, Gerhart, Weller, and Trevor (2008) demonstrated that the influence of job satisfaction, the most researched turnover antecedent in the literature, depended on whether one quit to search for another job, quit to take another job following a successful search, quit to take an unsolicited job offer, or quit for family reasons; these turnover paths approximated several of the distinct paths to leaving originally proposed in the unfolding model of turnover (T. W. Lee & Mitchell, 1994; T. W. Lee, Mitchell, Holtom, McDaniel, & Hill, 1999). Just as their path-specific results better explained how job satisfaction functions in various turnover decision processes, investigating layoff effects across separate paths can yield meaningful inferences into turnover decision processes influenced by layoffs. That is, should a layoff history reduce one's sense of being tied or tethered to post-layoff employment, we should not only see layoff effects on quits, but should also see a predictable pattern of effects across various turnover paths.

Two of the four paths studied by T. H. Lee et al. (2008)—quitting to accept an unsolicited job offer and quitting to accept a solicited job offer (i.e., an offer that arose from a successful job search)—appear particularly susceptible to the influence of psychological spillover and untethering. Because the job offer's presence means that, at the turnover decision

point, virtually no effort is required to secure and enter into a new job, these two paths are characterized by maximum levels of March and Simon's (1958) fundamental construct of ease of movement in the job market (T. H. Lee et al., 2008). As such, employees with a job offer and a layoff history possess both a viable alternative employment option and diminished psychological tethering to the post-layoff employer. Essentially, a reasonable job offer makes it easy for the reemployed layoff victim to move and the looser ties to the employer suggests there is little reason not to do so. Given this rather potent combination of ingredients for deciding to quit, we expect layoffs to readily predict voluntary turnover in paths where an offer preceded the quit decision (controlling for layoff effects that operate through job satisfaction and underemployment).

*Hypothesis 6: A layoff history increases the probabilities of quitting to accept an unsolicited job offer and of quitting to accept a solicited job offer.*

When one quits to search for a new job, March and Simon's (1958) ease of movement is considerably lower than the complete ease of movement enjoyed when an offer is present. This lower ease of movement in the job market presents uncertainty, as a reasonable offer must first be obtained by search, limiting the extent to which weakened ties to a post-layoff employer provide a known and straightforward transition into the next job. Further, when quitting to search, choosing to leave also means enduring unemployment, an undesirable condition into which layoff victims have particularly vivid insight. Thus, given that quitting to search for employment, relative to quitting with an offer in hand, yields reduced levels of both ease and desirability of movement, we predict the following:

*Hypothesis 7: The layoff effect on quitting to accept an unsolicited job offer and on quitting to accept a solicited job offer should each be greater in magnitude than the layoff effect on quitting to search for a new job.*

## Data and Method

### *Sample*

Our study draws data from the National Longitudinal Survey of Youth, 1979 Cohort (NLSY79), which is one of several national longitudinal survey programs administered by the Bureau of Labor Statistics. The NLSY79 is particularly suited to the research questions examined here as it includes multiple survey administrations that retrospectively capture the employment histories of a large and diverse sample of American men and women. The first survey was given in 1979 when respondents were between 14 and 22 years old. The NLSY79 has followed these same individuals across 23 follow-up surveys, conducted either annually or biennially, resulting in an ongoing panel that begins January 1, 1978. The most recent administration of the NLSY79 was completed in 2010. With each survey administration, data are collected for each of (up to) five jobs held since the last survey (or, in the case of the first administration, the five most recent jobs). From these data, we sampled “job spells”—periods of full-time employment with a single employer. Job spells begin when the surveyed individual joins an organization and end when there is a voluntary or involuntary separation (e.g. layoff, termination, quit, etc.). We note that the comprehensive employment history data contained within the NLSY79 yields multiple job spells per respondent, facilitating a within-subjects assessment of the layoff effect.

As the NLSY79 panel has progressed, some aspects of work history have been addressed only intermittently (e.g., respondents were polled about unsolicited job offers only in certain

survey years), while others are addressed with varying levels of specificity across survey administrations (e.g., response options for the item assessing the reason a respondent left a job are not standardized across the panel). As a consequence, it was necessary to draw two samples of job spells, each from different sections of the NLSY79 panel, in order to create measures that accurately reflect our constructs and fully address our research questions. In both samples, we exclude job spells during which the individual was less than 18 years old, was self-employed, was employed for no pay in a family business, was employed part-time (defined as working less than 30 hours in the first or last week of a job), was in the military, or for which data were missing on focal variables. This allowed us to confine our analyses to adult full-time employees who have the option to voluntarily separate from their current employer (see T. H. Lee et al., (2008) for similar exclusions). Because we use a fixed effects methodology (described below) that requires within-individual variation in variable values, our analyses further exclude all job spells of any NLSY79 respondent who did not report at least one instance of voluntary turnover and one layoff during the panel. We note that an overwhelming majority (over 90%) of NLSY79 respondents reported at least one voluntary exit. Layoffs, though not as widespread as voluntary turnover, also affect a significant proportion of NLSY79 respondents: approximately one third of respondents reported at least one layoff across the duration of their job histories while approximately 10% reported more than one layoff.

Our first sample of job spells—which we use to test Hypotheses 1 through 5—includes the full work history of each individual respondent: 12,035 job spells held by 2,439 individuals. Of these spells, 4,907 ended with a layoff. Across person-job-years, this sample was 66.2 % male and 48.4 % white, and ages ranged from 18 to 53 years (mean age = 31.8). A second sample was required to test Hypotheses 6 and 7, which proposed differential effects of layoffs across three

separate pathways to voluntary turnover. The need for the second sample arose because surveys administered prior to 1990 did not distinguish among (a) quits to accept an unsolicited job offer, (b) quits to accept a solicited job offer, and (c) quits initiated to engage in job search. The 1990 survey, however, revised the assessment of employment terminations such that jobs ending after 1990 can be classified into one of the three voluntary turnover groups. Consequently, while we are able to utilize each respondent's entire work history to generate our predictor (i.e., prior layoffs), all job spells ending before the 1990 survey administration are excluded from our analysis of voluntary turnover likelihood. Hence, the second sample contains 4,206 spells in total (held by 821 individuals). NLSY79 respondents in this sample reported 1,805 layoffs over the course of their full work histories (i.e., between 1978 and 2010). The second sample was 71.6% male, 50.1% white, and ranged in age from 20 to 53 (mean age = 34.6). See Table 1 for a summary of the sample differences.

[Insert Table 1 about here]

### *Measures*

In addition to the narrative information provided in this section, we list and describe all variables in Tables 1 and 2.

*Voluntary turnover.* With the survey following the conclusion of a job, NLSY79 respondents report, from a list of pre-determined options, the “main reason” that they left that job. From this item, we created a dichotomous outcome variable that identifies, for all job spells included in Sample 1, instances of voluntary turnover (coded 1 for voluntary turnover, 0 otherwise). All exits explicitly identified as a quit (e.g., “Quit to look for another job”, “Quit for pregnancy or family reasons”, “Quit for other reasons”, etc.) or that otherwise suggested an employee-initiated separation (e.g., “Moved to another geographic area”, “Found a better job”,

“Pay too low”, etc.) were coded as voluntary turnover. Responses indicating an employer-initiated separation (e.g., “Layoff”, “Fired”, “End of temporary/seasonal job”, etc.) were coded 0. Censored job spells (i.e., those that did not end within the observation window or for which no reason for separation was given) were also coded 0 (e.g., Salamin & Hom, 2005; Trevor, 2001).

Sample 2, which is used to assess Hypotheses 6 and 7, requires greater specificity in the voluntary turnover outcome measure. We followed the methodology described by T. H. Lee et al., (2008) to create three additional dichotomous variables that distinguish among the voluntary turnover paths of interest. The first variable (*Quit to search for offers*) is coded 1 if an NLSY79 respondent had selected the response option “Quit to look for another job” as the main reason for leaving a job. Job spells that end for all other reasons, as well as those that are censored, are coded 0. The second variable (*Quit to take a solicited offer*) is coded 1 if an NLSY79 respondent had selected the response option “Quit to take another job” as the main reason for leaving a job and, additionally, had indicated on a follow-up survey item that he or she had been engaged in job search at the time the job offer was received (and was coded 0 otherwise). The third variable (*Quit to take an unsolicited offer*) similarly required that the NLSY79 respondent had indicated that he or she “Quit to take another job”, but was coded 1 only when the follow-up item indicated that job search had not taken place (and was coded 0 otherwise). Because the follow-up item used to determine whether job search had preceded the quit was included only on surveys administered between 1990 and 2000, these latter two groups’ paths (i.e., *Quit to take a solicited offer* and *Quit to take an unsolicited offer*) reflect quits occurring between 1990 and 2000. The first path (i.e., *Quit to search for offers*) reflects quits occurring between 1990 and 2010.

*Prior Layoffs.* The occurrence of layoffs, like instances of voluntary turnover, is captured by responses to the item assessing the main reason that the respondent had left a job. We used

the response options “layoff” and “layoff/job eliminated” to identify layoff victims throughout the survey window, except during the period from 1979 to 1983, where these options conflated job exits due to layoff with those due to the temporary/seasonal nature of a job. Because this latter exit is inconsistent with our definition of layoffs, we chose to exclude from consideration “layoffs” reported before the 1984 survey. Thus, for both Sample 1 and Sample 2, the layoff measures reflect all layoffs incurred between 1984 and 2010.<sup>2</sup>

From the NLSY79’s record of layoffs, we created three separate measures that emphasize, for each of the job spells that make up an individual’s work history, a different facet of the individual’s layoff history (as of the job spell in question). The first measure is a dummy variable—*layoff (ever)*—that indicates whether the focal job began at any point after the respondent’s first layoff (i.e., coded 1 if a layoff had ever previously occurred; 0 otherwise). This specification allows assessment of the effect of the first layoff on voluntary turnover likelihood

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<sup>2</sup> The inability to accurately identify layoffs occurring before 1984 presented two imperfect options. The first was to simply exclude all job spells that ended before 1984. However, this approach systematically left-censors the work history of any respondent with employment that ended prior to 1984. This is problematic as a majority of the observations dropped correspond to the “control group” (i.e., job spells occurring prior to a layoff), compromising fixed effects estimates that rely on a comparison of the duration of pre- and post-layoff job spells. Instead, we chose to retain all job spells ending before 1984 and right censor (i.e., code as 0) all pre-1984 layoffs. This second approach, although it inadvertently assigns job spells that should be included in the “treatment group” (i.e., spells occurring after a layoff) to the “control group,” is preferable as it affects fewer individuals and results in a more conservative (or weakened) test of the effect of layoffs on voluntary turnover.

across all subsequent jobs. A second dummy variable—*layoff (most recent job)*—is similarly coded, except that it is coded 1 only when the focal job spell immediately follows (i.e., is adjacent to) a job that ended in layoff. Job spells initiated after the occurrence of a layoff that do not immediately follow it (i.e., job spells that are separated from the layoff by an intermediate job) are ignored in this operationalization. This measure therefore isolates a layoff's effect on the likelihood of voluntary turnover in the next consecutive job. A final measure of prior layoffs specifies, as of the focal job, the number of previously experienced layoffs in the individual's job history (*cumulative layoffs*), facilitating assessment of the individual effect of each additional layoff. We squared this cumulative index in order to model potential curvilinear effects of layoffs on voluntary turnover (*cumulative layoffs*<sup>2</sup>).

We note that the NLSY79 distinguishes between layoffs targeting individuals within the workplace (reduction-in-force layoffs) and layoffs in which the entire workplace is shuttered (workplace closure layoffs). Victims of workplace closures have been shown to deem the organization's explanation for the layoff as more acceptable and to evaluate the layoff itself as more fair than do those targeted for a reduction-in-force layoff (Wanberg et al., 1999). As such, the NLSY79's workplace closure layoffs are less well-suited to our conceptual framework. All layoffs used in this study are of the reduction-in-force type, which comprise the considerable majority of the layoffs reported within the NLSY79.

*Underemployment.* Scholars (e.g., Feldman, 1996; Feldman et al., 2002) describe underemployment as reemployment in jobs that are of lower quality; this is reflected in less-advantageous compensation, skill utilization, and work status (i.e., involuntarily engaged in part-time employment or in work that offers fewer hours than is desired). We assess the role of two facets of underemployment on the layoff-voluntary turnover relationship.

First, because a reduction in wage should be particularly relevant to the decision to quit, we examine the compensation facet of underemployment. Layoffs may leave their victims both financially strained and without ready alternatives for comparable employment, compelling them to accept jobs that pay less than those held previously. Research demonstrates that pay and pay growth are important to the voluntary turnover decision (e.g., Nyberg, 2010; Shaw, Delery, Jenkins, & Gupta, 1998; Trevor et al., 1997). A large decrease in one's wage may evoke perceptions of inequity (Adams, 1963) or relative deprivation (Crosby, 1976; 1984), each of which is linked to voluntary turnover (Aquino, Griffeth, Allen, & Hom, 1997; Zenger, 1992). We include two measures of the compensation dimension of underemployment. The first measure, *wage underemployment (cat.)*, is a 3-point scale in which 0 indicates an hourly pay increase (or no change) between the previous job and the current job, where 1 indicates a decrease that does not exceed 20% of the hourly pay received in the previous job, and where 2 indicates a decrease equal to 20% or more of the hourly pay received in the previous job. This categorical measure is with consistent Feldman's (1996) definition of the wage underemployment construct, with others' use of dichotomous underemployment measures using pay reduction cutoffs of 20% (Zvonkovic, 1988) and 33% (Elder, 1974), and with Feldman et al.'s (2002) use of a 20% or more pay reduction as the truncated anchor of their self-report underemployment scale.<sup>3</sup> Nevertheless, we recognize that the categorization of continuous data risks both the misrepresentation of continuous phenomena as discrete categories and loss of power (Cohen,

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<sup>3</sup> We recognize that our categorical operationalization is nevertheless somewhat arbitrary.

Accordingly, we explored several alternatives, including the use of a 10% pay decrease criterion, a 30% pay decrease criterion for the 2 coding, and a dichotomized scale with a 20% cutoff. In each case, we found similar evidence of mediation.

1992). Accordingly, we also include a second, continuous measure, *wage underemployment (cont.)*, equal to the change in CPI-adjusted hourly pay between the current job and the job held most recently (as a percentage of the pay received in the job held most recently). We multiplied this change by -1 such that greater underemployment (i.e., a larger reduction in pay) takes a positive value.

In addition to the compensation dimension, we also investigate the mediating role of *hours underemployment*, defined as a desire for more working hours than are currently offered (McKee-Ryan & Harvey, 2011). While an ideal measure of this construct should incorporate the individual's preference for working hours (e.g., Abrahamsen, 2010; Wilkins, 2007), the NLSY79 data do not include this information, and we are therefore constrained to operationalize this construct according only to the change in working hours after a job change. Specifically, we created a continuous measure of the change in weekly hours worked between the current job and the job held most recently (as a percentage of the hours worked in the job held most recently). We multiplied this index by -1, such that greater underemployment (i.e., a decrease in hours worked) takes a positive value.

*Job satisfaction.* Job satisfaction is assessed at each administration of the NLSY79 with a single item that asks respondents “*How do you feel about your job with [employer name]?*”. Responses are given according to a four-point scale where “like it very much” is coded 1, “like it fairly well” is coded 2, “dislike it somewhat” is coded 3, and “dislike it very much” is coded 4. We reversed this coding scheme to ease interpretation.

Our use of a single-item global job satisfaction measure is consistent with prior research investigating voluntary turnover within the NLSY79 panel (e.g., Ganzach, 1998; T. H. Lee et al., 2008; Maltarich, Nyberg, & Reilly, 2010; Trevor, 2001). Scholars have demonstrated that single-

item measures, while often conjuring expectations of low reliability, are in fact preferable to summated measures of facet satisfaction when assessing overall job satisfaction (Scarpello & Campbell, 1983) and, moreover, exhibit substantial convergent validity with facet job satisfaction scales (Wanous, Reichers, & Hudy, 1997).<sup>4</sup> Indeed, Ganzach (1998) noted that his results remained “very similar” when a multi-item measure, available for limited survey years only, was substituted for the NLSY79’s single-item satisfaction measure.

*Control variables.* We control for the influence of several additional variables that describe the focal job (or that describe the respondent at the time of the focal job) and are associated with voluntary turnover and/or layoff likelihood. These include industry, as individuals employed in Manufacturing, Mining, Agriculture/Forestry/Fishing, Transportation/Communication, Recreational Services, Professional Services, Finance/Real Estate, and Other industry categories were, generally speaking, more likely to quit than those employed in Public Administration but less likely to quit than those employed in Construction, Business Services, and Trade/Personal Services. We also control for occupation, age, marital status, number of children, education level, residential area (i.e., urban or rural), hourly pay, mean occupational pay, weekly hours worked, employer size, number of prior non-layoff job changes, and the number of months spent unemployed after a layoff. Table 2 lists these variables and the rationale for their inclusion.

[Insert Table 2 about here]

### *Analytical Method*

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<sup>4</sup> Wanous and Reichers (1996) additionally determined that a reliability of .70 was realistic, though likely conservative, for single-item scales, including a measure of job satisfaction.

We use survival analysis, also called event history analysis, to investigate the relationship between prior layoffs and voluntary turnover in the present job (see Allison (1984) for an introduction to survival analysis, or Morita, T. W. Lee, and Mowday (1993) for a discussion within the context of turnover research). Survival analysis is an increasingly utilized methodology in the prediction of turnover (e.g., Dickter, Roznowski, & Harrison, 1996; Kammeyer-Mueller, Wanberg, Glomb, & Ahlburg, 2005; T. H. Lee et al., 2008; Nyberg, 2010; Trevor, 2001). Rather than model a binary outcome, survival analysis makes use of longitudinal duration data to predict the instantaneous hazard of failure—the transition from one state to another. Applied to voluntary turnover, survival models predict the likelihood of transition out of employment (i.e., a quit) at time  $t$ , given “survival” up to time  $t-1$  (we use *weeks* as the unit of time in our analyses). Because these models rely on the duration of each job spell to model the hazard of voluntary turnover, our estimates of layoff effects are conditional on job tenure.

The use of “time-to-event” data carries analytical advantages over regression techniques that model only binary dependent variables. For example, survival analysis allows the use of data from right-censored spells, those that do not end within the study window and those that end for reasons other than the focal event (e.g., rather than drop spells in which the individual was fired for disciplinary reasons, survival analysis makes use of the fact that the individual did not quit during his or her tenure). Additionally, survival analysis’s handling of time-varying covariates is advantageous, particularly in the turnover context where change in antecedents over time is known to influence the decision to quit (Harrison, Virick, & William, 1996; Kammeyer-Mueller et al., 2005; Sturman & Trevor, 2001).

We estimate voluntary turnover hazard using the Cox (1972) proportional hazards model. Models estimated to test Hypotheses 1 through 5 describe the instantaneous hazard of exit due to

any form of voluntary turnover (e.g., Nyberg, 2010; Salamin & Hom, 2005; Trevor et al., 1997). Models estimated to test Hypotheses 6 and 7, on the other hand, take on a competing risks specification (e.g., Allison, 1984; T. H. Lee et al., 2008) wherein the hazard of voluntary turnover is separately modeled across three distinct pathways (i.e., quits to accept unsolicited job offers, quits to accept solicited offers, and quits to engage in job search). These pathways are competing in the sense that once one of them is followed through, a job spell is no longer at risk of ending by way of the other two. The independence of the processes underlying these turnover paths (e.g., T. H. Lee et al., 2008) allows for cross-model comparisons within the context of competing risks survival analysis (Narendranathan & Stewart, 1991).

The Cox model is considered semi-parametric in that it does not require that a functional form of the baseline hazard (i.e., the change in risk over time at baseline levels of covariates) be specified. Indeed, under the assumption of proportionality, the Cox model does not require a baseline function to compute hazard ratios, the coefficients that describe the multiplicative effect of a covariate on the baseline hazard. The semi-parametric nature of the Cox model is advantageous not only because a priori identification of the baseline is unnecessary, but also because it allows for a fixed effects specification of the model (Allison, 2009). Within the NLSY79 panel, most individuals report multiple job spells over the length of their work histories, allowing for the removal of unobserved sources of variation that remain constant within individuals. This is achieved within the Cox proportional hazards model by stratifying by individual, which affords each respondent a unique baseline function into which the unobserved, person-constant error is moved (Allison, 2009). Because the baseline is not used to compute hazard ratios, estimates are free of constant, person-specific error. However, because we allow a

unique baseline for each of the 2,439 individuals sampled, summary statistics that are typically presented with survival analysis models (e.g., cumulative baseline hazard) are not possible.

Our use of the recently developed (Allison, 2009) fixed effects specification of the Cox model is notable in that it appears to be the first such instance within the management literature, although scholars have begun to advocate for the use of this method (Weller, Michalik, & Mühlbauer, 2013). We find that this Cox model, in which we stratify by individual, provides several advantages over a non-stratified model. Substantively, a fixed effects approach minimizes threats to internal validity; most critically, the methodology eliminates the likelihood that an unobserved, person-specific characteristic that is constant over time (e.g., motivation, ability, job performance, etc.) is driving both layoffs and voluntary turnover hazard. This problem, which amounts to dependence across spells, has commonly been addressed through the use of robust standard errors (e.g., Lin & Wei, 1989), though this method in no way accounts for potential bias in the effect estimates themselves. A few authors have attempted to address the potential for this bias through controlling for proxies for the omitted individual-level characteristics. Such attempts (Fichman, 1988; Trevor, 2001) follow Allison's (1984) suggestion to control for the number of prior events and previous spell length. While this seldom-used methodology may be helpful, the fixed effects specification is a far more direct and comprehensive approach to eliminating the bias caused by a within-person omitted variable.<sup>5</sup>

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<sup>5</sup> Nevertheless, there are drawbacks to the use of fixed effects survival analysis. Primary among them is the potential loss of power. Specifically, Allison (2009) notes that: a) individuals reporting only one spell, and those reporting only two spells where the duration of the second exceeds the first, are dropped from the analysis; and (b) because the analysis is driven by within-individual variation on the covariates, biased estimates and inflated standard errors are possible

Although the fixed effects specification allows for a unique baseline hazard function for each of the individuals within the sample, predictors are nevertheless assumed to carry a proportional effect on the hazard across these baselines. We used Grambsch and Therneau's (1994) proportionality test, which evaluates a null hypothesis of no relationship between Schoenfeld (1982) residuals and time, to test the proportionality assumption. Individual and omnibus tests indicated that our predictors did not violate the proportionality assumption. We employed the Efron (1977) method to account for transition events with tied job spell durations.

### Results

Means, standard deviations, and correlations among variables are presented in Table 3. Estimates of hypothesized effects are reported in Tables 4 through 6. Within the fixed effects survival models estimated here, a statistically significant layoff effect indicates the change in the hazard of voluntary turnover, given a one-unit increase in the layoff variable. Unless otherwise stated, survival estimates are given in terms of the hazard ratio (HR), an exponential transformation of the raw coefficients. Subtracting 1 from the hazard ratio and multiplying by 100 reveals the percent change in voluntary turnover hazard associated with a one-unit change in the predictor. For example, in Model 1 of Table 4, the hazard of quitting decreases 20%  $[(.80 - 1) * 100]$  with each one-unit increase in national unemployment rate.

[Insert Table 3 about here]

We first examined whether an individual's hazard for voluntary turnover increases after the experience of a layoff (Hypothesis 1). Fixed effects survival estimates for each of three operationalizations of the layoff construct are given in Table 4. We compared an individual's

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when this variation is low. Hence, using this improvement in survival analysis requires data with multiple spells (jobs in our case) per unit (individual in our case).

likelihood of voluntary exit prior to the first occurrence of a layoff to that across all jobs succeeding the layoff (Model 2). The positive, statistically significant hazard ratio associated with the *layoff (ever)* measure ( $HR = 1.56, p < .001$ ) indicates that a quit is 56% more likely when at least one layoff appears in the individual's previous job history. Consistent with this positive general effect, our test of the cumulative influence of multiple layoffs found that each additional layoff is associated with a 39% increase ( $HR = 1.39, p < .001$ ; see Model 4) in an individual's voluntary turnover hazard. Our analyses additionally isolate the effect of a layoff on an individual's risk of voluntary turnover in the job immediately following displacement. Compared to the individual's likelihood of quitting prior to their first layoff, voluntary turnover is 65% more likely ( $HR = 1.65, p < .001$ ) in the next job after a layoff (see Model 3). Thus, across measures accounting for different temporal and types of effects, we found consistent evidence that prior layoffs predict a greater likelihood of voluntary exit, supporting Hypothesis 1.

[Insert Table 4 about here]

### *Curvilinearity*

Hypothesis 2 predicted that the positive layoff effect on quit hazard weakens with each additional layoff accumulated. We tested this assertion by including both linear and quadratic specifications of the *cumulative layoffs* measure in a fixed effects survival model predicting voluntary turnover likelihood. The statistically significant effect estimates given in Model 5 of Table 4 ( $HR_{\text{Cumulative layoffs}} = 1.63, p < .001$ ;  $HR_{\text{Cumulative layoffs}}^2 = 0.97, p < .001$ ) indicate that the likelihood of quitting increases—at a declining rate—with each layoff incurred.<sup>6</sup>

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<sup>6</sup> We additionally conducted a sensitivity analysis that eliminated job spells associated with extreme numbers of prior layoffs from our assessment of curvilinearity. While the maximum number of layoffs associated with any job spell was 17, we found statistically significant

To illustrate the nature of the attenuation of the positive layoff effect, we plotted point estimates of the likelihood of voluntary turnover associated with successive increases to one's total accumulated layoffs (see Figure 2). We graph raw coefficients as they allow a more accurate depiction of the change in turnover likelihood. Additionally, although the maximum number of reported layoffs was 17, we excluded layoff values greater than 10 from the plot because 99.9% of all observations were associated with 10 or fewer total layoffs. Figure 2 therefore charts the change in the likelihood of voluntary turnover (relative to that under the condition of no layoffs) as the total accumulated layoffs increase. Analysis of the plot's simple slopes (Aiken & West, 1991) provides a more formal assessment of attenuation (a simple slope is the tangent to the curvilinear plot at a particular level of the predictor). We found positive and statistically significant simple slopes for each additional layoff up to the sixth instance. Subsequent layoffs did not affect the likelihood of voluntary turnover (i.e., the simple slopes were not statistically different from zero), suggesting a plateauing quit hazard. Notably, our results indicate that, all else equal, those with four, five, and six layoffs in their work histories are 3.5, 4.7, and 5.9 times more likely to quit (relative to their quit likelihood in pre-layoff employment).<sup>7</sup> Thus, survival effect estimates and simple slopes analysis each supported Hypothesis 2.

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curvilinear effects when prior layoffs were first capped at 15 and then at 10. Moreover, the effect persisted until all spells associated with five or more prior layoffs were eliminated. Even then, marginal support for the curvilinear effect remained until prior layoffs were capped at three.

<sup>7</sup> We arrive at this final effect estimate by subtracting 1 from the relevant hazard ratio, which is equal to the exponentiated sum of the linear and quadratic raw coefficients associated with six layoffs (i.e., we exponentiate 6 times the 0.4868 raw coefficient on *cumulative layoffs* plus 6<sup>2</sup>

[Insert Figure 2 about here]

### *Mediation*

Hypotheses 3 through 5 proposed that layoff effects on voluntary turnover are partially mediated through underemployment and job satisfaction. To the extent that layoff effects emerge due to psychological spillover, they would not operate through a layoff's influence on the quality of the ensuing employment; hence, we expected only moderate levels of underemployment and job satisfaction mediation. Of our three layoff operationalizations, the *layoff (most recent job)* measure affords the most conservative test of this expectation of modest indirect effects, as the influences of a layoff on underemployment and job satisfaction are likely to be strongest in the job immediately following the layoff. Accordingly, we report mediation only for the effect of a layoff on the hazard of voluntary turnover in the first job following displacement.<sup>8</sup> We present raw coefficients here rather than hazard ratios because they better illustrate mediation. To obtain the corresponding hazard ratio, the raw coefficient is exponentiated, i.e.,  $HR = e^b$ .

We applied Sobel's (1982) test of the joint effect of the predictor and mediator to the survival analysis context (Maltarich et al., 2010; Tein & MacKinnon, 2003) to assess the indirect effects carried through underemployment and job satisfaction separately (i.e., hypotheses 3 and

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times the -0.028 raw coefficient on *cumulative layoffs*<sup>2</sup>, or  $e^{[(6*0.4868)+(36*-0.028)]} = 6.86$ ).

Subtracting one from this hazard ratio ( $6.86 - 1 = 5.86$ ) gives the 5.9 times greater likelihood of voluntary turnover associated with 6 layoffs.

<sup>8</sup> We additionally tested for mediation across all jobs following the layoff (i.e., using the *layoff (ever)* and *cumulative layoffs* measures) and found support for the indirect influence of both underemployment and job satisfaction. However, as expected, these effects were smaller than those associated with the *layoff (most recent job)* measure.

4). We first examined the relationship between the *layoff (most recent job)* predictor and the mediators.<sup>9</sup> As expected, we found that a layoff is positively related to both the continuous ( $b = 0.21, p < .001$ ) and categorical ( $b = 0.25, p < .001$ ) measures of wage underemployment in the subsequent job and negatively related to subsequent job satisfaction ( $b = -0.02, p < .001$ ). Surprisingly, while a layoff also predicted subsequent hours underemployment, the effect estimate was negative ( $b = -0.004, p < .001$ ), indicating that weekly hours worked tend to increase in post-layoff employment. With statistically significant relationships between the predictor and mediators established, Table 5 reports fixed effects survival analyses estimating the effect of the mediators on voluntary turnover hazard. Survival models indicate that wage underemployment ( $b = 0.24, p < .001$  and  $b = 0.21, p < .001$ ; see Models 2 and 3, respectively), hours underemployment ( $b = 0.43, p < .05$ ; see Model 4), and *job satisfaction* ( $b = -0.53, p < .001$ ; see Model 5) are statistically significant predictors of the likelihood of voluntary turnover (controlling for the influence of the layoff). While this pattern of results suggests mediation (Baron & Kenny, 1986), Sobel tests provide a more formal assessment of indirect effects. These tests supported mediation of the layoff effect only through wage underemployment ( $z = 3.83, p < .001$  for the continuous measure and  $z = 3.39, p < .001$  for the categorical measure) and job satisfaction ( $z = 6.30, p < .001$ ); no support was found for mediation through hours underemployment ( $z = 1.88, p > .05$ ). And indeed, comparison of the baseline estimate of the total layoff effect (see Model 1) to those estimates that account for the indirect influence of the mediators reveals that while 5% to 6% of total layoff effect is attributable to an increased likelihood of subsequent wage underemployment (see Models 2 and 3, respectively) and 3% is due to negative effects on post-layoff job satisfaction (see Model 5), none of the effect is carried

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<sup>9</sup> These analyses are not reported in our tables but are available from the first author.

through hours underemployment. That hours underemployment did not mediate the layoff-voluntary turnover relationship may owe to restriction of range in our operationalization of this underemployment predictor (e.g., our sample includes only full-time jobs and therefore omits decreases to weekly working hours following movement from full-time to part-time employment). We therefore took a separate sample that included both full-time and part-time jobs to create more hours underemployment variation. Model 8 of Table 5 shows that even here, there is no hours underemployment mediation (i.e., the layoff effect estimate in Model 8 is not substantially different from that in the baseline Model 7, and, moreover, hours underemployment does not predict voluntary turnover likelihood). We found similar lack of support when hours underemployment was measured in a categorical fashion (i.e., a dichotomous variable coded as “1” for any job change in which the individual moved from full-time to part-time employment). In sum, these mediation analyses, while consistent with Hypothesis 4, provide only partial support for Hypothesis 3. As indicated by Model 6, nine percent of the total effect of layoffs operated through wage underemployment (measured categorically) and job satisfaction.

Hypothesis 5 predicted a three-path mediation model wherein an indirect effect of prior layoffs on voluntary turnover likelihood was expected to operate sequentially through underemployment and job satisfaction (i.e., the causal chain of Layoff→Underemployment→Job Satisfaction→Voluntary Turnover). We assessed the validity of this mediated path with a joint significance test and a product of coefficients test (using exact standard errors; (A. B. Taylor, MacKinnon, & Tein, 2008)). Both supported three-path mediation of the layoff effect through wage underemployment and job satisfaction, although we note that a relatively small proportion of the total effect operates through this causal chain (i.e., less than 1%).

[Insert Table 5 about here]

*Distinct Paths to Voluntary Turnover*

We used a competing risks analysis to separately model the effect of prior layoffs on three distinct groups of voluntary leavers: (a) those who quit to accept an unsolicited job offer, (b) those who quit to accept a solicited offer (i.e., those who quit to accept a job that had been identified through job search), and (c) those who quit to search for a job. While the layoff predictors included in these competing risks analyses reflect each respondent's full work history, only those job spells that ended (or were censored) after the 1990 survey were available to estimate the layoff effect for each voluntary turnover path (see Data and Method). Because this segmentation of the sample, coupled with the competing risks, constrained the degree of within-individual variation available for the *layoff (ever)* and the *layoff (most recent job)* predictors, we estimate the effect of layoffs across these three competing turnover pathways using only the *cumulative layoffs* temporal specification.

Estimates of the effects of accumulated layoffs across each of the three turnover paths are presented in Table 6. In each model, we control for the influence of wage underemployment and job satisfaction in order to assess the total direct (unmediated) effect of layoffs on voluntary turnover likelihood.<sup>10</sup> We found, consistent with Hypothesis 6, that a history of layoffs carries positive, statistically significant effects on the likelihood of voluntary turnover when the exit is associated with absolute ease of movement but not when the exit suggests restricted ease of movement. That is, the layoff effect emerged only when predicting quits facilitated by in-hand job offers. With each additional layoff, the likelihood of quitting to accept an unsolicited offer and the likelihood of quitting to accept a solicited offer, considered separately, increase by 88%

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<sup>10</sup> Recall that our rationale addressing the psychological spillover of layoffs into reemployment is best captured by the layoff influence that is independent of reemployment in an inferior job.

(HR = 1.88,  $p < .05$ ; see Model 1) and by 63% (HR = 1.63,  $p < .01$ ; see Model 2), respectively. Model 3 indicates that when these turnover paths are considered jointly (quitting to accept an offer), each additional layoff increases the likelihood of quitting to take another job by 66% (HR = 1.66;  $p < .001$ ). As expected, however, a layoff history did not influence the likelihood of quitting to engage in job search (see Model 4).

Hypothesis 7 proposed statistically larger positive layoff effects for turnover paths characterized by complete, rather than restricted, ease of movement. In order to evaluate the magnitude of the differences among the effects reported in Table 6, we made use of Lunn and McNeil's (1995) interaction methodology for competing risks applied to Cox regression analysis. Statistically significant interaction effects indicated that the effect of accumulated layoffs on the likelihood of quitting to accept a solicited offer and on the joint likelihood of quitting to accept either an unsolicited or solicited offer were each larger ( $p < .05$ ) than the layoff effect on quitting to engage in job search. However, the interaction assessing the difference in layoff effect between quits to accept an unsolicited offer (considered independently) and quits to engage in job search reached only marginal levels of significance ( $p < .10$ ). Overall, these results support Hypothesis 7, particularly given that statistical power in tests of interaction effects is extremely sensitive to range restriction among predictors, and sample size (Aguinis, 1995), both of which are concerns here (e.g., only 171 sampled individuals reported quitting to take unsolicited job offers). Thus, our competing risks analyses support the expectation that a layoff history increases the likelihood of voluntary turnover in paths characterized by complete ease of movement (i.e., when one has an outside offer) and that the evidence is generally consistent with the expectation these effects exceed the layoff effect for exit paths characterized by restricted ease of movement (i.e., when quitting to search).

[Insert Table 6 about here]

## Discussion

The results of this study provide a nuanced assessment of the relationship between layoffs and their victims' voluntary turnover behavior upon reemployment. We found generally robust support for our contentions that layoffs do lead to voluntary turnover from post-layoff jobs, that the effect size attenuates as layoff events accumulate, that the layoff effect is partially mediated by victims' tendencies to work in lower quality post-layoff jobs, and that the layoff effect is greater when quitting to accept an existing offer (high ease of movement) than when quitting to search. Notably, the relative magnitudes of the direct and indirect effects in the mediation analyses and the pattern of results in the competing risks models examining different voluntary turnover paths are consistent with—and thus indirectly supportive of—our model of layoff effects on victims' psychological experience of subsequent employment. And while our data do not allow us to validate this model, what is lost in theoretical precision is offset by the opportunity for methodological rigor: the NLSY79's comprehensive employment history data allow both for within-subjects estimates of layoff effects and a thorough approach to the operationalization of the layoff construct, each strengthening the validity of our findings.

### *The Layoff Construct*

Given an absence of research addressing layoff effects on post-layoff work behaviors, we took several perspectives on what constituted a layoff history. Indeed, it seemed likely that victims conceive a layoff history differently (and vary in their post-layoff behaviors) according to the current job's temporal distance from prior layoffs and to the accumulated layoff total. Consequently, we utilized three layoff operationalizations (the presence of at least one layoff in one's work history prior to the current job, a layoff from the job held immediately prior to the

current job, and the cumulative number of layoffs prior to the current job). This approach allows for more exacting conclusions surrounding the effects of layoffs. We urge researchers addressing layoffs as a key outcome or antecedent in future studies to carefully consider our variety of approaches and the implications of layoff operationalization in their work.

### *Key Findings and Theoretical Contributions*

Our broadly conceived theoretical argument that a layoff's psychological effects spill over into subsequent employment relationships advances the reach of four (largely) separate but complementary theoretical literatures, each converging on this expectation of spillover. The literature surrounding psychological contracts, for example, suggests that layoffs affect victims' general employment schemas, which influence subsequent expectations of reciprocal obligation between employee and employer. The trust literature similarly provides a mechanism for spillover through the construct of trust transfer, which allows that a layoff-precipitated loss of trust in the layoff employer transfers to post-layoff employers. Scholars studying job insecurity have drawn links between this construct and both organizational commitment and voluntary turnover (Cheng & Chan, 2008; Griffeth et al., 2000; Sverke et al., 2002), providing a theoretical basis for the frequent observation by the lay press that layoffs decrease perceptions of job insecurity and, subsequently, loyalty. Finally, to the extent that layoffs act as employment shocks, our adaptation of the unfolding model of turnover suggests that victims become more vigilant about opportunities for alternative employment and remain so even in post-layoff reemployment. Notably, our arguments represent the first extension of these theoretical perspectives to predict cross-organizational spillover effects on *behavior*. As such, our framework addresses a conceptual gap in the literature surrounding layoff victimhood and provides a platform for the first study of layoff consequences on victim's post-layoff behaviors.

Absent measures of the psychological mechanisms that underlie the spillover framework, we pursued an analytical strategy that assesses the validity of hypotheses consistent with, though not directly evaluative of, this theoretical perspective. Support for psychological spillover is first suggested by the finding, across each of three operationalizations of the prior layoffs construct, that layoffs carry large positive effects on post-layoff voluntary turnover likelihood. Compared to the probability of voluntary turnover prior to ever experiencing a layoff, the hazard of quitting is 56% higher (across all subsequent jobs) after suffering at least one layoff and 65% higher in the job immediately following a layoff. Further, voluntary turnover likelihood increases 39% for each individual instance of layoff. The finding that the layoff effect endures across all post-layoff jobs is particularly amenable to the psychological spillover argument—should a layoff alter victims' general employment schemas and their attention given to alternative employment opportunities, the influence of the layoff could reasonably persist across multiple post-layoff employment relationships.

Analyses additionally demonstrated that the marginal effect of each additional layoff waned, and ultimately disappeared, as the total number of layoffs experienced increased from one to six (see Figure 2). This declining positive effect is consistent with our expectation that layoff effects, and thus psychological spillover and untethering from (looser ties to) subsequent employment, are bounded by the natural range of variation in general employment schemas, trust and job security perceptions, and the employee's capacity for attending to alternative employment opportunities (e.g., with repeated layoffs, felt trust and expectations of job security are likely to reach floor values, stemming any influence of additional layoffs). Nevertheless, each additional layoff up to the sixth instance did increase the likelihood of voluntary turnover, even though additional layoffs after the sixth yield no further increase in turnover likelihood (e.g., the

risk of quitting increased by 350%, 471%, and 586% after four, five, and six layoffs respectively).

The results of our mediation analyses are additionally consistent with the argument for psychological spillover and untethering. While we found that the effects of layoffs did partially operate through underemployment and job satisfaction, the fact that these indirect effects were modest in size implies a relatively small role of layoff victims subsequently being hired into subpar employment. Indeed, of the total influence of a layoff on the voluntary turnover likelihood in the job immediately following the layoff, only 5% to 6% operated through wage underemployment and only 3% operated through job satisfaction. Because the vast majority of the layoff effect appears to be attributable to processes other than low quality reemployment, the psychological spillover explanation seems increasingly reasonable.

As a fourth and potentially most compelling source of analytic evidence for our conceptual frame, we conducted competing risks analyses to examine the pattern of layoff effects across various turnover paths (i.e., quit reasons). As described in the rationale for Hypothesis 6, while a reasonable job offer makes it easy for the former layoff victim to move, the untethering effect of layoffs suggests there is little reason not to do so. Hence, though layoffs do not push the reemployed worker out the door, they make it easier to step through a door that is open. Consistent with this argument, layoffs clearly left victims more likely to quit to take a solicited or an unsolicited job offer. On the other hand, competing risks analyses revealed that layoffs do not affect the likelihood of quitting to engage in job search, a voluntary turnover pathway in which job satisfaction has a strong (negative) impact (T. H. Lee et al., 2008). In short, it appears that layoffs did not so much lead employees to quit post-layoff jobs in which they were unhappy, but rather freed them to quit when job opportunities were readily available. This interpretation is

also supported by statistically stronger layoff effects for quitting to accept an offer (i.e., quitting when there is complete ease of movement) than for quitting to search (i.e., quitting when ease of movement is less certain).

Perceived ease of movement depends on the perception of the availability of other suitable jobs and is often described as a “pull” factor in turnover research (T.H. Lee et al., 2008). Interestingly, the NLSY79 data also allow us to explore an expansion of the notion of the pull of an outside job to a broader pull construct, in which we acknowledge that some leavers are pulled into quitting by factors other than ease of movement in the job market. Specifically, if our spillover framework is correct, we should see that prior layoffs enable those pulled to leave by family reasons to quit. Indeed, post hoc analyses confirmed this notion, as layoff history was positively related to the likelihood of quitting for family reasons, with the effect statistically greater than the effect of layoffs on quitting to search. Thus, it appears that family reasons for quitting, much like an extant job offer, likely represent a pull factor that the layoff victim, having been untethered from the post-layoff employer, is more free to pursue.

### *Methodological Considerations*

Several methodological issues warrant discussion. First, we applied a fixed effects specification of the Cox proportional hazards model, which, because it eliminates time-invariant, person-specific influences, affords greater confidence in effect estimates than do survival models that cannot account for unobserved heterogeneity across individuals. Indeed, this methodological approach provides the rigorous comparison of layoff and non-layoff conditions that has been notably absent from previous studies of layoff effects. Accordingly, we join Weller and colleagues (2013) in encouraging turnover researchers (as well as those studying other time-to-event phenomena) to pursue fixed effects survival models. We do caution, however, that the

validity of this analytical approach is contingent on the nature of the available data. For example, the within-subjects comparisons require that each subject contribute multiple spells to the analysis. Further, because effect estimates are unreliable under conditions of low within-subject predictor and outcome variation (Allison, 2009), the methodology is best suited to phenomena that allow subjects to accumulate a multitude of spells.

Our choice of a within-subjects methodology reflects an overriding concern that layoff victims and non-victims may be materially different across unmeasured variables, biasing the validity of estimates obtained through alternative (i.e., between-subjects) analytical strategies. Although minimization of this potential for unobserved heterogeneity bias is paramount for causal inference, we recognize the value of (valid) between-subjects comparisons. To that end, and as a second methodological consideration, we report supplementary analyses that estimate the voluntary turnover likelihood of layoff victims relative to that of non-victims (i.e., those who reported no layoffs across the full NLSY79 panel). We utilized three estimation strategies to generate these between-subjects estimates, each intended to correct for unobserved heterogeneity between the two groups. The *Control Variable Correction* approach includes controls for respondent cognitive ability, locus of control, race, and sex (because these controls do not vary within person, they could not be included in our fixed effects analyses). The *Selection Dummy Correction* approach includes a dummy covariate that indicates whether the individual ever suffers a layoff (i.e., 0 if the individual reports no layoffs across all job spells, 1 otherwise). We refer to this variable as a “selection dummy” because it should account for unobserved selection effects that may have influenced the likelihood of being a layoff victim versus a non-victim. Finally, the *Shared Frailty Correction* approach estimates layoff effects under a shared frailty specification of survival analysis (Raffiee & Feng, 2014). This approach is analogous to

estimating a random effects model (Allison, 2009; Gutierrez, 2002), here accounting for heterogeneity in the risk of voluntary turnover between layoff victims and non-victims.

[Insert Table 7 about here]

Consistent with the fixed effects estimates of the layoff effect on voluntary turnover likelihood, these between-subjects survival analyses indicate a positive, though muted by comparison, layoff-turnover relationship, with support for curvilinearity also emerging (see Table 7). We speculate that the reason for larger estimates in the fixed effects models is that some aspect of the unobserved heterogeneity is acting as a suppressor variable, with failure to account for this aspect then artificially deflating the effect size. This speculation is supported by the finding that between-subjects estimates are lowest when demographic controls were used to account for unobserved heterogeneity, as this specification is the least rigorous of our three between-subject approaches to the elimination of such bias. Unfortunately, even under the more rigorous frailty and selection dummy approaches, we cannot know whether we have in fact accounted for the unobserved heterogeneity that can bias results. Fixed effects, on the other hand, is so rigorous (and conservative) precisely because it cleanly controls for the (time-invariant) unobserved heterogeneity that we attempt to get at indirectly across each of these between-subjects analyses, thereby allowing the better estimate of layoff influence to emerge. In fact, to the extent that such heterogeneity is perfectly time-invariant, it is fully accounted for through fixed effects analysis.

As a third methodological consideration that merits discussion, we address the role of time and layoffs in more depth. Although our alternative operationalizations of the layoff construct address the influence of the proximity of the layoff to the focal job, the NLSY79 data allow for deeper examination of the role of time on layoff effects, prompting us to consider

additional exploratory post hoc analyses. For example, we examined when, in the individual's tenure with the employer, the layoff occurred and found that early-tenure layoffs and later-tenure layoffs did not differ in their subsequent influence on voluntary turnover likelihood in post-layoff jobs. Moving from job tenure to career tenure, we found positive layoff effects of similar magnitude among individuals who experienced their first layoff early in the NLSY79 panel (between 1978 and 1995) and those who experienced their first layoff in the latter portion of the panel (between 1996 and 2010), suggesting a general consistency of effect over the prior four decades. We also found evidence to indicate that the influence of a layoff dissipates as the event that ended the last job (e.g., a layoff or quit) becomes more distant in time (here, time since last job is a function of both time between jobs and in subsequent employment). However, we note that the layoff effect nevertheless appears to persist as the victim moves between post-layoff jobs. Hence, the effect appears to decay over time, but not necessarily across post-layoff jobs.

A fourth methodological consideration concerns the role of industry on the relationship between layoff history and voluntary turnover. It is perhaps reasonable to expect that psychological spillover is more pronounced in industry contexts that are characterized by a relatively low incidence of layoffs. Unfortunately, the NLSY79 does not provide adequate variation in focal variables for industry-specific fixed-effects survival analyses of layoff effects on turnover. However, we note that our analyses control for industry-specific influences on the layoff-turnover relationship.

Finally, the lack of support for mediation of the layoff-voluntary turnover relationships through hours underemployment can perhaps be explained by the relatively weak construct validity. Feldman (1996) defines the working hours dimension of underemployment as a state of involuntary part-time, temporary, or intermittent employment, with relevant research indexing

the discrepancy between the employee's preferred and actual working hours (Abrahamsen, 2010; Holtom, Lee, & Tidd, 2002; D. C. Maynard et al., 2006; Wilkins, 2007). Because the NLSY79 data provide no indication of whether the individual is satisfied with his or her work hours, our measures are built on the crude premise that, post-layoff, a decrease in hours and that a move from full-time to part-time employment are each indicative of involuntary underemployment.<sup>11</sup> Thus, it is unlikely that our hours underemployment measures afford a rigorous assessment of the construct's true effect.

### *Future Research*

The absence of research addressing the impact of layoffs on victims' subsequent work behaviors in general, and voluntary turnover in particular, leaves intriguing avenues available for future study. Layoffs could, for instance, carry positive effects for job performance in ensuing employment, as it is not unreasonable to expect layoff victims to redouble their efforts as an inoculation strategy against future layoffs (Boswell et al., 2014; Greenhalgh & Rosenblatt, 1984). To the extent that victims worry that their past layoffs are a signal of low quality, these individuals may be motivated to perform in order to shed the negative association. Similar arguments can be applied to the effect of layoffs on alternative withdrawal behaviors, including absenteeism and tardiness. Additionally, to the degree that our arguments surrounding negative effects of layoffs on expectations of job security, perceptions of trust, and schema-based beliefs surrounding the threat of layoffs are valid, layoffs may also encourage permanent withdrawal from the labor force. Competing risks analyses indicated that layoffs made it easier to embrace

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<sup>11</sup> We note that the literature surrounding the wage facet of underemployment is, in contrast, defined according to objective wage change (Feldman, 1996). Our measure of this construct does not, therefore, suffer a similar threat to its validity.

both outside offers and the lure of staying home for family reasons, presumably through a reduced tethering to the job that simplified the individual's responding to the outside pull (be it job-related or family-related). Thus, should permanent exit from the labor force offer a pulling force (e.g., via reduced stress from unsuccessful search, the opportunity to pursue non-work initiatives, etc.), layoffs may be even more damaging to the external talent pool than is implied by our results.

Furthermore, should our spillover theorizing be valid, there is potential that the subsequent quit behavior of layoff victims in reemployment, and their possible permanent withdrawal from the labor force, vastly understates layoff effects on workforce stability. Trevor and Nyberg (2008) reported positive effects of downsizing on the likelihood that layoff *survivors* subsequently voluntarily exit the offending organization. Although surviving employees are spared the direct negative effects of the layoff (i.e., unemployment and stigmatization), the psychological consequences that we expect for layoff victims should still apply (e.g., surviving employees worry about future job security and experience loss of trust in the employer; Brockner, Grover, Reed, & DeWitt, 1992; Cascio, 1993). As such, surviving employees who have quit and become reemployed elsewhere may follow the same causal avenue toward voluntary turnover in the post-layoff work environment that we posited for layoff victims in subsequent employment. Given the unknown but certainly enormous number of layoff survivors in the labor force, survivors' turnover behavior in subsequent jobs could have a massive, disruptive impact on workforce stability in the U.S. economy. As such, the future investigation of layoff effects should expand beyond the study of layoff victims to consider behavioral consequences for layoff survivors upon turnover and reemployment.

### *Limitations*

We find that layoffs are associated with a greater likelihood of voluntary turnover in subsequent employment, which we interpreted as consistent with our characterization of layoffs as resulting in psychological spillover to post-layoff employment. Unfortunately, we were unable to measure perceptions associated with our psychological contract, trust, job insecurity, and employment shock arguments. Future research that allows scholars to directly measure these perceptual constructs and test whether they mediate layoff effects on voluntary turnover would substantially help to clarify the extent to which our proposed explanations are valid.

As we described in the methods section, early administrations of the NLSY79 do not allow the separation of temporary and seasonal job exits from layoffs. We therefore chose to exclude layoffs reported before 1984 in order to preserve the construct validity of our measures. To the extent that “layoffs” reported prior to 1984 do reflect layoffs as we have defined them (rather than temporary and seasonal job exits), the applicability of our results for layoffs experienced very early in the career is called into question. We note, however, that when we do include the questionable pre-1984 layoffs into our analyses (which would assume that layoffs are only trivially contaminated by temporary and seasonal job exits), our overall results do not change, and are in fact greater in magnitude.

An additional limitation, one that alternative data sources may be better suited to address, concerns our examination of the mediating effect of underemployment. We were only able to test mediation through two of several potential operationalizations of the underemployment construct—the degree of the reduction in pay and working hours following a layoff, with the latter of compromised construct validity. It would therefore be beneficial to explore whether mediation is robust to a composite measure of underemployment, i.e., one reflecting person-job fit on the dimensions of education, skill, hours worked, and wages (Feldman, 1996). To the

extent that our underemployment (and job satisfaction) measures are deficient, we may have underestimated the degree to which these factors mediate the layoff-turnover relationship.

### *Practical Implications*

Our data indicate that layoff victims, once reemployed, are more likely to voluntarily separate from the organization. One ramification for these reemployed victims is that such behavior may ultimately cost them future opportunities, as each additional quit makes one more likely to be branded a “job hopper.” Thus, the layoff and subsequent unemployment likely do not comprise the final blow to the employee, as stigmatization for excessive quitting may join underemployment as long term detrimental consequences.

At the firm level, given that turnover cost estimates, which include employee replacement, training, and outplacement (Cascio, 2000; Sturman, Trevor, Boudreau, & Gerhart, 2003), are substantial, as estimates of per-leaver costs sometimes double leaver salary (e.g., Johnson, 1995; Solomon, 1988), it is no surprise that attempting to recruit and select employees who are more likely to stay is of considerable concern to management. Our findings, which might suggest that a layoff history acts as a turnover-based warning sign when hiring, could therefore suggest that managers view a layoff history as a potential liability when making hiring decisions. At this early stage of the research on layoff effects, however, we strongly caution that there may well be various moderators that would reduce or even reverse the general layoff effects reported here. Indeed, the increasing prevalence of layoffs (and therefore layoff victims) indicate that employers may be better served by adopting human resource management policies and practices that bolster trust, job security perceptions, and the psychological contract of new hires, as such approaches may enhance existing employee performance as well as mitigate post-layoff turnover. Moreover, as ours is the first study of post-layoff behaviors, nothing is yet

known about the job performance of layoff victims upon reemployment. Given concerns about being targeted once again for layoff, these hires may be more motivated to perform than comparable applicants who have never experienced a layoff (Boswell et al., 2014; Greenhalgh & Rosenblatt, 1984). Additionally, the performance differences between post-layoff hires who leave and those who stay have relevant ramifications. Hence, future research on job performance in post-layoff employment, and on post-layoff performance's interaction with turnover, would provide intriguing implications for considering layoff history in the hiring process. For example, it could be that the general positive layoff effect on post-layoff job turnover disappears, or even becomes negative, for high performers, who may more fully embrace their newfound rewards and opportunities, which could make hiring layoff victims beneficial for the organization. In sum, the potential practical implications of our findings are intriguing, particularly given the scope of layoffs in the U.S. economy, but more research is necessary to extract concrete recommendations on using layoff history as relevant data in hiring decisions.

### *Conclusion*

While considerable research indicates that layoff victims are adversely affected in a multitude of ways by the layoff experience (e.g., impaired physical and mental health, reduced trust, eventual underemployment), our work is the first to illustrate that layoffs produce behavioral consequences in subsequent employment. The turnover behavior of post-layoff hires also begins to reframe concerns over layoffs. Various authors have reported that the organizational performance consequences of downsizing are mixed (e.g., Cappelli, 2000; Madrick, 1995). Our findings, however, suggest a new, and rather ominous, concern – what is the standard downsizing practice doing to a substantial portion of the domestic labor pool? With millions of layoffs over recent years, what type of future employees are being created? Our study

suggests, all else equal, that commonplace layoffs may be contributing to a vast population of employees who are less likely to remain with their subsequent employers. Furthermore, recent work illustrates that companies engaging in layoffs experience increases in subsequent voluntary turnover (Trevor & Nyberg, 2008). Hence, while laying off employees provides a short-term cost reduction for the organization, this tactic may at the same time be contributing to turnover both within the company and economy-wide. Consequently, because increased turnover often results in reduced productivity and impaired financial performance (Hausknecht & Trevor, 2011), the layoff culture in American business might, ironically, begin to be seen as a problem for management, rather than simply as a strategy whose costs are born solely by layoff victims.

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Table 1. Description of Samples, Dependent Variables, and Key Independent Variables.

| <b>Sample</b>                | <b>Description</b>   |
|------------------------------|--|
| Sample 1                     | Includes all job spells reported across the duration of the NLSY79 panel (1978-2010). Used to assess the main effect of layoffs on voluntary turnover, curvilinearity, and mediation.  |
| Sample 2                     | Includes all job spells reported as ending (or censored) between the 1990 and 2010 administrations of the NLSY79 survey. Used to assess the influence of layoffs on three distinct paths to voluntary turnover.  |
|                              |  |
| <b>Dependent Variable</b>    | <b>Description</b>   |
| Voluntary turnover           | All employee-initiated separations.  |
| Quit for unsolicited offer   | Employee-initiated separation following the acceptance of an unsolicited job offer.  |
| Quit after successful search | Employee-initiated separation following job search and the acceptance of job offer.  |
| Quit to search               | Employee-initiated separation to engage in job search.   |
|                              |  |
| <b>Independent Variable</b>  | <b>Description</b>   |
| Layoff (ever)                | Coded 0 if the job spell precedes the individual's first layoff. Coded 1 if the job spell follows the individual's first layoff.   |
| Layoff (most recent job)     | Coded 0 if the job spell precedes the individual's first layoff. Coded 1 if the job spell is the next job held after a layoff. Undefined otherwise (e.g., the previous job ended in a quit but the job prior to that ended in layoff).                           |
| Cumulative layoffs           | As of each of an individual's job spells, the total number of layoffs previously reported.   |
|                              |  |
| <b>Mediator Variable</b>     | <b>Description</b>   |
| Wage underemployment (cat.)  | Reflects the change in adjusted hourly pay between the current job and the most recent job. Coded 0 for no decrease in pay, 1 for a decrease that is less than 20% of previous pay, and 2 for a decrease that is equal to or greater than 20% of previous pay.   |
| Wage underemployment (cont.) | Reflects the change in CPI-adjusted hourly pay between the current job and the job held most recently (as a percentage of pay in the job held most recently). We multiplied this change by -1 such that underemployment (i.e., a pay decline) is positive.       |
| Hours underemployment        | Reflects the change in weekly hours worked between the current job and the job held most recently (as a percentage of the hours worked in the job held most recently). We multiplied this change by -1 such that greater underemployment takes a positive value. |
| Job satisfaction             | Employee reported job satisfaction, assessed at each administration of the NLSY79. Response options ranged from 1 (dislike job very much) to 4 (like job very much).   |

Table 2. Description of Control Variables.

| <b>Control</b>             | <b>Description</b>  |
|----------------------------|---|
| Industry                   | We identified 12 common industrial categories across the coding schemes used in the NLSY79. Individuals employed in Manufacturing, Mining, Agriculture/Forestry/Fishing, Transportation/Communication, Recreational Services, Professional Services, Finance/Real Estate, and Other industry categories were, generally speaking, more likely to quit than those employed in Public Administration but less likely to quit than those employed in Construction, Business Services, and Trade/Personal Services. |
| Occupation                 | We included dummies for seven broad occupational classifications: Professional/Technical Workers, Managers, Sales Workers, Clerical Workers, Craftsman, Laborers, and Service Workers.  |
| Months unemployed          | Number of months unemployed between the current job and the most recent job if there was a layoff in the most recent job. Coded as 0 if there was no previous job or if the unemployment was not due to layoff. Controls for differences in voluntary turnover propensity due to the severity of the layoff.  |
| National unemployment rate | Yearly national unemployment rate. Controls for differences in turnover propensity due to ease of movement differences (e.g., March & Simon, 1958; Trevor, 2001).   |
| Local unemployment rate    | Yearly local unemployment rate. Controls for differences in turnover propensity due to ease of movement differences (e.g., March & Simon, 1958; Trevor, 2001).  |
| Job changes                | The number of job changes prior to the present job, excluding changes due to layoff, following Judge and Watanabe's (Judge & Watanabe, 1995) finding that past quits predicted future quits.  |
| Age                        | Age in years. Controls for non-work responsibilities that vary with age and influence voluntary turnover likelihood (e.g., financial responsibility).   |
| Marital status             | Coded as 0 for unmarried, 1 for married. Controls for the finding that married individuals are less likely to quit (Abelson, 1987)  |
| Number of children         | The respondent's number of living children. Controls for differences in turnover propensity attributable to children (e.g., Abelson, 1987).   |
| Residential area           | Coded as 0 for rural area, 1 for urban area. Controls for individual differences in turnover propensity resulting from environmental characteristics (Judge & Watanabe, 1995; March & Simon, 1958).   |
| Education level            | The highest grade completed as of May of the survey year. Controls for mobility differences stemming from differences in education level (e.g., March & Simon, 1958; Trevor, 2001).   |
| Log hourly pay             | Logarithm of CPI-adjusted hourly pay rate. Controls for effects of hierarchical level and individual earnings on turnover propensity (e.g., Trevor, 2001).  |
| Mean occupational pay      | CPI-adjusted hourly pay, averaged across year and 3-digit occupational code (2-digit code when < 20 respondents; e.g., Lee et al., 2008; Trevor, 2001).   |
| Weekly hours               | Hours worked per week. Controls for differences in voluntary turnover propensity arising from weekly work hours.  |
| Employer size              | Number of individuals employed at current job location (in 100s). Controls for individual differences in turnover propensity resulting from visibility or internal transfer (e.g., March & Simon, 1956; Mobley et al., 1979).   |

Table 3. Means, Standard Deviations, and Correlations<sup>a</sup>

| Variable                         | Mean  | SD    | 1     | 2     | 3     | 4     | 5     | 6     | 7     | 8     | 9     | 10    | 11    | 12    |
|----------------------------------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|
| 1. Voluntary Turnover            | 0.21  | 0.41  | 1.00  |       |       |       |       |       |       |       |       |       |       |       |
| 2. Layoff (ever)                 | 0.62  | 0.49  | 0.05  | 1.00  |       |       |       |       |       |       |       |       |       |       |
| 3. Layoff (most recent job)      | 0.36  | 0.48  | 0.03  | 1.00  | 1.00  |       |       |       |       |       |       |       |       |       |
| 4. Cumulative layoffs            | 1.06  | 1.31  | 0.02  | 0.63  | 0.71  | 1.00  |       |       |       |       |       |       |       |       |
| 5. Wage underemployment (Cat.)   | 0.46  | 0.73  | 0.08  | 0.11  | 0.21  | 0.07  | 1.00  |       |       |       |       |       |       |       |
| 6. Wage underemployment (Cont.)  | -0.21 | 0.68  | 0.05  | 0.05  | 0.11  | 0.03  | 0.45  | 1.00  |       |       |       |       |       |       |
| 7. Hours underemployment         | -0.02 | 0.21  | -0.01 | 0.01  | 0.02  | 0.01  | -0.02 | -0.01 | 1.00  |       |       |       |       |       |
| 8. Job satisfaction              | 3.20  | 0.79  | -0.18 | 0.01  | -0.02 | -0.02 | -0.05 | -0.03 | 0.01  | 1.00  |       |       |       |       |
| 9. Months unemployed             | 2.46  | 11.78 | 0.01  | 0.16  | 0.36  | 0.11  | 0.08  | 0.01  | 0.01  | -0.02 | 1.00  |       |       |       |
| 10. National unemployment rate   | 6.06  | 1.17  | -0.13 | -0.21 | -0.16 | -0.16 | -0.03 | 0.01  | 0.01  | 0.03  | -0.04 | 1.00  |       |       |
| 11. Local unemployment rate      | 6.64  | 2.81  | -0.07 | -0.05 | 0.01  | -0.01 | 0.02  | 0.01  | 0.01  | 0.01  | 0.02  | 0.42  | 1.00  |       |
| 12. Job changes                  | 5.70  | 4.41  | 0.06  | 0.30  | 0.13  | 0.30  | 0.06  | 0.01  | -0.01 | -0.01 | -0.05 | -0.21 | -0.08 | 1.00  |
| 13. Age                          | 31.82 | 7.03  | 0.02  | 0.41  | 0.36  | 0.33  | 0.06  | 0.01  | 0.01  | -0.01 | 0.13  | -0.30 | -0.11 | 0.46  |
| 14. Marital status (Married = 1) | 0.53  | 0.50  | 0.01  | 0.11  | 0.09  | 0.04  | 0.01  | 0.01  | 0.01  | 0.03  | 0.03  | -0.09 | -0.01 | 0.11  |
| 15. Number of children           | 1.17  | 1.27  | 0.03  | 0.19  | 0.17  | 0.16  | 0.04  | 0.01  | 0.01  | 0.01  | 0.06  | -0.16 | -0.01 | 0.18  |
| 16. Residential area (Urban = 1) | 0.79  | 0.41  | -0.01 | -0.04 | -0.05 | -0.05 | -0.03 | -0.02 | 0.01  | 0.01  | -0.01 | 0.03  | -0.11 | -0.03 |
| 17. Education level              | 12.55 | 2.15  | -0.02 | -0.02 | -0.02 | -0.08 | -0.05 | -0.05 | -0.02 | -0.01 | -0.01 | -0.06 | -0.08 | 0.02  |
| 18. Log hourly pay               | 1.83  | 0.56  | -0.12 | 0.02  | 0.01  | 0.04  | -0.25 | -0.22 | 0.01  | 0.08  | -0.03 | -0.05 | -0.08 | 0.04  |
| 19. Mean occupational pay        | 7.95  | 3.69  | -0.02 | 0.11  | 0.09  | 0.06  | -0.05 | -0.06 | -0.02 | 0.05  | 0.01  | -0.14 | -0.09 | 0.11  |
| 20. Weekly hours                 | 42.62 | 8.15  | 0.04  | 0.05  | 0.02  | 0.05  | 0.04  | -0.01 | -0.41 | 0.01  | -0.02 | -0.05 | 0.01  | 0.11  |
| 21. Employer size                | 4.89  | 26.59 | -0.03 | -0.04 | -0.03 | -0.04 | -0.03 | -0.06 | -0.03 | -0.01 | -0.01 | 0.02  | -0.02 | -0.05 |

(table continued on next page)

Table 3 (Continued). Means, Standard Deviations, and Correlations<sup>a</sup>

| Variable                         | 13    | 14    | 15    | 16    | 17   | 18   | 19   | 20    | 21   |
|----------------------------------|-------|-------|-------|-------|------|------|------|-------|------|
| 1. Voluntary Turnover            |       |       |       |       |      |      |      |       |      |
| 2. Layoff (ever)                 |       |       |       |       |      |      |      |       |      |
| 3. Layoff (most recent job)      |       |       |       |       |      |      |      |       |      |
| 4. Cumulative layoffs            |       |       |       |       |      |      |      |       |      |
| 5. Wage underemployment (Cat.)   |       |       |       |       |      |      |      |       |      |
| 6. Wage underemployment (Cont.)  |       |       |       |       |      |      |      |       |      |
| 7. Hours underemployment         |       |       |       |       |      |      |      |       |      |
| 8. Job satisfaction              |       |       |       |       |      |      |      |       |      |
| 9. Months unemployed             |       |       |       |       |      |      |      |       |      |
| 10. National unemployment rate   |       |       |       |       |      |      |      |       |      |
| 11. Local unemployment rate      |       |       |       |       |      |      |      |       |      |
| 12. Job changes                  |       |       |       |       |      |      |      |       |      |
| 13. Age                          | 1.00  |       |       |       |      |      |      |       |      |
| 14. Marital status (Married = 1) | 0.28  | 1.00  |       |       |      |      |      |       |      |
| 15. Number of children           | 0.36  | 0.35  | 1.00  |       |      |      |      |       |      |
| 16. Residential area (Urban = 1) | -0.03 | -0.11 | -0.04 | 1.00  |      |      |      |       |      |
| 17. Education level              | 0.18  | 0.01  | -0.11 | 0.11  | 1.00 |      |      |       |      |
| 18. Log hourly pay               | 0.20  | 0.14  | 0.01  | 0.08  | 0.29 | 1.00 |      |       |      |
| 19. Mean occupational pay        | 0.26  | 0.12  | 0.04  | 0.01  | 0.28 | 0.36 | 1.00 |       |      |
| 20. Weekly hours                 | 0.06  | 0.04  | 0.03  | -0.05 | 0.02 | 0.03 | 0.09 | 1.00  |      |
| 21. Employer size                | -0.01 | 0.01  | -0.01 | 0.02  | 0.05 | 0.09 | 0.04 | -0.01 | 1.00 |

<sup>a</sup>N = 12,035 job spells for all variables except "Layoff (most recent job)", where N = 6,747 job spells

All correlations greater than  $|\text{.01}|$  are statistically significant,  $p < .05$ .

Table 4. Results of Survival Regression Analyses Predicting Voluntary Turnover

| Independent Variable <sup>c</sup>     | Model 1 <sup>a</sup> |          | Model 2 <sup>a</sup> |          | Model 3 <sup>b</sup> |           | Model 4 <sup>a</sup> |          | Model 5 <sup>a</sup> |          |
|---------------------------------------|----------------------|----------|----------------------|----------|----------------------|-----------|----------------------|----------|----------------------|----------|
|                                       | HR                   | s.e.     | HR                   | s.e.     | HR                   | s.e.      | HR                   | s.e.     | HR                   | s.e.     |
| Months unemployed                     | 0.9995               | 0.002    | 0.998                | 0.002    | 0.99                 | 0.003 *** | 0.998                | 0.002    | 0.997                | 0.002    |
| National unemployment rate            | 0.80                 | 0.02 *** | 0.81                 | 0.02 *** | 0.79                 | 0.04 ***  | 0.81                 | 0.02 *** | 0.81                 | 0.02 *** |
| Local unemployment rate               | 1.0002               | 0.01     | 1.0001               | 0.01     | 1.012                | 0.02      | 1.00003              | 0.01     | 0.9996               | 0.01     |
| Job changes                           | 0.87                 | 0.01 *** | 0.86                 | 0.01 *** | 0.83                 | 0.02 ***  | 0.86                 | 0.01 *** | 0.86                 | 0.01 *** |
| Age                                   | 1.08                 | 0.01 *** | 1.07                 | 0.01 *** | 1.10                 | 0.02 ***  | 1.06                 | 0.01 *** | 1.06                 | 0.01 *** |
| Marital status                        | 1.35                 | 0.11 *** | 1.33                 | 0.11 *** | 1.55                 | 0.21 ***  | 1.33                 | 0.11 *** | 1.32                 | 0.11 *** |
| Number of children                    | 1.23                 | 0.06 *** | 1.24                 | 0.06 *** | 1.25                 | 0.10 **   | 1.25                 | 0.06 *** | 1.24                 | 0.06 *** |
| Residential area                      | 1.04                 | 0.09     | 1.03                 | 0.09     | 1.22                 | 0.18      | 1.03                 | 0.09     | 1.04                 | 0.09     |
| Education level                       | 0.99                 | 0.05     | 0.99                 | 0.05     | 1.03                 | 0.08      | 1.001                | 0.05     | 0.998                | 0.05     |
| Log hourly pay                        | 0.45                 | 0.03 *** | 0.46                 | 0.03 *** | 0.39                 | 0.05 ***  | 0.46                 | 0.03 *** | 0.46                 | 0.03 *** |
| Mean occupational pay                 | 0.99                 | 0.01     | 0.98                 | 0.01 *   | 0.97                 | 0.02      | 0.99                 | 0.01     | 0.98                 | 0.01 *   |
| Weekly hours                          | 1.006                | 0.003 *  | 1.006                | 0.003 *  | 1.0001               | 0.01      | 1.006                | 0.003 *  | 1.006                | 0.003 *  |
| Employer size                         | 0.998                | 0.001 *  | 0.998                | 0.001 *  | 0.99                 | 0.004     | 0.998                | 0.001 *  | 0.998                | 0.001 *  |
| Layoff (ever)                         |                      |          | 1.56                 | 0.13 *** |                      |           |                      |          |                      |          |
| Layoff (most recent job)              |                      |          |                      |          | 1.65                 | 0.19 ***  |                      |          |                      |          |
| Cumulative layoffs                    |                      |          |                      |          |                      |           | 1.39                 | 0.06 *** | 1.63                 | 0.11 *** |
| Cumulative layoffs <sup>2</sup>       |                      |          |                      |          |                      |           |                      |          | 0.97                 | 0.01 *** |
| Log pseudolikelihood                  | -3655.44             |          | -3640.36             |          | -1153.63             |           | -3626.17             |          | -3620.27             |          |
| Wald test statistic (X <sup>2</sup> ) | 637.03***            |          | 667.18***            |          | 375.06***            |           | 695.57***            |          | 707.38***            |          |

<sup>a</sup>N = 12,035 job spells; <sup>b</sup>N = 6,747 job spells.

<sup>c</sup>Industry and occupation dummies are included but not reported.

\*p < .05, one-tailed test; \*\*p < .01, one-tailed test; \*\*\*p < .001, one-tailed test.

Table 5. Mediation Analyses for Underemployment and Job Satisfaction in the Layoff - Voluntary Turnover Relationship

| Independent Variable <sup>c</sup>      | Model 1 <sup>a</sup> |           | Model 2 <sup>a</sup> |           | Model 3 <sup>a</sup> |           | Model 4 <sup>a</sup> |           | Model 5 <sup>a</sup> |           | Model 6 <sup>a</sup> |           | Model 7 <sup>b</sup> |          | Model 8 <sup>b</sup> |          |
|--|----------------------|-----------|----------------------|-----------|----------------------|-----------|----------------------|-----------|----------------------|-----------|----------------------|-----------|----------------------|----------|----------------------|----------|
|  | b                    | se        | b                    | se       | b                    | se       |
| Months unemployed                      | -0.01                | 0.003 *** | -0.01                | 0.003 *** | -0.01                | 0.003 *** | -0.01                | 0.003 *** | -0.01                | 0.003 *** | -0.01                | 0.003 *** | -0.007               | 0.003 ** | -0.01                | 0.003 *  |
| National unemployment rate             | -0.24                | 0.05 ***  | -0.25                | 0.05 ***  | -0.24                | 0.05 ***  | -0.24                | 0.05 ***  | -0.23                | 0.05 ***  | -0.23                | 0.05 ***  | -0.194               | 0.05 *** | -0.19                | 0.05 *** |
| Local unemployment rate                | 0.01                 | 0.02      | 0.01                 | 0.02      | 0.01                 | 0.02      | 0.01                 | 0.02      | -0.003               | 0.02      | -0.01                | 0.02      | -0.006               | 0.02     | -0.01                | 0.02     |
| Job changes                            | -0.19                | 0.03 ***  | -0.19                | 0.03 ***  | -0.19                | 0.03 ***  | -0.19                | 0.03 ***  | -0.18                | 0.03 ***  | -0.18                | 0.03 ***  | -0.142               | 0.02 *** | -0.14                | 0.02 *** |
| Age                                    | 0.10                 | 0.14 ***  | 0.09                 | 0.02 ***  | 0.09                 | 0.02 ***  | 0.10                 | 0.02 ***  | 0.10                 | 0.02 ***  | 0.09                 | 0.02 ***  | 0.08                 | 0.01 *** | 0.08                 | 0.01 *** |
| Marital status                         | 0.44                 | 0.02 ***  | 0.42                 | 0.14 **   | 0.42                 | 0.14 **   | 0.44                 | 0.14 ***  | 0.42                 | 0.14 **   | 0.40                 | 0.14 **   | 0.39                 | 0.12 *** | 0.40                 | 0.12 *** |
| Number of children                     | 0.22                 | 0.08 **   | 0.23                 | 0.08 **   | 0.22                 | 0.08 **   | 0.22                 | 0.08 **   | 0.19                 | 0.09      | 0.19                 | 0.09      | 0.11                 | 0.07     | 0.11                 | 0.07     |
| Residential area                       | 0.20                 | 0.15      | 0.22                 | 0.15      | 0.20                 | 0.15      | 0.20                 | 0.15      | 0.13                 | 0.15      | 0.14                 | 0.15      | 0.18                 | 0.14     | 0.18                 | 0.14     |
| Education level                        | 0.03                 | 0.08      | 0.03                 | 0.08      | 0.01                 | 0.08      | 0.04                 | 0.08      | 0.02                 | 0.09      | 0.001                | 0.09      | 0.07                 | 0.07     | 0.07                 | 0.07     |
| Log hourly pay                         | -0.95                | 0.12 ***  | -0.83                | 0.13 ***  | -0.76                | 0.13 ***  | -0.95                | 0.12 ***  | -0.80                | 0.12 ***  | -0.64                | 0.13 ***  | -0.678               | 0.10 *** | -0.68                | 0.10 *** |
| Mean occupational pay                  | -0.03                | 0.02      | -0.03                | 0.02      | -0.03                | 0.02      | -0.03                | 0.02      | -0.03                | 0.02 *    | -0.03                | 0.02 *    | -0.019               | 0.01 *   | -0.02                | 0.01     |
| Weekly hours                           | 0.0001               | 0.01      | 0.0001               | 0.01      | -0.001               | 0.01      | 0.01                 | 0.01      | 0.00                 | 0.01      | 0.00                 | 0.01      | -0.008               | 0.004    | -0.01                | 0.004 *  |
| Employer size                          | -0.01                | 0.004     | -0.01                | 0.004     | -0.01                | 0.004     | -0.01                | 0.004     | -0.01                | 0.004     | -0.01                | 0.004     | -0.006               | 0.004    | -0.01                | 0.004    |
| Layoff (most recent job)               | 0.50                 | 0.12 ***  | 0.48                 | 0.12 ***  | 0.47                 | 0.12 ***  | 0.50                 | 0.12 ***  | 0.49                 | 0.12 ***  | 0.46                 | 0.12 ***  | 0.38                 | 0.11 *** | 0.38                 | 0.11 *** |
| Wage underemployment (cont.)           |                      |           | 0.24                 | 0.07 ***  |                      |           |                      |           |                      |           |                      |           |                      |          |                      |          |
| Wage underemployment (cat.)            |                      |           |                      |           | 0.21                 | 0.05 ***  |                      |           |                      |           | 0.19                 | 0.06 ***  |                      |          |                      |          |
| Hours underemployment                  |                      |           |                      |           |                      |           | 0.43                 | 0.22 *    |                      |           |                      |           |                      |          |                      |          |
| Job satisfaction                       |                      |           |                      |           |                      |           |                      |           | -0.53                | 0.05 ***  | -0.52                | 0.05 ***  |                      |          |                      |          |
| Hours underemployment (with PT Jobs)   |                      |           |                      |           |                      |           |                      |           |                      |           |                      |           |                      |          | 0.004                | 0.03     |
| Log pseudolikelihood                   | -1153.63             |           | -1147.10             |           | -1147.10             |           | -1151.65             |           | -1093.39             |           | -1087.87             |           | -1437.91             |          | -1437.90             |          |
| Wald test statistic (X <sup>2</sup> )  | 375.06***            |           | 388.12***            |           | 388.12***            |           | 379.01***            |           | 495.54***            |           | 506.57***            |           | 330.93***            |          | 330.94***            |          |
| Decrease in layoff effect <sup>d</sup> |                      |           | 5%                   |           | 6%                   |           | 0%                   |           | 3%                   |           | 9%                   |           |                      |          | 0%                   |          |

<sup>a</sup>N = 6,747 job spells; <sup>b</sup>N = 7,470 job spells

<sup>c</sup>Industry and occupation dummies are included but not reported.

<sup>d</sup>Decrease in layoff effect (i.e., percent mediation) reflects the difference between 'layoff (most recent job)' coefficients in models with and without mediators and is created using unrounded coefficients.

\*p < .05, one-tailed test; \*\*p < .01, one-tailed test; \*\*\*p < .001, one-tailed test.

Note: Estimates (b) are raw coefficients from survival analyses. Hazard ratio = e<sup>(b)</sup>.

Table 6. Results of Competing Risks Survival Regression Analyses Predicting Voluntary Turnover Pathways

| Independent Variable <sup>b</sup>     | Model 1                                      |          | Model 2                                   |          | Model 3 <sup>a</sup>   |          | Model 4                              |          |
|---------------------------------------|--|----------|---|----------|--|----------|--------------------------------------|----------|
|                                       | Predicting Quit to Take an Unsolicited Offer |          | Predicting Quit to Take a Solicited Offer |          | Predicting Quit to Take Either an Unsolicited or a Solicited Offer |          | Predicting Quit to Search for Offers |          |
|                                       | HR   | s.e.     | HR  | s.e.     | HR   | s.e.     | HR                                   | s.e.     |
| Months unemployed                     | 1.0026                                       | 0.01     | 0.99                                      | 0.01     | 0.9904   | 0.01     | 1.003                                | 0.01     |
| National unemployment rate            | 0.53   | 0.11 *** | 0.55                                      | 0.06 *** | 0.56   | 0.05 *** | 0.90                                 | 0.09     |
| Local unemployment rate               | 0.98   | 0.06     | 0.95                                      | 0.04     | 0.95   | 0.03     | 1.03                                 | 0.05     |
| Job changes                           | 0.63   | 0.08 *** | 0.70                                      | 0.04 *** | 0.69   | 0.04 *** | 0.82                                 | 0.05 **  |
| Age                                   | 1.03   | 0.06     | 1.08                                      | 0.04 *   | 1.05   | 0.03 *   | 1.12                                 | 0.04 **  |
| Marital status                        | 1.46   | 0.63     | 1.35                                      | 0.36     | 1.38   | 0.30     | 0.65                                 | 0.21     |
| Number of children                    | 1.54   | 0.43     | 1.22                                      | 0.23     | 1.33   | 0.20 *   | 1.24                                 | 0.28     |
| Residential area                      | 0.51   | 0.27     | 1.67                                      | 0.49 *   | 1.17   | 0.29     | 1.03                                 | 0.33     |
| Education level                       | 1.57   | 0.43     | 1.49                                      | 0.32 *   | 1.39   | 0.22 *   | 0.89                                 | 0.21     |
| Log hourly pay                        | 0.55   | 0.17 *   | 0.27                                      | 0.07 *** | 0.38   | 0.07 *** | 0.32                                 | 0.09 *** |
| Mean occupational pay                 | 0.89   | 0.06     | 0.996                                     | 0.03     | 0.97   | 0.03     | 0.97                                 | 0.04     |
| Weekly hours                          | 1.02   | 0.02     | 0.99                                      | 0.01     | 1.00   | 0.01     | 1.00                                 | 0.01     |
| Employer size                         | 1.00   | 0.01     | 0.96                                      | 0.02 *   | 0.98   | 0.01 *   | 0.99                                 | 0.01     |
| Wage underemployment (cat.)           | 0.77   | 0.14     | 1.13                                      | 0.12     | 1.06   | 0.09     | 1.12                                 | 0.13     |
| Job satisfaction                      | 0.50   | 0.09 *** | 0.43                                      | 0.04 *** | 0.46   | 0.04 *** | 0.53                                 | 0.06 *** |
| Cumulative layoffs                    | 1.88   | 0.55 *   | 1.63                                      | 0.26 **  | 1.66   | 0.22 *** | 1.07                                 | 0.20     |
| Log pseudolikelihood                  | -133.18                                      |          | -361.72                                   |          | -516.08  |          | -265.95                              |          |
| Wald test statistic (X <sup>2</sup> ) | 96.91***                                     |          | 422.90***                                 |          | 477.18***  |          | 137.05***                            |          |

N = 4,206 job spells.

<sup>a</sup>Because a job offer, whether unsolicited or solicited, affords the individual complete ease of movement out of one job and into another, we evaluate Hypotheses 6 and 7 by assessing the influence of a layoff history on these turnover pathways both separately (Models 1 and 2) and combined (Model 3).

<sup>b</sup>Industry and occupation dummies are included but not reported.

\*p < .05, one-tailed test; \*\*p < .01, one-tailed test; \*\*\*p < .001, one-tailed test.

Table 7. Layoff Effect Estimates from Between-Subjects Survival Regression Analyses Predicting Voluntary Turnover

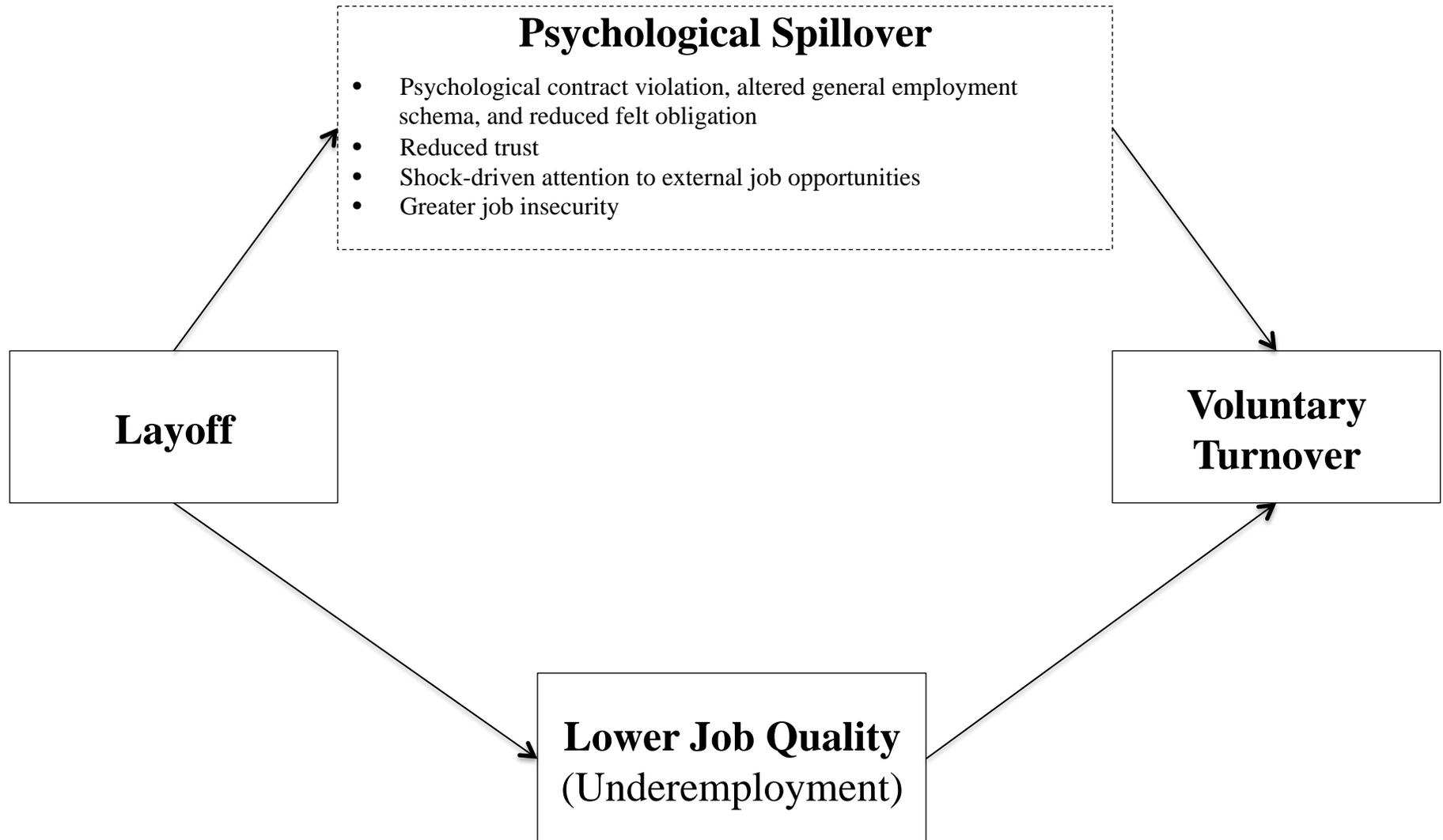
| <b>Layoff Predictor</b>         | <b>Control Variable Correction</b> |             | <b>Selection Dummy Correction</b> |             | <b>Shared Frailty Correction</b> |             |
|---------------------------------|------------------------------------|-------------|-----------------------------------|-------------|----------------------------------|-------------|
|                                 | <b>Model 1<sup>a</sup></b>         |             | <b>Model 2<sup>b</sup></b>        |             | <b>Model 3<sup>b</sup></b>       |             |
|                                 | <b>HR</b>                          | <b>s.e.</b> | <b>HR</b>                         | <b>s.e.</b> | <b>HR</b>                        | <b>s.e.</b> |
| Layoff (ever)                   | 1.03                               | 0.03        | 1.11                              | 0.05 **     | 1.07                             | 0.04 *      |
|                                 | <b>Model 4<sup>c</sup></b>         |             | <b>Model 5<sup>d</sup></b>        |             | <b>Model 6<sup>d</sup></b>       |             |
|                                 | <b>HR</b>                          | <b>s.e.</b> | <b>HR</b>                         | <b>s.e.</b> | <b>HR</b>                        | <b>s.e.</b> |
| Layoff (most recent job)        | 1.10                               | 0.05 *      | 1.17                              | 0.07 **     | 1.12                             | 0.05 **     |
|                                 | <b>Model 7<sup>a</sup></b>         |             | <b>Model 8<sup>b</sup></b>        |             | <b>Model 9<sup>b</sup></b>       |             |
|                                 | <b>HR</b>                          | <b>s.e.</b> | <b>HR</b>                         | <b>s.e.</b> | <b>HR</b>                        | <b>s.e.</b> |
| Cumulative layoffs              | 1.05                               | 0.02 *      | 1.09                              | 0.03 **     | 1.06                             | 0.03 **     |
| Cumulative layoffs <sup>2</sup> | 0.99                               | 0.00 *      | 0.99                              | 0.00 **     | 0.99                             | 0.00 **     |

<sup>a</sup>N = 27,480 job spells; <sup>b</sup>N = 28,937 job spells; <sup>c</sup>N = 22,170; <sup>d</sup>N = 23,369.

\*p < .05, one-tailed test; \*\*p < .01, one-tailed test; \*\*\*p < .001, one-tailed test.

Note: Models include all covariates listed in Table 2 of the revised manuscript and, following Allison’s (1984) recommendations for panel data, each individual’s number of previous quits and the length of the job spell prior to the focal job spell.

Figure 1. Conceptual Model Linking Layoffs to Voluntary Turnover



**Note:** Constructs surrounded by solid lines are measured; broken lines denote the conceptual explanation for unmeasured processes.

Figure 2. Effect of Accumulated Layoffs on Voluntary Turnover Likelihood

