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Consequences of Routine Work Schedule Instability for Worker Health and Wellbeing

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Abstract

The American labor market is increasingly unequal, characterized by extraordinary returns to work at the top of the market but rising precarity and instability at the bottom of the market. Research on precarious work and its consequences has overwhelmingly focused on the economic dimension of precarity, epitomized by low and stagnant wages. But, the rise in precarious work has also involved a major shift in the temporal dimension of work such that many workers now experience routine instability in their work schedules. This temporal instability represents a fundamental and under-appreciated manifestation of the risk shift from firms to workers and their families. To date, a lack of suitable existing data has precluded empirical investigation of how such precarious scheduling practices affect the health and wellbeing of workers. We use an innovative approach to collect survey data from a large and strategically selected segment of the US workforce: hourly workers in the service sector. These data reveal relationships between exposure to routine instability in work schedules and psychological distress, poor sleep quality, and unhappiness. While low wages are also associated with these outcomes, unstable and unpredictable schedules are much more strongly associated. Further, while precarious schedules affect worker wellbeing in part through the mediating influence of household economic insecurity, a much larger proportion of the association is driven by work-life conflict. The temporal dimension of work is central to the experience of precarity and an important social determinant of worker wellbeing.

Introduction

From the 1970s through the 2010s, the U.S. labor market experienced a pronounced risk shift from employers to employees, characterized by an increase in job insecurity as well as retrenchment in employer-provided health insurance, retirement plans, and other fringe benefits (Kalleberg, 2009; Cappelli, 1999; Pugh, 2015). During this period, American workers experienced increasingly precarious employment and higher levels of economic insecurity (Jacoby, 2001; Hacker, 2006). At the same time, the social safety net became a less reliable and less sufficient source of fallback support for low-wage or unemployed workers, and household resources were further stretched by a rise in single-parent families (Breen, 1997). Against this backdrop, the rise in precarious employment could have major implications for worker health and wellbeing (Kalleberg, 2018) and the dramatic increases in the disability, morbidity, and mortality of working class and less educated American men and women (Zajacova and Montez, 2017; Sasson, 2016; Case and Deaton, 2015; Montez and Berkman, 2014), while likely caused by a number of factors, is suggestive of the dire possible consequences of these transformations.

This rise in precarious employment was widespread, but most dramatically affected workers in low-wage occupations. Although precarity is complex and multifaceted, key dimensions include both low and stagnant wages and rising uncertainty about the amount and timing of work hours their employer will offer from one week to the next. A great deal of research has emphasized low wages as a marker of precarity (Kalleberg, 2011; Osterman and Shulman, 2011), and policy debates have often centered on raising the minimum wage (Card and Krueger, 1995; Neumark and Wascher, 2007; Dube, Lester and Reich, 2010; Cengiz et al., 2017) or augmenting low wages with social safety net benefits like the earned income tax credit (Cooper, 2017; Hoynes, 2017). Meanwhile, the temporal dimension of employment relations - related to the predictability and stability of work hours - has received far less attention in research and policy domains. Yet, there are reasons to expect that the temporal dimension of precarious employment could be at least as important as wages in shaping worker health and wellbeing.

The service sector represents a strategic site for examining the consequences of the risk shift for American workers. In the service sector, which employs over 10 percent of all American workers and contains the single largest concentration of low-wage workers (Osterman and Shulman, 2011), employers' drive towards the "efficient husbandry" of workers' time (Thompson, 1967) has been taken to a new extreme with the widespread use of "just-in-time" scheduling practices. Many service sector employers use a combination of human resource management strategies to closely align staffing with demand (Lambert 2008; Rubery et al. 2005). Under this system, workers receive their weekly work schedules as little as a few days in advance, their scheduled work hours and work days may change substantially week-to-week, and workers may have their shifts changed, cancelled, or added at the last minute (Golden, 2001; Appelbaum et al., 2003; Clawson and Gerstel, 2015). Recent estimates suggest that nearly 90% of hourly retail workers experience some degree of instability (Lambert et al., 2014). It is no coincidence, then, that the retail and food service sector has been the focus of recent regulatory efforts to address unstable schedules (CLASP, 2018).

Although a large body of theoretical and empirical research in sociology focuses on time as a fundamental component of everyday life (Zerubavel, 1981), the literature on precarious work has typically not emphasized the temporal dimension to the same degree in either defining and describing precarious work or in proposing policy remedies (Capelli, 1999; Kalleberg, 2013). Nevertheless, reducing the precarity of work schedules has recently emerged as a new frontier in organizing campaigns and has been the objective of "secure scheduling" legislation passed in several cities and states over the past three years. Policymaking related to scheduling has often come on the heels of successful local minimum wage campaigns. Each of these avenues for reducing precarity - increasing wages or stabilizing schedules - could possibly improve the health and wellbeing of lower-SES American workers.

Yet, the research on the effects of wages on health is mixed, and the evidence base on the effects of routine uncertainty in work schedules is quite limited. Data sources containing information on both work scheduling and health outcomes are rare, and workers in low-wage unstable jobs are difficult to sample. There is a real lack of data with which to understand the connection between low wages, unpredictable and unstable work schedules, and workers' health and wellbeing outcomes.

To fill this gap, we use an innovative survey method to collect data from hourly retail workers in the United States. Our study, The Shift Project, is unique in collecting detailed measures on routine uncertainty in work schedules as well as measures of worker and family health, social wellbeing, and household financial security for a national sample of retail workers employed at the large firms that are the subject of new regulatory efforts.

Our paper makes several contributions to the literature on the consequences of precarious employment. First, we show that routine uncertainty about work time is a strong predictor of worker health and wellbeing. Second, we show that routine uncertainty in work schedules is even more strongly predictive of worker health and wellbeing than hourly wages are. Third, we demonstrate that routine uncertainty about work time affects health and wellbeing in part through an economic pathway, but even more dramatically owing to the work-life conflict that it causes. Our findings strongly suggest that the temporal dimension of precarious work is an important social determinant of health and wellbeing deserving of greater attention and our estimates provide important information to guide policymaking in this domain. Finally, we demonstrate the utility of an innovative survey recruitment technique and analytic tools that can be flexibly applied in other topic areas in which data are lacking.

Rising Precarity

Throughout the second half of the twentieth century and into the first decades of the twenty-first, a wave of neo-liberal policymaking led to the deregulation of industries, reduction in union power, and retrenchment of the safety net (Snyder, 2016; Kalleberg, 2009). At the same time, firms fundamentally re-oriented, shifting from a Fordist employment model of living wages and stable employment to a set of employment practices tailored to short-term profit making and shareholder value maximization (Fligstein, 1990; Fligstein, 2001). The new firm orientation has fundamentally altered employment relations in ways that afford employers maximum flexibility to nimbly respond to market demands, while requiring workers to contend with work unpredictability and instability (Capelli, 1999; Kalleberg, 2013; Snyder, 2016). Together, these complementary transformations of the state and the firm served to transfer risk from institutional actors to individuals and households (Hacker, 2006; Kalleberg, 2009).

In the domain of labor and employment, these social forces have led to employment precarity - that is, work that is "uncertain, unpredictable, and risky from the point of view of the worker" (Kalleberg, 2009, p. 2). While this increase in precarity has been widespread, those at the bottom of the income and occupational distribution have fared the worst (Fligstein and Shin, 2004).

Scholars suggest that this transformation of employment and rising precarity of work is likely to have broad consequences for social life. Exposure to precarious work has been hypothesized to spill over to negatively affect workers' own health and wellbeing as well as that of their families (i.e. Kalleberg, 2008; Benach, et al., 2014). In this way, precarious work could play an important role in the stratification process, inhibiting both intra- and inter-generational mobility.

While there is fairly broad agreement that employment has become more precarious and more polarized, how to actually conceptualize and operationalize precarious work remains unsettled (Vosko et al., 2009; Kalleberg, 2018). A variety of overlapping typologies have been proposed, with some emphases, such as on contract work, that are more relevant in Europe than the U.S., but a common denominator across typologies and geographies is the inclusion of wages as a key dimension of precarious employment (Rodgers 1989; Lappara et al. 2004; Vosko 2006; Blossfield et al. 2005). In contrast, the temporal dimension of precarious work related to routine uncertainty in work schedules has played a less central role.

However, more recent empirical and conceptual work on precarious employment in the U.S. focuses specifically on two dimensions of precarity - an economic dimension and a temporal dimension. This is reflected in Kalleberg's (2011) classification that distinguishes economic compensation from non-economic aspects of work including pace and scheduling as well as in Kalleberg's (2018) revised typology that defines precarious work in terms of being "limited economically" and being "uncertain" with respect to work time and work scheduling. We see a similar emphasis on these two core dimensions of precarious work in Carre and Tilly's (2018) ambitious cross-national study of retail work - where they emphasize the dimensions of wages and of work hours.

Precarious Wages

Of these two key dimensions of precarious work, the economic has been the overwhelming focus of attention, especially with respect to wages (Kalleberg, 2011). Indeed, in their influential book on job quality and high-road labor practices, Osterman and Shulman (2011) remark that "everyone agrees that wages are the most important feature of work" (p. 4). Vosko (2006) similarly notes that "income is, arguably, the most important dimension of precarious employment" (p. 49).

This focus on wages is evident in the voluminous literature which shows very clearly the rising precarity manifest in stagnant wages for the bottom 50% of the income distribution. While economic growth led to fairly equal wage growth across the income distribution following World War II through the 1970s, the next decades saw approximately zero growth in real wages for the bottom half of earners (Duncan and Murnane, 2011; Mischel et al., 2015).

The focus on wages is also seen in the literature in economics and policy analysis on the minimum wage. This literature documents the declining real value of the minimum wage since the 1980s (Barany, 2016), the political economy of the regulation of wages (Bartels, 2016), and, perhaps most prominently, the debate over the employment effects of minimum wage increases (Card and Krueger, 1995; Neumark and Wascher, 2007; Dube, Lester and Reich, 2010; Cengiz et al., 2017).

That wages are low and stagnant, particularly in the large sector of the economy made up of retail trade and food service (Carre and Tilly, 2018; Osterman and Shulman, 2011), is in and of itself a measure of the severity of the problem of precarious work. However, scholars have also examined how wages matter for employee health and wellbeing. Here the literature is, perhaps surprisingly, quite equivocal with respect to the importance of wages for wellbeing.

One set of studies uses changes to the minimum wage as a way of identifying wage effects on health and wellbeing. Several studies find positive effects of wage increases on mental health (Reeves et al., 2017) and on subjective wellbeing (Flavin and Shufeldt, 2016; Kuroki, 2018). Other work finds heterogeneous effects, with positive effects on mental health confined to women (Horn et al., 2017), and, in a study of teenagers, positive effects on health among white females and negative effects for Hispanic males (Averett et al., 2016). Still other work finds null or even negative effects of wage increases on health (Horn et al., 2017). Outside of the minimum wage effects literature, and focusing specifically on the retail sector, Maume and colleagues analyze a sample of approximately 600 retail food workers employed at Kroger and find relatively weak associations between hourly wage and sleep quality (Maume et al., 2009).

Overall, then, wages are not as consistently predictive of the health and wellbeing of lower-SES workers as might be expected. But, wages are only one dimension of job quality valued by workers. Research shows that workers also place a premium on schedule predictability, which can be quantified in terms of wage trade-offs. One experimental study finds that workers would take a 20% cut to wages in exchange for a job that provided one week of advance notice of work schedules (Mas and Pallais, 2018). Along similar lines, using observational data from the Survey of Household Economic Decision-Making, the Federal Reserve (2018) finds that half of workers would prefer a stable job over a variable one that paid "somewhat more" and 40% of workers would take the stable job over a variable one that paid "a lot more." These results are echoed in interviews with hourly workers who explained that "they would trade higher pay in positions with irregular schedules and short duration for lower paid positions with regular schedules" (Halpin and Smith, 2017).

The economic dimension of precarious work has attracted the weight of attention in the scholarly literature. Wages matter in and of themselves. However, taken together, studies of the effects of wages on wellbeing, as well as workers' willingness to trade wages for stability, point quite clearly to the need to consider other dimensions of precarious work, in particular the temporal.

Precarious Schedules

A rich theoretical literature establishes the centrality of time - clock time, schedules, and "social" time - to the rhythm and patterns of everyday life and to wellbeing (Zerubavel, 1981; Adams, 1990). An appreciation of the temporal dimension of work is also evident in recent research in sociology and industrial relations, which highlights work schedules as a source of inequality and disadvantage (Clawson and Gerstel, 2015; Rubery et al., 2005). The literature on precarity has also begun to take notice of the importance of work time in the experience of precarious work (Snyder, 2016).

The shift to the industrial economy began an era in which many workers "clock in" and "clock out" and are paid for their time rather than for their output (Thompson, 1967; Kalleberg, 2009). When "time is currency," as it is for workers paid by the hour, the regularity of schedules takes on heightened importance. In his seminal book, *Hidden Rhythms*, Zerubavel (1981) described work time as usually taking place "at certain normatively prescribed standard hours" and extolled the benefits of regular and rigidly-patterned work time for allowing workers some protected time and the ability to plan. By the 2010s, however, regular and predictable work schedules had become increasingly rare (Lambert, et al., 2015). Instead, irregularity of work time is common, and fewer workers are able to consistently protect their non-work time from the creeping demands of work (Lambert, 2008; Rubery et al., 2005).

Work time constitutes a major portion of everyday life and, as such, has the potential to shape health and wellbeing. Basic health-related behaviors such as diet, exercise, and sleep require some semblance of control over one's time and the ability to plan (Zerubavel, 1981; Fenwick and Tausig, 2001, 2004; Allen and Armstrong, 2006). When predictability and control over work time are lacking, economic resources can sometimes buffer against ill effects - e.g., outsourcing of domestic tasks can reduce role strain - and research in fact shows that resources spent on time saving services increase happiness (Whillans et al., 2017). Time is also central to quality of life and subjective wellbeing (Mogilner, Whillans, and Norton, 2018).

The relationship between work time and health has received empirical attention with respect to non-standard work hours that encompass evenings, nights, early mornings, and weekends (Presser, 1999). These non-standard work schedules interfere with circadian rhythms and are negatively associated with sleep quality (Vogel et al., 2012; Maume et al., 2009; Wight, Raley, and Bianchi, 2008; Costa, 2003). Non-standard work schedules are also associated with stress (Bara and Arber, 2009), anxiety and irritability (Costa, 2003), and reports of worse self-rated health and mental health (Presser 2003; Fenwick and Tausig, 2001, 2004; Rajaratnam and Arendt, 2001; Knutsson 2003; Costa, 2003; Cho, 2017).

Substantial scholarship has also focused on work time among professional white-collar workers and their struggles to balance work and care commitments (Galinsky et al., 2011; Schulte, 2014). This work finds that professional workers often lack the necessary autonomy and control to shape their own work schedules (Kelly and Moen, 2007; Kelly, Moen, and Tranby, 2011), and this lack of schedule control has negative impacts on health and wellbeing (Marmot et al., 1997; Ala-Mursula et al., 2002). Workplace experiments provide strong evidence that increasing control over work time causes reductions in work-family conflict, as well as reductions in stress and psychological distress and improvements in sleep, among other outcomes (Moen et al., 2016; Kelly et al. 2014; Olson et al. 2015). However, especially among white-collar workers, increased schedule control may lead to role-blurring by allowing, and even obligating, workers to "take work home" (Schieman and Young, 2010; MacEachen et al., 2008).

The cases of non-standard work shifts among low-income workers and schedule control among high-SES workers provide support for the idea that work time is an important contributor to worker health and wellbeing. But, neither of these cases capture precarious schedule dimensions of unstable and unpredictable work time, a "routine uncertainty" that is common among service sector workers.

Routine Uncertainty in Work Time

In the modern service sector, the longstanding employer interests in maximizing control of labor and offloading risk onto workers have taken a new form with just-in-time scheduling practices (Thompson, 1967; Lambert, 2008; Rubery et al., 2005). Under this system, workers receive their weekly work schedules as little as a few days in advance, their scheduled work hours and work days may change substantially week-to-week, and workers may be asked to work on-call or have their shifts changed, cancelled, or added at the last minute (Golden, 2001; Appelbaum et al., 2003; Clawson and Gerstel, 2015; Halpin, 2015). Workers are often required or assumed to have total open availability and schedules are created without consideration of employee preferences and without employee input. As a result, for example, many employees are expected to work so-called "clopening" shifts in which they close the establishment late at night only to return a few hours later to reopen it (Kantor, 2015).

This set of practices allows employers to effectively transfer financial risk to their employees. Rather than commit to a set of stable employee schedules, employers now seek to maintain as lean staffing as possible and do so by scheduling workers for minimal regular hours, adding shifts at the last minute, asking workers to leave shifts early, and requiring "on call" shifts (Houseman, 2001; Lambert, 2008). In turn, employees encounter substantial uncertainty about when and how much they will work (Henly Shaefer, and Waxman, 2006; Carillo et al., 2017). Unstable and unpredictable work schedules are now common in the service sector (Golden, 2001; Appelbaum et al., 2003; Enchautegui et al., 2015) and can be found among low-wage workers in other industries as well, such as health care (Clawson and Gerstel, 2015). Recent estimates show that 87% of early-career retail workers reported instability in their work hours from week to week over the past month. Of those retail workers who reported unstable work hours, the fluctuations were substantial, averaging almost 50% of their usual weekly hours (Lambert et al., 2014).

From the employee perspective, this should not be mistaken for the "desirable flexibility" sought by many white-collar professionals (Galinsky et al., 2011). Instead, it appears that these scheduling practices are experienced as "undesirable instability" by low-wage hourly workers (Henly, Shaefer, and Waxman, 2006; Halpin, 2015). However, there is very little research that actually examines how exposure to these practices might spill over to affect worker health and wellbeing.

Why Routine Uncertainty in Work Time Might Affect Wellbeing

Although little prior research directly examines the relationship between schedule unpredictability and instability and health and wellbeing for service-sector workers, theory and prior research provide ample reason to expect that unpredictable and on-call scheduling for hourly employees will have a range of negative effects. In particular, we expect that unstable and unpredictable work schedules could negatively affect health and wellbeing by increasing household economic insecurity and by increasing work-life conflict.

Household Economic Insecurity

First, while unpredictable and unstable work schedules capture a distinct dimension of precarious work from economic factors such as wages, it may in fact be the case that these temporal aspects of job quality matter for wellbeing primarily because of their negative consequences for household economic security. Variable hours may, mechanically, lead to income volatility, especially if that variability makes it difficult for workers to hold secondary jobs that might otherwise be used to smooth earnings. Last minute changes to work schedules may similarly make it difficult for workers to actually make the shifts that they are scheduled for, increasing income volatility, but also household material hardship. In order to smooth consumption in light of volatile earnings, workers may need to rely on credit products, including high-cost sources of credit such as payday loans and pawn shops.

Prior research has shown that schedule instability leads to economic insecurity (Ben-Ishai, 2015; Golden, 2015, Haley-Lock, 2011; Luce et al., 2014; Zeytinoglu et al., 2004). In a 2013 survey of workers with low to moderate income, among those who reported income volatility, having an irregular work schedule was the most common reason given (Federal Reserve Board, 2014). Similarly, in a financial diary study of 235 households, negative income shocks were common and a drop in work hours was one of the main culprits (Murdoch and Schneider, 2014). Further, prior research indicates that income volatility negatively affects sleep and food sufficiency (Wight, Raley, and Bianchi, 2008; Leete and Bania, 2010).

Work-Life Conflict

Second, unstable and unpredictable work schedules could affect health and wellbeing through non-economic pathways, by making it difficult for workers to balance the demands of employment and personal life (Ben-Ishai, 2015; Golden, 2015, Haley-Lock, 2011; Luce et al., 2014; Morsy and Rothstein, 2015; Zeytinoglu et al., 2004). Work-life conflict may be an intervening mechanism in the relationship between unpredictable and unstable work schedules and health outcomes.

The work-life conflict model identifies underlying time and strain-based conflicts that result from competing and conflicting demands of work and life. Time conflict results when work is scheduled at times that directly interfere with family responsibilities, while strain-based conflicts stem from the stress that schedules cause and can spill over to affect family life (Greenhaus and Beutell, 1985). These conflicts are prevalent in the U.S. workforce, with about half of U.S. workers reporting that work "sometimes" or "frequently" interferes with their family life (Schieman, Milkie, and Glavin, 2009).

Work schedules have important influences on work-life conflict. Data from the General Social Survey in the 2000s show that working non-standard hours, or an irregular or on-call schedule is a strong predictor of work-life conflict (Golden, 2015). Further evidence linking unpredictable schedules to perceptions of work-life conflict comes from a study of 21 stores of a single women's apparel company in the Midwest. In this very useful study, Henly and Lambert (2014a) report that workers who were exposed to limited advance notice, last-minute schedule changes, and variability in days of the week worked report higher levels of general work-life conflict. However, an examination of the health implications of this work-family conflict was beyond the scope of the study.

Nevertheless, there is a separate, strong evidence base linking work-life conflict to worse health and wellbeing (Kelly and Moen, 2007). A recent study also provides evidence that work-life conflict plays a mediating role in the relationship between working non-standard schedules and worse mental health for workers (Cho, 2017). The study does not address the influence of advance notice, variable timing and number of work hours, on-call, or cancelled shifts, which may be expected to have similar effects on time-based conflict and on health.

In sum, while we lack empirical evidence of the association between routine schedule instability and worker health and wellbeing, prior research provides two well-specified pathways by which unstable and unpredictable schedules could affect on health and wellbeing.

The Policy Context: Regulating Precarious Work

Despite clear evidence of the rising precarity of work for many Americans, the Federal Government has taken only limited steps to address the rising precarity of employment evident in low wages and unstable and unpredictable work schedules. However, cities and states around the country have embraced a kind of new federalism, passing legislation that seeks to "raise the floor" in terms of job quality (Bernhardt, 2012), with cities such as San Francisco (Reich, Jacobs, and Deitz, 2014) and states such as California (Milkman and Appelbaum, 2013) at the vanguard.

While states and localities have passed laws to regulate paid family leave (Milkman and Applebaum, 2013) and paid sick time (Colla et al., 2014), the weight of legislative attention has focused on the minimum wage (Tilly, 2015). As of 2018, 30 states, 32 cities, and 6 counties around the country had passed minimum wages in excess of the federal rate of $7.25, ranging from $15 in San Francisco to $7.50 in New Mexico (IRLE, 2018). These laws can be seen as addressing the economic dimension of precarious work by mandating higher wages for low-wage workers. The benefits of such laws are overwhelmingly concentrated in the service sector. Two-thirds of minimum wage workers are employed in service occupations and nearly three-quarters in the retail trade or leisure and hospitality industries (BLS, 2018).

More recently, a coalition of workers, organizers, and unions have advanced a legislative agenda related to the temporal dimension of precarious work. This policymaking is focused on unstable and unpredictable work hours (Figert, 2017). Under the mantle of "fair scheduling" and "secure scheduling," this coalition has successfully pressed for the passage of laws to regulate these scheduling practices in San Francisco, CA, Emeryville, CA, Seattle, WA, New York, NY, and the State of Oregon (CLASP, 2018).

Whereas minimum wage laws effectively focus on workers in the retail and food service sectors, these scheduling laws explicitly apply to only those workers. While the specific coverage rules vary somewhat, these laws only cover workers employed by firms that are in the retail, food service, and full-service restaurant industries (SMC 14.22; Senate Bill 828; NYC Administrative Code, Title 20, Chapter 12; SF Police Code Article 33F and 33G). Further, these ordinances are written to also only apply to large firms. Thus, the worker population of policy interest is retail and food service employees working for large firms.[1]

The scheduling ordinances passed to date also have a common set of provisions. First, the laws generally require advanced notice of work schedules and in cases where shift timing is changed with less notice, employees are owed "predictability pay." However, there is variation in the amount of required notice, with some ordinances requiring two weeks (Seattle, San Francisco, Emeryville, and

[1] In Seattle and Oregon, only those firms with more than 500 employees world-wide are covered (SMC 14.22; Senate Bill 828), in San Francisco only those with more than 40 establishments (SF Police Code Article 33F and 33G), and in New York City only chain fast food restaurant and retail employers with more than one location and more than 20 employees in New York City (NYC Administrative Code, Title 20, Chapter 12) are covered.

New York City for fast food), others requiring one week of notice (Oregon) and another requiring just 72 hours (New York City for retail). Second, several of the ordinances specifically regulate on-call shifts. For instance, in Seattle, employees who are not "called-in" are owed partial pay, and in New York City, such shifts are simply outlawed for retail workers. But, the other ordinances do not specifically regulate on-call shifts. Third, there is also variation in the rules around consecutive closing then opening shifts, referred to as "clopenings." In New York City, fast food workers must give written consent and receive an extra $100 for any two shifts that are separated by less than 11 hours. Oregon and Seattle have a similar rule, though the rest period is shorter (10 hours) and the compensation lower. However, there are no such rules in San Francisco. Fourth, several of these laws include "access to hours" provisions that are designed to make more work hours available to part-time employees as well as "right to request" provisions that protect workers from retaliation should they request input into their work schedules. These two provisions do not directly regulate unstable and unpredictable scheduling practices, but the existing literature on managerial practices and scheduling suggests that they could function to induce more regularity in schedules and more schedule control. Other laws, again broadly similar but with important distinctions in terms of provisions, have been proposed and considered in Washington, D.C., Philadelphia, PA, and the State of Connecticut (Reyes, 2018; Anzilotti, 2018).

However, there is very little evidence that demonstrates that these specific scheduling exposures are associated with worker health and wellbeing. Similarly, while there is variation in the provisions regarding advanced notice and on-call work, there is a pronounced lack of evidence to inform best practices around the amount of advanced notice to require and the case for regulating on-call work and clopening shifts specifically. We also have little information on how the impacts of these measure to regulate the temporal dimension of precarious work would compare to the impacts of wage increases.

Hypothesized Effects of Unstable Schedules on Wellbeing

Between the two key dimensions of precarious work - the economic dimension (wages) and the temporal (unstable work schedules) - the sheer weight of scholarly attention would suggest that wages are by far the more important determinant of employee health and wellbeing. Yet, the literature is surprisingly mixed on the actual empirical associations. In contrast, while theory and a small body of existing research suggests that routine uncertainty in work schedules might affect employee health and wellbeing, data limitations have precluded empirical tests of the association.

Our study focuses on three outcome measures that have been emphasized in prior research because they are expected to be sensitive to work conditions and represent overarching indicators of overall health and wellbeing: sleep quality, psychological distress, and happiness. We then test the following hypotheses relating precarious employment to these outcomes:

H1: Routine uncertainty in work schedules interferes with sleep and increases psychological distress and unhappiness.

H2: Prior research has not made head-to-head comparisons of the relative importance of wages and schedules for worker health and wellbeing. We hypothesize that schedules will be as strongly related to health and wellbeing as wages.

H3: The effects of the routine uncertainty in work schedules on sleep, psychological distress, and happiness will operate, in part, through an economic pathway by affecting household economic insecurity, and, in part, through a temporal pathway by affecting work-life conflict.

Limitations of Existing Data

To date, it has proven difficult to test these hypotheses and especially difficult to do so for the policy-relevant population of employees of large retail and food service firms because there is a pronounced lack of available data. There are three interrelated limitations of existing data: (1) few data sets include measures of scheduling practices, (2) data sets that include measures of scheduling practices rarely include measures of health and wellbeing, and (3) existing data cannot be used to describe the scheduling practices at the large retail firms that are at the center of policy debate and organizing activity.

One important exception is the 2011-2015 waves of the National Longitudinal Survey of Youth-1997 (NLSY97). In those three waves, the NLSY97 contained items that gauged amount of advance notice of schedules that the respondent received at work, the degree of control the respondent had over her schedule, and the week-to-week variability in the respondent's work hours. The NLSY97 also contains useful measures of adult health and wellbeing.

However, the NLSY97 is limited in some important respects. First, by design, it captures a specific cohort of workers - all of whom were born between 1975 and 1982 and were aged 29 to 41 in 2011-2015. That age restriction excludes more than two-thirds of the retail and food service workforce - both the 25% of workers under age 29 and the 43% over age 41 (Author's calculations from ACS). Second, because the NLSY97 is designed to be nationally representative of that age cohort, the sample size of hourly workers in the retail industry is limited, with 1,564 total observations on 1,037 unique respondents working in retail in 2011, 2013, or 2015. Third, while the NLSY97 contains some of the most detailed scheduling measures available to date, these remain quite limited. For example, there are no questions that capture on-call scheduling, clopening, or cancelled shifts. Yet, these practices constitute precarious schedules and routine uncertainty and are central components of recent policymaking. Finally, though policy attention and organizing is focused on regulating large chain retailers, there are no data in the NLSY97 that can be used to describe scheduling practices at these companies. The names of employers are not available, and if they were, the sampling design ensures that we would lack any substantial number of cases within particular employers.[2]

In sum, there is an acute lack of data that contains measures of scheduling and outcomes of interest for sufficiently large samples of retail workers. A significant challenge in collecting this data is the effective recruitment of large samples of retail workers at reasonable cost.

Data and Methods

Survey Methodology

The Shift Project used an innovative method of collecting web-based surveys from a large population of service-sector workers. Our paper analyzes survey data from this study collected from 27,792 retail and food service workers employed at 80 large companies across the country.

Survey respondents were recruited using targeted advertisements on Facebook. Our innovation is to use the unique targeting capabilities that are at the heart of Facebook's business model to sample and recruit respondents from a specific population of substantial scholarly and policy interest

[2] Despite these limitations, it is still potentially useful to benchmark the data we describe below against the NLSY97 data on the scheduling variables in common. It is important to bear in mind that the company samples are not the same in our data as the NLSY97 and the period differs as well, with NLSY97 collection in 2011, 2013, and 2015 and our data in 2017-2018. That said, we find a high degree of similarly. 50% of workers in the Shift data report no control over scheduling as opposed to 49% of NLSY97 workers at companies with at least 10 employees. 64% of Shift respondents report more than 1 week of advanced notice as opposed to 57% of NLSY97 respondents. Finally, we estimate 24% variation in work hours week-to-week in the NLSY97 against 35% in the Shift data.

- hourly workers employed by large firms in the retail sector. Facebook compiles detailed data on its users through a combination of user self-reports, user activity, and third-party vendors. Facebook then offers advertisers the opportunity to use this data at the group level to target advertisements to particular populations of interest. We take advantage of this infrastructure to target survey recruitment messages to active users on Facebook who reside in the United States, are over the age of 18 and under the age of 64, and are employed by one of 80 large retail or food service companies.

Our survey recruitment and data collection approach yields a strategically-targeted, non-probability sample. Although the use of non-probability internet samples is well-established in experimental psychology (Birnbaum, 2004; Skitka and Sargis, 2006), survey methodologists have raised reasonable concerns about inferences drawn from non-probability samples in observational research (Groves, 2011; Smith, 2013). Nevertheless, traditional probability sample surveys are facing steeply declining response rates (Keeter et al., 2017), and an emerging body of work has demonstrated that non-probability samples drawn from non-traditional platforms, in combination with statistical adjustment, can yield similar distributions of outcomes and estimates of relationships as probability-based samples. This work has drawn data from Xbox users (Wang et al., 2015), Mechanical Turk (Goel, Raod, and Sroff, 2015; Mullinix et al., 2015), and Pollfish (Goel et al., 2015). Yet, of all of these platforms, Facebook is the most commonly and widely used by the public (Perrin, 2015).

Using Facebook as our sampling frame is novel and departs from conventional survey sample frames such as address-based samples or random digit dialing. While earlier research noted selection into Facebook activity (Couper, 2011), recent estimates show that approximately 81% of Americans age 18-50 are active on Facebook (Greenwood et al., 2016). Thus, the sampling frame is now on par with coverage of telephone-based methods (Christian et al., 2010). Further, Facebook use is not especially stratified by demographic characteristics (Greenwood et al., 2016).

There is some recent precedent for using Facebook as a recruitment tool for academic research. Bhutta (2012) uses Facebook to recruit Catholic respondents to a survey through Facebook's Catholic affinity groups and chain referrals. In an approach more akin to ours, Zhang et al. (2017) compare respondents drawn from Facebook and the American Community Survey in terms of veteran status, homeownership, and nativity and find a high degree of similarity.

Below, we discuss the logistics of our approach using targeted advertising in greater detail, and then describe several steps that we take to gauge and guard against sample selection bias.

Fielding the Survey

We purchase advertisements on the Facebook platform which then appear in the Desktop Newsfeed, Mobile Newsfeed, and on Instagram accounts of our target sample. Each advertisement is made up of four main elements. The top banner of the advertisement displays the text "[Name of Author's University] Work and Family Study." This text is hyperlinked to our official Facebook study page. Below the banner, we include the text of our advertisement. Third, the center of the advertisement is dedicated to a picture designed to resemble workers at the targeted employer workplace. Finally, below the picture, we include a "headline" that reads "Chance to win an iPad!" A sample advertisement is shown as Appendix Figure 1.

Each advertisement is targeted to users age 18-64, in the United States, who speak English and are employed by one of 80 large service-sector companies. We selected these 80 companies purposefully by drawing from the top 100 retailers by sales in the United States (National Retail Federation, 2015). The full list of companies is included as Appendix Table 1.[3] These firms were strategically chosen because, given their size and business type, they will be covered by local labor laws aimed at regulating work schedules (Author's Correspondence, 2017).

Users who click on the link in our ad are redirected to an online survey hosted through the Qualtrics platform. The front page of the survey contains introductory information and a consent form. Respondents provide consent by clicking to continue to the survey instrument. Respondents who complete the survey and provide contact information are entered in the iPad drawing.

Survey data was collected in June, September, and October of 2016, March, May, and June of 2017, and late August, September, and October of 2017. We pause our data collection between November of 2016 and February of 2017 and in July to early August of 2017 to avoid the seasonal effects of holiday shopping and changes to family routines due to the school summer break.

In total, our advertisements were shown to Facebook users 5,024,362 times, including some users who saw our advertisements on multiple occasions. These advertisements generated 337,098 link clicks through to our survey at a total advertising and prize cost of $160,000. Then, 60,409

[3] The NRF list ranks parent companies that may include more than one consumer-facing brands (for instance, Yum! Brand owns both KFC and Taco Bell). Our sample of employers includes one or more consumer-facing brands owned by 61 of the top 100 retailers including all of the top 30 firms (excluding Apple and Amazon which are primarily internet-based sales businesses), and all but 6 of the top 50 retailers. We also include an additional 11 firms that do not appear on the NRF list, but are among the top 50 largest restaurant chains in the United States (National Restaurant News, 2017).

respondents contributed at least some survey data. In all, 6.7% of ad displays led to clicks through to begin the survey and 18% of those clicks led to some survey data. Overall, 1.2% of advertisement displays yielded survey data.

From the 60,409 responses, we eliminate 8.5% who report that they were not paid hourly. We also exclude almost 4% of respondents who failed a data quality check included in the survey that instructed respondents to select a particular response category to demonstrate their attention. After these exclusions, the remaining sample includes 53,077 respondents. Of these 53,077 respondents who began the survey, 27,792 fully completed the survey. We use multiple imputation for those respondents who completed the survey, but had item non-response using the mi impute chained commands in Stata. Our final analysis sample for a single implicate is 27,792 responses. As a robustness check, we impute missing data for respondents who broke off mid-survey and, for this larger sample, we find results consistent to those we present.

These response rates are lower than obtained in many probability-sample phone surveys. However, a sample such as ours would be difficult if not impossible to reach through traditional methods given the absence of an appropriate sampling frame. Nevertheless, we are attentive to issues of sample selectivity and potential bias, as described below.

Methods of Mitigating Bias

Bias on Observables

As noted above, Facebook use is so widespread as to diminish concerns about its use as a sampling frame. However, a second source of bias arises from non-random non-response to the recruitment advertisement. Statisticians have developed a set of post-stratification and calibration methods that are often deployed in the analysis of non-probability sample data (Wang et al., 2015; Goel et al., 2016; Zagheni and Weber, 2015). This approach allows us to adjust our data to account for discrepancies in the demographic characteristics of our sample compared with the characteristics of a similar target population of workers captured in high-quality probability-sample data. We describe our approach to weighting in detail in Appendix A and all of our results use these weights.

Bias on Unobservables

Post-stratification weighting can effectively adjust for bias in observed characteristics, including for data with much more extreme demographic bias than we observe in our data (i.e. Wang et al., 2015). However, this approach assumes that within narrowly defined cells, the sample is drawn randomly. We address potential biases in observed sample characteristics with two approaches. First, we use variation in "social sharing" of our advertisements as one gauge of unobserved bias. If respondents who are selected into the survey via advertisements that were shared more widely differ on a potential confounder, than testing for interactions between the extent of sharing and schedule instability in predicting our outcomes should reveal the presence of that bias. Second, rather than speculate about forms of non-specific bias, we generate hypotheses about potential, specific unobserved characteristics that might both alter survey response and bias the relationship between schedule instability and health and wellbeing outcomes. We then run advertisements that elicit these "unobservable" characteristics in their messaging (for instance, contrasting a message referencing insufficient work hours with one referencing overwork) and examine if the relationship between schedule instability and health and wellbeing varies for respondents recruited through these opposing channels. We provide further detail on these two approaches in Appendix B.

Key Measures

We fielded an online survey containing approximately 70 questions. The survey was divided into five modules that collected information on job characteristics, household finances, demographics, worker health and wellbeing, and parenting and child wellbeing.

Dependent Variables

We gauge adult health and wellbeing with three measures. First, we use a psychological distress scale that includes 5 of 6 items from the Kessler-6 index of non-specific psychological distress (namely, how often in the past month the respondent felt sad, restless, nervous, hopeless, or that everything was an effort) and an additional item about feeling overwhelmed by difficulties. The scale of psychological distress that combines these six items has a Cronbach's α reliability of .91. We create a dichotomous measure that separates scores below 13 (those with little or no distress, on average) from those between 13 and 24 (those with more than a little distress, on average). The results are not affected by using the full continuous range of the scale. Our measure of distress is distinct from the familiar Kessler-6 measure in that our measure includes an item about feeling that "difficulties are piling up so high you could not overcome them," and does not include the K6 item that asks about feelings of worthlessness.

Independent Variables: Routine Uncertainty in Work Schedules

We measure the instability of respondents' schedules with a set of items that have been carefully developed and tested by the Employment Instability Network (Henly and Lambert, 2014b). First, we ask respondents to classify their usual schedule as a regular day shift, a regular evening shift, a regular night shift, a variable schedule, a rotating shift, or some other arrangement. Second, we ask respondents for the amount of advance notice they are given of their schedule, differentiating 0-2 days of notice, 3-6 days, 1-2 weeks, or 2 weeks or more. Third, we calculate a measure of hour volatility by asking respondents to report the most and the fewest weekly hours they worked over the past 4 weeks and taking the difference in hours divided by the maximum weekly hours. Fourth, we ask respondents if "in the last month, was one of your scheduled shifts cancelled with less than 24 hours notice?" and create a dichotomous indicator distinguishing those that had ("1") from those who had not ("0") experienced a cancellation. Fifth, we ask respondents if "in the last month, you worked on call?" and create a dichotomous indicator distinguishing those that had ("1") from those who had not ("0") worked on-call. Sixth, we ask respondents if "in the past month or so, have you ever worked a closing shift and then worked the very next opening shift with less than 11 hours off in between your shifts at [EMPLOYER]? This is sometimes called 'clopening.' " We create a dichotomous variable indicating those respondents who had ("1") and had not ("0") worked such a shift sequence. In addition, we include a seventh measure, of schedule control, and compare those who say their work schedules are (1) determined completely by the employer with no worker input (2) determined by the employer with some worker input, and (3) determined by the worker with some employer input or entirely by the worker.

Finally, we create an eighth measure, an additive index that combines several measures of schedule instability and unpredictability. The items in this index are (1) having a variable schedule, (2) having less than two-weeks advanced notice, (3) having had a shift cancelled, (4) having worked on-call, (5) having worked a clopening shift, and (6) having no input into scheduling. Just 1% of respondents have a score of six on the scale and so we top-code at five exposures.

Independent Variables: Wages

We also measure respondents' hourly wages. This data is collected by self-report from respondents who report being paid hourly. They are first asked a screening question, "Are you paid by the hour at [EMPLOYER]?" then, if yes, "How much are you paid by the hour by [EMPLOYER]?" In related work (Authors, 2017), we seek to validate the wage data used here by comparing wages against reports for workers in the same industries and occupations who are surveyed in the Current Population Survey and the NLSY97. We find that mean wages in the Shift data are between those reported in the CPS and the NLSY97. We also assess if the canonical association between job tenure and wages is similar across the CPS, NLSY97, and Shift data. Here too, we find that our estimate in the Shift data is closer to the estimate in the CPS and the estimate in the NLSY97 than they are to each other (Authors, 2017).

Mediating Variables

Economic Insecurity. We measure five indicators of household economic insecurity. First, we use a measure of household income volatility, similar to an item from the Federal Reserve's SHED survey, by directly asking respondents, "would you say that week to week your household income is basically the same or goes up and down." We treat this as a dichotomous variable.

Second, we ask respondents, "in a typical month, how difficult is it for you to cover your expenses and pay all your bills" and ask respondents to rate it as very difficult, somewhat difficult, or not at all difficult. We recode responses into a dichotomous variable contrasting "very difficult" with "somewhat" or "not at all difficult." This measure was included in the National Financial Capability Survey and has been used in studies of household financial fragility (i.e. Henager and Wilmarth, 2018; Thedos et al., 2014).

Third, we create a dichotomous measure that captures whether the respondent experienced material hardship in the 12 months prior to survey. Respondents are assigned a ("1") if they used a food pantry, went hungry, did not pay utilities, took an informal loan, moved in with family or friends, stayed in a shelter, or defered needed medical care. Material hardship is a commonly used gauge of deprivation (i.e. Meyer and Jencks, 1989; Beverly, 2001) and these measures are included in the Fragile Families Study and the SIPP, among many other surveys.

Fourth, we create a measure of the use of alternative financial service credit products that is coded as "1" if respondents took out a payday loan or used a pawnshop in the prior 12 months and "0" otherwise. These alternative financial services may be used by workers to smooth erratic income or deal with expense shocks, yet they may also lock respondents into high costs debts that are difficult to retire and so ultimately depress wellbeing (Stegman, 2007). Use of these products has been previously measured in the Detroit Area Study, the Survey of Consumer Finances, and the Federal Reserve's SHED survey, among others.

Finally, we include a measure of respondent's perceived financial insecurity. Following Lusardi, Schneider, and Tufano (2011), we ask respondents to rate their confidence in their ability to cope with a hypothetical expense, in this case of $400. We code respondents as financially fragile if they reported that they certainly could not or probably could not come up with that amount of funds.

Work-Life Conflict. Our survey includes four items capturing work-life conflict drawn from the Fragile Families and Child Wellbeing study (Ciabattari 2007; Nomaguchi and Johnson 2014). Respondents are asked to rate their agreement/the truth of four statements, each on a four-point scale: (1) "My work schedule makes it hard to be there for my family," (2) "my shift and work schedule cause extra stress for me and my family," (3) "where I work, it is difficult to deal with family or personal problems during working hours," and (4) "in my work schedule, I have enough flexibility to handle family needs." Items number 1, 2, and 3 are reverse coded such that lower values signal less conflict. We then combine these four items in a single scale (Cronbach's $\alpha = 0.82$). This scale of work-life conflict differs from a commonly used 5-item scale developed by Netemeyer et al. (1996). Unlike the Netemeyer et al. (1996) items, three of four items in our work-family conflict scale directly reference work schedules, which is a good fit for our research purposes.

Controls

The rise in precarious employment involved declines in wages and shorter job tenure, in addition to the changes in scheduling practices. These same factors could plausibly confound the relationship between work schedules and our key outcomes of interest. For instance, workers who have longer tenure may be rewarded with more stable and predictable schedules and may benefit in terms of economic security and wellbeing through other channels as well. To address these potential sources of confounding, we control for job tenure with a measure of length of employment with current employer (less than 1 year, 1-2 years, 3-5 years, or 6 years or more). We also adjust for usual hours worked per week and whether the respondent reported being a manager.

In addition to these aspects of work, demographic characteristics could also confound any relationship between scheduling practices and our outcomes of interest. Prior research suggests that women and people of color may be more likely to experience unstable and unpredictable work schedules in the service sector (Golden, 2015; Pugh, 2016) and there may also be demographic variation on our key outcome measures. To guard against this source of confounding, we control for gender, race/ethnicity (Black, non-Hispanic; Hispanic; or other/two-or-more-races, non-Hispanic; versus white, non-Hispanic), as well as educational attainment (high school diploma or less, some college, or BA or more), marital status, school enrollment, and whether the respondent lived in a household with children. Last, we include year and month fixed effects in our models to control for seasonal variation in work and wellbeing outcomes.

Analytic Models

We estimate associations between our eight key measures of schedule instability (variation in weekly hours, schedule type, advanced notice, cancelled shifts, on-call shifts, clopening shifts, schedule control, and the index) and our three outcome variables (psychological distress, sleep quality, and happiness). While these estimates are not causal, we note that by design our sample has limited heterogeneity: everyone is an hourly retail worker at one of 80 large firms and we control for economic and demographic characteristics. We estimate the following model:

$$ln\left(\frac{P_i}{1-P_i}\right) = \alpha + \beta X_i + \lambda J_i + \mu + \omega \qquad (1)$$

where our outcome of interest, P for individual i, is the probability of reporting (1) more than a little psychological distress, (2) very good or good sleep quality, or (3) being very or pretty happy regressed on a set of control variables, X, and a set of job scheduling characteristics, J described above. The coefficients of interest are represented by λ and summarize the relationship between work schedules and of hourly wage and the wellbeing of workers in terms of the dependent variables described above. The set of individual-level controls, X_i, are respondent-level measures of race/ethnicity, age, education, household composition, marital status, hourly wage, usual work hours, household income, job tenure, and managerial status. The terms μ and ω represent year and month fixed effects, which control for unobserved period effects. Equation (1) shows the logistic regression model we estimate for our dichotomous outcomes. The results are substantively similar if we estimate the models using a linear probability model. For each outcome, we estimate eight separate models, entering the key measures of scheduling one at a time.

We test our second hypothesis by re-estimating our model above, but without the measures of work scheduling. Here, we focus on the association between hourly wage and each of our three outcome measures. As before, we include the same set of controls for workplace, household, and demographic factors. We then compare the magnitude of these associations with wages against the magnitude of the associations with unstable and unpredictable work schedules estimated above. We do so first by contrasting the predicted values of our outcome measures across the observed range of values for wages and the observed range of values for the instability scale.

We also make a set of policy-relevant comparisons. We examine the full set of minimum wage increases enacted by cities, counties, and states over the period 2015-2018. The median increase was \$0.75 (p25 = \$0.35; p75 = \$1.22). However, several minimum wage increases had stepped introductions. Examining the cumulative increase in the minimum wage over a three year period, we see a median of \$2.14 (p25 = \$0.97; p75 = \$3.32). We contrast the estimated differences in the values for each of our dependent variables from making such an increase from \$7.25 against the estimated differences in each of our dependent variables from three specific provisions of the work scheduling ordinances - having 3-6 days' notice, having 1-2 weeks' notice, and having more than 2 weeks' notice versus having 0-2 days' advance notice, experiencing on-call shifts versus not, and experiencing a clopening shift versus not.

Third, we assess how household economic insecurity and work-family conflict mediate any relationships between unstable and unpredictable scheduling and worker health and wellbeing. Here, we focus on our combined scale measure of schedule instability as the "treatment" variable (though of course recognizing that it is not randomly assigned) and estimate its total effect on each of our three outcome measures. We then use the four-step procedure outlined by Baron and Kenny (1986) to establish that there is partial mediation of the relationship between schedule instability and each of our outcomes by each of our two mediating variables - economic insecurity and work-life conflict. We then estimate the proportion mediated using the assisted product method for binominal outcomes described by MacKinnon et al. (2008). Finally, we use a bootstrap to estimate confidence intervals for each of the estimated proportions mediated.

Robustness

We first test the sensitivity of our results to including employer fixed-effects. This focuses the analysis on within-employer, rather than between-employer, variation. We also test robustness to the inclusion of state fixed-effects and to the inclusion of state and employer fixed-effects. The results are presented in Appendix C. Second, in our main models, we present results weighted to the ACS and by employer size. To test robustness, we re-estimate each of the regression models using alternative weights derived from the CPS and ACS. These results are also presented in Appendix C. Finally, we present the results of our two tests of selection into the survey on an observed confounder, summarized in Appendix B.

Results

Descriptive Results: Worker Health and Wellbeing and Scheduling Experiences

We begin by presenting means for our outcome variables in Table 1. Half of service-sector workers report "more than a little" psychological distress, on average, which is high compared to the broader U.S. population (Weissman et al., 2015). Three quarters of the workers (76%) report fair or poor sleep quality. More than one quarter of workers (29%) report being not too happy.

Table 1 also presents means for our mediators. Household economic insecurity is high. Forty percent of respondents report week-to-week variation in income, one-quarter report difficulty paying bills, and one-fifth make use of alternative credit products. Sixty-five percent of respondents report experiencing at least one serious material hardship in the past twelve months and more than half (50%) of respondents report that they would probably or certainly not be able to cope with an emergency expense of $400. For the mediation analysis, we use a scale created from these five items (Cronbach's $\alpha = 0.62$). Work-life conflict is also common in the sample with a mean and median score of 2.3 out of a maximum of 4.

Table 2 describes the schedules of the service-sector workers in our sample. Schedule variability and short-notice are common. The plurality of workers, 37%, report having variable schedules with another 19% reporting a rotating shift. A smaller share, 22%, has a regular day-time schedule, while another 8% has a regular evening schedule and 9% has a regular night shift. Overall then, just a fifth work a regular, standard-time shift, another 15% work a regular non-standard shift, and almost 60% work some kind of variable schedule.

Workers also receive little advance notice of their weekly schedules. Sixteen percent receive fewer than 3 days of notice and another 18% receive 3 to 6 days' notice. Thirty percent of workers receive 1 to 2 weeks' notice and the final 37% receive more than two weeks' advance notice. Together, 34% of workers have less than one week and 63% of workers have less than two weeks of advance notice.

Workers also experience substantial variation in the total hours they worked each week over the month prior to interview. The mean percent variation is 32%, which implies that a worker who averaged 25 hours per week in the prior month likely worked as few as 20 hours at least one week of the month and as many as 30 hours in another week.

A minority of workers, 13%, report that they have had a work shift cancelled on short-notice within the past month. About twice as many (26%) report that they work on-call shifts. A much larger share of workers, 50%, report working a consecutive closing then opening ("clopening") shift.

Workers also have very little control over their work schedules, with half reporting no input at all and another 35% that their employer makes their schedule, but that they have some input. Just 13% have primary control over their schedule.

These various manifestations of routine work schedule uncertainty also cohere into a set of exposures for some workers. Seven percent of workers are exposed to five or six such scheduling practices and an additional 15% of workers report exposure to 4 such scheduling practices. Another 24% are exposed to 3 and an additional quarter to two such practices. In contrast, only a fifth are exposed to one unstable or unpredictable work scheduling practice and only 6% of workers report no recent exposure to unstable and unpredictable scheduling practices.

Regression Results: Worker Health and Wellbeing and Scheduling Practices

We now turn to our estimates of the relationship between our key indicators of routine work schedule uncertainty and our three measures of worker health and wellbeing. After reporting the main effects, we examine whether these associations are mediated through the household economic insecurity pathway and/or the work-life conflict pathway.

Psychological Distress

In the models in the first column of Table 3, we see that each of our measures of unstable and unpredictable scheduling are positively associated with psychological distress. Respondents whose hours vary more week-to-week have a higher likelihood of experiencing psychological distress, as do respondents who work variable schedules or rotating schedules compared to those working regular day shifts. Workers with less than 3 days of notice and workers with just 3-6 days of notice fare significantly worse than those with more than two weeks of advance notice of their schedules, though we find no difference between those with 1-2 weeks and 2 weeks or more. We also find that workers exposed to cancelled shifts, on-call work, and clopening shifts are significantly more likely to experience psychological distress. Schedule control is also a key predictor of psychological distress with workers who have no input faring substantially worse than those who have some control or even just some input. Finally, workers exposed to multiple forms of unstable and unpredictable scheduling are at highest risk of psychological distress, with an essentially monotonically increasing risk with exposure.

Figure 1 plots the predicted share of respondents experiencing psychological distress by values of the key scheduling variables, after adjusting for the model covariates and weighting. We see that the relationships are both statistically significant and substantively large. For instance, 65% of workers who have had shifts cancelled report psychological distress against less than half of those who have not. There is a similarly large gap between those who work on-call shifts and those who do not. The gap is still larger, at about 30 percentage points, between workers exposed to one or two forms of schedule instability and those exposed to five or more sources of instability (top left panel of Figure 4).

Sleep Quality

The models in the second column (2) of Table 3 present similar estimates for the association between scheduling and sleep quality. Here, we see that week-to-week variability in work hours is negatively associated with reporting very good or good sleep quality, as is working a variable schedule as opposed to a regular day shift. Unsurprisingly, working a night shift is most strongly negatively associated with sleep quality - though working a regular evening shift is not. The distinction between having less than 3 days' notice or 3-6 days' notice versus at least 1 week of advance notice is again evident as those workers with less than two weeks' notice report worse sleep. Shift cancellation and working on call are negatively associated with sleep quality, as are working a clopening shift and having little control over one's schedule. Taken together, exposure to the constellation of unstable and unpredictable scheduling practices raises the risk of fair or poor sleep, particularly among the half of workers reporting exposure to 3 or more such practices.

Figure 2 again plots predicted probabilities from these models. There are substantively significant gaps between those with more and less hours variation and between respondents who have unstable and unpredictable schedules and those with more stable and predictable schedules. For instance, nearly 30% of those whose work hours vary relatively little (10%) week-to-week report very good or good sleep as compared to 25% of those whose work hours vary a great deal (70%). There is a similarly sized gap between those working a regular day shift and those working a variable shift. This same 5 percentage point gap is evident between those who receive less than one week's notice of their work schedules and those receiving at least two weeks' notice. There is a somewhat larger gap - about ten percentage points - between those who have had a shift cancelled and not, those who work on-call and not, and those who experienced a clopening and not. The gap is even wider between those with few exposures to unstable and unpredictable scheduling practices (35%) and those with five or more exposures (15%), nearly half a standard deviation (top middle panel of Figure 4).

Happiness

Finally, the models in the third column (3) of Table 3 report how scheduling practices are related to respondent reports of being very or pretty happy as opposed to not too happy. While there is no significant relationship with week-to-week variability in work hours, the other measures of scheduling show similar patterning as for psychological distress and sleep quality. Respondents who work a variable schedule are less likely to report being very or pretty happy compared to those who work a regular day shift, and those with 0-2 days and 3-6 days of advance notice are significantly less happy than those with at least 1 week of advance notice. There are strong relationships between happiness and exposure to cancelled shifts, on-call work, clopening, limited schedule control, and multiple exposures to unstable and unpredictable scheduling practices.

For these relationships, the association is both statistically and substantively significant. Figure 3 plots predicted values of happiness from the model estimates. There is a nearly 15 percentage-point gap (a third of a standard deviation) between those who have had cancelled shifts (58%) and those who have not (72%). Similarly, those who work on call are much less likely to be very or pretty happy (65%) than those who do not (75%). Finally, as for the other outcomes, respondents who have few exposures to unstable and unpredictable scheduling practices fare far better than those exposed to a constellation of such practices (top right panel of Figure 4).

The Relative Roles of Time and Money: Schedule Stability and Hourly Wages

Exposure to unstable and unpredictable work scheduling practices is negatively associated with psychological wellbeing, sleep, and happiness. The temporal dimension of precarious work matters for health and wellbeing. At the bottom of Table 3, we also show the associations between the key indicator of the economic dimension of precarious work, hourly wages, and our three outcome measures. For each model, we see that hourly wages are significantly associated with our outcomes - workers who earn more are less likely to be psychologically distressed, and more likely to be happy and to report good or very good sleep quality.

However, these estimates do not tell us the relative importance of the temporal and economic dimensions of precariousness for worker health and wellbeing. In Figure 4, we explicitly make these comparisons, contrasting the magnitudes of the associations of schedule instability versus hourly wages with our outcomes by plotting predicted values for each outcome across the observed range of variation in our schedule instability scale and in hourly wages. While both wages and instability are significantly associated with each of the measures of wellbeing, the associations are substantially larger for schedule instability. Contrasting these indicators of the two core dimensions of precarious work clearly shows the primacy of unstable and unpredictable work schedules for psychological distress, sleep, and happiness.

Another way to compare the substantive significance of these two dimensions of precarious work for wellbeing is to size the associations in terms of expected changes in psychological distress, sleep, and happiness that would be implied to result from policy-relevant changes to scheduling or to wages. In Table 4, we present differences in predicted probabilities for our three outcomes that our models suggest would result from increasing advance notice from 0-2 days to 3-6 days (as mandated in New York City), or to 1 week (as mandated in Oregon) or to 2 weeks (as mandated in Seattle and San Francisco). We also show the changes estimated from banning on-call shifts (as mandated for retail workers in NYC) and from eliminating clopening shifts (which are regulated in NYC for fast food workers and in Oregon and Seattle). We contrast these "effect sizes" with those that our model suggests would result from increasing the minimum wage from $7.25, where we bound the effect using the sizes of actual minimum wage increases enacted between 2015 and 2018.

Table 4 shows the substantial impacts of changes to scheduling on wellbeing. For instance, as shown in column 1, eliminating on-call shifts would reduce psychological distress by 15 percentage points for affected workers, and requiring 72 hours of advanced notice would reduce psychological distress by 4.5 percentage points for affected workers. Wage increases also reduce distress, but the magnitudes are smaller: a $4 increase would reduce distress by 2 percentage points.[4] We see similar results for happiness (column 2) and for sleep quality (column 3).

It is possible that even though the effect sizes for a wage increase are smaller than for scheduling changes, the total effect on the full sample would be larger if a larger share of workers would be affected by a wage increase than by a scheduling change. The fourth column of Table 4 shows the percent of workers in our sample that would be affected by each type of policy change. Sixteen percent of workers would be affected by a mandate to provide 72 hours of advance notice, a third of workers by a mandate to provide one week advance notice, and two-thirds by a mandate to provide two weeks' notice. One-quarter of workers would be affected by the mandate to end on-call shifts and as many as half of workers by regulations on clopening. In contrast, in our data, 46% of workers would receive a raise (though the amount would vary) if the minimum wage increased by

[4]It is of course possible the minimum wage increases could have additional effects on workers beyond the treatment group. One possibility is reductions in employment (Neumark and Wascher, 2007), though the scholarly consensus is that there is little evidence of such effects (Dube et al., 2010). Another possibility is that wages would rise for those already above the new minimum wage, though the evidence suggests that wage compression is more likely (Schmitt, 2015).

$3.50 (the 75th percentile of cumulative stepped increases), from $7.25 to $10.75 and 56% would be affected by an even larger increase to $11.25.

Columns 5, 6, and 7 then size the simulated effects of these policy changes for the total sample of workers by multiplying the estimated effect of a change (from Cols 1, 2, and 3) by the share of sample estimated to be affected (from Col 4). Here, the effects are smaller, because the benefits are distributed across all workers. For advance notice, requiring 1 week of notice returns larger benefits than requiring just 72 hours, reducing distress by 2 percentage points in the sample and increasing happiness by 1.8 points and sleep quality by 1.4 points. Requiring two weeks' notice would have larger effects for happiness and sleep, increasing them by 2.8 and 2.5 points, respectively. Eliminating on-call shifts and, especially, clopenings, would also have substantial effects on the total sample, reducing distress by 3.8 and 5.6 points, increasing happiness by 2.4 and 3.8 points, and increasing sleep quality by 2.1 and 3.9 points, respectively. In contrast, though a larger share of workers would be affected by a $4.00 wage increase, the total effect on the sample is significantly smaller, with effect sizes of between 0.5 and 1 percentage point (the same as requiring 72 hours of advance notice in the case of distress and happiness).

Mediation by Economic Insecurity and Work-Family Conflict

In Table 5 we present the key results from the mediation analysis. Here, we focus on the extent to which our two key hypothesized mediators - household economic insecurity and work-life conflict - account for a portion of the total effect of our scale measure of schedule instability on each of the three outcome variables. Table 5 presents the percentage of the total effect that can be accounted for by each of the two mediators for each of the three outcomes. We present the point estimate of the mediation percentage as well as the 95% confidence interval around the proportion.

We see that household economic insecurity substantially mediates the relationship between work scheduling practices and psychological distress, accounting for 42% of the total effect (95% CI: 40%, 44%). Household economic insecurity plays a similar role in accounting for the total effect of schedule instability on sleep and happiness, explaining 37% and 45% of the total effect, respectively. However, even after accounting for household economic insecurity, the relationship between schedule instability and psychological distress remains negative and statistically significant. Economic instability is not the main reason why unstable and unpredictable schedules matter for worker health and wellbeing.

Instead, work-life conflict explains a much larger proportion of the total effect of schedule instability on each of the three outcome measures. We are able to mediate 76% of the total effect of schedule instability on psychological distress (95% CI: 73%, 79%) as well as 82% of the total effect on happiness and 76% of the total effect on sleep quality.

In all, our mediation hypotheses are strongly supported. The negative associations between wellbeing and unstable and unpredictable work schedules is partially mediated by household economic insecurity, but work-life conflict plays the more important mediating role.

Robustness

We assess the robustness of our main regression results to four checks: (1) to the inclusion of employer and state fixed effects, (2) to the use of alternative weights, (3) to using social engagement and sharing on Facebook to check for selection on unobserved confounders, and (4) to the consideration of possible unobserved confounders using a message test. As detailed in Appendices B and C, the results are quite robust to these checks.

Discussion

Since the 1970s, a risk shift from employers to employees has led to an increase in employment precarity for U.S. workers (Hacker, 2006; Jacoby, 2001), but particularly so for workers with low levels of educational attainment and human capital (Kalleberg, 2009). Research and policy mobilization in response to rising precarity have emphasized the economic dimension of precarious wages far more so than the temporal dimension of precarious schedules. Yet, this temporal dimension is a central feature in the lives of many workers that is fundamental to their wellbeing. The use of just-in-time and on-call scheduling practices represents a stark manifestation of the risk shift in that these scheduling practices allow employers to transfer the risk associated with uncertainty in consumer demand onto workers. Although these practices may achieve a short-term business objective of minimizing labor costs, they potentially exact a heavy toll in terms of worker health and wellbeing. To date, this unmeasured cost of precarious schedules has been suspected, but not put to a rigorous empirical test because of a lack of necessary data.

Using a new source of data, we estimated the associations between routine instability in work schedules and worker health and wellbeing. The evidence is strong and consistent in connecting scheduling practices - including short notice of work schedules, irregular work schedules and hours, cancelled shifts and on-call shifts - to psychological distress, worse sleep quality, and unhappiness. These findings align with but extend the strong evidence base linking schedule control to improved health outcomes among white-collar workers (Moen et al., 2016; Olson et al., 2015) as well as the literature on non-standard work schedules and worker wellbeing (Presser, 2003; Bara and Arber, 2009; Costa, 2003).

The vast majority of prior research has focused on the economic dimension of precarious work, and more specifically, on wages. In this context, it is striking that exposure to unstable and unpredictable work schedules has substantively larger negative associations with psychological distress, sleep quality, and happiness than wages. We size these effects in terms of enacted and proposed policies that would change scheduling practices and raise wages. Our simulations show much larger population-level benefits for changes to scheduling than to wages. All of this evidence points to the central importance of the temporal dimension of precarious work and calls for a reorientation in how we think about precarious employment. Although the economic dimension of precarity is of clear importance, the temporal dimension is arguably even more important and deserves more serious and concentrated attention.

Work schedules have an inherent economic component for hourly workers, because schedules together with hourly wage determine earnings. Our mediation analysis confirms that a portion of the association between schedules and wellbeing is attributable to economic insecurity. However, the far more important pathway is through work-life conflict engendered by these scheduling practices. Workers who receive little advance notice, and are exposed to shift cancellation, on-call shifts, and clopenings, among other practices, experience a great deal of conflict between work demands and personal life, which depresses wellbeing. This mediation shows that the temporal dimension of precarious work is consequential over and above any economic pathway.

Alongside this substantive contribution of highlighting the central role of time in the relationship between economic precarity and worker wellbeing, our research makes a methodological contribution in developing a flexible and accessible means to fill a gap in available survey data as well as tools for assessing and addressing selection bias in the resulting non-probability sample. We demonstrate that sophisticated advertisement targeting capabilities available on the social media site Facebook allow for highly targeted survey recruitment. We harness these capabilities in the service of building a large and policy-relevant database of employees at large retail and food service employers. But, the same basic recruitment techniques could be used to build survey samples for a wide variety of research aims. Because we rely on a non-probability sample, we are attentive to issues of potential sample selectivity. We partially address selection issues through post-stratification weighting techniques, which are well-established and easily replicated. In addition, we develop more novel tests of bias on unobservables that could also be applicable to research relying on non-probability samples . Using these strategies, we find no evidence to suggest important selection on an unobserved confounder. Beyond the utility in this particular case, these two tests of bias could be useful in future research that makes use of Facebook or other social media sites as a sampling frame and recruitment channel.

In interpreting our novel and policy relevant findings that work scheduling is strongly related to worker health and wellbeing, some limitations and cautions should be kept in mind. Our analyses are cross-sectional, and unobserved characteristics of individuals could lead some workers to sort into jobs with particular scheduling practices or to be subject to certain scheduling practices within jobs and to experience worse outcomes for reasons unrelated to those scheduling practices. Because we can identify employer and incorporate employer fixed effects into our models, we can address the issue of positive or negative selection into particular employers. For instance, high road employers that offer stable schedules and offer better-than-average work conditions may attract the happiest and healthiest workers, whereas employers with the least desirable working conditions are likely to negatively select the least capable and healthy workers. Inclusion of employer fixed effects accounts for these differences across employers, which is one advantage of the newly-available data from The Shift Project. Nevertheless, a selection process may still influence within-employer variation in the stability and predictability of workers' schedules, if managers exercise discretion and reward or punish workers based on their performance or favoritism. This source of selection cannot be addressed in the current analysis. Therefore, when interpreting our results, we recognize that the unobserved characteristics of workers may in part confound the reported relationships. While we have taken steps to guard against sample selectivity and conducted numerous robustness checks, we cannot eliminate the possibility of residual confounding.

Our research comes against the backdrop of a rapidly changing policy landscape, as many localities have increased the local minimum wage and a few now offer paid time off for sickness or parental leave. In the domain of work schedules, San Francisco, Seattle, Emeryville (CA), and New York City have all passed and implemented legislation that requires chain stores to provide two weeks' of advance notice of work schedules and access to more work hours. New York State and the State of Oregon have written regulation or passed laws and other cities and states are considering similar legislation. Our research provides concrete support for the notion that requiring 72 hours of advance notice would be beneficial to workers, that requiring a week of advance notice would be better still, and that in some domains, two weeks' of advance notice would be best of all. Our estimates also clearly support the idea that reducing on-call and clopening shifts would improve the lives of retail workers, specifically, improving workers' mental health, sleep quality, and happiness. If these provisions served to also reduce schedule variability, hour volatility, shift cancellation, and increase schedule control, our estimates show that those changes too would promote wellbeing.

While our estimates show that these schedule effects are large compared with those of wage increases, our estimates should not be interpreted to suggest that wage increases are immaterial to wellbeing. Quite to the contrary, we find significant associations between wages and psychological distress, sleep quality, and happiness. Yet, these new findings point to a need to rethink what really matters most for job quality in the large, less-skilled sectors of the economy. The multiple dimensions of work schedules that represent the temporal dimension of precarious work are arguably at least as important, and perhaps more so, than the economic dimension as a social determinant of worker health and wellbeing.

The imminent changes in scheduling law and company practice provide a window into the consequences of the risk shift related to workers' time. The exogenous changes in work scheduling practices - in the direction of discouraging and penalizing just-in-time scheduling - offer an opportunity to gauge the effects of a reduction in the risk borne by service sector workers. Future research, capitalizing on these exogenous changes, would represent an important step forward in understanding the causal link between work schedule practices and the wellbeing of workers and their families. Our results add to a growing body of evidence that scheduling experiences are powerfully associated with worker wellbeing, and give us reason to expect that an increase in the stability and predictability of work schedules would be likely to have a range of beneficial effects.

Our study pertains to the retail and food service sector, a sizeable and policy-relevant segment of the U.S workforce. Yet, precarious scheduling experiences are not unique to these workers. Instead, precarious schedules have become a fact of life for a broad range of industry sectors and occupations ranging from the software sector (O'Carroll 2015), telecommunications, media, and government (Rubery et al. 2005), health care (Clawson and Gerstel 2015), to financial professionals and truck drivers (Snyder 2015). Although those working in higher paid occupations have more resources to buffer against routine uncertainty in work schedules, the connections we trace between the temporal dimensions of precarious work - above and beyond economic status - give some reason to expect health and wellbeing consequences of the scheduling risk shift that spread beyond the service sector. The temporal dimension of precarious employment: instability, unpredictability, and uncertainty about work schedules - deserves a place alongside the economic dimension in future research and policymaking on precarious employment and on work as a social determinant of health.

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Tables

Table 1. Descriptive Statistics - Measures of Outcomes and Mediator Variables

Psychological Distress	
More Than a Little	54%
Sleep Quality	
Very Good/Good (vs. Fair/Poor)	26%
Happiness	
Very/Pretty Happy (vs. Not Too Happy)	71%
Week-to-Week Income Volatility	
Varies (vs. Stays the Same)	43%
Difficulty Paying Bills/Expenses	
Somewhat/Not Difficult (vs. Very Difficult)	74%
Household Economic Hardships	
At Least One Hardship (vs. None)	65%
Use of Payday Loans or Pawn Shop	
Used Products (vs. Did Not)	19%
Confidence in Ability to Cope with Emergency Expense	
Certainly/Probably Able (vs. Certainly/Probably Unable)	46%
Economic Insecurity Scale (range 0-1)	
Mean	0.40
Median	0.40
Work-Life Conflict Scale (range 1-4)	
Mean	2.4
Median	2.3
N	27,792

Table 2. Descriptive Statistics - Measures of Work Schedules

Week-to-Week Hours Variation	
Mean	32%
Median	27%
Schedule Type	
Variable Schedule	37%
Regular Daytime Schedule	22%
Regular Evening Schedule	8%
Regular Night Shift	9%
Rotating Schedule	19%
Other	4%
Advance Notice	
0-2 days	16%
3-6 days	18%
Between 1 and 2 weeks	30%
2 Weeks or More	37%
Shift Cancelled in Last Month	
Yes	14%
Work On-Call Shifts	
Yes	26%
Clopening Shift	
Yes	50%
Schedule Control	
Decided by Employer	51%
Employer, with Employee input	33%
Employee with Employer or Solely Employee	15%
Instability Scale	
0	6%
1	22%
2	26%
3	24%
4	15%
5 or more	7%
Hourly Wage	
Mean	$11.60
Median	$10.60
N	27,792

Table 3. Schedules and Psychological Distress, Sleep Quality, and Happiness, Coefficients from Logistic Regressions

	(1) Psych Distress	(2) Good Sleep	(3) Happy
Week-to-Week Hours Variation			
Variation	0.36+	-0.30*	-0.07
Schedule Type			
Regular Day	ref	ref	ref
Variable	0.38***	-0.33***	-0.38***
Regular Evening	0.16	-0.16	-0.23
Regular Night	0.27+	-0.50***	-0.19
Rotating	0.20*	-0.26**	-0.16+
Other	0.23+	-0.29*	-0.29*
Advance Notice			
0-2 days	0.34***	–0.35**	-0.45***
3-6 days	0.15+	-0.27**	-0.25***
1-2 Weeks	0.00	-0.09	-0.09
More than 2 Weeks	ref	ref	ref
Shift Cancelled in Last Month			
No	ref	ref	ref
Yes	0.91***	-0.54***	-0.79***
Work On-Call Shifts			
No	ref	ref	ref
Yes	0.63***	-0.44***	-0.45***
Clopening Shift			
No	ref	ref	ref
Yes	0.47***	-0.41***	-0.39***
Schedule Control			
Decided by Employer	0.57***	-0.33***	-0.61***
Employer + Employee	-0.03	0.11	-0.01
Employee	ref	ref	ref
Instability Scale			
0	ref	ref	ref
1	0.42**	-0.06	0.02
2	0.58***	-0.25*	-0.49***
3	0.91***	-0.51***	-0.62***
4	1.37***	-0.85***	-1.17***
5 or more	1.85***	-1.23***	-1.28***
Hourly Wage	-0.02*	0.01+	0.02*
Observations	27,792	27,792	27,792

Note: This table excerpts key estimates from 21 separate regression models, each of which includes one of the seven schedule measures as a predictor. In this table, each panel x column shown represents a separate regression. All models include controls for race, age, gender, educational attainment, marital status, school enrollment, hourly wage, household income, average weekly work hours, employment tenure, managerial status and living with children as well as month and year fixed-effects.

Table 4. Estimated Effect Sizes of Work Scheduling Regulations and of Minimum Wage Increases

	(1) Change in Pr(Psych Distress)	(2) Change in Pr(V./Pretty Happy)	(3) Change in Pr(V. Good/ Good Sleep)	(4) Percent of Sample Affected	(5) Total Change in Psych Distress	(6) Total Change in Happiness	(7) Total Change in Sleep
Changes in Advance Notice							
0-2 days **to** 3-6 days	-0.045	0.044	0.014	16%	-0.007	0.007	0.002
0-2 days **to** 1-2 Weeks	-0.079	0.075	0.048	34%	-0.019	0.018	0.014
3-6 days **to** 1-2 Weeks	-0.035	0.031	0.034				
0-2 days **to** >2 weeks	-0.081	0.092	0.066				
3-6 days **to** >2 weeks	-0.036	0.048	0.052	64%	-0.020	0.028	0.025
1-2 weeks **to** >2 weeks	-0.001	0.017	0.018				
On Call Shift							
On-Call **to** No On-Call	-0.148	0.092	0.079	26%	-0.038	0.024	0.021
Clopening Shift							
Clopening **to** No Clopening	-0.111	0.075	0.077	50%	-0.056	0.038	0.039
Wages							
$7.25 **to** $7.50	-0.001	0.001	0.001	1%	0.000	0.000	0.000
$7.25 **to** $7.75	-0.003	0.002	0.001	2%	0.000	0.000	0.000
$7.25 **to** $8.25	-0.005	0.004	0.002	8%	0.000	0.000	0.000
$7.25 **to** $8.75	-0.008	0.005	0.003	11%	-0.001	0.001	0.000
$7.25 **to** $9.25	-0.010	0.007	0.004	21%	-0.002	0.001	0.001
$7.25 **to** $9.75	-0.012	0.009	0.006	25%	-0.003	0.002	0.001
$7.25 **to** $10.25	-0.015	0.011	0.007	40%	-0.006	0.004	0.003
$7.25 **to** $10.75	-0.017	0.012	0.008	46%	-0.008	0.006	0.004
$7.25 **to** $11.25	-0.020	0.014	0.009	56%	-0.011	0.008	0.005
Sample Mean (SD)	0.46 (0.50)	0.71 (0.45)	0.26 (0.44)				

Note: This table presents estimates of changes in predicted values of psychological distress, sleep quality, and happiness from a policy-relevant change in scheduling or wages. The estimated values are derived from 15 separate regression models, each of which includes either hourly wages or one of the three schedule measures as a predictor. In this table, each panel x column shown represents estimates from a separate regression. All models include controls for race, age, gender, educational attainment, marital status, school enrollment, hourly wage, average weekly work hours, employment tenure, managerial status and living with children as well as month and year fixed-effects. The estimates for hourly wage do not include any controls for work hours, while the models estimating scheduling effects do include a control for work hours.

Table 5. Mediation of Association between Schedule Instability Scale and Psychological Distress, Sleep Quality, and Happiness by Household Economic Insecurity and Work-Life Conflict: Percentage of Total Effect Mediated

	Economic Insecurity	Work-Life Conflict
Psychological Distress		
Percent Mediated	42%	76%
95% CI	[0.40, 0.44]	[0.73, 0.79]
Good Sleep		
Percent Mediated	45%	82%
95% CI	[0.46, 0.51]	[0.84, 0.91]
Happy		
Percent Mediated	37%	76%
95% CI	[0.31, 0.35]	[0.73, 0.80]
Observations	27,792	27,792

Note: Estimates are of the proportion of the total effect of schedule instability scale on each outcome that is mediated by household economic insecurity (left) and by work-life conflict (right). All models are estimated with survey weights and on multiply-imputed data and include controls for race, age, gender, educational attainment, marital status, school enrollment, hourly wage, household income, average weekly work hours, employment tenure, managerial status and living with children as well as month and year fixed-effects.

Figures

Figure 1: Predicted Probabilities of Psychological Distress by Scheduling Experiences

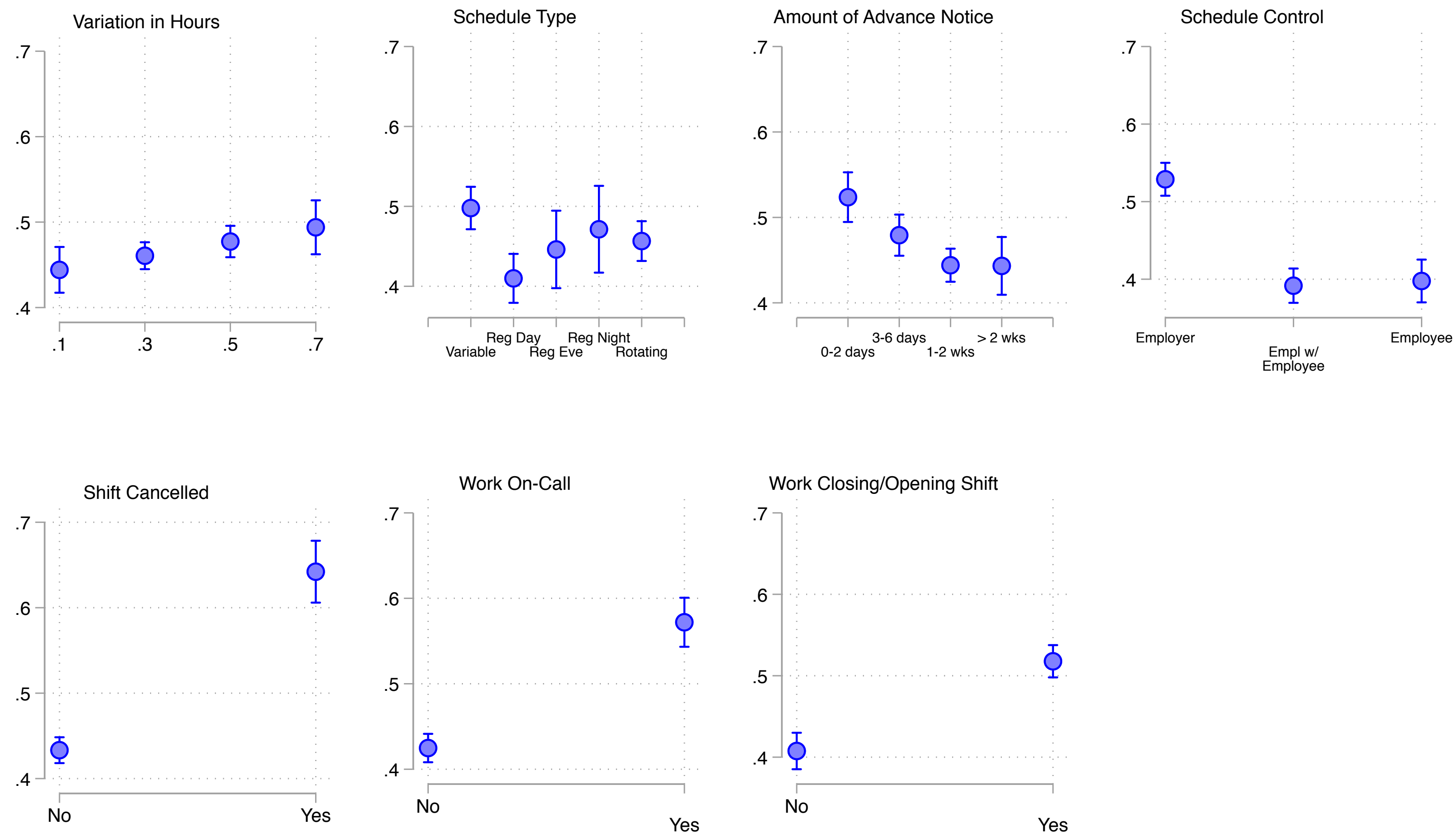


Figure 2: Predicted Probabilities of Very Good/Good Sleep Quality by Scheduling Experiences

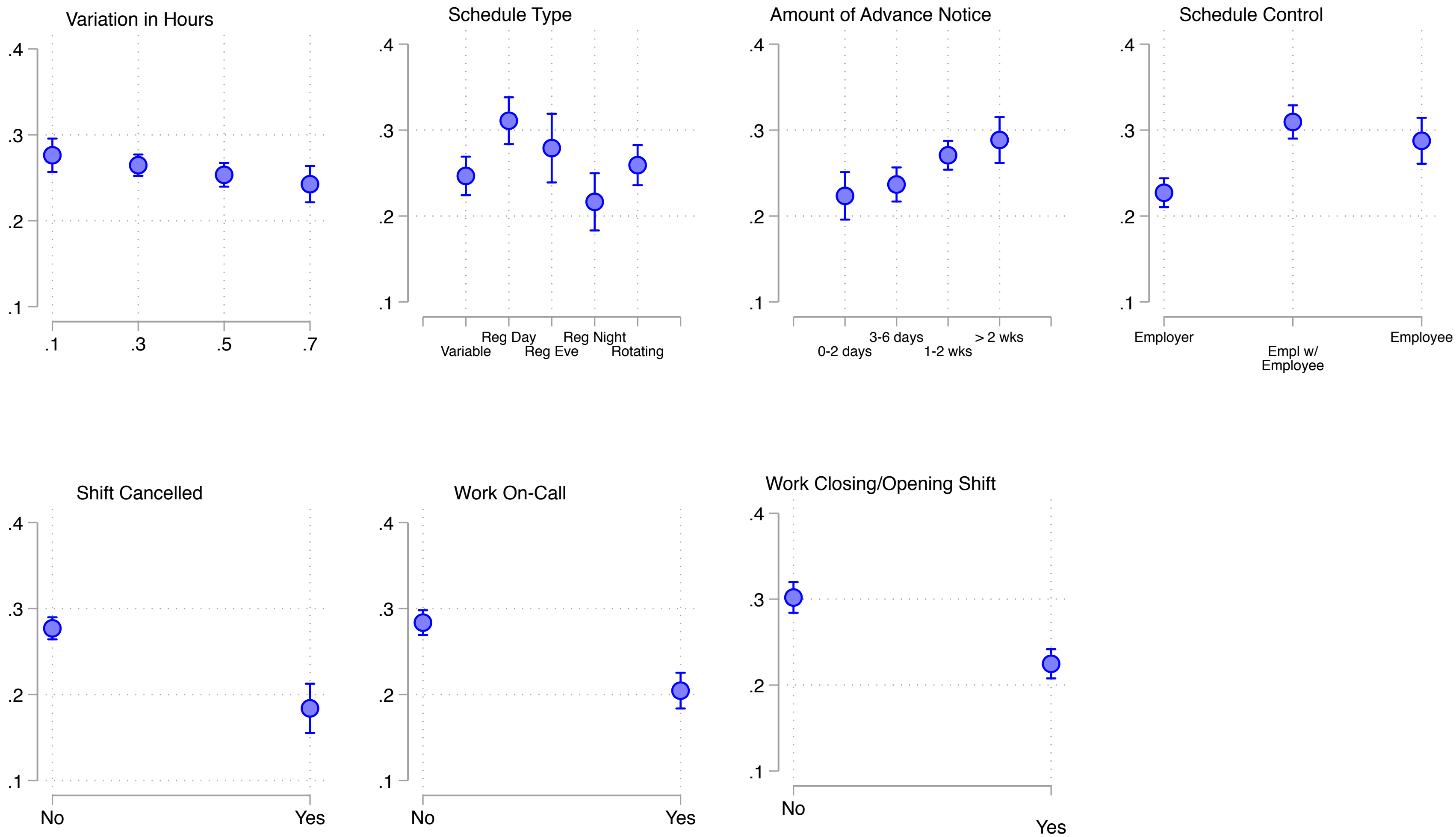


Figure 3: Predicted Probabilities of Being Very/Pretty Happy by Scheduling Experiences

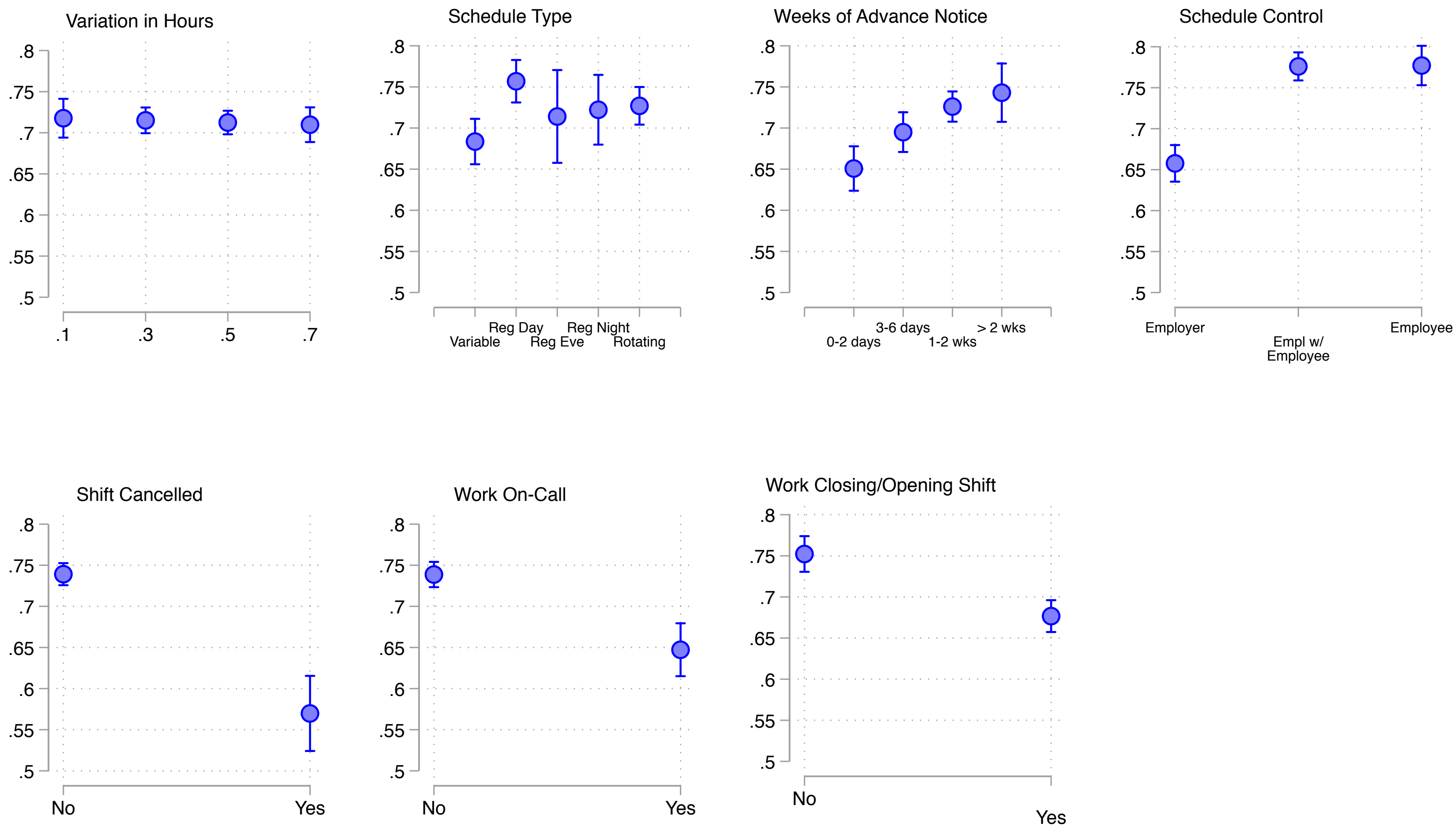
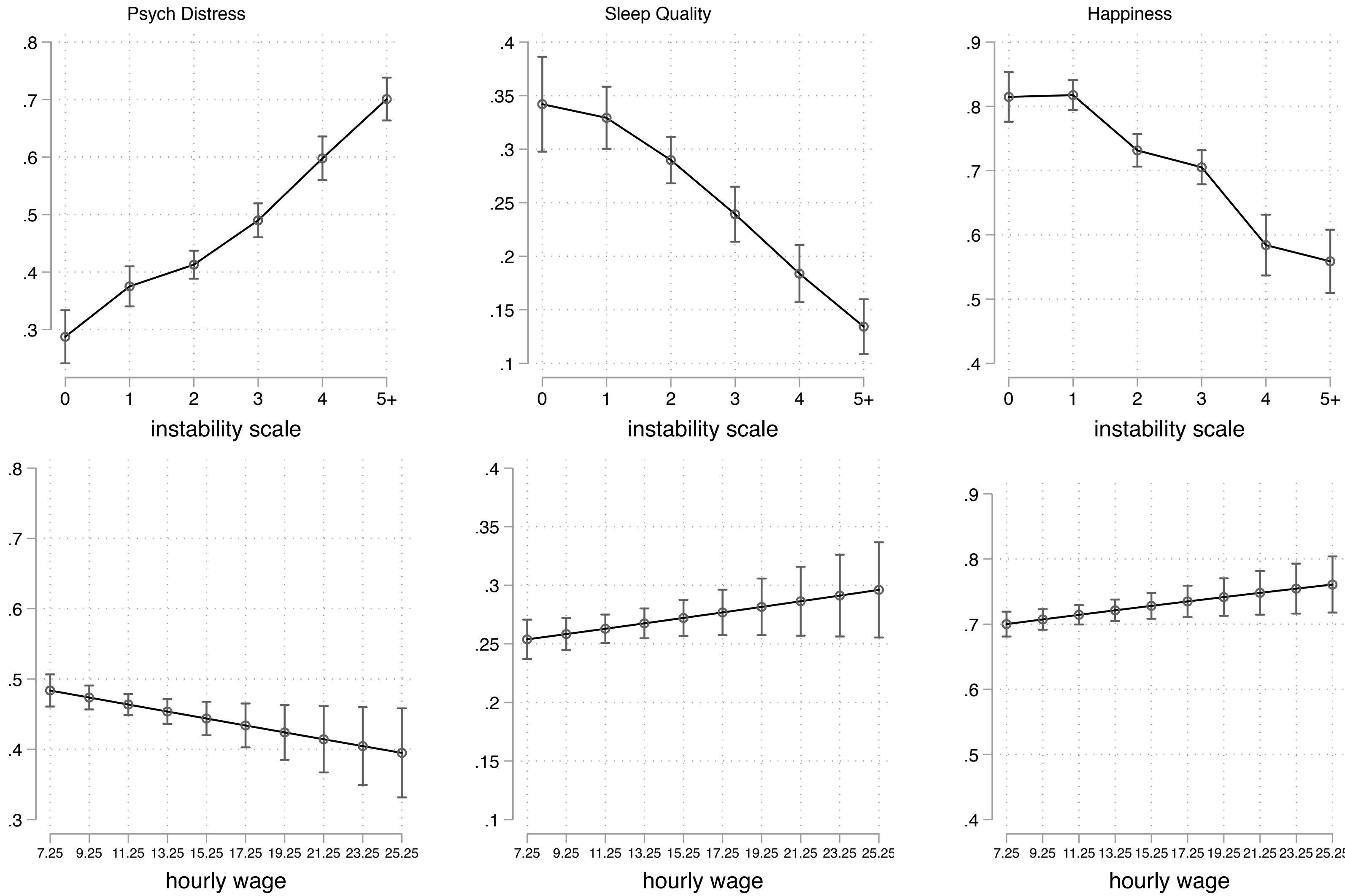


Figure 4: Predicted Probabilities of Outcomes by Scheduling Instability Scale and by Hourly Wage



Appendices

Appendix A. Weighting Procedure

A key contribution of our study is to construct a survey sample that contains relatively large numbers of employees at each of 80 employers. This is valuable precisely because such data are not readily available from existing survey or administrative sources. The consequence is that it is actually difficult to derive a good estimate of the demographic characteristics of our target population to use as a benchmark. We compare the demographics of our survey respondents against several candidate benchmark populations, none of which exactly capture our target population.

First, we pool data from the 2013-2015 American Community Surveys. We limit the sample to 482,608 working-aged respondents in the retail and food sectors, who are not in upper level managerial occupations. Second, we pool data from the 2010-2017 rounds of the Current Population Survey (CPS), focusing on the March Annual Social and Economic Supplement (ASEC). Notably, the ASEC includes a measure of firm size that allows us to restrict the sample to those employed at firms with greater than 1,000 employers. All of the firms in our data have substantially more than 1,000 employees, but conditioning on firm size at least allows us to exclude the many retail workers who are employed at small firms from our analysis. In total, we have data on 32,221 CPS-ASEC working-aged respondents employed at large firms in the relevant industries and occupations.

To construct post-stratification survey weights, we stratify respondents in our survey data and in the ACS and CPS benchmark samples into cells defined by age (18-29, 30-39, 40-49, or 50-64) x race/ethnicity (white, non- Hispanic; Black, non-Hispanic, Other or two-or-more races, non-Hispanic, or Hispanic) x gender (male/female). We construct post-stratification weights for each cell that are the ratio of the proportion of the benchmark sample in each cell to the proportion of our sample in that same cell. When we apply these weights, the demographic characteristics of our survey sample closely align with the respective benchmark sample.

We also construct two alternative weights for each of the ACS and the CPS-ASEC. First, we additionally post-stratify our data into nine industry groups (hardware, department stores, general merchandise, grocery, fast food, apparel, electronics, drug stores, or other retail). Second, we "right-size" the 80 employers in our sample to reflect their distributions in the population as a whole. To do so, we use the Reference USA U.S. Business Database to calculate total U.S. employment at each firm by aggregating up from establishment-level employment counts. Using this data, we observe the number of employees at approximately 200,000 establishments, aggregate at the firm level, and then adjust our weights so that each company contributes in proportion to its share of the 80 company total employment. The unadjusted sample size by employer and then the weighted sample size by employer is listed in Appendix Table 1.

Finally, we adjust each of the weights so that they sum to the original sample size in our survey sample so as not to affect standard errors. Our preferred final weight is post-stratified by age, gender, and race/ethnicity and calibrated to the ACS benchmark and then adjusted for employer size.

Appendix B. Robustness to Social Engagement/Sharing on Facebook and Message Tests of Selection on Confounders

We recruited respondents to the survey through paid advertisements on Facebook. We specified our target audiences and our advertisements were delivered to eligible users based on Facebook's advertisement placement algorithm. However, a unique feature of Facebook's paid advertisements is that users can engage with these paid posts in much the same way that they may engage with posts created by friends or institutions.

One form of engagement is social sharing and tagging. Here, Facebook users can share the advertisement to their own timelines or those of their friends. The extent of this sharing can be gauged by the "social reach" of an advertisement in terms of the number of unique users who see the advertisement through social channels and in terms of the number of "social impressions" obtained through such channels. These may then generate "social clicks" in which users click through to the survey from a social share rather than from a paid placement.

We see substantial variation in social sharing between advertisements. For instance, social impressions range from 0 to 2,326 (or from 0 to 0.22 when adjusted by reach) and social clicks range from 0 to 56 (or from 0 to .004 when adjusted by reach).

Respondents who take our survey because their friends shared or endorsed the content are likely to be different in meaningful ways than those who are targeted by our paid advertisements. Further, this social sharing may extend the reach of our advertisements beyond those who list their employer to those who do not list an employer, but whose employer is known to friends on Facebook. We leverage the fact that these forms of social engagement with our advertisements are then likely to shift the pool of respondents to the survey and introduce heterogeneity in the composition of the sample at the level of the recruitment advertisement. We compare those who came to the survey through advertisements that experienced high levels of engagement and social sharing with those who came through advertisements with little such social activity. If these unobserved characteristics importantly bias our estimates, we should see a significant interaction between the extent of engagement and social sharing and schedule precarity on our key outcome measures. We recognize that this induced heterogeneity is unobserved - we do not know exactly what it is about respondent pools made up of more "organic" respondents that might be different from those made up of more "paid" respondents. Nevertheless, this source of unobserved heterogeneity may be one source confounding bias in our core estimates. In short, this is not a definitive test, but one additional piece of evidence that may build confidence in our results.

We assess the importance of such dynamics by sequentially interacting post shares, social impressions, social reach, and social clicks with the instability scale measure to predict each of the three outcomes. In total, we estimate 12 interactions between these measures of social engagement and the instability scale. We find no significant interactions and the estimates are uniformly small in size with large p-values. The lack of significant interaction gives us some confidence that there is not serious selection into the survey on an unobserved confounder.

Effect of Sample Selection on Potentially Confounding Unobservables

The tests above using social sharing on Facebook give us further confidence that unobservable characteristics are not a major source of bias. Here, we conduct another test of the extent of any such selection on an unobserved confounder. To do so, we first specify several such confounders. The first is time pressure based on the argument that workers who feel that they are time constrained would both be less likely to take our survey and that time constraint could also bias the relationship between scheduling practices and health and wellbeing. The second is pride in work based on the argument that workers' might select into the survey based on such feelings and that pride (or the lack of pride) might both shape worker schedule instability (i.e. "team players" might get better schedules) and our outcomes of interest. The third is relations with managers based on the argument that workers again might select into the survey based on their feelings about their workplace dynamics and that poor relations with management (or good relations) might both shape worker schedule instability (i.e. managers might privilege those workers they get along with) and our outcomes of interest.

We next developed pairs of advertising recruitment messages designed to elicit responses by workers who were either "high" or "low" on each "unobserved" factor. For time pressure, we used two advertising messages to recruit respondents: either "Not getting enough hours at [EMPLOYER]?" or "Overworked at [EMPLOYER]?" For pride in work, we again developed two messages to recruit respondents: either "Proud to work at [EMPLOYER]?" or "Disrespected at [EMPLOYER]?" For managerial relations, we use the messages, "Get along with your manager at [EMPLOYER]?" and ?Issues with your manager at [EMPLOYER]?"

We next re-estimate the preferred models of the scheduling instability scale on each of the three outcome measures, now including an interaction between recruitment message and the instability scale. If the unobserved variable confounds our key relationships between scheduling instability and health and wellbeing, then we would expect that the interaction terms would be significant. In all, we estimate 18 interactions between the instability scale (entered as a continuous variable) and the message conditions. We find that just one of the interactions is significant. In all, this test also fails to find evidence that would suggest important selection on a confounding unobservable.

Appendix C. Robustness to State and Employer Fixed Effects and to Alternative Weights

Employer and State Fixed Effects

Our preferred models include controls for a set of demographic and economic characteristics. Respondents in our data are also identified by their employer (Appendix Table 1). A substantial amount of the variation in exposure to scheduling practices is between employers. Our instability scale ranges from zero sources of schedule instability to five sources of instability. To take this example, we see that at the employer in our data with the most unstable scheduling, 54% of employees report three or more sources of schedule instability (and just 1% report no instability) as compared with just 4% reporting 3 or more sources of schedule instability (and 53% no instability) at the employer with the most stable scheduling. If there is a process of positive selection in which the highest quality workers on unmeasured characteristics select into the employers with the best scheduling practices, then this selection process could confound the relationship between work schedules and health and wellbeing. Controlling for employer-fixed effects removes the confounding influence of such a selection process.

After controlling for employer, there is also substantial within-employer variation in scheduling practices. This variation is driven by the on-the-ground reality that store-level managers have substantial latitude in setting employee schedules (Lambert, 2008). This managerial discretion may drive between-store, but within employer variation in the instability of worker schedules. (This discretion may also drive within-store and between-worker variation in scheduling that is likely to be patterned by important potentially confounding variables).

The employer fixed-effects focus the analysis on the within-employer variation rather than the between-employer variation. We test the robustness of our results to this specification and find that they are virtually unchanged. Appendix Figure 2 presents coefficient plots for the relationship between the instability scale and each of our three key outcomes, contrasting the estimates without employer fixed-effects (red lines) and with employer fixed-effects (blue dot-dash lines).

We also test robustness to a set of state fixed-effects in order to net out state-specific characteristics that could confound the relationship between scheduling and wellbeing - for instance, in some states, regulatory climate might mean that employers engage in more precarious employment practices and that there are fewer policies in place to aid low-income families. Appendix Figure 2 shows that our estimates are quite robust to relying on within-state variation (blue dashed lines). Finally, we also show the robustness of our results to the inclusion of both state and employer fixed effects (blue dotted lines).

Alternative Weights

The regression results above are estimated using our preferred weights that are based on the demographics of industry sub-groups from the ACS and then adjusted to reflect employer size. We also created several alternative weights that benchmark our data to the Current Population Survey.

Appendix Figure 3 shows the robustness of our main results to various reasonable alternative weights, focusing for parsimony of presentation on the relationship between our scheduling instability scale and our three outcome measures. The blues lines show the preferred estimates (models in column 1 of Tables 3, 4, and 5) and the permutations of the ACS weights that do not stratify by industry and do not re-weight by employer size. The red lines report the estimates when using the CPS weights, which have the advantage of being limited to respondents at large employers. The two estimates are largely similar.

The weighted results above give us confidence that any demographic biases in our sample composition in terms of age, gender, and race/ethnicity are not skewing our estimates of the relationship between unstable and unpredictable scheduling practices and worker and family health and wellbeing. However, it remains possible that workers select into our survey sample on the basis of some unobserved characteristic and that same characteristic confounds the relationship between scheduling practices and worker health and wellbeing.

Appendix Table 1. Listing of Firms at which Survey Respondents are Employed

Employer	Unweighted N	Weighted N
7-Eleven	86	174
Ace Hardware	122	173
Advance Auto Parts	83	182
Albertson's	127	169
Aldi	142	50
Applebees	77	322
Arby's	108	194
AT&T	39	103
AutoZone	96	190
Bed Bath & Beyond	142	115
Best Buy	601	330
BJs	88	69
Buffalo Wild Wings	64	185
Burger King	623	544
Carl's Jr.	72	69
Chick-fil-A	188	299
Chili's	65	273
Chipolte	682	144
Costco	611	370
CVS	790	527
Dairy Queen	100	181
Dicks Sporting Goods	124	134
Dollar General	562	321
Dollar Tree	732	254
Domino's	723	483
Dunkin Donuts	779	305
Five Guys	83	175
Food Lion	148	198
GameStop	259	88
Gap Brands	635	360
Giant	53	117
Hannaford	153	78
Hardee's	84	128
HEB	80	171
Home Depot	861	1028
HomeGoods	19	9
Hy-Vee	98	204
IHOP	106	215
Ikea	81	58
JC Penny	576	457
Jimmy John's	438	155
KFC	232	300
Kohls	465	410
Kroger	577	765
Little Caesars	104	190
Lowes	639	748
Macys	261	361
McDonalds	1245	2031
Meijer	297	269
O'Reilly Auto Parts	152	154
Olive Garden	236	296
Panda Express	123	71
Panera	604	216

Cont....

Papa John's	138	195
Petco	177	81
PetSmart	173	143
Pizza Hut	610	477
Publix	752	467
Red Lobster	71	187
Rite Aid	281	257
Ross	121	233
Safeway	579	371
Sams Club	322	403
Sears	69	237
Sonic	59	289
Staples	107	136
Starbucks	1354	477
Subway	922	644
Taco Bell	550	466
Target	648	963
TJX	711	284
Toys-Babies R Us .	295	196
Trader Joe's	30	216
Trader Joes	493	93
Victorias Secret	465	125
Walgreens	752	588
Walmart	401	4109
Wegman's	173	36
Wendys	574	473
Whole Foods	530	231

Appendix Table 2. Full Regression Results for Models of Psychological Distress

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Hourly Wage	-0.02*	-0.02*	-0.02*	-0.02	-0.02*	-0.02+	-0.02*	-0.02*	-0.01
Hours Variation		0.36+							
Schedule Type									
Variable			0.38***						
Reg Day			0.00						
Reg Eve			0.16						
Reg Night			0.27+						
Rotating			0.20*						
Other			0.23+						
Amount of Notice									
0-2 days				0.34***					
3-6 days				0.15+					
1-2 wks				0.00					
>2 wks				0.00					
Cancelled Shift									
No					0.00				
Yes					0.91***				
On Call Shift									
No						0.00			
Yes						0.63***			
Clopening Shift									
No							0.00		
Yes							0.47***		
Schedule Control									
Employer								0.57***	
Employer with Employee								-0.03	
Employee								0.00	
Instability Scale									
0									0.00
1									0.42**
2									0.58***
3									0.91***
4									1.37***
5+									1.85***
Race/Ethnicity									
White, Non-Hispanic	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Black, Non-Hispanic	0.16	0.17	0.18	0.16	0.12	0.12	0.16	0.13	0.13
Hispanic	0.03	0.03	0.07	0.02	0.01	0.01	0.02	0.04	0.04
Other Race/Ethnicity, Non-Hispanic	0.12	0.11	0.13	0.11	0.10	0.07	0.14	0.15+	0.12
Gender									
Female	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Male	-0.24***	-0.24***	-0.25***	-0.25***	-0.25***	-0.25***	-0.25***	-0.26***	-0.28***
Has Children									
No kids	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Has kids	-0.07	-0.06	-0.05	-0.06	-0.05	-0.06	-0.06	-0.07	-0.04
Age									
18-29	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
30-39	-0.26**	-0.25**	-0.25**	-0.26**	-0.23*	-0.24**	-0.23*	-0.26**	-0.23*
40-49	-0.61***	-0.60***	-0.59***	-0.61***	-0.57***	-0.56***	-0.55***	-0.64***	-0.53***
50+	-0.97***	-0.95***	-0.94***	-0.95***	-0.92***	-0.92***	-0.89***	-1.01***	-0.84***
Education									
No degree or diploma earned	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
High school diploma/GED	-0.02	-0.02	-0.03	0.00	0.00	0.02	-0.05	0.00	0.00
Some college	-0.21+	-0.22+	-0.24*	-0.18	-0.18	-0.15	-0.26*	-0.17	-0.21+
Enrolled in School									
No	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Yes	-0.32***	-0.34***	-0.32***	-0.32***	-0.28***	-0.32***	-0.34***	-0.25**	-0.29***
Marital Status									
Married	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Cohabiting	0.42***	0.42***	0.43***	0.42***	0.43***	0.42***	0.41***	0.41***	0.41***
Single	0.36***	0.36***	0.36***	0.35***	0.35***	0.37***	0.37***	0.35***	0.35***
Job Tenure									
Less than 1 year	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
1-2 years	0.12	0.13	0.13	0.13	0.10	0.10	0.11	0.13	0.08
3-5 years	0.09	0.11	0.11	0.10	0.07	0.09	0.05	0.08	0.05
6+ years	-0.02	-0.00	0.01	-0.01	-0.05	-0.01	-0.03	-0.02	-0.01
Usual Work Hours									
0-10 hours	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
10-20 hours	-0.22	-0.19	-0.26	-0.22	-0.24	-0.25	-0.28	-0.19	-0.29
20-30 hours	-0.25	-0.21	-0.29	-0.27	-0.17	-0.27	-0.39+	-0.21	-0.31
30-40 hours	-0.31	-0.21	-0.31	-0.33	-0.20	-0.31	-0.45*	-0.28	-0.32
40+ hours	-0.30	-0.19	-0.31	-0.33	-0.19	-0.32	-0.46*	-0.27	-0.33
Manager									
No	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Yes	0.04	0.05	0.01	0.03	0.04	0.01	-0.01	0.10	-0.02
Constant	0.61*	0.39	0.37	0.51+	0.40	0.37	0.55*	0.29	-0.19
Observations	27792	27792	27792	27792	27792	27792	27792	27792	27792

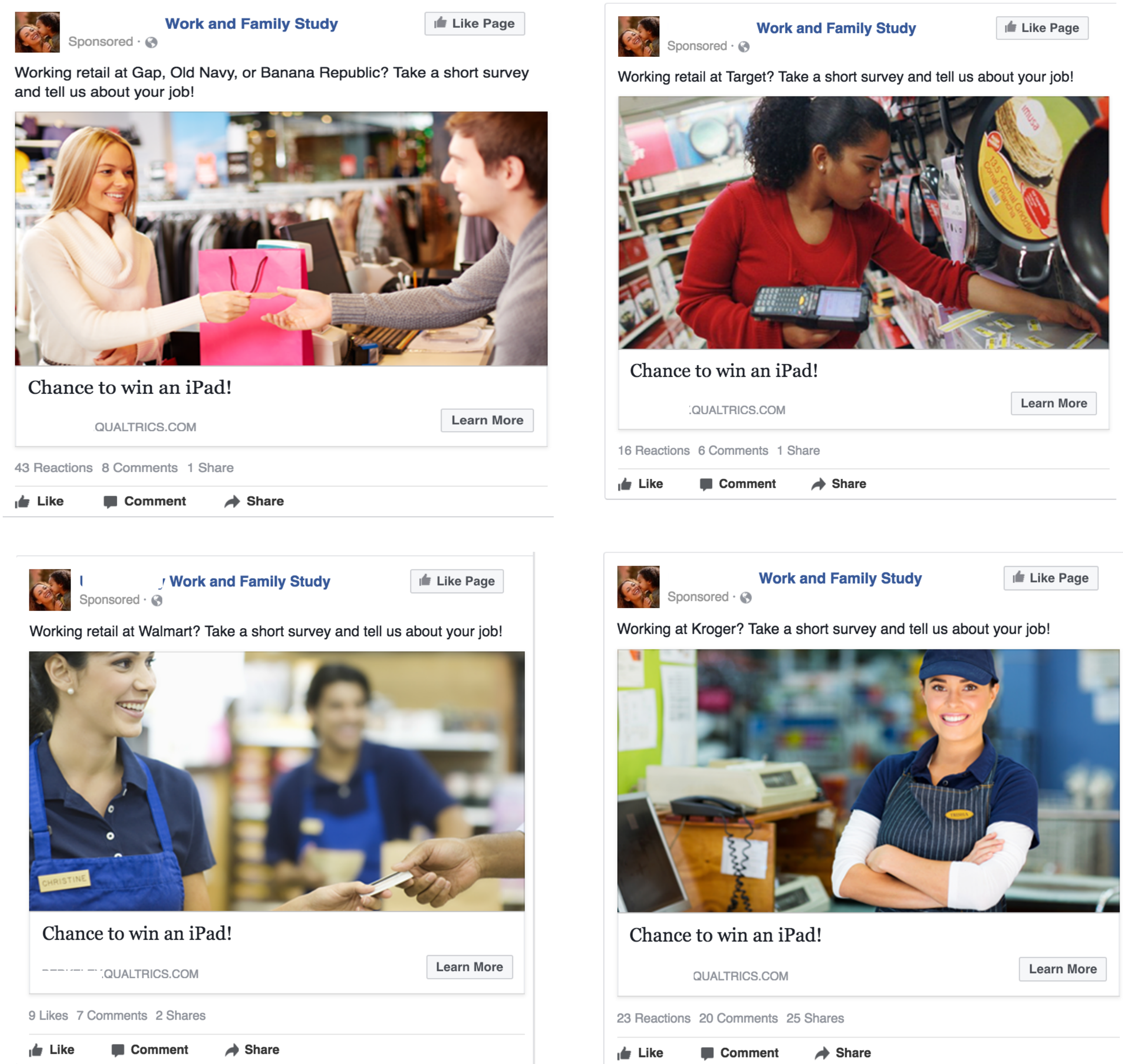
Appendix Table 3. Full Regression Results for Models of Sleep Quality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Hourly Wage	0.01+	0.01	0.01	0.01	0.01	0.01	0.01	0.01+	0.01
Hours Variation		-0.30*							
Schedule Type									
Variable			-0.33***						
Reg Day			0.00						
Reg Eve			-0.16						
Reg Night			-0.50***						
Rotating			-0.26**						
Other			-0.29*						
Amount of Notice									
0-2 days				-0.35**					
3-6 days				-0.27**					
1-2 wks				-0.09					
>2 wks				0.00					
Cancelled Shift									
No					0.00				
Yes					-0.54***				
On Call Shift									
No						0.00			
Yes						-0.44***			
Clopening Shift									
No							0.00		
Yes							-0.41***		
Schedule Control									
Employer								-0.33***	
Employer with Employee								0.11	
Employee								0.00	
Instability Scale									
0									0.00
1									-0.06
2									-0.25*
3									-0.51***
4									-0.85***
5+									-1.23***
Race/Ethnicity									
White, Non-Hispanic	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Black, Non-Hispanic	-0.17	-0.17	-0.19	-0.18	-0.15	-0.14	-0.17	-0.15	-0.16
Hispanic	-0.05	-0.05	-0.07	-0.04	-0.04	-0.03	-0.04	-0.05	-0.06
Other Race/Ethnicity, Non-Hispanic	0.02	0.03	0.01	0.03	0.04	0.05	0.00	-0.00	0.02
Gender									
Female	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Male	0.11+	0.11+	0.12+	0.12+	0.12+	0.11+	0.12+	0.12+	0.13*
Has Children									
No kids	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Has kids	-0.27**	-0.28**	-0.29**	-0.27**	-0.28**	-0.28**	-0.28**	-0.28**	-0.30**
Age									
18-29	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
30-39	0.00	-0.00	-0.00	0.01	-0.02	-0.01	-0.02	0.01	-0.03
40-49	0.03	0.01	0.01	0.02	-0.00	-0.01	-0.03	0.04	-0.05
50+	0.37**	0.35*	0.34*	0.34*	0.33*	0.33*	0.29*	0.39**	0.25+
Education									
No degree or diploma earned	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
High school diploma/GED	-0.04	-0.04	-0.04	-0.06	-0.05	-0.07	-0.02	-0.05	-0.05
Some college	0.32**	0.32***	0.33**	0.29**	0.30**	0.28**	0.36***	0.30**	0.32**
Enrolled in School									
No	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Yes	0.10	0.11	0.10	0.10	0.07	0.09	0.11	0.04	0.07
Marital Status									
Married	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Cohabiting	-0.32**	-0.31**	-0.31**	-0.31**	-0.32**	-0.31**	-0.30**	-0.30**	-0.31**
Single	-0.28***	-0.28***	-0.27**	-0.27**	-0.27**	-0.28***	-0.28***	-0.27**	-0.27**
Job Tenure									
Less than 1 year	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
1-2 years	-0.04	-0.05	-0.05	-0.05	-0.02	-0.03	-0.02	-0.04	-0.01
3-5 years	-0.03	-0.04	-0.04	-0.04	-0.02	-0.03	0.01	-0.02	-0.01
6+ years	0.10	0.08	0.07	0.09	0.12	0.09	0.11	0.11	0.09
Usual Work Hours									
0-10 hours	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
10-20 hours	-0.18	-0.17	-0.14	-0.19	-0.18	-0.17	-0.13	-0.20	-0.13
20-30 hours	-0.16	-0.17	-0.12	-0.15	-0.21	-0.16	-0.06	-0.20	-0.13
30-40 hours	-0.11	-0.17	-0.09	-0.11	-0.18	-0.12	0.00	-0.14	-0.11
40+ hours	-0.26	-0.32	-0.24	-0.25	-0.33	-0.26	-0.13	-0.29	-0.25
Manager									
No	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Yes	-0.17*	-0.17*	-0.13+	-0.15+	-0.17*	-0.15+	-0.12	-0.21**	-0.13
Constant	-1.16***	-1.00***	-0.92***	-1.03***	-1.04***	-1.01***	-1.11***	-1.02***	-0.76**
Observations	27792	27792	27792	27792	27792	27792	27792	27792	27792

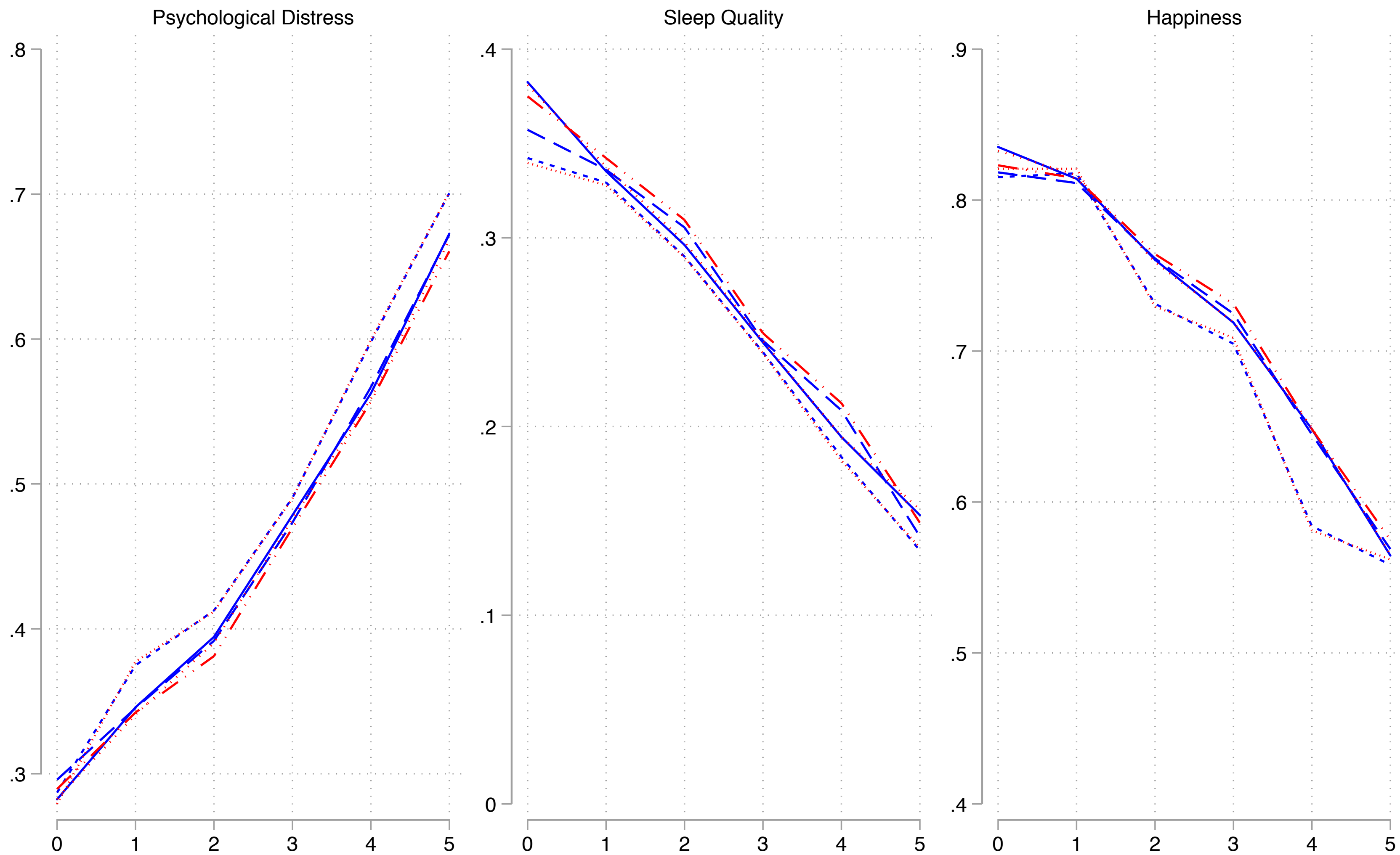
Appendix Table 4. Full Regression Results for Models of Happiness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Hourly Wage	0.02*	0.02*	0.02*	0.01	0.02*	0.01+	0.02*	0.02*	0.01
Hours Variation		-0.07							
Schedule Type									
Variable			-0.38***						
Reg Day			0.00						
Reg Eve			-0.23						
Reg Night			-0.19						
Rotating			-0.16+						
Other			-0.29*						
Amount of Notice									
0-2 days				-0.45***					
3-6 days				-0.25*					
1-2 wks				-0.09					
>2 wks				0.00					
Cancelled Shift									
No					0.00				
Yes					-0.79***				
On Call Shift									
No						0.00			
Yes						-0.45***			
Clopening Shift									
No							0.00		
Yes							-0.39***		
Schedule Control									
Employer								-0.61***	
Employer with Employee								-0.01	
Employee								0.00	
Instability Scale									
0									0.00
1									0.02
2									-0.49***
3									-0.62***
4									-1.17***
5+									-1.28***
Race/Ethnicity									
White, Non-Hispanic	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Black, Non-Hispanic	-0.14	-0.14	-0.16	-0.14	-0.09	-0.10	-0.13	-0.10	-0.11
Hispanic	0.02	0.02	-0.01	0.04	0.04	0.04	0.04	0.01	0.02
Other Race/Ethnicity, Non-Hispanic	-0.10	-0.10	-0.11	-0.09	-0.07	-0.07	-0.11	-0.13	-0.10
Gender									
Female	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Male	-0.10	-0.10	-0.09	-0.09	-0.10	-0.10	-0.10	-0.09	-0.08
Has Children									
No kids	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Has kids	-0.14	-0.15	-0.16+	-0.14	-0.16+	-0.14	-0.14	-0.14	-0.18+
Age									
18-29	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
30-39	-0.10	-0.10	-0.12	-0.09	-0.14	-0.12	-0.12	-0.10	-0.14
40-49	0.18	0.18	0.15	0.18	0.13	0.14	0.13	0.20	0.08
50+	0.20	0.20	0.17	0.17	0.14	0.15	0.13	0.23	0.04
Education									
No degree or diploma earned	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
High school diploma/GED	0.02	0.02	0.04	-0.00	0.00	-0.01	0.04	-0.00	-0.00
Some college	0.12	0.12	0.15	0.09	0.09	0.08	0.16	0.08	0.13
Enrolled in School									
No	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Yes	0.43***	0.43***	0.44***	0.43**	0.39**	0.43***	0.45***	0.36**	0.40***
Marital Status									
Married	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Cohabiting	-0.43***	-0.44***	-0.44***	-0.43***	-0.44***	-0.43***	-0.42***	-0.42***	-0.43***
Single	-0.61***	-0.61***	-0.60***	-0.59***	-0.59***	-0.61***	-0.61***	-0.60***	-0.60***
Job Tenure									
Less than 1 year	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
1-2 years	-0.14	-0.15	-0.15	-0.15	-0.11	-0.12	-0.13	-0.15	-0.10
3-5 years	-0.05	-0.06	-0.07	-0.06	-0.03	-0.05	-0.02	-0.04	-0.01
6+ years	0.05	0.05	0.02	0.04	0.09	0.05	0.06	0.06	0.04
Usual Work Hours									
0-10 hours	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
10-20 hours	-0.22	-0.21	-0.18	-0.23	-0.21	-0.21	-0.18	-0.26	-0.14
20-30 hours	-0.02	-0.01	0.02	-0.00	-0.10	-0.01	0.09	-0.07	0.05
30-40 hours	0.02	0.02	0.02	0.04	-0.09	0.02	0.14	-0.01	0.03
40+ hours	0.05	0.05	0.04	0.08	-0.07	0.05	0.18	0.01	0.06
Manager									
No	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Yes	0.07	0.07	0.10	0.10	0.08	0.10	0.11	0.02	0.13
Constant	1.23***	1.26***	1.48***	1.40***	1.46***	1.42***	1.29***	1.61***	1.83***
Observations	27792	27792	27792	27792	27792	27792	27792	27792	27792

Appendix Figure 1: Example Survey Recruitment Advertisements



Appendix Figure 2: Predicted Values by Scheduling Experiences, Alternative Weights



Appendix Figure 3: Predicted Values by Scheduling Experiences, with and without Employer Fixed Effects