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Can a Flexibility/Support Initiative Reduce Turnover Intentions and Exits? Results from the Work, Family, and Health Network

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ABSTRACT

We draw on panel data from a randomized field experiment to assess the effects of a flexibility/supervisor support initiative called STAR on turnover intentions and voluntary turnover among professional technical workers in a large firm. An unanticipated exogenous shock—the announcement of an impending merger—occurred in the middle of data collection. Both organizational changes reflect an emerging employment contract characterized by increasing employee temporal flexibility even as employers wield greater flexibility in reorganizing their workforces. We theorized STAR would reduce turnover intentions and actual turnover by making it more attractive to stay with the current employer. We found being in a STAR team (versus a usual practice team) lowered turnover intentions 12 months later and reduced the risk of voluntary turnover over almost three years. We also examined potential mechanisms accounting for the effects of these two organizational changes; STAR effects on reducing turnover intentions are partially mediated by reducing work-to-family conflict, family-to-work conflict, burnout, psychological distress, perceived stress, and increasing job satisfaction. The effect of learning about the merger on increasing turnover intentions is fully mediated by increased job insecurity. STAR also moderates the negative effects of learning about the merger on turnover intentions for different subgroups. Findings provide insights into the effectiveness of an organizational intervention, the dynamics of organizations, and how competing logics of two organizational changes affect employees' labor market expectations and behavior.

KEYWORDS: turnover; organizational change; merger; field experiment; flexibility.

The twenty-first-century employment contract seems to be evolving with shared understandings between employers and employees based on two types of flexibility. First, *work-time flexibility* is increasingly being allocated to more employees (Matos and Galinsky 2012). A decline in job security means

that some employers are seeking other ways to gain or sustain the commitment of valuable employees by offering them greater temporal flexibility.¹ Greater flexibility may be motivated by other reasons as well, including more executives and managers with work-life conflicts themselves, the ease of communication technologies, and a general goal of being an “employer of choice.” Leading employers aim to recruit and retain employees—or at least those deemed desirable—by providing them with greater flexibility as to when and where they work, along with more supportive, family-friendly work environments (e.g., Berg, Kalleberg, and Appelbaum 2003; Eisenberger et al. 2002; Frye and Breugh 2004; Hammer and Barbera 1997; O’Driscoll et al. 2003; Osterman 1995). The 2012 National Study of Employers found that 63 percent of employers allow “some” employees to work from home on an occasional basis; in 2005, only 34 percent did so (Matos and Galinsky 2012; Workplace Flexibility 2010). While the media has emphasized some backsliding in employers’ policies (such as YAHOO and Best Buy revocation of extensive telecommuting), the fact is that it is increasingly common to offer at least some employees greater control over where and when they work and to express support for work-life concerns.

The second element of the emerging social contract is that employers are claiming greater flexibility to downsize or contract their workforces; this flexibility is justified by a turbulent global digital economy and facilitated by the absence of policy protections in the United States (Blossfeld, Buchholz, and Kurz 2011; Kalleberg 2011; Rubin 1996; Sweet and Meiksins 2013). Greater employer flexibility means that fewer contemporary workers, even highly skilled professionals, can count on secure jobs. While the lives of “company men” have been critiqued (Bennett 1990; Whyte 1956), it is nevertheless the case that pension, promotion, and other policies developed in the middle of the twentieth century rewarded precisely this orderly career pattern (Wilensky 1961). Employers now invoke their flexibility in hiring, firing, wage setting, and demotions through restructuring and automating jobs, hiring contract workers, and early retirement “options” encouraged in the face of threatened job loss. Taken together with trends in technology, globalized workforces, and work intensification and “extensification” (Green 2006), these two new flexibilities—employee flexibility to modify their work schedules and location and employer flexibility to easily change their workforces—are poised to transform the nature of (especially professional) work.

Understanding the implications of this emerging social contract between employers and employees requires investigating the micro-level consequences for individual workers of the macro-forces producing a climate of time pressures and uncertainty, as well as, for some, greater work-time flexibility and support. This is what we begin to do in this article. Specifically, we examine the separate and combined effects on professional employees’ subsequent turnover intentions and actual turnover of (1) an initiative offering more schedule control and supervisor support for work-life concerns, and (2) an unexpected merger announcement reflecting employer flexibility in reorganizing workforces.

This study is part of a randomized field experiment conducted by an interdisciplinary research team (the Work, Family and Health Network or WFHN) to test the effects of an intervention called STAR; STAR aims to reduce work-family conflict and improve health and well-being by redesigning the work environment (Bray et al. 2013; Kelly et al. 2014; King et al. 2012; Kossek et al. 2014; Moen et al. 2016). We utilize longitudinal data from a Fortune 500 firm we call TOMO (a pseudonym, to comply with confidentiality agreements). TOMO presented STAR as a pilot initiative that the company was rolling out to some groups in its information technology (IT) division.

We draw on panel data collected before and after the STAR intervention from a sample of professionals and managers in the IT division of this large company as well as administrative data on

1 “Temporal flexibility” can range from some limited flexibility as to when (and sometimes where) employees work, contingent on obtaining their supervisors’ permission, to allocating all employees schedule control over when and where they work, with no need for informing or requesting permission from supervisors, as long as productivity and quality of results do not suffer (see also Kelly and Moen 2007 and Kelly, Moen, and Tranby 2011). The STAR intervention we analyze here offers both a high degree of schedule control and supervisor support in helping employees manage the multiple obligations of their lives.

voluntary exits from the firm to investigate whether there are direct or indirect effects of the STAR initiative on employees' turnover intentions over a period of 12 months and actual turnover over a period of almost three years. We theorize that STAR would make remaining with the organization more attractive to employees, reducing both their turnover intentions and actual exits. This expectation addresses the effects of one side of the emerging social contract, the move to offer employees greater temporal flexibility and greater support around family and personal goals and obligations.

We also simultaneously address the second form of flexibility in the emerging contract, employers' flexibility in reorganizing and downsizing their workforces. In the midst of the randomized field trial of the STAR intervention, employees were confronted with an unexpected announcement that TOMO would be merged with—actually acquired by—another firm. Thus this study became a *de facto* investigation of both a randomized field experiment of the effects of introducing employee flexibility and supervisor support (STAR) and a natural experiment to assess the effects of learning about an impending merger in the midst of data collection. Note that these represent two competing institutional logics (Greenwood et al. 2010) around the relationship between the employing organization and its employees: a reorganizing, streamlining logic as this firm prepares to be absorbed within another, and an employee-friendly logic of providing employees greater control over the temporal and spatial organization of their work along with encouraging supervisors to be more supportive of their personal and family lives.

These events thus provide a serendipitous opportunity to investigate: (1) whether an intervention designed to promote employee flexibility and supervisor support reduces turnover intentions and actual exits, as well as (2) whether a macro-level dislocation (in the form of the merger announcement and the layoffs it portends) increases turnover intentions and voluntary exits, and (3) whether the STAR intervention targeting temporal/spatial flexibility and supervisor support buffers the effects of such a dislocation for managers, non-supervisory employees, or other vulnerable subgroups such as women or those with less tenure in the organization.

The fact that the merger announcement occurred in the middle of a randomized field trial means that we collected data both before and after this exogenous shock as well as both before and after a randomly selected portion of this IT workforce was exposed to the STAR intervention. Since some employees were interviewed at baseline prior to the merger announcement (denoted as the early survey group), we can empirically investigate the impacts of both the randomized field experiment and the natural experiment introduced by the announcement of the merger. We can also examine STAR's potential buffering effects on managers' and non-supervisory employees' turnover intentions and actual exits. Thus we have two "treatments:" the STAR intervention and the merger announcement, though obviously respondents were not randomized as to where they were in the study when the merger announcement occurred (see Table A1). We are theorizing and testing the direct effects of each "treatment" and their combined effects on employee turnover intentions and exits.

Note that this study underscores the difficulty of conducting randomized trials in the field, in that real-world events such as this merger announcement complicate best practices associated with experimental designs. However, by exploiting the timing of this exogenous shock, we can make important contributions to understanding and theorizing how the emerging social contract with its two flexibilities affects both employees' commitment to their work organization and the organizational costs that come hand-in-hand with employee turnover.

DOES FLEXIBILITY/SUPPORT REDUCE TURNOVER INTENTIONS AND EXITS?

STAR may reduce both turnover intentions and the odds of actually leaving the firm by lessening respondents' stress and promoting their subjective well-being (Pearlin 2010; Pearlin et al. 1981). For example, we have previously shown that providing employees and managers with greater flexibility

and supervisor support for their personal and family lives reduces work-family conflict, burnout, perceived stress, and psychological distress, as well as promoting job satisfaction by enhancing their ability to successfully manage their multiple obligations (Kelly et al. 2014; Moen et al. 2016). Hence, the Work Family and Health Network theorized that a workplace intervention like STAR would reduce both turnover intentions and actual voluntary turnover (King et al. 2012).

Indeed, employers seem to be motivated to offer their workforces greater temporal/spatial flexibility and work-family support in part because they believe doing so will attract and retain valuable employees (Galinsky, Bond, and Swanberg 1998; Matos and Galinsky 2012; Osterman 1995; Richman et al. 2008). In support of this thesis, there is a body of observational (non-experimental, often cross-sectional) evidence linking employees' temporal flexibility with lower turnover expectations. For example, Rosemary Batt and Monique Valcour (2003) find that flexible scheduling policies lowered turnover intentions, and in a study of a multinational company, Lori Muse (2011) found turnover intentions to be two times greater among employees without flexibility compared to those with it. Laura McNall, Aline Masuda, and Jessica Nicklin (2010) show the availability of flextime and compressed work schedules are associated with lower turnover intentions. Masuda and colleagues (2012) also report positive effects of flexible work arrangements in the form of lower turnover intentions.

There is also a body of evidence on an increasingly popular form of flexibility, working from home or other locations outside the workplace (also called telecommuting or remote work), though, again, most of these studies rely on cross-sectional data. In addition to greater control over when they started or ended their workdays or the ability to take several hours off during the workday, the STAR intervention provided greater ease and normative acceptance for working remotely. A meta-analysis of 46 telecommuting studies by Ravi Gajendran and David Harrison (2007) shows telecommuting to be related to lower turnover intentions. Timothy Golden (2006) also finds that more extensive teleworking was negatively associated with turnover intentions. James Caillier's (2013) investigation of government employees shows that turnover intentions (but not actual turnover) were higher for employees *not* allowed to telework than those who could do so. Pamela Stone (2007) finds professional women leaving their jobs feel pushed out because of the inflexibility of their working conditions, including long hours and often rigid schedules in an office. This aligns with research showing that high levels of work-family conflict, that is, negative spillover from work to home or from home to work, is associated with both higher turnover intentions and actual turnover (Armstrong et al. 2007; Grandey and Cropanzano 1999; Greenhaus, Parasuraman, and Collins 2001; Jones et al. 2007; Moen and Huang 2010; Richman et al. 2008; Russo and Buonocore 2012).

Supervisor support has also been associated with lower turnover intentions (Batt and Valcour 2003; Maertz et al. 2007). Managerial support for employees' work and family obligations may be particularly consequential (O'Neill et al. 2009). Employee reports of family supportive supervisor behaviors have been shown to be significantly related to lower turnover intentions, as well as to less work-family conflict, higher positive work-family spillover, and greater job satisfaction, over and above measures of general supervisor support (Hammer et al. 2009).

While these studies point to potential relationships between flexibility and support on the one hand, and turnover intentions and actual turnover on the other, any causal argument is hampered by issues of selection. In other words, there may well be unexamined variables differentiating respondents who have flexibility regarding when or where they work and supportive supervisors from those who do not. The fact is, flexibility and supervisor support are not randomly distributed across employees, but, rather, tied to particular types of jobs, often those with higher status (Golden 2006; Schieman, Whitestone, and Van Gundy 2006). As Rebecca Glauber (2011) points out, "Women and men do not take jobs with lower pay in return for greater access to flexibility. Instead, jobs with higher pay offer greater flexibility" (p. 489). This difficulty of selection (certain types of workers are advantaged) is attenuated in the current study because the initiative targeting flexibility and support (STAR) was randomly assigned to different work groups within the organization, and we followed

the same people over time, charting whether they left voluntarily or whether their expectations of exiting changed.²

Only a few studies examine such effects of *within-person* changes in flexibility and/or support on within-person turnover intentions or on the odds of actual exits. Dan Dalton and Debra Mesch (1990) find no effect of the introduction of a flexible scheduling intervention on turnover for hourly white-collar workers, though it did reduce absence rates. In another study, medical coders who were allowed to start working from home had significantly higher organizational commitment than the control group who continued to work at the office (Hunton and Norman 2010). A recent experiment in which some workers in a Chinese call center were randomized to work at home found that turnover (or “attrition”) fell sharply, by 50 percent for those working at home (Bloom et al. 2015). Phyllis Moen, Erin Kelly, and Rachelle Hill (2011) used a natural experiment of white-collar employees in a corporate headquarters to show that the introduction of greater schedule control was related to lower turnover intentions as well as to less actual turnover. In a randomized-controlled study that also contributed to the development of the STAR intervention, Leslie Hammer and colleagues (2011) find employees with high work-family conflict who worked under supervisors trained to be more family supportive reported lower turnover intentions, compared to similar employees in a control group.

While the evidence to date is highly suggestive, most extant studies are about turnover *intentions*, not actual turnover, and there are no studies of the separate and combined effects of a randomized workplace initiative within the concurrent turbulence of an upcoming merger. This is important theoretically, because in today’s competitive global economy workers increasingly confront various combinations of initiatives touting new work arrangements along with—and to an extent as compensation for—the uncertainties of downsizing, mergers, and acquisitions. It is also an important business concern, given the high costs of turnover. Indeed, Sunil Ramlall (2004) estimates the cost of voluntary white-collar turnover to be at least a year’s compensation for the position, due to the costs of recruiting, interviewing, and training a replacement, as well as reduced productivity during the training period.

Figure 1 presents our conceptual framework of the effects of the STAR intervention as well as potential mechanisms (mediators) theorized to explain STAR effects on turnover intentions and voluntary turnover. It also includes the effects of the timing of the baseline survey and STAR in relation to the merger announcement by distinguishing the early from the late survey group—those who took the baseline and experienced STAR before the big announcement and those who did so after it. The research design called for the baseline survey and STAR to be rolled out to different (randomized) units of the workforce over an extended period of about one year (not all workers randomized to STAR could be exposed to it at once). The researchers, managers, and employees had *no* advance knowledge of an impending merger, which was publicly announced on a single day with a company-wide email and media coverage. Over half (52.63 percent; $N = 451$) of the respondents had already completed the baseline and STAR implementation (for those randomized to the treatment) *before* they learned about the merger; they are coded as the early survey group and the changes between their baseline and subsequent surveys reflect what happened as they learned of the impending merger. The remainder (47.37 percent; $N = 406$) completed the baseline survey and STAR implementation only *after* the merger announcement; they are coded as the late survey group because they knew about the impending merger prior to baseline data collection. Thus, when the late survey group took the baseline survey, they already had experienced the shock of the merger announcement; any effects of that stressor on their turnover intentions might already be captured in their responses to the baseline survey.

2 As detailed in Bray and colleagues (2013), groups of employees and their managers were aggregated into larger units, called study groups, for randomization. The units that were randomized to STAR or to usual practice were clusters of teams that either reported to the same executive, worked closely together, or both.

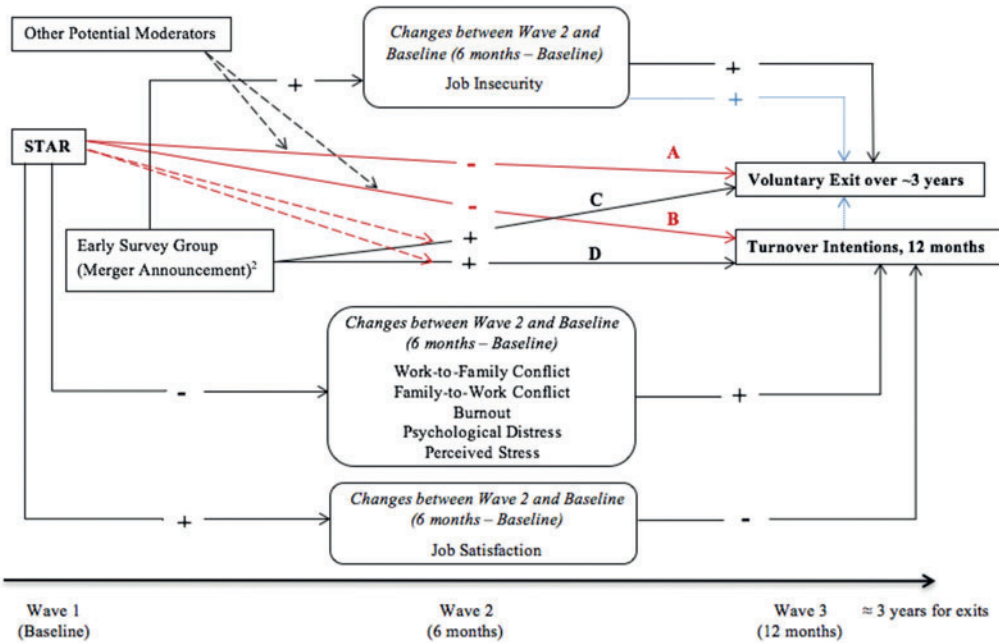


Figure 1. Conceptual Model of STAR's Impact on Turnover Intentions and Voluntary Turnover

Notes: Dashed lines indicate proposed moderating effects. Dotted lines refer to theorized relationships that we couldn't test in this study. The early survey group completed the baseline survey prior to the merger announcement. The late survey group completed their baseline survey after the merger announcement.

Those who learned about the merger only after completing the baseline survey (early survey group) are thus expected to respond by increasing their turnover intentions. Fortunately, we can capture both their responses before the merger was announced (at baseline) and their subsequent adjustments (in a follow-up survey 12 months later). By contrast, among the late survey group increases in turnover intentions most likely occurred before the baseline survey (see [Table A2](#)). Fortunately, this produces a natural factorial experiment, a design where the treatments are the combinations of the levels of two or more factors, in this case the STAR intervention and the timing of the merger announcement (see [Table A1](#)).

To summarize, the WFHN randomized field research design ([Bray et al. 2013](#)) permits comparisons between those employees in teams randomized to receive the STAR intervention and those in the control group continuing usual practices. The STAR intervention, which was introduced immediately after the baseline interview for teams randomized to treatment, was aimed at promoting both greater flexibility and greater supervisor support for family and personal issues. [Erin Kelly and colleagues \(2014\)](#) find that STAR reduced work-family conflict, while [Moen and colleagues \(2016\)](#) show STAR increased job satisfaction even as it decreased burnout, perceived stress, and psychological distress. In light of this evidence, as well as prior evidence from previous studies on other populations linking flexibility and supervisor support to lower turnover intentions and lower turnover (cf. [Hammer et al. 2011](#); [Moen et al. 2011](#)), we hypothesize that over a 12-month period:

H1 (direct effect): Exposure to the STAR intervention will have positive effects in reducing turnover intentions as well as voluntary turnover.

Additionally, given that their baseline survey reflects the situation of the early survey group before the merger news ([Chirumbolo and Hellgren 2003](#); [Lam et al. 2015](#)), we propose:

H2 (direct effect): Those who began the study early, with no initial knowledge of the upcoming merger (early survey group), are expected to increase their intentions to leave by their post-merger announcement 12-month interview, compared to those already aware of the merger/takeover before taking the baseline survey.

We also consider possible mechanisms linking the STAR intervention to reduced turnover intentions. The Work, Family, and Health Network (WFHN) theorized that the intervention would work by reducing work-family conflict and increasing job satisfaction, in other words, making employment at TOMO more attractive. We test whether changes in work-to-family and family-to-work conflict that STAR aims to affect constitute possible mechanisms by which the STAR intervention might reduce turnover intentions. We also examine changes in other well-being indicators such as increases in job satisfaction or reductions in burnout (emotional exhaustion), psychological distress, and perceived stress as mediators of STAR on turnover intentions. (Because our analysis of actual turnover begins with exits occurring before the six-month survey, we cannot examine changes between the first two survey waves as potential mediators for actual turnover.)

H3a (mediation): The STAR effect on reducing turnover intentions reflects a mediation process, operating by increasing job satisfaction and reducing work-to-family conflict, family-to-work conflict, burnout, perceived stress, and psychological distress.

We also test whether the merger timing effect (early versus late survey group) operates through increased job insecurity across the first two survey waves six months apart.

H3b (mediation). Any merger announcement effect increasing turnover intentions is mediated by an increase in job insecurity.

Our fourth hypothesis moves beyond these marginal models to propose possible moderation processes in which the intervention (STAR) provides resources (greater temporal flexibility and supervisor support) to deal with or react to the shock of being taken over by another company. STAR may therefore buffer any turnover effects of the merger announcement for the whole sample, for managers only, or for non-supervisory employees only. In other words, any negative effects of the exogenous shock of learning about the merger (early survey group) or being exposed to knowledge of the merger announcement longer (late survey group) might be weakened by the positive effects of being part of the STAR flexibility/support intervention. Thus:

H4 (moderation). Participating in the STAR intervention will moderate any negative effects of exposure to or learning about the exogenous shock of the merger announcement on turnover intentions or actual turnover, for the whole sample or else for managers or employees only.

Note that this is a more exploratory hypothesis. We do not theorize whether managers or non-supervisory employees would be most affected, but suspect there may well be differences by status position. Managers, like the employees working under them, were blindsided by the merger announcement. But then they were pulled into the process of reconfiguring teams and restructuring the workforce. Since managers were the first to receive more detailed information as to actual merger plans and are active in the decision making regarding shuffling personnel and planning for layoffs, they may feel more in control of their own situations and see themselves as having ongoing value to the firm. Accordingly, we examine possible differences by status within the organization, since these managers may be less apt to either voluntarily exit or increase their turnover intentions. We also

consider other potential moderators, such as gender, age cohort, family circumstances, tenure, and employability.

METHOD

Data Collection

The researchers first worked with company representatives to identify 56 study groups that were then randomly assigned to either the “treatment (STAR)” or the “control” groups. The latter continued with usual management practices regarding flexibility or traditional schedules, so are also called usual practice groups. Some study groups consist of large teams reporting to the same managers, while other study groups include multiple smaller teams under either one senior leader or working closely together on the same application. A randomization procedure modified from a biased-coin randomization technique (Bray et al. 2013) was developed to ensure that job function, division, and size of the study groups were balanced between the treatment groups (STAR) and usual practice groups.

We collected survey data at four points in time—baseline, 6 months, 12 months, and 18 months—from TOMO employees and managers who were part of the information technology (IT) division of the firm and located in the two cities with the largest workforces. Both cities had active IT industries but were recovering from the Great Recession during the study period, suggesting the local labor markets were similar. Respondents were recruited into the WFHN study by trained field staff who described the study goals as understanding the relationship between work and employees’ health and well-being. Participation in the WFHN Study was entirely voluntary; employees and managers were recruited by emphasizing the importance of the topic for employees, for the firm, and for promoting scientific understanding of the health effects of working conditions. The researchers’ involvement and the researchers’ aims of evaluating STAR’s impact were not mentioned, and STAR was framed in company communications as a pilot being rolled out by the firm (see Kelly et al. 2014; Kossek et al. 2014; Moen et al. 2016). Study participants were informed that no individual responses to the survey would be available to anyone at TOMO. Computer-assisted personal interviews (CAPI) lasting about 60 minutes were conducted by trained field interviewers at the workplace on company time at baseline, 6 months, 12 months, and 18 months. In addition, we drew on administrative data on voluntary termination for approximately three years after baseline.

Sample

The data collection process occurred over an extended period of time; for example, the baseline CAPI data collection lasted about one year. The baseline survey was completed by 73 percent of eligible workers ($N = 1,044$). Fully 85 percent ($N = 889$) of baseline respondents also completed the 12-month follow-up survey. Failure to randomize a small number of cases to either STAR or usual practice resulted in a final analytic sample of 857 for analysis of turnover intentions. Most (90.8 percent) of the initial survey respondents consented to sharing their administrative personnel data with the study. For those who did not provide consent, in the competing risks models of voluntary turnover we used the last wave they were interviewed as the date they were censored. We have an analytic sample of 977 employees and managers when estimating actual turnover.

Dependent Variables

Turnover intentions are captured in a two-item scale developed by Karen Boroff and David Lewin (1997). Responses range from 1 (*strongly disagree*) to 5 (*strongly agree*). Questions include “you are seriously considering quitting [company name] for another employer” and “during the next 12 months, you will probably look for a new job outside [company name].” The correlation between these two items at baseline is .78.

We also assessed the degree of *actual turnover* or terminations by drawing on administrative data from the company listing the dates and reasons why employees left the firm. We focus on *voluntary* exits, treating *involuntary* layoffs as a competing risk.

Focal Independent Variable: The STAR Intervention

The STAR intervention was presented as a corporate pilot initiative and announced by senior executives in the IT division. STAR included (1) supervisory training on strategies to demonstrate support for employees' personal and family lives while also supporting employees' job performance and (2) participatory training sessions among both employees and managers that identify new work practices and processes to increase employees' control over work time and focus on key results, rather than face time. Employees randomized to STAR could work when they chose to do so, offering considerable flexibility so long as deadlines were met and projects were accomplished well. Employees in STAR also received blanket approval to work at home as they deemed appropriate; previously (and for the usual practice control group) a telecommuting policy required approval on a case-by-case basis by top management, but STAR made this available without managerial approval. The STAR training included eight hours of participatory sessions for employees (with managers present) as well as four additional hours for managers only. Facilitators prompted discussions of the organization's expectations of workers, everyday practices, and company policies, and encouraged participants to enact new ways of working that increase employees' control over their work time and for managers to demonstrate greater support for others' personal obligations. Previous analyses of STAR effects at six months found that employees reported more variable schedules, worked more at home, experienced more control over their work time, and saw their supervisors as more supportive of their personal and family commitments, as compared to employees in the control group (Kelly et al. 2014). We build on and extend this work by looking at STAR's impact on outcomes of interest to both employees and organizations: increases in turnover intentions, increases in voluntary termination, and whether STAR buffered the effects of subsequently learning of the impending merger for those in the early survey group.

Independent Variables

The two key explanatory variables are exposure to STAR and the timing/sequencing of exposure to the merger announcement in relation to when respondents completed the baseline survey. Those randomized to the *STAR intervention* were coded (1) and the controls (usual practice) were coded (0). This is standard for the more conservative "intent to treat" analytic strategy we use (see Kelly et al. 2014).

Survey group captures when respondents were interviewed at baseline in relation to when the merger was announced. Recall that about half of respondents (in both the STAR and control groups) were interviewed at baseline and introduced to STAR before the merger announcement (early survey group). The remainder had their baseline interview and STAR training only after being exposed to the merger announcement (late survey group). Thus, when the respondents in the late survey group randomized to STAR went through the initiative, they already knew about the impending takeover by another organization. All respondents had been exposed to the merger announcement (which occurred on a single day) by the time they completed their 12-month follow-up survey.

Mediators

We test for mediational effects, expecting that improvements in several well-being measures might account for any STAR effects on turnover intentions by the third (12-month) wave. (We could not test mediational effects for actual turnover since some exited prior to the six-month survey.) To capture changes across the first two waves, we subtracted baseline score of the mediators from the same measure at Wave 2 (six months later), examining the effects of changes in work-to-family conflict, family-to-work conflict, burnout, perceived stress, psychological distress, and job satisfaction on

changes in turnover intentions at the 12-month follow-up. A detailed description of the well-being measures is provided in Table A3. If a respondent skipped an item but answered more than 75 percent of questions on a scale, we substituted the missing value with the mean of the individual's observed values on other items in that scale.

Work-to-family-conflict and *family-to-work conflict*, two variables STAR was designed to reduce, is assessed using a scale developed and validated by Richard Netemeyer, James Boles, and Robert McMurrin (1996). It reflects the degree to which work responsibilities are incompatible with home obligations and vice versa. Sample questions include "The amount of time your job takes up makes it difficult to fulfill your family or personal responsibilities" for work-to-family conflict and "Family-related strain interferes with your ability to perform job-related duties" for family-to-work conflict. Responses range from 1 (*strongly disagree*) to 5 (*strongly agree*), with higher scores indicating higher level of conflict. Cronbach's Alpha is .90 for work-to-family conflict and .83 for family-to-work conflict at baseline.

Burnout/emotional exhaustion is measured by a three-item subscale of Maslach Burnout Inventory (Maslach and Jackson 1986). A sample question: "You feel emotionally drained from your work. How often do you feel this way?" Responses range from 1 (*never*) to 7 (*every day*), with higher scores indicating higher levels of burnout. Cronbach's Alpha for burnout/emotional exhaustion is .89 at baseline.

Psychological distress is measured by a standardized six-item scale developed by Robert Kessler and colleagues (2003) for mental health. Higher scores (6 to 30) indicate higher level of non-specific psychological distress. Cronbach's Alpha for psychological distress is .75 at baseline.

Perceived stress is captured by a four-item scale validated by Sheldon Cohen, Tom Kamarck, and Robin Mermelstein (1983), which is a global measure of stress appraisals. This scale was only fielded to employees to provide time for other questions to managers. Cronbach's Alpha is .77 for perceived stress at baseline. Higher scores of perceived stress (4 to 20) indicate higher level of stress.

Job satisfaction is derived from a three-item scale developed by Cortlandt Cammann and colleagues (1983), including the question: "In general, you are satisfied with your job?" Responses range from 1 (*strongly disagree*) to 5 (*strongly agree*). Cronbach's Alpha of job satisfaction is .86 at baseline.

Job insecurity is captured by a single item asked at each survey wave: "Thinking about the next 12 months, how likely do you think it is that you will lose your job or be laid off?" on a 1 (*not at all likely*) to 4 (*very likely*) scale (Brim, Ryff, and Kessler 2004). Change in job insecurity is obtained by subtracting respondents' score at baseline from their score at Wave 2, with a positive number representing an increase in job insecurity.

Moderators

We test several demographic variables as possible moderators: *age-cohort* is a categorical variable distinguishing Leading-Edge Boomers (born between 1946 and 1954), Trailing-Edge Boomers (born between 1955 and 1964), and GenXers (born between 1965 and 1980). (Given the small numbers, five pre-boomers born between 1940 and 1945 are included with the Leading-Edge Boomers.) We also consider gender and marital status as potential moderators, coded with *women* = 1, and *married* = 1, respectively.

Several work-related variables are theorized as potential moderators. Managerial status is coded as a dummy, with *managers* = 1. Tenure at baseline greater than the median (11 years) is coded as *high tenure* (= 1), with those with low tenure expected to both be more likely to increase their turnover intentions and benefit from the buffering effects of STAR. We also include employability, since those with poor prospects are unlikely to increase their turnover intentions. *Employability* is measured by a single question: "How easy would it be for you to find a job with another employer with approximately the same income and fringe benefits as you have now?" Answers "very easy" or "somewhat easy" are coded as *high employability* (= 1) whereas "not easy at all" is coded as *low employability*. We theorized those with high employability would be more apt to have increased turnover intentions as well as more voluntary exits, and that STAR might reduce the turnover intentions and exits of the high employability group.

Analytical Procedures

Our objective is to understand the effects of the STAR intervention on turnover intentions and actual voluntary exits, recognizing as well the effects of the timing of STAR exposure in relation to the merger announcement. Conceptually, our analytic strategy aims to mimic the ideal experiment that would yield definitive estimates of the desired effects. This ideal factorial experiment would randomize many persons (or teams) to one of the four conditions defined by the two interventions: STAR (yes/no) and survey/merger announcement timing distinguishing the early survey group (pre-announcement) from the late survey group (post-announcement). Assuming perfect compliance, fidelity, measurement, and so forth, this experiment would identify the effects of STAR alone, of being in the early survey group alone, and their combination or joint effects (see [Table A1](#)). Accordingly, we conduct actual analyses with our imperfect data (with STAR randomized and early/late survey group timing not randomized but at least balanced across STAR treatment groups) that shed light on these effects.

In randomized experiments one can assume that subjects in each condition are similar on both measured and unobserved characteristics. Half (50.41 percent) of the respondents were randomized to the STAR intervention, half randomized to the control group. The STAR and control groups are balanced; we find few statistically significant differences ($p < .05$) between members in the STAR and control groups on demographic measures ([Table 1](#)). Although we did not expect early and late survey groups to be balanced given that the timing of baseline survey in relation to the merger announcement was out of our control, we find these groups are also balanced on most variables (see [Table A2](#)). We therefore include as covariates variables that were not balanced (purely by chance) between STAR and usual practice, i.e., *age cohort*, *race*, and *tenure*, in addition to managerial status as a measure of organizational location. We find the patterns of results regarding STAR's effects do not differ from the reduced models ([Tables 2 and 3](#)). We also include baseline levels of turnover intentions in the model estimating turnover intentions, since our goal is to capture any changes between baseline and twelve months.

Estimation

We assess changes in turnover intentions at the 12-month survey, prior to the period when the merger/acquisition was finalized with major organizational restructuring and before there was high voluntary and involuntary turnover (see [Figure 2](#)). We use linear mixed models with level 1 as individuals and level 2 as study groups to estimate turnover intentions by Wave 3, controlling for baseline turnover intentions. The models are fit using maximum likelihood with *xtmixed* in Stata 13. The basic models thus take the following form:

$$Y_{ij} = \beta_0 + \beta_1 \text{STAR}_j + \beta_2 \text{Timing of Knowing about Merger}_{ij} + \beta_3 \text{STAR}_j * \text{Timing of Knowing about Merger}_{ij} + \beta_4 \text{Age Cohort}_{ij} + \beta_5 \text{Race}_{ij} + \beta_6 \text{Managerial Status}_{ij} + \beta_7 \text{Tenure}_{ij} + u_j + \epsilon_{ij}$$

Here j is study group and i is the individual respondent. The fixed portion of the model estimates an overall regression line representing the population average. The random effect, u_j , serves to shift this regression line up or down according to each study group since the random effects occur at the study group level (*studygroup*).

In order to examine potential mediation effects, we first constructed change scores between Wave 2 and baseline for each mediator. We recoded change scores to 0 if a respondent had a missing value for either wave of mediators in order to include as many respondents as possible. (Robustness checks show that the results are the same using a more restricted sample.) We then included both STAR and mediators to predict turnover intentions and to assess whether decreases in work-family conflict, burnout, perceived stress and psychological distress, and/or increases in job satisfaction fully or partially mediate the effect of STAR on turnover intentions. We also assess whether increases in job insecurity fully or partially mediate the turnover intentions effect of being in the early survey group.

Table 1. Descriptive Statistics, Full Sample and by Experimental Condition (STAR)

	<i>Full Sample^a</i>		<i>STAR</i>		<i>Usual Practice (control)</i>		
	<i>Mean/%</i>	<i>StdDev</i>	<i>Mean/%</i>	<i>StdDev</i>	<i>Mean/%</i>	<i>StdDev</i>	<i>t-test^b</i>
Dependent variables							
Actual turnover (~3 years)							
Voluntary exits (<i>N</i> = 92)	9.42%		7.57%		11.27%		*
Involuntary exits (<i>N</i> = 58)	5.94%		7.16%		4.71%		
Stayers	84.65%		85.28%		84.02%		
Turnover intentions by Wave 3	2.34	1.06	2.18	1.01	2.50	1.09	***
Independent variables							
Study group level							
Experimental condition (STAR = 1)	50.41%		100%		0%		
Individual level							
Baseline turnover intentions	2.24	1.05	2.19	1.02	2.28	1.09	
Timing of survey in relation to merger announcement							
Early survey group ^c	52.63%		53.94%		51.29%		
Late survey group ^c	47.37%		46.06%		48.71%		
Potential mediators							
Δ Work-to-family conflict (W2-baseline)	-.18	.76	-.27	.79	-.10	.72	**
Δ Family-to-work conflict (W2-baseline)	-.01	.57	-.04	.55	.01	.58	
Δ Job satisfaction (W2-baseline)	.05	.59	.11	.59	-.01	.59	**
Δ Burnout (W2-baseline)	-.16	1.23	-.25	1.29	-.07	1.16	*
Δ Psychological distress (W2-baseline)	-.37	2.65	-.44	2.58	-.30	2.73	
Δ Perceived stress (W2-baseline) ^d	-.25	2.15	-.26	2.07	-.24	2.22	
Δ Job insecurity (W2-baseline) ^e	.12	.65	.08	.67	.15	.64	
Covariates							
Women (= 1)	38.04%		40.51%		35.53%		
Age cohort							
Leading-Edge Boomers (b. 1946-54)	15.29%		17.82%		12.71%		*
Trailing-Edge Boomers (b. 1955-64)	38.16%		40.05%		36.24%		
Gen Xers (b. 1965-80)	46.56%		42.13%		51.06%		**
Race							
White (= 1)	70.60%		72.45%		68.71%		
Indian Asian (= 1)	14.47%		11.81%		17.18%		*
Others (= 1)	14.94%		15.74%		14.12%		
Married (= 1)	82.96%		83.33%		82.59%		

(continued)

Table 1. Descriptive Statistics, Full Sample and by Experimental Condition (STAR)
(continued)

	Full Sample ^a		STAR		Usual Practice (control)		
	Mean/%	StdDev	Mean/%	StdDev	Mean/%	StdDev	t-test ^b
Have children under 18 at home (= 1)	58.58%		58.56%		58.59%		
Number of children under 18	1.04	1.10	1.01	1.07	1.07	1.13	
Managerial status (= 1)	21.70%		22.45%		20.94%		
Tenure at baseline (in years)	14.51	9.49	15.78	10.07	13.22	8.68	***
Tenure greater than median 11 years (= 1)	50.06%		53.01%		47.06%		†
High employability (= 1) ^f	43.97%		42.76%		45.22%		

^aAnalysis for actual turnover includes 977 cases, while analysis for turnover intentions has 857 cases due to exits and missing data.

^bMean differences between STAR and usual practice (control) are tested using t-tests.

^cEarly survey group completed the baseline survey prior to the merger announcement. Late survey group completed their baseline survey after the merger announcement.

^dPerceived stress was only measured among employees ($n = 671$).

^eΔ Job insecurity is theorized as a mediator for the timing of survey in relation to the merger announcement.

^fThe sample size is 846 for employability due to missing values.

† $p < .10$ * $p < .05$ ** $p < .01$ *** $p < .001$ (two-tailed tests)

We use competing risk survival models (*stcrreg* in Stata13) based on Fine and Gray's proportional subhazards model to assess the probability of voluntary turnover. As an extension of Cox proportional hazard regressions, competing risk regressions allows us to better estimate the association of predictors and the outcome of interest, i.e, voluntary exits, by adjusting for the rate of a competing event, in this case involuntary turnover. If respondents are laid off they are obviously no longer at risk of voluntarily leaving their jobs. The semiparametric nature of the competing-risk models permits us to capture the effects of covariates over time without assuming any particular form of distribution for the subhazard rate (Box-Steffensmeier and Jones 2004). We take the date of respondents' baseline survey as their starting point. For the stayers, the date of their last survey serves as the date they are right censored. The date of exit in the administrative data is treated as the date of the turnover "event" for those leaving the company. Standard errors were adjusted for clustering by study groups.

RESULTS

Table 1 presents descriptive statistics on variables at baseline for the total sample as well as separately for respondents randomized to STAR and usual practice control groups. Respondents at TOMO are on average 46.16 years old, reflecting the larger aging trend of the American workforce. A little over half are Boomers; 15 percent are in the leading edge of the Boomer cohort (born 1946-54) and 38 percent are in the trailing edge of the Boomer cohort (born 1955-64). The Gen X cohort (born 1965-80) constitutes almost half (47 percent) of the sample. Two in five (38 percent) respondents are women. While 14.5 percent of the sample is Indian Asian; this is not surprising as Current Population Survey data (from 2010) finds that 17 percent of full-time IT professionals in the United States are Asian.

Note there is no bivariate difference in baseline turnover intentions between STAR treatment and usual practice control respondents (prior to the intervention), but there is, as hypothesized, by the 12-month follow-up survey (Wave 3). Despite the uncertainty of a pending merger, the mean of turnover intentions for the STAR groups changed minimally if at all from 2.19 to 2.18 over the 12 months between the baseline and the Wave 3 surveys. In contrast, the mean for turnover intentions

Table 2. Linear Mixed Models of STAR Effects on Turnover Intentions by Wave 3

Turnover Intentions, Wave 3															
Coefficient															
Direct Effects			Mediation					(employees only)					Moderation		
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)			
STAR	-.243*** (.061)	-.248*** (.060)	-.243*** (.058)	-.213*** (.057)	-.235*** (.058)	-.244*** (.060)	-.204*** (.055)	-.183** (.059)	-.197*** (.055)	-.141 (.091)	-.079 (.095)	-.013 (.122)			
Early study group (= 1)	.129* (.060)	.123* (.059)	.119* (.059)	.112* (.057)	.114 [†] (.058)	.128* (.060)	.102 [†] (.055)	.048 (.061)	.077 (.056)	.222* (.088)	.277** (.091)	.348** (.111)			
Manager (= 1)	-.007 (.067)	.002 (.067)	.013 (.067)	.015 (.066)	.006 (.067)	-.004 (.067)	.016 (.065)	.018 (.043)	.018 (.065)	.006 (.113)	.128 (.128)	.004 (.067)			
Change in work-to- family conflict (W2-baseline)			.108** (.036)				.023 (.040)	.019 (.043)	.018 (.040)						
Change in family-to- work conflict (W2-baseline)			.131** (.049)				.056 (.050)	.112* (.056)	.061 (.050)						
Change in job satis- faction (W2-baseline)				-.311*** (.046)			-.268*** (.048)	-.278*** (.054)	-.266*** (.048)						
Change in burnout (W2-baseline)					.085*** (.022)		.039 (.025)	.031 (.027)	.036 (.024)						
Change in psycho- logical distress (W2-baseline)						.028** (.010)	.010 (.011)	.019 (.012)	.009 (.010)						
												.006			

(continued)

Table 2. Linear Mixed Models of STAR Effects on Turnover Intentions by Wave 3 (continued)

		Turnover Intentions, Wave 3												
		Coefficient												
		Direct Effects			Mediation				(employees only)				Moderation	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Turnover intentions at baseline		(.027)	(.027)	(.027)	(.027)	(.026)	(.026)	(.027)	(.026)	(.029)	(.026)	(.027)	(.027)	(.027)
Observations		857	857	857	857	857	857	857	857	671	857	857	857	857
Number of groups		56	56	56	56	56	56	56	56	56	56	56	56	56
Study group variance		.008	.007	.005	.004	.003	.005	.007	.001	0	.002	.005	.004	.003
Individual variance		.639	.636	.632	.633	.608	.628	.631	.603	.572	.598	.636	.634	.634
ICC		.012	.011	.007	.007	.005	.007	.011	.002	0	.003	.008	.006	.005
BIC		2,132	2,134	2,132	2,134	2,097	2,127	2,134	2,116	1,647	2,115	2,152	2,154	2,154
Proportion of study group level variance explained		.870	.881	.921	.925	.946	.923	.879	.980	1.000	.973	.918	.934	.943
Proportion of individual level variance explained		.397	.399	.403	.402	.426	.407	.404	.431	.460	.435	.399	.401	.401
R-squared		.422	.425	.431	.430	.454	.435	.429	.460	.489	.464	.427	.430	.430

Notes: Standard errors in parentheses were adjusted for clustering by study groups. Age cohort and race are controlled. Proportion of study group level and individual variance explained as well as R-squared are calculated in comparison with the null models that are not shown on the table. Early survey group completed the baseline survey prior to the merger announcement. Late survey group completed their baseline survey after the merger announcement. Results of models with all covariates listed on Table 1 are available from authors.

^aPerceived stress was only measured among employees.

^bΔ Job insecurity is theorized as a mediator for the timing of survey in relation to the merger announcement.

[†] $p < .10$ * $p < .05$ ** $p < .01$ *** $p < .001$ (two-tailed tests)

Table 3. Competing Risk Event History Analysis Estimating STAR Effects on Subhazard Ratios of Voluntary Turnover over ~ 3 years

	<i>Voluntary Exits (Involuntary Exits as Competing Event)</i>			
	<i>Subhazard Ratio</i>			
	<i>(1)</i>	<i>(2)</i>	<i>(3)</i>	<i>(4)</i>
STAR (= 1)	.599*	.600*	.470*	.485*
	(.128)	(.130)	(.155)	(.171)
Early study group (= 1) ^a		.857	.705	.589 [†]
		(.189)	(.211)	(.172)
STAR*early study group			1.624	1.824
			(.640)	(.765)
Managers (= 1)				.720
				(.164)
High tenure (= 1)				.529**
				(.126)
Age cohort (Gen Xers [b. 1965-80] as reference)				
Leading Edge Boomers (b. 1946-54)				.647
				(.213)
Trailing Edge Boomers (b. 1955-64)				.370**
				(.115)
Race (white as reference)				
Indian Asian				1.266
				(.313)
Others				1.068
				(.296)
Observations	977	977	977	977
Study groups	56	56	56	56
Event: voluntary exits	92	92	92	92
Competing event: involuntary exits	58	58	58	58

Note: Results of models with all covariates listed on Table 1 are available from the authors.

^aEarly survey group completed the baseline survey prior to the merger announcement. Late survey group completed their baseline survey after the merger announcement.

[†] $p < .10$ * $p < .05$ ** $p < .01$ *** $p < .001$ (two-tailed tests)

of the usual practice (control) groups increased from 2.28 to 2.50 ($p < .001$). We also found that the late survey group had marginally higher turnover intentions (see Table A2) than the early survey group at baseline, reflective of the fact that they had already experienced at least the first shock of the merger announcement prior to their baseline data collection.

Table 1 also shows that 9.42 percent of the sample left voluntarily during the observation period, while 5.94 percent were laid off. In preliminary support of our hypothesis, voluntary exits were significantly higher among usual practice groups (11.27 percent) than groups randomized to STAR (7.57 percent) using t -tests ($p < .05$).

Turning to potential mediators, note that there are greater changes in the expected direction in work-to-family conflict ($p < .01$), job satisfaction ($p < .01$), and burnout ($p < .05$) for those randomized to STAR as compared to those following usual practice. We also tested for differences across early and late survey groups. As expected, members of the early survey group (who completed their baseline survey before the merger announcement) experienced significantly greater increases in

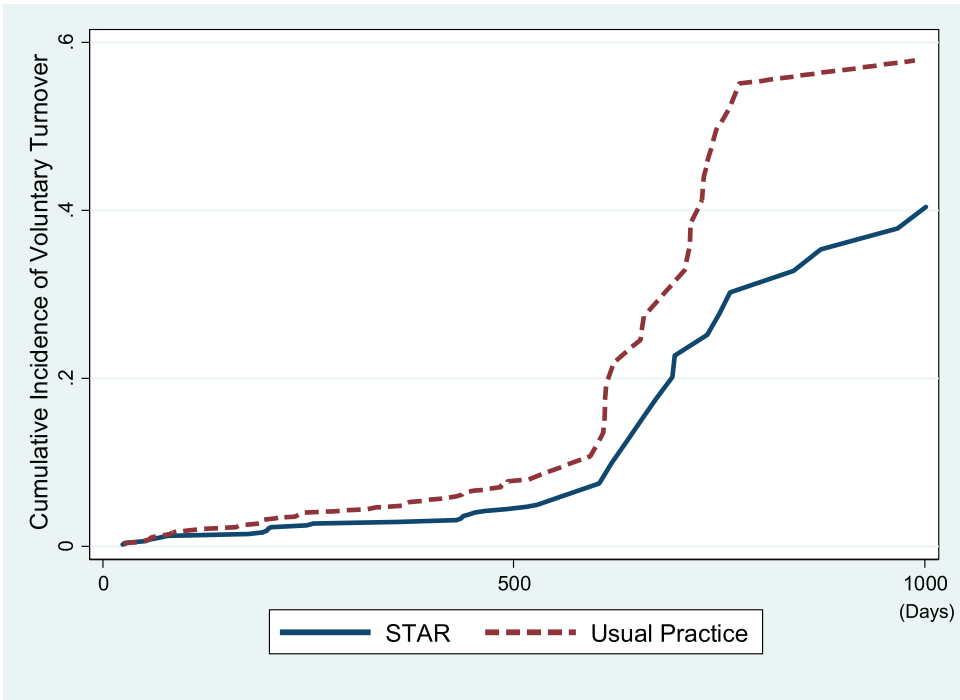


Figure 2. Cumulative Incidence of Voluntary Turnover

Notes: The cumulative incidence, the estimate of the probability of voluntary turnover ($n = 92$), is generated by specifying involuntary turnover ($n = 58$) as a competing risk.

job insecurity than did respondents in the late survey group ($p < .001$, Table A2). Specifically, the early survey group's job insecurity increased by .22, but there was no increase for the late survey group. Since those in the late survey group were already aware of the merger at baseline, it is likely their baseline job insecurity reflected that fact.

It just so happens that there are more Leading-Edge Boomers in STAR than in usual practice while there are more GenXers in the usual practice groups. In addition, there happen to be more Indian Asians in the usual practice than in the STAR groups. Moreover, respondents in STAR groups have longer tenure in the company compared to those randomized to usual practice. Coincidentally, these three variables—age cohort, race, and tenure—are also the variables that were not balanced between early and late survey groups (see Table A2). Therefore, we control for these variables, as well as managerial status in our analyses.

Hypotheses 1 and 2 (Direct Effects)

Models 1 and 2 in Table 2 show that, as hypothesized, STAR reduces turnover intentions, while being in the early survey group increases them. Another way of examining possible effects of the merger announcement is to examine baseline differences, when only about half the sample knew about the merger. As Table A2 shows, we also find a marginally significant difference in turnover intentions at baseline, with those in the late survey group having higher turnover intentions than those in the early survey group.

Table 3 shows the result of these changes for voluntary turnover after adjusting for the competing risk of involuntary exits, using administrative data over almost three years (up to 1,023 days). As hypothesized (see Figure 1), STAR reduces the rate of voluntary turnover; those randomized to this intervention giving them more flexibility and control over their time and more supportive supervisors

are 40 percent more likely to remain with the company. The direct effect of STAR remains significant after adding covariates that were not balanced at baseline (Model 4, Table 3). We do not find differences in the odds of voluntary exits between early and late survey groups. Compared to Generation Xers, Trailing-edge Boomers are more likely to stay with the firm, as are employees with long tenure. Figure 2 shows the cumulative incidence estimation of voluntary exits (using *stcompe*t in Stata 13), with those in STAR groups having lower odds of voluntary turnover compared to usual practice control groups.

Hypothesis 3 (Mediation)

Following Reuben Baron and David Kenny's (1986) approach to testing mediation, we first established the causal relationship between STAR and turnover intentions (as shown in testing Hypothesis 1). Second, we established the relationship between STAR and theorized mediators. Kelly and colleagues' (2014) study had already accomplished this, indicating that STAR significantly reduced work-to-family and family-to-work conflict between baseline and Wave 2, and other research shows STAR effects in reducing burnout, perceived stress, and psychological distress, while increasing job satisfaction (Moen et al. 2016).

Models 3 through 10 in Table 2 suggest that the positive effects of STAR are partially mediated by declines in work-to-family conflict, family-to-work conflict, burnout, psychological distress, and perceived stress (which was only asked of non-supervisory employees), as well as increases in job satisfaction. We see that adding these mediators reduces STAR's effects on turnover intentions. However, STAR remains a statistically significant (negative) predictor of turnover intentions even net of changes in these theorized mechanisms. When including all these mediators within the same model for the combined sample of employees and managers we find changes in job satisfaction remains a statistically significant predictor (see Model 8, Table 2). Increase in job satisfaction also remains significant once increase in job insecurity is included (Model 10). Looking at employees only and including perceived stress (Model 9), we see that declines in family-to-work conflict and increases in job satisfaction are statistically significant predictors of turnover intentions. But all of these measures are correlated (correlation matrix available from authors) and they may reflect underlying latent processes reducing stress and promoting well-being. As hypothesized, an increase in job insecurity fully mediates the effect of merger timing (Models 9 and 10); the early survey group variable is no longer a significant predictor of turnover intentions when changes in job insecurity are included.

Hypothesis 4 (Moderating or Buffering Effects)

In the three-way interaction model between STAR, merger announcement timing, and managerial status (Model 12), the three-way interaction term is statistically significant, suggesting that STAR's buffering of the effects of the early survey group learning about the merger differs greatly between employees and managers. Tests of differences by STAR/usual practice among the four categories defined by early/late survey groups and manager status indicate that turnover intentions are significantly different by STAR status among two groups: employees who are in early survey group ($p < .001$) and managers who are in the late survey group ($p < .05$).

Figure 3 offers a visual representation of these buffering effects separately for non-supervisory employees and managers, based on Model 12 in Table 2. Among both employees and managers, members of the STAR groups report lower levels of turnover intentions by Wave 3 compared to those in the usual practice groups, controlling for baseline turnover intentions. Employees in early survey group randomized to STAR report lower turnover intentions compared to other groups. For managers, it is the late survey group who benefits most from the STAR intervention, with managers who knew about the merger but were also in the STAR treatment group less apt to increase their turnover intentions. This latter finding is surprising, and may reflect managers exposed to STAR seeing the tradeoff of greater individual flexibility and support even though the merger brings more possibilities of layoffs.

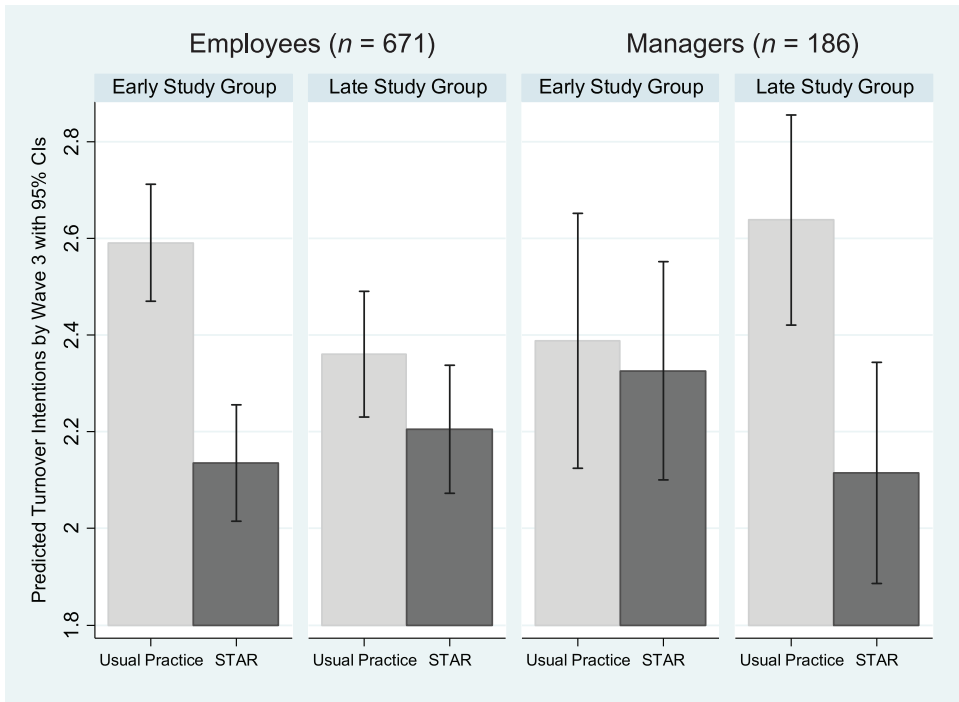


Figure 3. Predicted Turnover Intentions by STAR, Managerial Status, and Survey Group

Note: These effects are net of baseline turnover intentions, age cohort, race, and tenure.

Age cohort, gender, marital status, and respondents' assessments of how easy it would be for them to get a comparable job (employability) were all examined as potential moderators, but only tenure with the organization conditioned the relationship between the focal independent variables and turnover intentions. Model 13 in Table 2 shows the results of tenure as a moderator in a three-way interaction with early survey group. Figure 4 presents the predicted values of turnover intentions by Wave 3 with all the covariates included in the model, using tenure in years. We find STAR buffers the effects of low tenure, but only for those in the early survey group. This finding is consistent with our hypothesis. In addition, workers with longer tenure may be less responsive to either stressful changes (like the merger) or positive changes (like STAR) because they are affected by the benefits and status they have gained in the firm. Note that tenure is somewhat correlated with age, but not entirely. Fully 75 percent of Leading-Edge Boomers, 65 percent of Trailing-Edge Boomers, and only 30 percent of GenXers have more than 11 years (median) tenure.

DISCUSSION AND CONCLUSIONS

Increasingly the story of work in the twenty-first century is about both employers and employees managing change, as the traditional contract linking seniority with security is unraveling and new ways of working are emerging. This study of employees in a single organization helps to inform the meaning of macro-level trends in this emerging social contract. One component of the contract is reflected in the move by leading employers to offer employees greater schedule control and support (employee flexibility), often in the face of mounting time pressures, expectations, and job intensity. We examined, in a rigorous field trial, the effects on turnover intentions and voluntary turnover of an intervention called STAR that offered randomized groups of IT workers greater flexibility and support for their family and personal lives, while other groups continued usual practices, functioning as controls for this analysis.

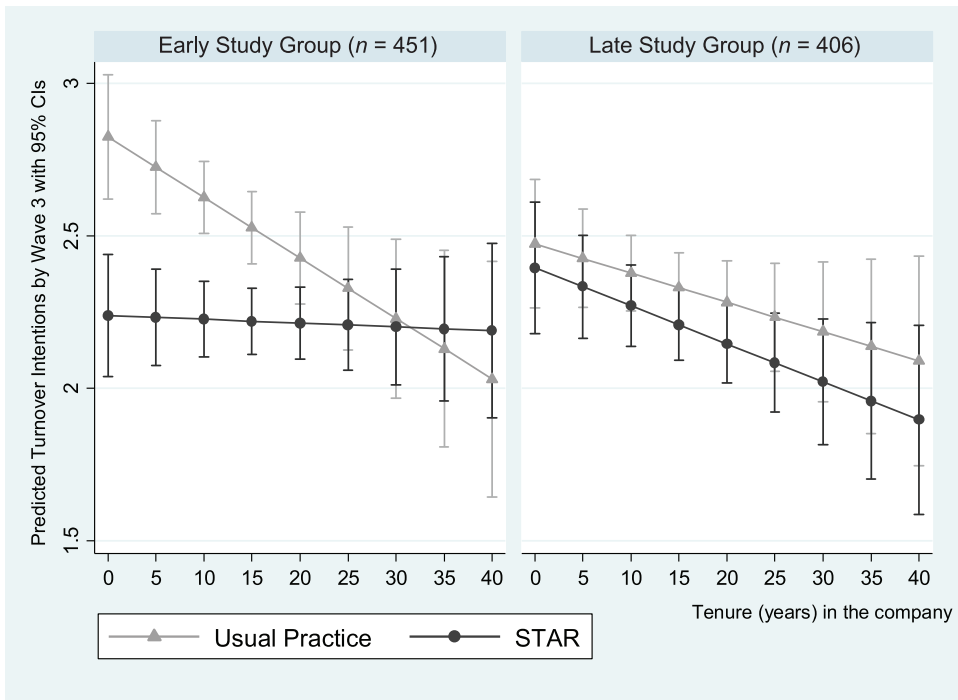


Figure 4. Predicted Turnover Intentions by STAR, Tenure, and Survey Group

Notes: These effects are net of baseline turnover intentions, age cohort, race, and managerial status. Twelve percent of the analytical sample has more than 30 years tenure. The maximum tenure in the company is 43.5 years.

We find that STAR, an initiative that gives employees more control over when and where they work and conveys managerial and co-worker support for attending to personal life, reduces employees' plans to leave the firm over 12 months and also reduces voluntary turnover over a period of about three years. In fact, employees whose teams were randomly assigned to STAR were 40 percent less likely to choose to leave the company as their counterparts in the usual practice groups. These are important findings for the work-life field and the sociology of work because they demonstrate that workers respond to the specific work conditions they face and choose to stay connected to organizations that provide greater flexibility, control, and support even in a broader context of globalization, insecurity, and, in this case, a weak labor market tied to the Great Recession.

We also investigated mediating mechanisms, and find that STAR partially works through increases in well-being (i.e., less work-family conflict, burnout, perceived stress and psychological distress, and higher job satisfaction). In particular, changes in turnover intentions are different for the STAR groups in part because STAR increases job satisfaction, a well-established predictor of turnover intentions.

Previous research (Batt and Valcour 2003; McNall et al. 2010; Muse 2011) suggests that employee flexibility and managerial support for work-life concerns affect turnover intentions and turnover, but randomized controlled trials are extremely rare in this field and most studies have not recognized the potential confounding of employee selection—i.e., who has access to flexibility—in their analyses of the apparent effects of flexibility.

Our findings are consistent with previous studies but the experimental design provides strong and clear evidence of the effects of flexibility and support in initiatives like STAR. One open question is whether a less comprehensive (and therefore less expensive) initiative would have similar effects; experimental and quasi-experimental analyses of other organizational changes would be an important

addition to the field. The findings here show that STAR's broad access to flexibility and explicit support for employees' personal and family lives produces different effects than the usual practice conditions, even though some employees in the control groups negotiated flexible work arrangements with their supervisors. In other words, even compared to a usual practice of some flexible schedules and limited work at home if one's manager approves, STAR's broad message that employees could and should have more control over their own work practices produced lower turnover intentions and lower voluntary turnover—with important implications for employees' careers and for the organization's labor costs. One implication is that flexible work policies that clearly recognize and support employees' personal and family lives and that provide broad access to flexible work arrangements may reduce turnover intentions and voluntary turnover more dramatically than the common approaches requiring case-by-case managerial approval, even if those approaches are not implemented through participatory workshops like we saw in STAR. These questions deserve further research.

Our study also recognizes that offering workers considerable temporal flexibility and work-life support is often tied to employers' own considerable flexibility in changing their workforces, including laying off employees at will. In fact, employers often see initiatives increasing work-time flexibility and family-friendly environments as a way to retain workers in a climate of downsizing and restructuring. It so happened that in the midst of data collection the corporation announced a merger, specifically an acquisition by another firm. This enabled us to test the effects of increased insecurity as a result of this exogenous shock for part of the sample, as well as to test whether the STAR initiative buffered the effects of learning about the merger on turnover intentions. We find, first, that turnover intentions increase following the merger announcement. Responses for the early survey group, whose survey timing allows us to see before and after values, suggest that workers start thinking seriously about leaving the firm when the merger is announced. We also find that STAR's effects on turnover intentions are greater for those in the early survey group, for employees (as compared to frontline managers), and for those with less tenure in the firm (and perhaps less investment in the benefits and status hierarchies of that organization).

While our study design is intended to provide clear and simple estimates of “treatment,” the intervention effects are tangled up in the context of the exogenous stressor, the merger announcement that unexpectedly occurred in the midst of the field trial. This study (and other evaluations of planned changes) must carefully attend to such unplanned changes in the environment (Biron, Gattrell, and Cooper 2010; Hasson et al. 2012; Olsen et al. 2008). What we show is that: (1) STAR has direct effects, reducing both turnover intentions over a 12-month period and actual turnover over a three-year period; (2) being in the early survey group (and therefore learning about the merger after baseline) increases turnover intentions, and (3) the STAR intervention moderates these deleterious effects, with greater effects for different subgroups: employees but not managers, and those with low tenure in the early survey group.

Strengths and Limitations

A principal limitation of this study is the non-randomized merger announcement, because it raises the threat of ineffective confounder control and biased effect estimates. However, we chose to treat this as a serendipitous event, using it to test its effects on turnover intentions and voluntary turnover. Other potential limitations include treatment/exposure contamination across groups (e.g., members of a STAR group discussing the effort with non-members), though any contamination would decrease our odds of finding treatment effects.

There are also limitations as to scope and hence the ability to generalize to larger populations. We have shown that the STAR intervention targeting all workers, not just those most at risk of work-family conflicts such as women and parents, reduces turnover intentions and voluntary turnover. But IT workers are all less “at risk” than those without the education and skills they possess. Future studies need to test similar interventions and specify mechanisms in different workforce populations. A

real limitation, though a deliberate one, has been our focus on IT workers who have demanding, stressful jobs but who were not simultaneously income strapped. Those seeking to develop and test interventions reducing turnover should move beyond professional workforces to consider blue-collar, service, and lower-paid, customer-facing white-collar workforces. STAR increased latitude regarding when and where work occurs and implementing this type of initiative in other settings may require new coordination systems. The goal of interventions in these settings would be that employees feel supported and in control of their work schedules even as all shifts are covered, shift trades are handled fairly, and overtime costs do not increase dramatically (though the cost savings from reduced turnover may offset some labor costs) (Bailyn, Collins, and Song 2007; Lambert, Haley-Lock, and Henly 2012; Williams and Huang 2011).

The principal strength of this study is its ability to look at the effects of a flexibility/support intervention on change in turnover intentions and actual turnover during a period of dislocation. The combination of a randomized trial of an intervention that offers workers greater flexibility and supervisor support for family and personal life and a natural experiment related to learning about an upcoming merger/takeover is unique. Other strengths include the use of a strong design, validated measures, and strong data collection procedures. We recognize that randomized field trials such as this are expensive in terms of money, time, and effort. They are simply not always feasible. But natural experiments—where scholars investigate changes occurring whether they study them or not—would be a real advance from cross-sectional studies, as are longitudinal studies following the same individuals over time.

Especially innovative is our focus on an organizational-level intervention, rather than trying to change individual behavior (such as stress reduction through meditation, for example). As Leonard Syme and Abby King (2013) note, “One key fact about intervention trials is that they rarely attempt to intervene in the fundamental driving forces in society—both social and environmental” (p. 294). We see the world of work as just such a fundamental driving force; indeed, most waking hours of adults are spent on paid work. What we have shown is the value of structural changes in the temporal and social organization of work by dramatically increasing employees’ schedule control and supervisor support for all aspects of their lives.

Yet another strength lies in our capturing the dynamics of organizations. As Katherine Stovel and Mike Savage (2006:1081) point out, mergers (and new initiatives) occur at specific moments in time, while the careers of employees (including their turnover intentions and actual exits) unfold over time, making it difficult to “untangle various clocks” and identify the longer-term employment consequences of shorter-term organizational changes.

Policy Implications

The findings from this study can also inform both corporate and public policy and practice by showing the salutary effects of the STAR initiative on turnover intentions and in actually reducing turnover, a key and costly business concern. The STAR intervention’s combination of schedule control with supervisor support appears to be a promising, cost effective way of promoting employee satisfaction and lowering turnover expectations as well as actual turnover (Barbosa et al. 2014). Thus the increasingly commonly-held belief that offering employees’ temporal flexibility and support will have positive effects for both employees and employers is buttressed. But such initiatives offering flexibility and support should be part of new ways of working for all employees in a work unit or organization. Many flexibility initiatives only offer some minimal and negotiated flexibility as to starting or stopping times or the ability to telecommute for some select employees (see Kelly and Moen 2007). In contrast, STAR empowers all employees to work with their team members in ways that promote their effectiveness on and off the job.

This study also promotes understanding of the changing perspectives of workers in the face of organizational dislocations at a time when such dislocations are increasingly common, given

employers' growing flexibility in managing the size and nature of their workforces. We were able to demonstrate the effects of a merger announcement on subsequent turnover expectations. An old institutional logic assumes that hard work and seniority pay off in job, economic, and retirement security. Public and organizational policies as well as individual and family plans for the future have assumed this was the case. But layoffs and downsizing are now common, regardless of age or seniority. Finally, our results are provocative in pointing to the moderating effects on turnover-related outcomes of initiatives like STAR, emblematic of similar initiatives increasingly being adopted by leading-edge corporations even as they drop any pretense of offering job security.

Implications for Future Research

Two fruitful trends in social research are (1) the move to randomized field experiments, and (2) the growing emphasis on incorporating context in studies of individual actors. Increasingly sociologists and other scholars are proposing randomized experiments as the gold standard for understanding social processes (Gangl 2010; Morgan and Winship 2007; Winship and Morgan 1999). Others underscore the importance of locating lives in context to better specify the conditions under which some processes operate (Brooks-Gunn et al. 1993; Clampet-Lundquist et al. 2011; Ludwig et al. 2008; Morenoff 2003; Sampson 2008). While experimental designs are indeed a tremendous improvement over traditional cross-sectional studies and even observational panel studies, what we have shown is the importance of situating them in the changing contexts in which they are invariably embedded. The results of this study point to the dynamics of the relationship between an employing organization and workers (Stovel and Savage 2006), underscoring the importance of considering other, often unexpected, changes that might affect the outcomes of randomized field research trials testing the efficacy of interventions.

APPENDIX

Table A1. 2 x 2 Factorial Design

	<i>Early Survey Group</i>	<i>Late Survey Group</i>	<i>Total</i>
STAR group	Moderating effects		STAR intervention effects
Control group			
Total	Merger timing (survey group)		

Table A2. Descriptive Statistics, Full Sample and by Timing of Survey Relative to Merger Announcement

	Early Survey Group ^a		Late Survey Group ^a		
	Mean/%	StdDev	Mean/%	StdDev	t-test ^b
Dependent variables					
Actual turnover (~3 years)					
Voluntary exits (N = 92)	8.77%		10.13%		
Involuntary exits (N = 58)	5.65%		6.25%		
Stayers	85.58%		83.62%		
Turnover intentions by Wave 3	2.36	1.03	2.31	1.09	
Independent variables					
Study group level					
Experimental condition (STAR = 1)	51.66%		49.01%		
Individual level					
Baseline turnover intentions	2.18	1.02	2.30	1.09	+
Timing of survey in relation to merger announcement					
Early survey group	100%		0%		
Late survey Group	0%		100%		
Potential mediators					
Δ Work-to-family conflict (W2-baseline)	-.17	.79	-.20	.74	
Δ Family-to-work conflict (W2-baseline)	.01	.57	-.04	.56	
Δ Job satisfaction (W2-baseline)	.03	.62	.07	.55	
Δ Burnout (W2-baseline)	-.10	1.32	-.24	1.12	+
Δ Psychological distress (W2-baseline)	-.35	2.78	-.40	2.51	
Δ Perceived stress (W2-baseline) ^c	-.15	2.16	-.37	2.12	
Δ Job insecurity (W2-baseline) ^d	.22	.69	.00	.60	***
Covariates					
Women (=1)	37.47%		38.67%		
Age cohort					
Leading Edge Boomers (b. 1946-54)	11.53%		19.46%		**
Trailing Edge Boomers (b. 1955-64)	38.58%		37.68%		
Gen Xers (b. 1965-80)	49.89%		42.86%		*
Race					
White	67.63%		73.89%		*
Indian Asian	14.63%		14.29%		
Others	17.74%		11.82%		*
Married (= 1)	82.26%		83.74%		
Have children under 18 at home (= 1)	58.31%		58.87%		
Number of children under 18	1.03	1.09	1.05	1.11	
Managerial status (= 1)	18.85%		24.88%		*

(continued)

Table A2. Descriptive Statistics, Full Sample and by Timing of Survey Relative to Merger Announcement (continued)

	<i>Early Survey Group^a</i>		<i>Late Survey Group^a</i>		<i>t-test^b</i>
	<i>Mean/%</i>	<i>StdDev</i>	<i>Mean/%</i>	<i>StdDev</i>	
Tenure at baseline (in years)	14.15	9.39	14.92	9.59	
Tenure greater than median 11 Years (= 1)	45.01%		55.67%		**
High employability (= 1) ^c	44.47%		43.42%		

Note: Analysis for actual turnover includes 977 cases, while analysis for turnover intentions has 857 cases due to exits and missing data.

^aEarly survey group refers to the respondents who completed their baseline survey before the merger announcement. Late survey group refers to those who were aware of the upcoming merger prior to baseline survey.

^bMean differences between early survey and late survey groups are tested using *t*-tests.

^cPerceived stress was only measured among employees ($N = 671$).

^d Δ Job insecurity is theorized as a mediator for the timing of survey in relation to the merger announcement.

^eThe sample size is 846 for employability due to missing values.

[†] $p < .10$ * $p < .05$ ** $p < .01$ *** $p < .001$ (two-tailed tests)

Table A3. Description of Measurements

Scale	Source	Items	Values
Dependent variable			
Turnover intentions (1-5)	Boroff and Lewin 1997	You are seriously considering quitting TOMO for another employer. During the next 12 months, you will probably look for a new job outside TOMO.	Strongly agree = 5 Agree = 4 Neither = 3 Disagree = 2 Strongly disagree = 1
Mediators			
Work-to-family conflict (1-5)	Netemeyer et al. 1996	The demands of your work interfere with your family or personal time. The amount of time your job takes up makes it difficult to fulfill your family or personal responsibilities. Things you want to do at home do not get done because of the demands your job puts on you. Your job produces strain that makes it difficult to fulfill your family or personal duties. Due to your work-related duties, you have to make changes to your plans for family or personal activities.	Strongly agree = 5 Agree = 4 Neither = 3 Disagree = 2 Strongly disagree = 1
Family-to-work conflict (1-5)	Netemeyer et al. 1996	The demands of your family or personal relationships interfere with work-related activities. You have to put off doing things at work because of demands on your time at home. Things you want to do at work don't get done because of the demands of your family or personal life. Your home life interferes with your responsibilities at work, such as getting to work on time, accomplishing daily tasks, and working overtime. Family-related strain interferes with your ability to perform job-related duties.	Strongly agree = 5 Agree = 4 Neither = 3 Disagree = 2 Strongly disagree = 1

(continued)

Table A3. Description of Measurements (continued)

Scale	Source	Items	Values
Emotional exhaustion/burnout (1-7)	Maslach et al. 2001	<p>You feel emotionally drained from your work. How often do you feel this way?</p> <p>You feel burned out by your work. How often do you feel this way?</p> <p>You feel used up at the end of the workday. How often do you feel this way?</p>	<p>Every day = 7</p> <p>A few times a week = 6</p> <p>Once a week = 5</p> <p>A few times a month = 4</p> <p>Once a month or less = 3</p> <p>A few times a year or less = 2</p> <p>Never = 1</p>
Psychological distress (6-30)	Kessler et al. 2003	<p>During the past 30 days, how much of the time did you feel so sad nothing could cheer you up?</p> <p>During the past 30 days, how much of the time did you feel nervous?</p> <p>During the past 30 days, how much of the time did you feel restless or fidgety?</p> <p>During the past 30 days, how much of the time did you feel hopeless?</p> <p>During the past 30 days, how much of the time did you feel that everything was an effort?</p> <p>During the past 30 days, how much of the time did you feel worthless?</p>	<p>All of the time = 5</p> <p>Most of the time = 4</p> <p>Some of the time = 3</p> <p>A little of the time = 2</p> <p>None of the time = 1</p>
Perceived stress (4-20)	Cohen et al. 1983	<p>During the past 30 days, how often have you felt that you were unable to control the important things in your life?</p> <p>During the past 30 days, how often have you felt confident about your ability to handle your personal problems?</p>	<p>Very often = 5</p> <p>Fairly often = 4</p> <p>Sometimes = 3</p> <p>Almost never = 2</p>

(continued)

Table A3. Description of Measurements (continued)

Scale	Source	Items	Values
Job satisfaction (1-5)	Cammann et al. 1983	<p>During the past 30 days, how often have you felt that things were going your way?</p> <p>During the past 30 days, how often have you felt difficulties were piling up so high that you could not overcome them?</p> <p>In general, you like working at your job.</p> <p>In general, you are satisfied with your job.</p> <p>You are generally satisfied with the kind of work you do in this job.</p>	<p>Never = 1</p> <p>Strongly agree = 5</p> <p>Agree = 4</p> <p>Neither = 3</p> <p>Disagree = 2</p> <p>Strongly disagree = 1</p>
Job insecurity (1-3)	General Social Survey (GSS) (Smith et al. 2016)	Thinking about the next 12 months, how likely do you think it is that you will lose your job or be laid off?	<p>Very likely = 3</p> <p>Somewhat likely = 2</p> <p>Not likely at all = 1</p>

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