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TEMPORARY WORK AND DEPRESSIVE SYMPTOMS: A PROPENSITY SCORE ANALYSIS

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Abstract

Recent decades have seen a tremendous increase in the complexity of work arrangements, through job sharing, flexible hours, career breaks, compressed work weeks, shift work, reduced job security, and part-time, contract and temporary work. In this study, we focus on one specific group of workers that arguably most embodies nonstandard employment, namely temporary workers, and examine the consequences of this type of employment on depressive symptoms. This study aims to estimate the effect of being a temporary worker on depressive symptom severity. Accounting for the possibility of mental health selection into temporary work through propensity score analysis, we isolate the direct effects of temporary work on depressive symptoms with varying lags of time since exposure. We use prospective data from the U.S. National Longitudinal Survey of Youth 1979 (NLSY79), which has followed, longitudinally, from 1979 to the present, a nationally representative cohort of American men and women between 14 and 22 years of age in 1979. Three propensity score models were estimated, to capture the effect of different time lags (immediately following exposure, and 2 and 4 years post exposure) between the period of exposure to the outcome. The only significant effects were found among those who had been exposed to temporary work in the two years preceding the outcome measurement. These workers report 1.803 (95% CI 0.552; 3.055) additional depressive symptoms from having experienced this work status (than if they had not been exposed). Moreover, this difference is both statistically and substantively significant, as it represents a 50% increase from the average level of depressive symptoms in this population.

Recent decades have seen a tremendous increase in the complexity of work arrangements, through job sharing, flexible hours, career breaks, compressed work weeks, shift work, reduced job security, and part-time, contract and temporary work (Patterson, 2001; Rogers, 2006). While a fraction of workers engage in these non-standard arrangements out of choice, many others are in these positions often out of necessity, not preference, as the jobs themselves are not expected to last (Barker & Christensen, 1998; Cohany, Hipple, Nardone, Polivka, & Stewart, 1998; Polivka, 1996). In this study, we focus on one specific group of

workers that arguably most embodies nonstandard employment, namely temporary workers, and examine the consequences of this type of employment on depressive symptoms.

Data from the U.S. Bureau of Labor Statistics indicated that in 1999, 5.6 million workers held a position they believed to be temporary (Hipple, 2001). Without an explicit or implicit contract for long-term, stable employment, temporary workers have referred to themselves as 'disposable', 'second class', 'commodities' and 'tremendously underutilized' (Vosko, 2000). In addition, work conditions often include reduced wages, status, security and benefits, such as pension, health insurance and sick leave (Cohany, et al., 1998; Hadden, Muntaner, Benach, Gimeno, & Benavides, 2007; Polivka, 1996). Despite the obvious deleterious nature of these work conditions, evidence is only beginning to emerge regarding the effects of temporary work on mental health (M. Virtanen, et al., 2005; P. Virtanen, Liukkonen, Vahtera, Kivimäki, & Koskenvuo, 2003).

A meta-analysis found greater psychological morbidity among temporary employees relative to permanent employees (OR=1.25, 95%CI: 1.14; 1.38) (M. Virtanen, et al., 2005). This modest relationship was accompanied by much heterogeneity between studies, according to exposure definitions, the variety of mental health outcomes and contextual factors ($Q=32.91$, $P=0.0012$) (ibid.). Most studies in the systematic review were also cross-sectional, and thus, it was not possible to conclude from this meta-analysis that temporary work causally affects mental health, as the association uncovered could have been due to selection of those with poor mental health into more temporary work (Kawachi, 2008; Kim, Kim, Park, & Kawachi, 2008). Indeed, transitions between fixed term and permanent employment have been associated with mental health selection such that, relative to distressed cases, the odds of becoming permanently employed were 1.80 higher among non-distressed workers (95%CI=1.01–3.20), with borderline statistical significance ($p=0.048$) (M. Virtanen, Kivimäki, Elovainio, & Vahtera, 2002).

However, health selection and social causation are not mutually exclusive processes, and may even vary by psychiatric outcome (Dohrenwend, et al., 1992; Ritscher, Warner, Johnson, & Dohrenwend, 2001). While this has not been examined with temporary workers per se, other forms of nonstandard employment have been examined for evidence of this interplay between health selection and social causation. In fact, a study that assessed the concurrent effects of underemployment, by income, on CES-D depressive symptom score, found a statistically significant association, despite controlling for health selection into insecure employment ($p<0.01$) (Friedland & Price, 2003).

This study aims to estimate the effect of being a temporary worker on depressive symptom severity. Accounting for the possibility of mental health selection into temporary work through propensity score analysis, we isolate the direct effects of temporary work on depressive symptoms with varying lags of time since exposure.

Methods

Data

We use prospective data from the U.S. National Longitudinal Survey of Youth 1979 (NLSY79), which has followed, longitudinally, from 1979 to the present, a nationally representative cohort of American men and women between 14 and 22 years of age in 1979. Details on the survey methodology have been described elsewhere (Zagorsky & White, 1999). Our sample is based on longitudinal records from this dataset collected biennially between 1992 and 2002. Data are publicly available online and were recoded for analysis as described below. Ethics approval was granted for this study by McGill University's Faculty of Medicine Review Ethics Board.

Design and Samples—Three propensity score models were estimated, to capture the effect of different time lags (immediately following exposure, and 2 and 4 years post exposure) between the period of exposure to the outcome. The timing of the measurement of variables is schematically represented in Table 1. As noted in the legend, P indicates the predictors used in the predicting equation of temporary work, which, except for race and gender (measured in 1979), typically refer to the current status at the time of survey (age, employment status, CES-D, marital status), or to the past calendar year (education, household income, hours worked), while the dots in grey indicate a potential exposure to temporary work, and O refers to the outcome measurement.

Model 1 examines the impact of having experienced at least one temporary job between 1992 and 1994 (in the two-year period since the last interview) on depressive symptoms in 1994. Models 2 and 3 reflect constraints in the data imposed by the fact that the outcome was assessed *either* in 1998, 2000, or 2002, whenever respondents first turned 40 years old. As a result of the staggered data structure of these outcomes, we constructed two cohort sequential, cross-lagged designs. Model 2 thus estimates the effect of temporary work on depressive symptoms two years after the last possible exposure, while Model 3 looks at a four-year lag.

Sample sizes presented in the table reflect the full models with all missing values deleted, and excluding respondents who reported being limited in the amount or type of work they could perform because they were pregnant. Analyses not shown here were performed with these respondents in the models, but did not differ substantively. However, we felt that these workers' condition introduced enough heterogeneity in their labor force experience that they should be excluded from our models. Finally, the number of respondents having experience at least one temporary job in the relevant period of observation is also reported.

Measures

Temporary Work—In 1994, 1995, and 1998, questions based on the Supplement on Contingent Work of the Current Population Survey (CPS) were fielded, and detailed information was collected on up to five jobs in the period since last interview. Temporary work exposure was based on self-reported answers to the following question: “[Are/Were] you a regular employee at this job, do you consider yourself a temp worker, consultant or

contractor, or are you an employee of a contractor? By “THIS JOB”, we mean the one you are actually doing the work for - NOT a temporary agency, or a consulting or contracting firm that may have sent you there at first.” Respondents were then offered the following set of answers:

1. Regular employee
2. Temp worker, sent by a temporary agency
3. Temp worker, hired directly by the company
4. Consultant
5. Contractor
6. Employee of a contractor
7. Other (specify)

We selected responses to categories 2 and 3 as indicative of temporary worker status. As workers could report on this status for up to five jobs in the period since the last interview (roughly two years), we aggregated the responses for any given survey year to produce a binary variable indicating any exposure over the past two years to a temporary job. Models (not shown here) were also estimated using only the job in the week preceding the survey, and the results were substantively similar.

Depressive symptoms—These were assessed using a seven-item subscale of the Centre for Epidemiologic Studies Depression Scale (CES-D), in 1992, 1994, 1998, 2000 and 2002. The CES-D (Centre for Epidemiologic Studies Depression Scale) was developed for both general and clinical populations, has been validated across subpopulations, and measures current depressive symptom severity (Radloff, 1977). While the full CES-D was measured in 1992, starting in 1994, only a modified seven-item scale was used (validated by Mirowsky and Ross 1989), and so we restricted the 1992 measure to that same scale. Furthermore, starting in 1998, only those respondents who were 40 years and over were asked an expanded health questionnaire, of which the CES-D scale was part. This has led to the staggered design described above. CES-D measures in 1992 were used as the baseline level of mental health in most models. These seven item scales were summed, and had good internal consistency (Cronbach $\alpha > 0.70$). This variable ranges from 0 to 21, with higher scores corresponding to a greater frequency of more depressive symptoms, or higher severity.

Predictors of temporary work—*Gender*, *race* and *marital status* were coded as dichotomous variables, with the indicator categories of male=1, white=1 and married=1. *Educational attainment* was measured as the highest grade completed as of May 1st of the survey year. *Age* is measured in years at the time of the survey. *Employment status* indicates whether individuals were employed (indicator category) or not during the week preceding the survey. The employed category included those employed as well as those employed but absent from their job. The non-employed group includes varying degrees of attachment to the labour force: unemployed (on layoff), unemployed (looking for work), out of the labour force (retired, disabled or other) and a small number in the active military forces. It is

dichotomized, with non-employed individuals as the reference group. This measure changed over the years such that we were not always able to distinguish the unemployed from those out of the labor force, so we opted for this broader measure that is less specific, but allows us to use all years. *Household income* was measured by natural log (plus a small constant) of the annual net household income (in thousands of dollars, and adjusted for purchasing power in the year 2000). *Hours worked* reflect the actual number of hours worked in the preceding calendar year.

Propensity score analyses

We follow the strategy described by Oakes and Johnson (2006) and use `psmatch2` in Stata 10 for our analyses (Leuven & Sianesi, 2003). As mentioned in the introduction, temporary workers may differ substantially from permanent workers in a number of ways, one of them being that they may be selected into these positions as a result of pre-existing poor mental health. This situation can pose a problem with methods that simply control for confounders, as they assume overlapping distributions of these confounders for both groups of temporary and permanent workers. A way to circumvent this problem would be to match each temporary worker with a permanent worker on each characteristic thought to affect both employment status and depressive symptoms.

However, this matching would quickly get unwieldy, and may even prove impossible when several confounders are thought to play a role. An alternative matching method is therefore to use a propensity score, which relies on a prediction equation of the likelihood of being a temporary (vs. permanent) worker to match cases. We therefore address this concern by matching temporary workers (the “exposed” group) to permanent workers (the “unexposed” group) on the basis of their propensity score, as predicted by the following variables: age, gender, race, baseline depressive symptoms, education, marital status, household income, and hours worked in the past calendar year. These variables were selected on theoretical grounds, as potential predictors of both temporary worker status and depressive symptoms level, and were kept in the prediction equation even if they proved to be nonsignificant predictors of temporary work status (Oakes and Johnson 2006).

Propensity scores were estimated through nearest-neighbour within caliper matching, setting the caliper at 0.01. As recommended by Oakes and Johnson (2006), we performed sensitivity analyses by setting the caliper at different levels, but the results (not shown) did not change. Next, we assessed covariates balance between the exposed and unexposed groups using both standardized differences (as obtained in Equation 1) and the percent bias reduction (from Equation 2).

$$\text{standardized difference} = \frac{100(\bar{x}_{\text{exposed}} - \bar{x}_{\text{unexposed}})}{\sqrt{\frac{s_{\text{exposed}}^2 + s_{\text{unexposed}}^2}{2}}} \quad (1)$$

$$\% \text{ Bias Reduction} = 1 - \left(\frac{|\text{Standardized Difference}_{\text{matched}}|}{|\text{Standardized Difference}_{\text{unmatched}}|} \right) \quad (2)$$

We expect to see improvement in the covariate balance after matching, as indicated by standardized differences of less than 10%, and a positive bias reduction.

Once the two samples are matched, we can calculate the average treatment effect for the treated (ATT). With respect to our study, the ATT is a comparison of the average depressive symptoms among workers in temporary jobs (exposed) with what the average would have been had these workers not been temporary (unexposed). Again, following Oakes and Johnson (2006), we may formalize this as in Equation 3,

$$\widehat{ATT} = \frac{\sum y_{ei} - y_{ui}^*}{n_e} = \frac{\sum \Delta_e}{n_e} = \bar{\Delta}_e \quad (3)$$

Where y_{ei} is the outcome of an exposed (e) subject (i); y_{ui}^* is the outcome of the unexposed matched subject; n_i is the sample size of the pair-matches; and Δ_e is the pair-matched difference in outcomes. The extent to which our estimate of ATT approximates the true ATT depends on the exchangeability of our cases, as measured by the criteria elaborated in Equations (1) and (2). We use bootstrapping (1000 repetitions) to create a sampling distributions of ATTs from which we can calculate the standard error around this estimate.

Results

Model 1

Table 2 presents the results of the matching process for Model 1. For each predictor, the averages among temporary workers and non-temporary workers are presented, before and after matching. As the standardized differences in the fifth column indicate, most of the differences are well below 10%, with race and age just above that cut-off point. All the predictors saw an improvement in the bias as a consequence of matching, as indicated by the uniformly positive column of % of reduction in bias. Figure 1 reinforces this observation, as it shows that only a small number of temporary workers were not matched (off-support).

Table 3 thus presents the results of the matching, which indicate that temporary workers report 1.803 (95% CI 0.552; 3.055) additional depressive symptoms from having experienced this work status (than if they had not been exposed). Moreover, this difference is both statistically and substantively significant, as it represents a 50% increase from the average level of depressive symptoms in this population.

Model 2

In contrast to Model 1, the evaluation of the covariate imbalance for Model 2 presented in Table 4 indicates that exposed and unexposed individuals are not interchangeable, as the balance gets even worse after matching. Age is particularly problematic in this regard, as

indicated by a negative value for the percentage reduction in bias, and a large standard difference twice the size of the accepted cut-off. Similarly, though the balance is improved with matching for these variables, education and marital status still exhibit unbalanced covariates. Figure 2 underscores this point, as it shows the substantial amount of unmatched temporary workers. Because they cannot be trusted to represent the true ATT, the results of the matching will therefore not be presented for Model 2.

Model 3

Model 3 suffers from some of the same limitations as Model 2, but to a lesser extent: the evaluation of the covariate imbalance presented in Table 5 shows elevated standardized differences for race and CES-D in 1992, but Figure 3 actually shows perfect matching of temporary workers. In light of these results, the ATT will still be presented, but should be interpreted with caution.

Table 6 shows a point estimate that suggests a higher level of depressive symptoms among temporary workers of 0.836 (95% CI -0.442; 2.114). However, this estimate is not shown to be statistically significantly different from 0.

Discussion

This study finds an important increase in depression symptom severity associated with exposure to temporary work at any point in time in the two preceding years (including concurrently). Most importantly, these results were obtained through propensity score matching on a number of covariates affecting the likelihood of temporary work status, including, most notably, prior depressive symptoms. Moreover, the exposed group was limited to workers who reported temporary employment status, and did not include those with perceived job insecurity or alternative contractual arrangements (Bernhard-Oettel, Sverke, & De Witte, 2005; P Virtanen, Vahtera, Kivimäki, Pentti, & Ferrie, 2002). These findings are consistent with a previous meta-analysis, but remain a novel contribution using US data (M. Virtanen, et al., 2005). Models with different lag periods of two and four years between the most recent possible exposure and the outcome were not conclusive however, as the models' assumptions of covariate balance were not met.

Our results contributed to the literature by examining these processes among a US population, with longitudinal controls for factors that may predict entry into temporary work. Yet, other selective factors that may have an influence on the likelihood of being a temporary worker were not included in the models here, and could consist of introducing confounding, such as region and employer (demand-side), variables (Belman & Golden, 2000; Coleman & McLaughlin). Region may also be an indicator for mobility, or opportunity for employment - in this case, exposure to temporary employment. Moreover, these analyses did not consider the industry, which may have provided additional predictive power (but would have proved difficult to balance due to the large number of empty cells it would have created). Dimensions of inadequate employment not addressed here include: skill mismatch; status discord; inadequate hours, or involuntary part-time work, or the work conditions of jobs performed by temporary workers (Friedland & Price, 2003).

The fact that the lagged models did not meet the propensity scores assumptions could be due to the small number of workers in temporary positions, though the very significant results of Model 1, achieved with similar numbers of respondents, would tend to discredit this hypothesis. Another likely explanation is that temporary work is for the most part a transitory status in this population: among the 940 respondents having experienced at least one temporary job from 1992 to 1998, 80% experienced only one such job, 18% two, and 2% three temporary jobs. Therefore, the lagged models assume that temporary work has long-lasting effects on mental health, while this may not be the case in the face of changing, and potentially improving, situations. Thus, while the “scarring” effects of a similarly insecure situation such as job displacement have been found in Sweden (Eliasson & Storrie, 2006), we did not find evidence of this effect for temporary work in the US.

An important limitation of our analyses is that it is assumed here that temporary workers would prefer a permanent position if it were available. Yet, a study of female temps in New Zealand reported the positive aspects of being in a non-committal work relationship: as a lifestyle choice, an escape from an unsatisfactory conventional job or a self-defined transitional period (Casey & Alach, 2004). In this sample, it is possible that the composition of temporary jobs and expectations of temporary workers was not representative of all sectors. As a result, despite the specific definition of temporary work exposure, a mixture of low-income, skill mismatched, status discordant work and preferred work conditions were likely found.

Previous studies may shed some light on the possible mechanisms through which temporary work affects mental health, such as the insecurity that characterizes these positions. Indeed, previous studies have argued that there may be a need among employees for the availability, or perception of the availability, of a long-term, continuing relationship with one’s employer, based on the need for job and/or income security, as a crucial factor in determining emotional well-being and depression risk (Barnes, 1980). In particular, our results are comparable with the findings of Ferrie and colleagues, who estimated the effect of chronic job insecurity over a 2.5 year period among Whitehall II participants (2002), and found that workers who were in chronically insecure positions had higher mean depression scores (1.84, 95% CI: 1.28; 2.40).

Policy implications on the demand-side, suggest a need for increasing the awareness of employers of the long-term or global health impact of relying on a temporary work force, to meet current demands, and in planning future employment needs. From a governmental perspective, temporary workers could be protected by organizational structures involving certification of occupational skills, as in Germany, where an institutionalized transition pathway, linked to a strong occupational segment of the workforce, appears to be more resistant to the increasing flexibility in work patterns in the organization of labour (Heinz, 2003).

In conclusion, our results suggest that the proximate effect of temporary work appears to be significantly associated with greater depressive symptoms. This effect is not trivial, as it constitutes an increase by more than 50% in depressive symptoms compared to the average of the population. Moreover, it is likely that the health effects of job insecurity are mediated

by the availability of other opportunities in the labour market, which makes these results all the more pressing in these times of economic crisis (Polanyi, Tompa, & J., 2004). Finally, our results suggest that the expectation that temporary workforces increase productivity may be perversely affected by increases in depressive symptoms: indeed, the WHO found that, in many developed countries, anywhere between a third to almost half of the cases of absenteeism from work are due to mental health problems (World Health Organization, 2003), and even when employees are present at work, productivity at work may be impaired (World Health Organization, 2005). Our results suggest then that it could be profitable for both employers and employees to consider employee mental health effects when responding to labour demands in the context of an increasingly competitive globalized economy.

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Fig. 1.
Overlap on propensity scores by temporary or non-temporary work status. Model 1

Fig. 2.
Overlap in propensity scores by temporary or non-temporary work status, Model 2.

Fig. 3.
Overlap in propensity scores by temporary or non-temporary work status, Model 1

Table 1

Time period of measurement of variables and sample sizes for each model

	1992	1994	1996	1998	2000	2002	N	Temp Wrkrs
Model 1	P*****	O					3577	133
Model 2							2186	99
C1	P (CES-D)	P*****		O				
C2	P (CES-D)	P (CES-D)	P*****		O			
Model 3							3177	116
C1	P*****			O				
C2	P (CES-D)	P*****			O			
C3	P (CES-D)		P*****			O		

Note: P refers to predictors of the propensity score of temporary work, ***** indicate possible periods of exposure to temporary work and O indicate the measurement of outcomes; N is the total sample used in the analyses, and Temp Workers is the number of temporary workers that were matched (on support).

Table 2

Estimates for the matching on the propensity score for Model 1

Predictor variables	Sample	Mean		Std Diff	% Reduct bias
		Temp	Non-Temp		
Male	Unmatched	0.451	0.511	-11.9	36.5
	Matched	0.455	0.417	7.6	
White	Unmatched	0.406	0.561	-31.3	60.8
	Matched	0.409	0.348	12.3	
Age 1992	Unmatched	31.902	32.211	-19.2	33.8
	Matched	31.917	31.712	12.7	
Education 1992	Unmatched	12.466	13.253	-34.2	83.6
	Matched	12.561	12.689	-5.6	
Married 1992	Unmatched	0.519	0.608	-18.0	83.0
	Matched	0.523	0.508	3.1	
CES-D 1992	Unmatched	5.203	3.776	34.4	83.5
	Matched	5.182	4.947	5.7	
Log(HH inc) 1991	Unmatched	1.346	1.701	-57.2	88.7
	Matched	1.354	1.394	-6.5	
Hours worked 1991	Unmatched	1327.400	1977.800	-73.4	99.8
	Matched	1337.400	1339.000	-0.2	
Employment stat. 92	Unmatched	0.639	0.895	-63.2	100
	Matched	0.644	0.644	0	

Table 3

ATT estimate for Model 1

	ATT	Std. Err.	z	P > z	[95% Conf. interval]
Temporary work 1992–1994	1.803	0.639	2.820	0.005	0.552
					3.055

Note: Std. Err. obtained by bootstrapping (1000 repetitions).

Table 4

Estimates for the matching on the propensity score for Model 1

Predictor variables	Sample	Mean		Std Diff	% Reduct bias
		Temp	Non-Temp		
Male	Unmatched	0.404	0.508	-20.9	89.1
	Matched	0.420	0.432	-2.3	
White	Unmatched	0.394	0.555	-32.7	100
	Matched	0.443	0.443	0	
Age	Unmatched	36.394	36.414	28.7	68.9
	Matched	36.420	36.568	8.9	
Education	Unmatched	12.394	13.312	-37.4	56.7
	Matched	12.625	12.227	16.2	
Married	Unmatched	0.485	0.611	-25.6	28.2
	Matched	0.511	0.602	-18.4	
CES-D	Unmatched	4.960	3.754	28.7	68.9
	Matched	4.602	4.227	8.9	
Log(HH inc)	Unmatched	1.293	1.731	-70.1	88.6
	Matched	1.370	1.420	-8	
Hours worked	Unmatched	1322.400	1991.800	-69.3	93.6
	Matched	1481.300	1438.200	4.5	
Employment status	Unmatched	0.737	0.891	-40.2	70.5
	Matched	0.807	0.852	-11.9	

Note: please see Table 1 for the time period of measurement predictors

Table 5

Estimates for the matching on the propensity score for Model 3

Predictor variables	Sample	Mean		Std Diff	% reduct bias
		Temp	Non-Temp		
Male	Unmatched	0.414	0.529	-23.2	77.6
	Matched	0.414	0.388	5.2	
White	Unmatched	0.397	0.565	-34	38.4
	Matched	0.397	0.293	21	
Age	Unmatched	34.491	34.489	0.3	-271
	Matched	34.491	34.483	1.3	
Education	Unmatched	12.698	13.381	-29.3	78.5
	Matched	12.698	12.552	6.3	
Married	Unmatched	0.500	0.615	-23.2	70
	Matched	0.500	0.534	-7	
CES-D 1992	Unmatched	4.871	3.659	30.8	48.8
	Matched	4.871	4.250	15.8	
Log(HH inc)	Unmatched	1.402	1.754	-60.2	98.9
	Matched	1.402	1.405	-0.6	
Hours worked	Unmatched	1571.400	2044.200	-51.9	83.4
	Matched	1571.400	1649.800	-8.6	
Employment status	Unmatched	0.707	0.913	-54.3	95.8
	Matched	0.707	0.716	-2.3	

Note: please see Table 1 for the time period of measurement of predictors.

Table 6

ATT estimate for Model 3

	ATT	Std. Err.	z	P > z	[95% Conf. Interval]
Temporary work	0.836	0.652	1.280	0.200	-0.442 2.114

Note: Std. Err. Obtained by bootstrapping (1000 repetitions); please see Table 1 for the time period of measurement of temporary work exposure.