Does task-specific human capital exist and is it transferable? Evidence from displaced workers in the PSID

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DRAFT

Abstract

Task-specific human capital exists and is (partially) transferable. I characterize task-specific human capital as the abilities required to complete tasks performed on the job and then I define a task knowledge space where distances between jobs can be measured. To show the transferability of task-specific human capital, I use the tools of program evaluation to explore the effect of losing varying amount of task-specific human capital on displaced workers from the PSID. Not only do displaced workers who switch tasks post-displacement see substantial long-run drops in earnings, those losses in earnings are larger the more different their post-displacement job is from their pre-displacement job, with respect to the tasks performed in the job. Using common estimates of discount rates and amortizing, this loss amounts to a lifetime cost of around 7 log points of earnings per year for workers that move the median distance in task space post-displacement (relative to those displaced workers that do not switch tasks).

1 Introduction

A literature initiated by Neal [1995] and Parent [2000] has found there is non-firm-specific component to human capital. Workers take much of their skills with them from firm to firm. This early literature identified this domain-specific human capital as industry-specific. They found that those displaced workers — known to lose substantial fractions of their earnings upon losing their job (see Jacobson et al. [1993] or Stevens [1997]) — who switch industries see much more dramatic decreases in their earnings than those displaced workers who don't switch industries. After better data became available, a later literature found while industry tenure was correlated with human capital, human capital was not entirely industry-specific. Kambourov and Manovskii [2007] found human capital is occupation-specific and recent papers by Gathmann and Schonberg [2010] and Poletaev and Robinson [2008] have found a task-specific component to human capital. These papers go further and attempt to directly estimate the returns to occupation- and task-specific human capital, respectively, in a standard Mincerian framework. Its clear from the literature that a non-general component of human capital exists, is domain specific and is important. Its less clear to what extent domain specific human capital is mobile. How well does it move with workers as they navigate the labor market throughout their careers?

The first step in my analysis is to define a task space in which jobs are located. The vector connecting two jobs in this space defines a distance between them and a direction that defines a qualitative difference between them. In general, jobs that are far apart in the space have much different tasks associated with them and jobs that are closer in the space are more similar. I construct this space by using the occupational abilities ratings found in the O*Net database (see Willison et al. [2008]). I then assign distance measures to individuals by matching their occupations to this database.

The second step in the analysis uses the Panel Survey of Income Dynamics (PSID) to show the existence and extent of transferable domain specific human capital. Because this data set is a panel, I can control for individual-specific unobserved factors and I can look at the effect of various treatments (e.g. switching the tasks performed in a job) over a long period of time. This long-run perspective is important because the simple model of transferable human capital that I develop in the next section is agnostic on the timing of the effects of treatment.

In an ideal experiment workers are randomly treated by being displaced from their jobs and then randomly assigned positions in the task space. The analysis in this paper deviates from that ideal experiment in two ways. First, displacements are not random. Lower quality workers are more likely to be displaced. The literature has dealt with this problem extensively, but the internal validity of my results do not hinge on this issue because in my analysis both the treated and the untreated have been displaced. However, it may be the case that because I am estimating the effects of treatment on a group of workers that have been displaced, my results are not indicative of the effect of treatment on those workers that do not get displaced. I will have more to say about this and I attempt to prove external validity in a number of ways. Second, placement in the task space post-displacement is not random. To deal with this problem I use a task isolation score (think geographical isolation but in the task space) in an instrumental variable approach.

Two results emerge from my analysis. Job task-specific human capital exists and the more different two jobs are with respect to the tasks performed on the job, the harder it is to transfer that knowledge when workers switch between them. In other words, task-specific human capital is transferable.

After setting up a simple model of transferable human capital in section 1, I describe the data in section 2. After these preliminaries, I explore the effects of various treatments assuming exogenous displacements and exogenous post-displacement outcomes in section 3. In section 4, I discuss the implications of the assumption of exogenous displacements in the context of this analysis and then in section 5 I propose and implement an IV strategy to attempt to get around the assumption of exogenous post-displacement outcomes. Robustness of the results is checked in a number of ways in section 6 and then I conclude in section 7.

2 Model

Workers have three types of human capital: general, firm-specific and transferable. General human capital — often made synonymous with education in popular discourse — is perfectly mobile and stays with the worker from job to job. Firm-specific human capital is lost when workers change employers. Transferable human capital is job task knowledge that the worker can, at least partially, take with them from job to job. It is “transferable” because various jobs share many similar tasks and some of the task-specific knowledge gained at one job can be easily transferred to another job. If a worker only has to make small adjustments in the bundle of tasks he performs when he switches jobs (e.g. switching from being a bus driver to being a taxi driver), its unlikely he loses much human capital in the switch. If, on the other hand, he has to significantly change the bundle of tasks he performs after a job switch (e.g. switching from a bus driver to a medical doctor), its likely he does lose a significant fraction of his human capital.

Suppose there are only two types of job tasks, a (for analytical) and m (for manual). Each job requires a discrete binary amount of each task, none or some. Thus, the task space has four points: analytical and manual jobs, non-analytical but manual jobs, analytical but non-manual jobs and jobs with neither sort of task[1]. Building from Neal [1995], then, the following is a simple and stylized model of human capital and wages:

$$w^1_{i,j,s,t} = \eta E_{i,s} + \kappa_a J_{a,i,s} + \kappa_m J_{m,i,s} + \gamma F_{i,s} + X_{i,t}\beta + \epsilon^1_{i,j,t} \tag{1}$$

$$w^2_{i,j,s,t} = \eta E_{i,s} + \kappa_a J_{a,i,s} + \kappa_m J_{m,i,s} + X_{i,t}\beta + \epsilon^2_{i,j,t} \tag{2}$$

$$w^3_{i,j,s,t} = \eta E_{i,s} + \kappa_m J_{m,i,s} + X_{i,t}\beta + \epsilon^3_{i,j,t} \tag{3}$$

$$w^4_{i,j,s,t} = \eta E_{i,s} + \kappa_a J_{a,i,s} + X_{i,t}\beta + \epsilon^4_{i,j,t} \tag{4}$$

$$w^5_{i,j,s,t} = \eta E_{i,s} + X_{i,t}\beta + \epsilon^5_{i,j,t} \tag{5}$$

The left hand side are potential wages. On the right hand side are at-displacement human capital variables (E_s, J_s and F_s), non-at-displacement human capital variables and other factors effecting wages (X) and an error term, $\epsilon_{i,j,t}$, for individual i at time t in task space location $j \in \{1, 2, 3, 4\}$. The time of displacement is s. The worker's non-specific human capital is E and F is the worker's firm-specific human capital.

A worker's wages would be w^1 if the worker was not displaced and they would be w^2 if the worker was displaced at time s but continued in a job in the same location in task space after the

[1] An economy with jobs that have no tasks seems unrealistic, but in this stylistic model, these would correspond to low-skill jobs in the actual economy.

displacement. If the worker changes location in the task space, potential wages can be any of w^3, w^4 or w^5 depending on where in the task space the worker ends up after displacement.

The *ideal experiment* would be to randomly displace workers and then to randomly assign displaced workers into job tasks. In this experiment, if we observed significant differences in wages of job tasks changers and job task stayers[2], we would conclude domain-specific human capital exists. Similarly, if among the switchers we observed significant differences in wages of those that moved further in the task space, we would conclude that domain-specific human capital is transferable [3].

As Neal notes, he is not able to run the ideal experiment. His data, the Displaced Worker Survey (DWS), make a difference in differences approach impossible. The DWS asks about *past* displacements. This means many years may have transpired between the loss of the job and the survey. Given retrospective demographic questions are not asked, we can't know what other factors may be driving changes in wages since the displacement. For example, the DWS does not ask about subsequent displacements or the number of jobs in intervening years. Stevens [1997] demonstrated subsequent displacements explain much of the persistent effects of displacement (and presumably some of those long term impacts have their effects in the long period between the displacement asked about on the DWS and the administration of the survey itself).

Besides data issues, its problematic for a causal interpretation of a straightforward analysis to assume the post-displacement choices of workers are exogenous. The ideal experiment requires random placement of workers in job tasks post-displacement. If workers or firms are aware of the workers' non-firm-specific human capital, wage offers after the job loss will be higher for the job tasks for which the worker has more knowledge. Assuming workers respond to incentives, we will observe more productive workers staying in their job tasks. Consider the relationship between the observable human capital variables and the unobserved job task-worker match quality [4]:

$$\epsilon_{i,j,f,t} = \psi_i + \rho_t + \tau_{i,j} + \zeta_{i,f} + \xi_{i,j,f} \tag{6}$$

Whereψ_i and ρ_t are individual and time specific fixed characteristics, $\tau_{i,j}$ is the quality of the

[2]For example, if we were to test the hypothesis $E[w^5 - w^2|X_t] < 0$.
[3]For example, a test of this hypothesis $E[w^5 - w^2|X_t] - E[w^3 - w^2|X_t] < 0$.
[4]The firm subscript, f, was omitted from the above.

job task match for the individual, $\zeta_{i,f}$ is the firm match quality and $\xi_{i,j,f}$ is the cross firm-task match quality. The problem is the job task match, $\tau_{i,j}$, is correlated with job task human capital, J. The better match a worker is for the set of tasks he performs the more he will know about how to perform those tasks. One mechanism that might be driving this correlation is that better task matched workers will have been doing those tasks for a longer time and so have more knowledge about doing them.

If human capital is measured by tenure variables, this problem can be dealt with by using an IV strategy developed by Altonji and Shakotko [1987] and used by Parent [2000]and Kambourov and Manovskii [2007]. They use the difference between the individual occupation tenure and the mean of tenure in that occupation as an instrument. That IV strategy does not deal with another problem, though. As pointed out by Pavan [2009], there are cross match-human capital correlations as well. The error term will be correlated with firm tenure through $\xi_{i,j,f}$, for example.

Instead, my analysis exploits the panel nature of the data and observes the effects of a displacement and treatment around the time of the displacement. Also, my analysis is agnostic on the form of human capital (i.e. my main focus is not the return to tenure variables). Because pre-displacement human capital and match values are fixed for the individual, the problems addressed by the Altonji and Shakotko [1987] IV strategy do not arise in the panel setting. Also, some of the concerns raised by Pavan [2009] are addressed because the firm-task cross term is eliminated during the displacement. However, there may be selection effects such that $\tau_{i,j}$ is correlated with task human capital.

I assume that the coefficients on the human capital variables are positive. This assumption implies displaced workers will see a drop in their wages due to the loss of their firm specific human capital. It also implies w^5 is less than all the other potential wages and $w^3$and $w^4$are each less than w^2. The theory cannot predict a priori differences between w^3 and w^4, however. In any case, there is a sense in which some pairs of jobs are further from each other than other pairs in the task space. The prediction of the model, then, is displaced workers that move further in the task space will see a larger hit to wages.

3 Data

Two data sources are used in this analysis, the PSID and the O*Net Abilities database. The Panel Study of Income Dynamics was an annual survey from 1969 to 1995 and has been biannual since. An attempt is made to resurvey the same individuals in each wave. 3.4% of the respondents have been interviewed in every year of the survey, 36.4% of respondents have answered survey questions in at least 10 survey waves and 87.0% of respondents have answered the survey in more than one year. The survey sample has expanded because as respondents' children grew to be adults with independent households the survey administrators attempted to add the new households to the sample. The sample has also been expanded and contracted over the years with various new initiatives and budget cuts. In 1969, 4,802 heads of household were interviewed and in 2001, 7,574 heads of household were interviewed. In the early 90's, over ten thousand head of households were interviewed.

The survey asks questions about several members of the household, but most data is collected about and via the head of household. Head of household is defined to make the survey results heavily skewed towards prime age working men.

The data can be downloaded from the PSID website [5], but it comes in a very inconvenient format. There is one "individual" file for all years that ostensibly contains fixed observable individual-level data. Each wave's data are contained in separate year files. Each file contains hundreds of variables. While most of these variables contain data on questions asked in previous and subsequent waves, these variables are not connected systematically. Its tedious putting the data in "panel format" by hand, so I have created a user-configurable script to process the raw files. This script and its documentation is available upon request.

The analysis sample is constructed as follows:

- Sample is limited to 1968 to 1999 male heads of household
- Only looking at first displacements

[5] http://psidonline.isr.umich.edu/

- Removed individuals with reported displacements in 1968 (i.e. those that said their most previous job in the last 10 years was lost because of being displaced)
- Include all individuals that entered the sample
- Exclude records with zero earnings or that don't have task-measurable occupations

Heads of household were used to simplify data scrubbing (non-heads have variables in separate columns). The analysis sample uses only men to control for the well-known differences between genders in human capital. However, my results do not seem sensitive to gender as the analysis run on women-only yield qualitatively similar results.

Displacements are defined as narrowly as possible in the PSID. Respondents are asked if they have different employers then they had in the previous survey wave and if so, why. The typical wording of the follow-up question is "what happened with that employer–did the company go out of business, were you laid off, did you quit, or what?". For the purposes of this paper, the respondent is considered having been displaced in the previous year if they answer "company folded/changed hands/moved out of town" or "employer died/went out of business". Before doing the study I expected actual displaced workers to have different results than workers who choose to switch jobs. The data do not seem to support this prior belief. This and its implication are discussed in the "robustness checks" section below.

Why first displacements? First, it simplifies analysis. Second, there is evidence (see e.g. Stevens [1997]) the effects of subsequent displacements are different than the effects of the first displacement (or workers who get multiply displaced are different from workers that are only displaced once). Previous displacements, but no other factors observable by the econometrician, can predict future displacements. Assuming the same variables are the only factors observable by workers, only the first displacement is unforecastable by the worker. Ignoring the possibility that workers have private information about themselves, but allowing them private information about their firm, we can only assume displacements are exogenous if information about the closure or relocation of the firm has yet to reach the worker. In the analysis in this paper, I assume the first displacement is exogenous two years prior to the displacement. This assumption suggests, for example, that two years before the first displacement, the will-be displaced worker has as much likelihood of being displaced as all other workers. These assumptions about information (particularly the lack of private information) are problematic for the external validity of displacement studies [6], but are common in the literature.

The O*Net abilities measures (Willison et al. [2008]) are meant to measure "enduring attributes of the individual [in the occupation] that influence performance" which is to say they are meant to describe attributes of occupations that remain stable over time. Each occupation was originally scored by occupational analysts on 52 types of abilities. These abilities are very detailed (e.g. "*The ability to coordinate two or more limbs (for example, two arms, two legs, or one leg and one arm) while sitting, standing, or lying down. It does not involve performing the activities while the whole body is in motion.*"). These ability scores make up the O*Net measured task space. Occupations are points in this space. The distance between two occupations in this space is called their TASK DISTANCE. This distance is measured using the Mahalanobis distance (Mahalanobis [1936]) which is weighted Euclidean distance where the weights are determined by the correlation structure of O*Net measured task space. This weighting scheme is necessary because many of the O*Net abilities are highly correlated with each other, e.g. occupations requiring high deductive ability often require high inductive ability. These correlations would artificially increase the distance between otherwise similar occupations because the correlated dimensions are in effect being double counted in an unweighted distance measure [7].

The TASK ISOLATION score for an occupation is the average task distance from the occupation to all other occupations in the economy weighted by the observed supply for each occupation. This is the expected task distance a worker would travel if they were randomly assigned a new occupation. Figure 1 shows both the actual distribution of task distance between old and new occupations when workers change occupations and the expected distribution if they just randomly picked new occupations.

[6] For example, workers that know they are low quality might self select into occupations and industries that are more likely to experience displacements.

[7] Also, this distance measure, unlike the angular separation for example, is able to distinguish two occupations that require the same proportions of tasks, but differ in magnitudes. For example, suppose both occupation A and occupation B consist of equal measures of analytical tasks and physical tasks, but B requires twice as much of both. Even though both occupations lie on a ray out of the origin of the task space, the Mahalanobis distance would be positive whereas the angular separation would be zero. The latter distance measure systematically underestimates distances between occupations.

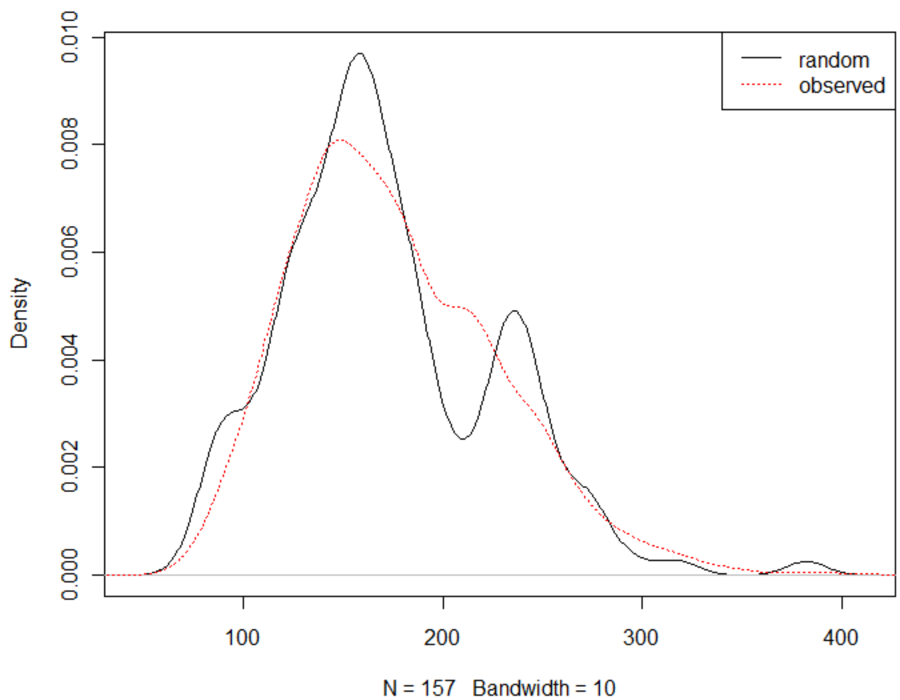

Figure 1: The density of the task distance between old and new occupations for occupation switchers. The black solid line is the expected density if new occupations were assigned randomly. The red dotted line is the actual density of occupation movers.

Task tenure measures the relationship between knowledge of job tasks and the time spent working on those job tasks. For job task i, the O*Net database gives a maximum value for knowledge of i, $\hat{T}_i^o$, for a worker in a particular occupation, o. A worker first observed in the PSID database is assumed to have the average amount of knowledge of i,$\bar{T}_i = \sum_o \hat{T}_i^o$. Over time, T_i, the amount of tenure in job task i for the worker, approaches $\hat{T}_i^o$as the worker spends time doing that job task. If the worker's current occupation has a maximum knowledge of i greater than $T_i$then T_i is increasing over time. If the worker's current occupation has a maximum knowledge of i less than $T_i$then T_i is decreasing over time. The following is assumed to be the law of motion for T_i:

$$
\begin{aligned}
T_{i,0} &= \bar{T}_i \\
T_{i,t} &= (1-\alpha)T_{i,t-1} + \alpha \hat{T}_i^{o_t}
\end{aligned}
$$

where $\alpha = 0.25$ puts task tenure at 95% of the maximum after 10 years. This calibration matches the observation that worker's become experts in an occupation after 10 years (as measured by when wages flatten out as a function of occupational tenure). Because job tasks are multidimensional (e.g. 52 dimensions as in the O*Net database), task tenure is multidimensional. A single summary measure of task tenure makes analysis easier. The analysis below uses the maximum task tenure ($\max_i T_i$) as this summary measure. Another obvious choice would have been average tenure ($\frac{1}{52}\sum_i T_i$). The reported results do not depend on this choice.

Workers with task tenure below the median (among workers) are classified as low task tenure. High task tenure workers, symmetrically, are those workers with task tenure above the median.

3.1 Summary statistics of analysis sample

Table **??** displays the summary statistics for the analysis sample with the treated and control groups broken out. For nearly every statistic there is no significant difference between the treated and control. However, there is a noticable difference in earnings with the treated having nearly 10% lower earnings than the control. Interestingly, those workers that end up with higher levels of the treatment (i.e. greater than the median task distance moved post-displacement) have higher earnings than those with lower levels of the treatment. That said, these statistics are calculated over the whole sample period, pre- and post-displacement. This table, then, suggests the treated sample is not that much different from the untreated except for the primary outcome variable.

Table **??** shows some of these statistics two-years before displacement. Here we see the treated are younger than the control at the time of treatment. As such and a time-varying attribute of the worker, I explicitly control for age in all regressions. That said, the task tenure averages are very similar which suggests the treated have as many years experience working in the same tasks as the untreated. Both groups have similar levels of education as well. Before displacement, as in the table discussed above the treated have lower earnings than the control. The differences in age cannot account for these differences and it will be assumed in the analysis below that whatever is causing this difference, it is a fixed feature of the individual. Fixed individual effects, then, control for this difference.

Its interesting to note that unlike in table **??**, those with greater levels of treatment (i.e. those that move more than the median in task space post-displacement) earn less than those exposed to smaller treatments. This difference accentuates the importance of dealing the fact that treatment level is endogenous. Workers choosing to move far in the task space seem to have better outcomes in the long run relative to those that choose not to. In section **??** I use task isolation as an instrument for the treatment. Table **??** shows that those with high levels of treatment are more task isolated before displacement than those with low levels of treatment. This shows that while task isolation is plausibly exogenous to outcomes it is correlated with treatment. It is a good candidate to be an instrument for treatment.

Table 2: Summary Statistics two years before displacement

Data	Control	Treated (< median)	Treated (> median)
age	mean: 37.5	34.7	34.2
	(SD: 11.4)	(10.4)	(10.8)
years of education	11.4	11.6	11.5
	(2.7)	(2.8)	(2.4)
Occupation isolation	41.5	34.0	51.4
	(16.4)	(8.6)	(21.0)
Task tenure	0.77	0.77	0.78
	(0.13)	(0.12)	(0.12)
Hours worked	2160	2183	2158
	(710)	(689)	(756)
Earnings (82-84$)	19,392	18,156	17,716
	(16,154)	(14,300)	(14,318)

Note: Mean values are reported for task stayers (the control group), task changers were the task distance was less than the median and task changers where the task distance was greater than the median. Standard deviation is reported in parenthesis.

Table 1: Summary Statistics for Analysis Sample

Data	Whole sample	Undisplaced	Control	Treated
age $\in [17, 99]$	mean: 39.0 (SD: 12.4)	39.2 (12.7)	38.2 (11.4)	38.7 (11.4)
gender	male (0)	male (0)	male (0)	male (0)
years of education $\in [0, 19]$	11.8 (2.9)	11.8 (3.0)	11.7 (2.7)	11.6 (2.6)
number of observations $\in [1, 34]$	20.5 (9.7)	19.5 (9.9)	22.8 (8.8)	25.2 (7.6)
Occupation isolation$\in (16, 131)$	44.0 (18.2)	44.3 (18.5)	42.5 (17.5)	42.5 (17.2)
Task tenure$\in [0, 1]$	0.78 (0.14)	0.78 (0.14)	0.77 (0.15)	0.80 (0.12)
Ever displaced	23.4%	0%	100%	100%
Displaced year prior to survey	1.2%	0%	5.7%	4.8%
Switch 3-digit occupation prior year | displaced year prior	65.3%	-	0%	100%
Task distance | displaced year prior, occ switch$\in [0, 204)$	-	-	-	78.6 (29.6)
Hours worked $\in [0, 7800]$	2126 (709)	2119 (705)	2135 (737)	2,118 (698)
Earnings (82-84\$)$\in (0, 653300]$	20,301 (17,960)	20,584 (17,955)	19,715 (18,752)	18,169 (17,853)
Hours | task distance greater than median post displacement	-	-	-	2164 (697)
Earnings | task distance greater than median post displacement	-	-	-	18,880 (15,873)

Note: Mean values are reported for the whole sample, those workers who were never displaced, task stayers (the control group) and those that moved the median distance in task space (the treated group). Standard deviation is reported in parenthesis.

4 Exogenous post-displacement outcomes

In this section, the analysis proceeds assuming displacements are exogenous and treatments (occupations or tasks) are exogenous. Changes in occupations and tasks are calculated between those variables' values two years before and after the displacement. This four year interval is long enough before the displacement that earnings haven't started to drop and its long enough after the displacement such that most workers have found employment. On the other hand, this window of time is narrow because I want to reduce the number of other factors besides the original displacement (including a second displacement) that may effect changes in outcomes. I want to replicate as much as possible the ideal experiment.

In this experiment, the counter-factual non-displaced group is identified on observations from individuals that were displaced but three or more years before displacement. The control group is displaced workers that did not move in the task space. By in large, these are workers that did not change occupations. The treatment group is displaced workers that changed occupations after displacement. Following Jacobson et al. [1993], Stevens [1997] and Lindo [2009], the following generalized difference in difference model is estimated:

$$w_{i,t} = \sum_s \alpha_s D_{i,s} T_i + \sum_s \delta_s D_{i,s} + X_{i,t}\delta + \beta_i + \gamma_t + \epsilon_{i,t} \tag{7}$$

$w_{i,t}$is the earnings for worker i at time t. $D_{i,s}$ is a dummy variable indicating the time of displacement relative to time t and T_i is the treatment of interest. There is one dummy variable for every year from two years before displacement to nine years after. There is a single dummy variable for displacement having happened ten or more years in the past. $X_{i,t}$ is a set of time varying attributes of the worker. A quadratic in age is always among the controls as is this interacted with the treatment measure. The β_i is a individual fixed effect and the γ_t is a year fixed effect.

My first task is to replicate the findings in the literature. Figure 2 shows the replication of the Stevens [1997] specification (i.e. equation 7) on my analysis sample with the treatment terms removed. The results are qualitatively similar to hers. Displaced workers see a substantial decline in earnings that starts to manifest itself before the displacement. Earnings recover slightly, but the decline in earnings seems persistent. These results also replicate the "inverted hump-shaped" estimates of Lindo [2009] who uses a similar analysis sample and specification.

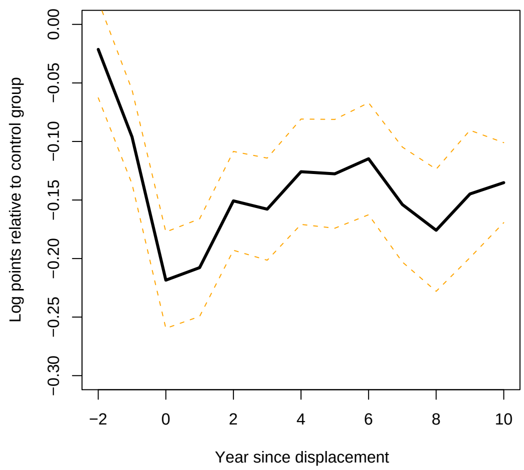

Figure 2: Effects of displacement: Coefficients on displacement dummies from estimating equation 7 (with the treatment terms removed) on the analysis sample. The analysis sample is described in section 3. An age quadratic is the only control. The thick black line represents the estimates for each dummy. Lines representing the confidence intervals (±2∗S.E.) of these estimates are also plotted. Standard errors are panel robust.

My second task is to show workers who lose more transferable human capital after a displacement are made worse off by displacement than those that didn't lose, or lost less, human capital. As I demonstrated above, all displaced workers see reductions in their earnings post-displacement. The important distinction is between the displaced workers that were treated and those that were untreated. In the analysis that follows, the treatment is occupation switch or task switch.

Because the most interesting comparison is between the treated and untreated workers (and not between displaced and undisplaced), in the following analysis I will report two statistics. The p-stat is the likelihood, given the imprecision of the estimates, that lifetime earnings of the treated are less than the lifetime earnings of the untreated. The q-stat is the likelihood that earnings in every period post-displacement are less for treated than the untreated. Both statistics are computed using bootstrap simulations.

The first treatment is occupation switches, a binary treatment. Figure 3 displays the estimates for the effects on earnings of displacement for occupation switchers versus occupation stayers. The red line are occupation stayers and the black line are occupation switchers. Along the x-axis are years since displacement. The reference group, and so the zero line, is the the displaced in the counter-factual case where they were not displaced. This counter-factual is identified using observations of the displaced previous to two years before displacement.

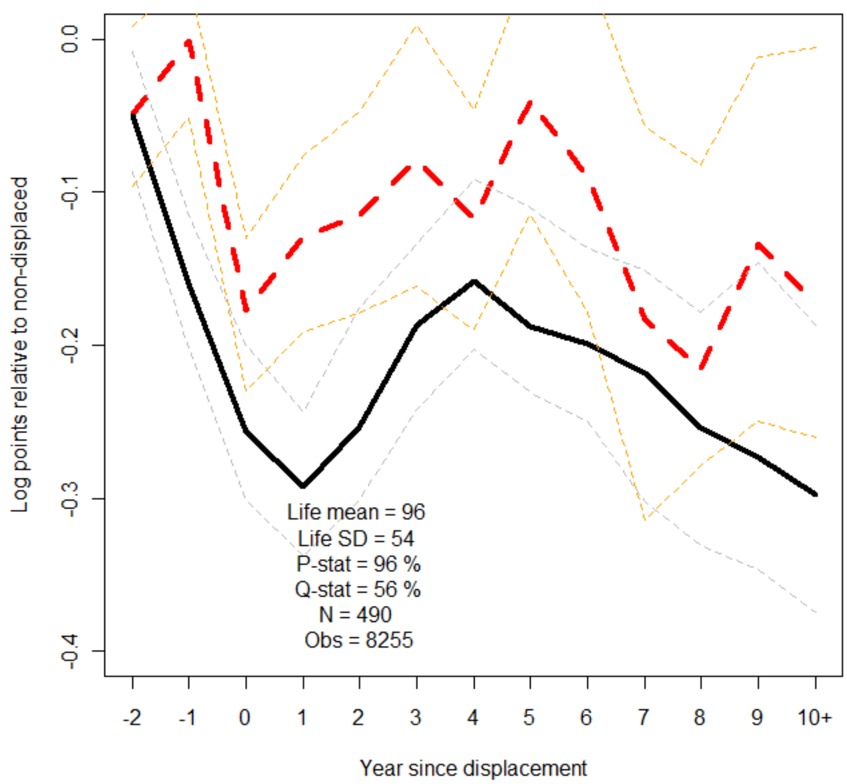

Figure 3: Effects on earnings of displacement for occupation switchers (vs. stayers): Coefficients on displacement dummies from estimating equation 7 on the analysis sample. The analysis sample is described in section 3. An age quadratic is the only control. The thick red (dotted) line represents the estimates on the occupation stayers (three digit occupation two years after the displacement is the same as two years before displacement) and the thick black line represents the estimates on the occupation switchers. Lines representing the confidence intervals ($\pm 2 *$S.E.) of these estimates are also plotted. Standard errors are panel robust.

There are a couple potential hypotheses to test. The main prediction of the transferable human capital story regarding this and the following experiments is that given exogenous shifts in the task space, those that move further should lose more human capital and thus see larger wage declines. The theory predicts little about the timing of this loss because it says nothing about when transferable human capital is utilized in a worker's career[8]. This suggests the appropriate hypothesis test is whether the integral of the difference between the red and black lines is positive. The p-stat is the appropriate statistic to test this hypothesis. A much more strict test of this prediction is whether the red line is above the black line in all periods. The q-stat is the appropriate statistic for this test. Evidence for the second hypothesis is necessarily evidence of the first, but as its a much more strict test, it gives a sense of the magnitude of the effect of treatment.

As in Stevens [1997], occupation switchers see much bigger costs from displacements than occupation stayers: the p-stat is 95% and the q-stat is 50%. This means its very likely that occupation switchers see a decline in their earnings over their lifetimes after their displacement and there is a very good chance they have lower earnings in every period after displacement.

The evidence for occupation switchers is strong evidence for the existence of transferable human capital. It does not tell us, however, the nature of that sort of human capital. Transferable human capital may be occupation specific, as has been suggested by the previous literature. This is unlikely as the structure of occupation taxonomies (e.g. Census 1990 occ codes) are arbitrarily constructed relative to the actual tasks being done and the products or services being produced on the job[9]. Occupations in these taxonomies are essentially labels on a set of job tasks performed by the people in that occupation. A truck driver for example is someone who drives trucks, has high stamina, is physically fit enough to occasionally lift heavy objects, etc. In some cases the label also identifies the product or, more usually, the service performed by those in the occupation. Doctors are people who have to verbally communicate, use deductive reasoning, etc, but they also provide health care services, for example.

This criticism of the idea that human capital is occupation-specific has two prongs. First, as

[8]Assuming any wage contract is available, a firm might, for example, pay a worker to accumulate human capital through most of his career and then utilize it in a burst of productivity just before the worker retires. The inconsistent productivity of artists and academics is another example.

[9]TODO: cite: there must be an account of how these taxonomies are constructed somewhere.

alluded to above, occupation labels confuse types of transferable knowledge (product knowledge versus knowledge of job tasks). Second, some occupations are more similar to others in terms of the types of tasks performed by people in those occupations. Also, some occupations are similar in the types of products and services being produced. Consider the examples of a truck driver switching occupations to become a taxi cab driver, a nurse becoming a cab driver and a nurse becoming a doctor. Cab drivers do similar things on the job as truck drivers and nurses provide similar services as doctors. A framework that treats transferable human capital as occupation-specific, however, would treat the truck driver becoming a cab driver as equivalent to a nurse becoming a cab driver. In this case, the truck driver doesn't have to learn very many new tasks in her new job that are different from her old job as a truck driver, but the nurse would be required to learn a whole new set of tasks. Also, this framework would treat the nurse becoming a doctor as equivalent to the nurse's switch to being a taxi cab driver. The nurse though is leaving a lot of product domain knowledge behind when he becomes a taxi cab driver that he wouldn't be leaving behind if he were to become a doctor. The occupation-specific human capital framework doesn't distinguish between product knowledge and job task knowledge and it doesn't account for the fact that some occupation moves require more changes in product and task knowledge than other occupation changes.

In response to this criticism, figure 4 displays the estimates for the effects on earnings of displacement for job task switchers (the black line) versus job task stayers (the red line). While treatment in the regression is continuous, in the figure task switchers are somewhat arbitrarily set to be occupation switchers that moved more than the average distance in task space. Changing this threshold, though, doesn't change the results depicted on this figure. While all displaced workers see persistent costs of displacement, task switchers see significantly higher costs of displacement than task stayers (p-stat=100% and q-stat=58%). Another statistic reported in that figure is the mean discounted lifetime difference between the control and treatment. For this treatment, in the bootstrap simulations this is estimated to be 99 log points with a standard deviation of 50.

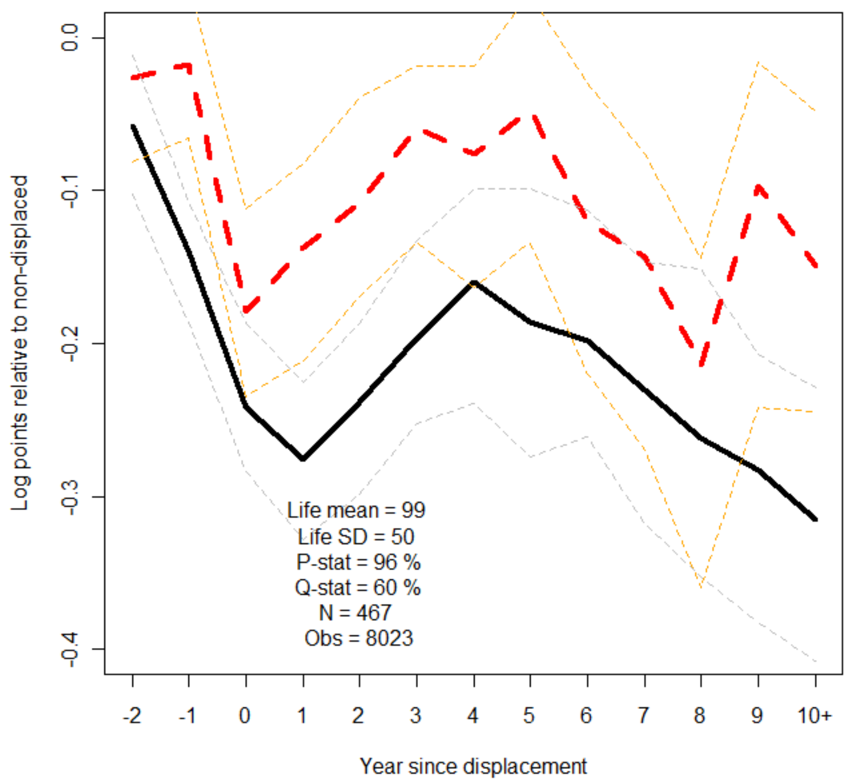

Figure 4: Effects of displacement on job task switchers (vs. non-switchers): Coefficients on displacement dummies from estimating equation 7 (stratification on task distance moved) with labor earnings as the dependent variable. This regression used the analysis sample which is described in section 3. An age quadratic is the only control. The thick red (dotted) line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals ($\pm 2*$S.E.) of these estimates are also plotted. Standard errors are panel robust.

At first glance you might expect the black line to converge with the red line over time. In the story of transferable human capital, knowledge is acquired through ones career, so you would not expect a one time destruction of human capital (i.e. the displacement) to have permanent effects. Its important to remember, though, that these figures show outcomes relative to the counter-factual non-displaced group. Because age is being controlled for, the counter-factual group consistent of non-displaced workers in ones cohort. Members of ones cohort accumulate human capital at the same pace and so a gap in knowledge persists between the treated and the untreated.

All of the results reported in this section task switches are verified in table 4. Regressions similar to the ones used to construct the figures in the section are reported there. The results for task switching are reported as continuous treatment of distance between the pre- and post-displacement occupations.

4.1 Outcomes before displacement

In the ideal experiment the treatment and control groups would not differ in their unobserved characteristics. Unobserved characteristics can not be controlled for and they may have an impact on the treatment effect. One way to infer that the treatment and control groups do not differ in unobservables is to check to see if their outcomes are different before treatment. In figure 5, we see a modification of the analysis on task switches above. As before the red line is the untreated group, the task stayers, and the black line is the treated group, the task movers. Now, however, all of the post-displacement years have been grouped together, we see more years before displacement and the reference group counter-factual is identified on observations of the displaced ten years and more before their displacement. Four and more years before displacement we see the point estimates almost overlap suggesting outcomes in those years were nearly identical. Between three and two years before displacement, the wages relative to the reference group of the untreated appear to be a little higher than those for the treated group but given the error bars not significantly different.

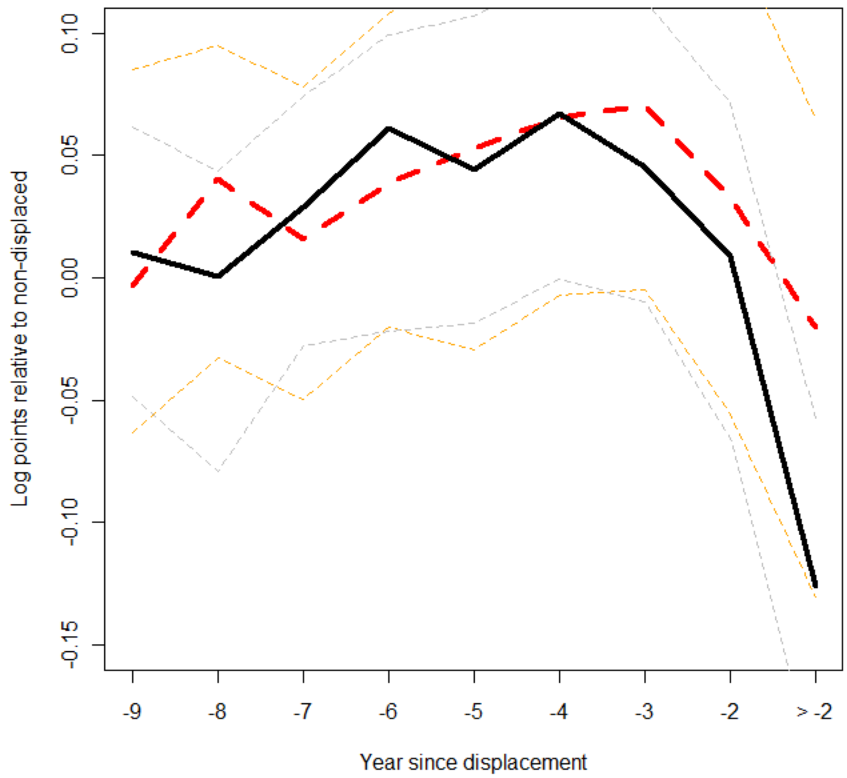

Figure 5: Pre-displacement outcomes for job task switchers (vs. non-switchers): Coefficients on displacement dummies from estimating equation 7 (stratification on task distance moved) with labor earnings as the dependent variable. This regression used the analysis sample which is described in section 3. An age quadratic is the only control. The thick red (dotted) line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals ($\pm 2{*}$S.E.) of these estimates are also plotted. Standard errors are panel robust.

5 Endogenous displacements

The first concern with the experiments in the previous section is one that haunts all studies of the effects of displacements: displacements, even mass lay-offs, are not independent of the quality of workers. Unless the whole plant or firm is shut down, managers have discretion over who to lay-off. Introspection suggests they will lay-off less productive workers. Thus, displacements are not acting causally on outcomes and our estimates of the effect of displacements are biased and probably biased negatively.

While a problem for the displacements literature both in terms of internal and external validity, this is only a problem for this study in regards to external validity. This is because the treatments contemplated in the previous section and the control group where conditional on displacement. The treated and the untreated were both displaced.

This issue still brings into question the external validity of my estimates. Its unlikely that workers that are displaced are as a group similar to workers in general in respect to their human capital characteristics. To verify external validity, I ran the experiments in the previous section but limited displacements to those that occurred in counties where unemployment was above 9% (a standard deviation above the national average). These displacements are more likely to be exogenous. The results are qualitatively similar to the results reported in the previous section and increasing the unemployment threshold does not overturn the results either.

6 Endogenous treatments

As mentioned above, the second problem with the experiments in section 4 is that occupation and task outcomes are not random. Those workers that switch occupations (and thus tasks) are less likely to be harmed by this switch.

Take the experiment $E[w_2|D=1, Sw=0] - E[w_1|D=0, Sw=0]$ implicit in red line of figure 4, where "Sw" stands for whether (=1) or not (=0) there was an task switch after the displacement. For the same worker, the outcomes, w_j, may be different whether or not they are displaced (i.e. D=1) , but also the decision to switch may depend on the displacement outcome. This leads to a "bad controls" problem as follows[10]:

[10] I was introduced to the bad controls problem on page 64 of Angrist and Pischke's *Mostly Harmless Econometrics*.

$$
\begin{aligned}
E[w_2^1|D=1,Sw^1=0]-E[w_1^0|D=0,Sw^0=0] &= E[w_2^1|Sw^1=0]-E[w_1^0|Sw^0=0] \\
&= E[w_2^1-w_1^0|Sw^1=0]+ \\
&\quad E[w_1^0|Sw^1=0]-E[w_1^0|Sw^0=0]
\end{aligned}
$$

The superscripts on outcome variables indicate the potential value of that variable under displacement (=1) or no displacement (=0). The first equality is valid because displacements are assumed to be exogenous. The second equality is just a re-writing of the first. The first term on the right hand side of the second equality is interpreted as the causal effect of displacements on earnings and the next two terms are the selection bias introduced by the bad control. Because switching tasks is more likely when a worker is displaced due to decreased returns to staying (e.g. destruction of human capital), the selection bias term is likely to be positive. Those that stay in their task despite this destruction of human capital, on average, had more human capital before the displacement than those that decide to stay without the trauma of displacement.

From the discussion above the red line in 4 should be lower in an unbiased estimate. Similarly, the black line should be lower as well:

$$
\begin{aligned}
E[w_5^1|D=1,Sw^1=1]-E[w_1^0|D=0,Sw^0=1] &= E[w_5^1-w_1^0|Sw^1=1]+ \\
&\quad E[w_1^0|Sw^1=1]-E[w_1^0|Sw^0=1]
\end{aligned}
$$

Displaced workers that switch tasks would have higher human capital than undisplaced workers that switch tasks anyway. This suggests the bias term is positive in this experiment as well. The question becomes: are these bias terms empirically important?

Using task isolation scores from before the displacement is a way around this bad controls problem yet maintain the spirit of the experiments in the previous section. Just as geographically isolated workers will have higher costs in adjusting their labor supply[11], task isolated workers will

[11]Its common in the literature to use labor market geographic isolation (measured by the level or share of employment in a geographical unit) as an instrument. Also, see Blank [2005] for a discussion of the effects of geographic

find more costly to switch industries or occupations. If transferable job task human capital exists, we would expect task isolated workers to made worse off than non-isolated workers when they are forced to move in task space. Figure 6 shows the effects of displacements on earnings for task isolated and non-task isolated workers.

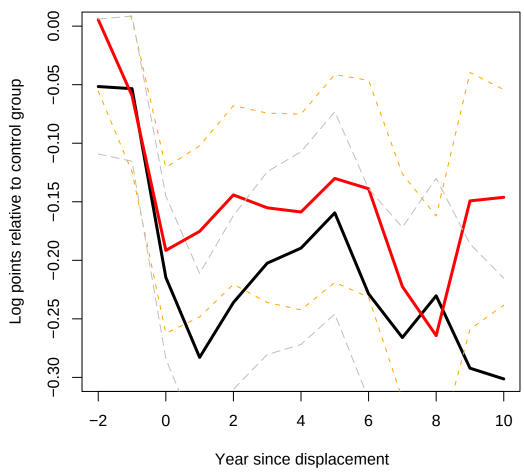

Figure 6: Effects of displacement for task isolated and non-task isolated workers: Coefficients on displacement dummies from estimating equation 7 on the analysis sample. The analysis sample is described in section 3. An age quadratic is the only control. The thick red (dotted) line represents the estimates on the non-isolated sample (isolation score for the worker's occupation is less than the median) and the thick black line represents the estimates on the isolated sample (isolation score greater than the median). Lines representing the confidence intervals ($\pm 2*$S.E.) of these estimates are also plotted. Standard errors are panel robust.

These results are not very exciting. There are significant differences between task isolated and non-isolated workers one year after treatment, but given we're looking at 13 coefficients, we would expect with high probability observing a significant result in at least one year. If the red line is above the black line, its not by much (p-stat=77% and q-stat=13%). However, restricting the sample to just task switchers increases the p-stat to 98% and the q-stat to 58%.

In theory, whether or not a worker was task isolated before the displacement should not have an effect on their earnings after displacement given a particular distance moved in task space. In other

isolation on poverty.

words, what should matter for post-displacement earnings is how much task specific knowledge was lost and pre-displacement isolation only affects earnings through this channel by making more likely for isolated workers to move far in the task space post-displacement (conditioned on making a move in task space). This suggests pre-displacement task isolation can be used as an instrument for distance moved post-displacement.

I instrument for task distance using pre-displacement task isolation. A worker is more isolated if she has to move further in the task space for any occupation change than other workers in other occupations. When displaced, the worker pays a higher cost of searching. This could have two effects: first, she will be less likely to change tasks (she will want to find work in the same occupation at a different firm) and second, if she does change tasks, she'll have to move further in the task space. Thus, in theory the instrument has ambiguous effects on task distance. However, a linear probability model suggests a worker that has a standard deviation higher isolation score is about 4% more likely to switch tasks at displacement. These results suggest the first effect is, at most, very small.

The isolation metric is constructed for each worker by looking within education and state cells. The distance to the occupation that is nearest the worker's pre-displacement occupation is the isolation score; a worker is more isolated if their nearest neighbor is far away. The further the distance, the higher the isolation metric and the more isolated the worker is in her education and state cell.

The correlation between task isolation and task distance is 0.40, but there are actually several endogenous variables on the right hand side of equation 7, one for each year/task distance interaction term. In a first stage regression, pre-displacement isolation does appear to increase the distance displaced workers travel in task space especially in the years directly post-displacement. A regression of the endogenous variable on just the excluded instrument, task isolation, has an F statistic of 2363.

In figure 7 are the second stage estimates. While qualitatively these IV estimates are similar to the OLS results, the error bars are too wide to take away any clear conclusions about the size of the biases of OLS. However, the difference between task stayers and task movers remains significant as these estimates have a p-stat of 96% and a q-stat of 45%. Also, as predicted in the above discussion of bad controls, the point estimates for the treated group are shifted down. The estimates for the control group are too noisy to support a similar claim about them.

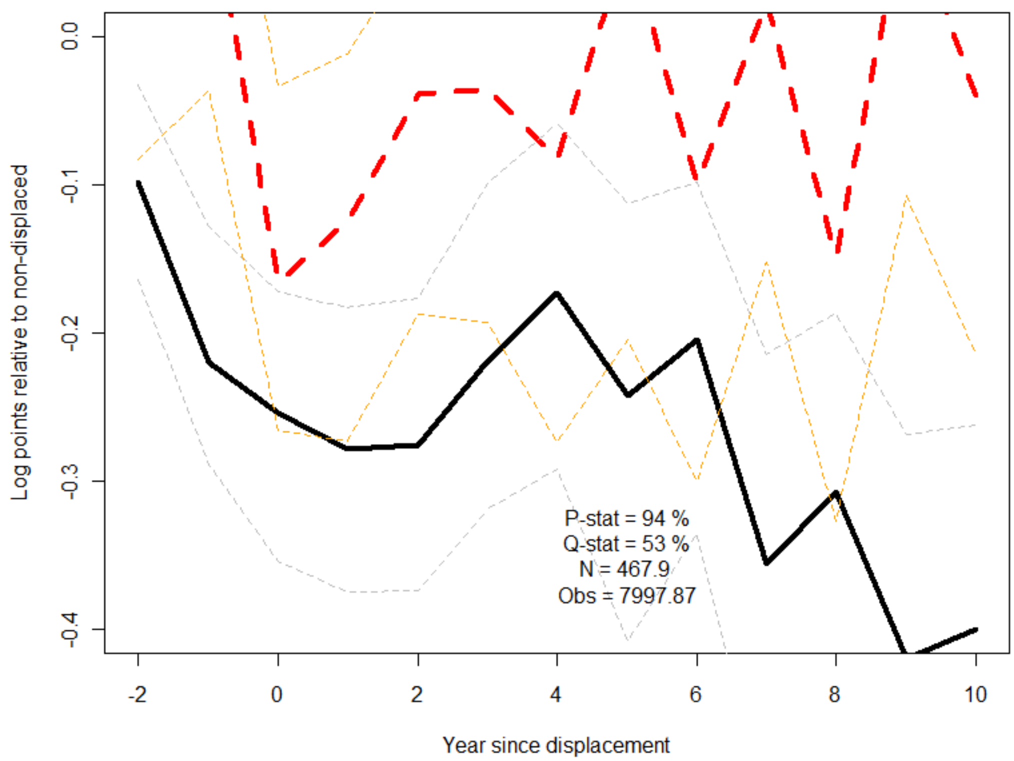

Figure 7: Effects of displacement on job task switchers (vs. non-switchers) with pre-displacement task isolation as an instrument for task distance: Coefficients on displacement dummies from estimating a stratified version of equation 7 (stratification on task distance moved) with labor earnings as the dependent variable. This regression used the analysis sample which is described in section 3. An age quadratic is the only control. The thick red (dotted) line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals ($\pm 2*$S.E.) of these estimates are also plotted. Standard errors are panel robust.

Task isolation is a relatively successful instrument for task distance. The evidence from this instrument suggest the bias caused by having an endogenous treatment is not large. I have also tried an employment based instrument, a la Neal [1995]. Using his reasoning, if a task has high levels of employment, this crowding will make job search more costly. Task employment, however, is not a construct easily found in the data. One definition of task employment is the number of workers the same occupation, but this has a very low correlation with task distance. Another tack is to calculate task employment by taking a weighted average of occupation employment where the weights are an inverse function of task distance. Both of these definitions turn out to be very weak instruments.

7 Robustness checks

7.1 Alternative displacements

In previous sections of the paper I have assumed the treatment is conditional on a narrowly defined displacement. Only workers that had their firm go out of business are considered for treatment. This definition of the treatment increases the likelihood that the displacement is exogenous. If displacements are exogenous then there is little chance for firm match specific effects to contaminate my results.

In this section, I relax this definition. First I use more broadly defined displacements to condition treatment. In addition to firm closures, I include layoffs and at-fault firings. Figure 8 shows the result of conditioning on broad displacements. In the early years after displacement there appears to be a significant difference between the treated (task switchers) and the control (task stayers) groups. This difference disappears after three or four years.

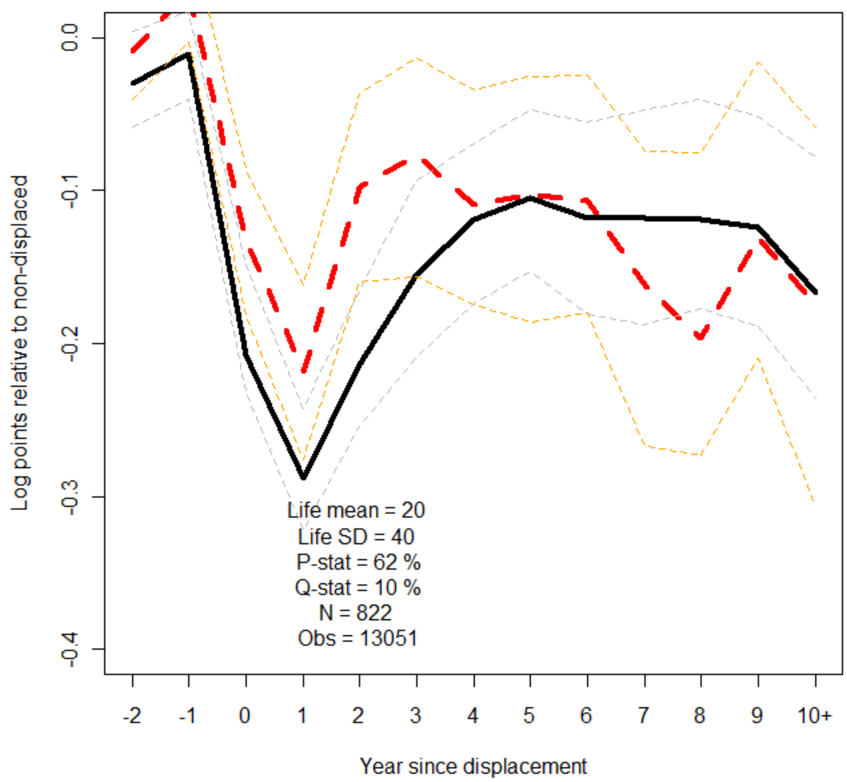

Figure 8: Effects of broadly defined displacement on job task switchers (vs. non-switchers): Coefficients on displacement dummies from estimating equation 7 (stratification on task distance moved) with labor earnings as the dependent variable. This regression used the analysis sample which is described in section 3. An age quadratic is the only control. The thick red (dotted) line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals ($\pm 2 *$S.E.) of these estimates are also plotted. Standard errors are panel robust.

Second I use all job switches to condition treatment. Figure 9 shows results very similar to the main results using just narrowly defined displacements. The magnitude of the difference between the control and treated groups are similar and the p- and q-stats are nearly identical.

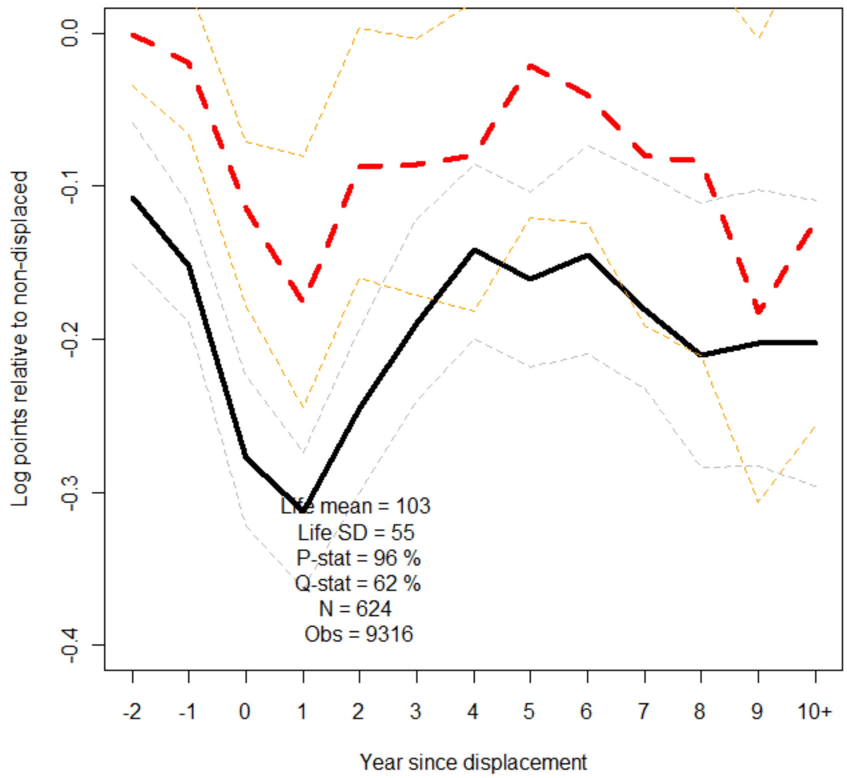

Figure 9: Effects of job switching on job task switchers (vs. non-switchers): Coefficients on displacement dummies from estimating equation 7 (stratification on task distance moved) with labor earnings as the dependent variable. This regression used the analysis sample which is described in section 3. An age quadratic is the only control. The thick red (dotted) line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals ($\pm 2*$S.E.) of these estimates are also plotted. Standard errors are panel robust.

7.2 Alternative treatments

In the section on endogenous treatments, I explored using task isolation as the treatment rather than task distance. The results are qualitatively similar to the main results, but there were a lot of noise in the estimates. In this section, I explore other alternative treatments.

When I allow the treatment to have quadratic effects, the resulting estimates are qualitatively similar to but weakened somewhat compared to the main results. Figure 10 shows the outcomes for task switchers (black line) and task stayers (red line). In the years around the displacement, task switchers appear to be much more impacted by the displacement than task stayers. In later years, the lines converge suggesting the disparate effect vanishes.

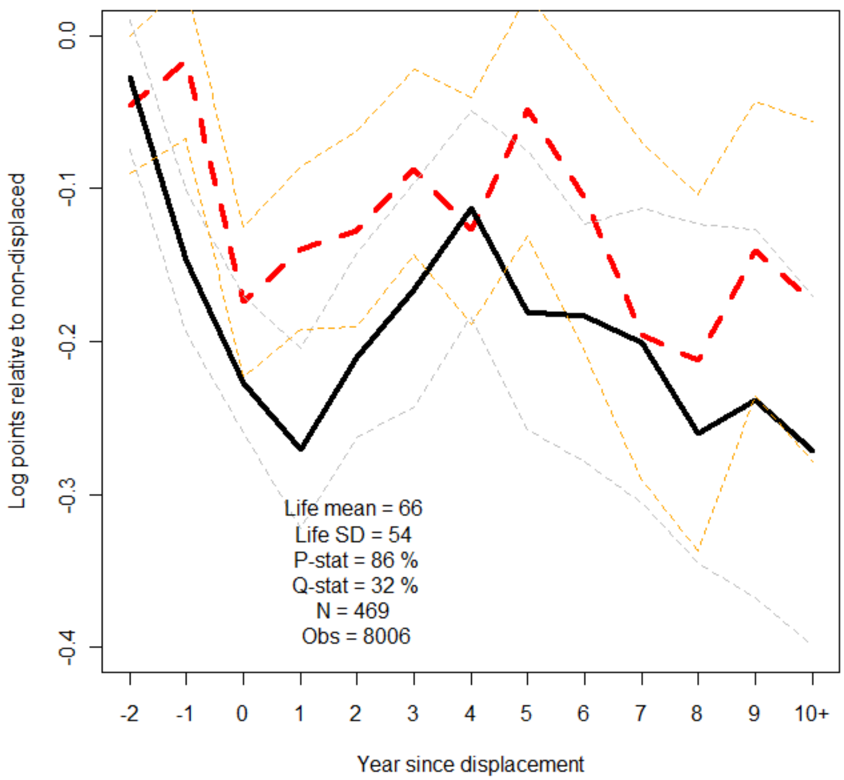

Figure 10: Effects of displacement on job task switchers (vs. non-switchers): Coefficients on displacement dummies from estimating equation 7 (stratification on a quadratic of task distance moved) with labor earnings as the dependent variable. This regression used the analysis sample which is described in section 3. An age quadratic is the only control. The thick red (dotted) line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals ($\pm 2*$S.E.) of these estimates are also plotted. Standard errors are panel robust.

7.3 Alternative outcomes

In this section, I explore other employment outcomes of displacement.

Besides affecting earnings, displacements might affect labor supply on the extensive or the intensive margins. The PSID data is not granular enough for us to explore the extensive margin. Displaced workers may experience bouts of unemployment, but we might not observe such bouts in the yearly snapshot captured in the survey. Instead, I will look at the intensive margin; the number of hours worked per year. Figure 11 shows the effect of displacement on the probability of working full time (here defined as working more than 2000 hours in the survey year). While task stayers (the red line) have a reduced chance of working full time in the year of their displacement, they don't experience a reduced chance of doing so in later years. Those that switch tasks and move the mean distance in task space (the black line) see significant drops in their probability of working full-time (p-stat=93% and q-stat=57%).

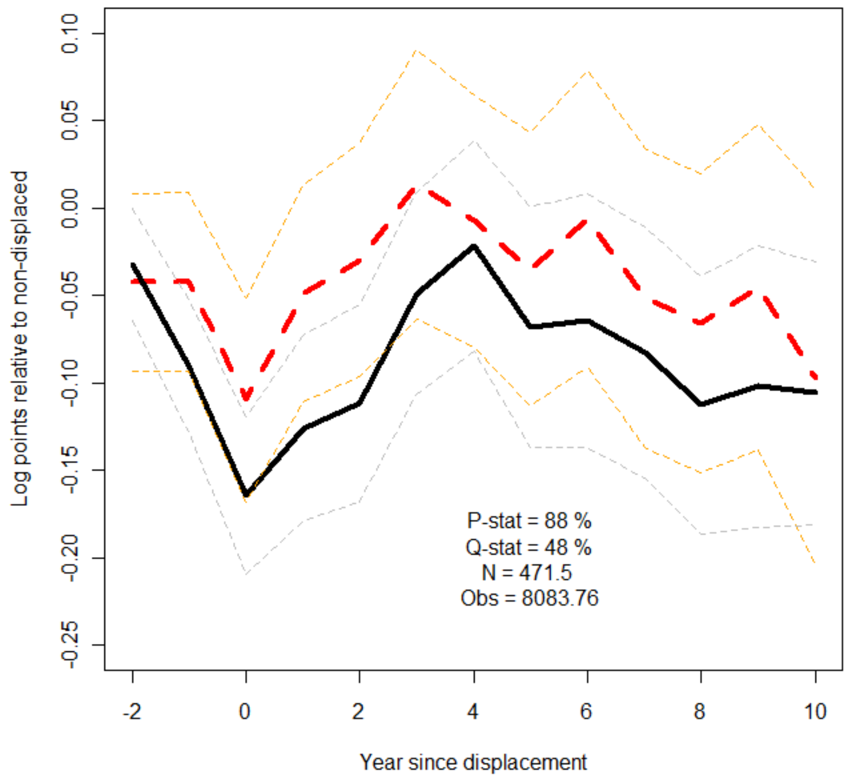

Figure 11: Effects of displacement for mean task switchers vs. non-switchers on the probability of working "full time": Coefficients on displacement dummies from estimating a stratified version equation 7 (stratification on task distance moved) with an indicator on whether or not the individual worked more than 2000 hours as the dependent variable. This regression used the analysis sample which is described in section 3. An age quadratic is the only control. The thick red dotted line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals ($\pm 2*$S.E.) of these estimates are also plotted. Standard errors are panel robust.

Job, occupational and industry switches have all been used as treatments in the literature, but it is interesting to see if job, occupation and industry switching become more frequent with treatment (where, recall, treatment is at-displacement task distance). Similarly, an interesting question is whether or not treatment affects the distance traveled in task space as workers navigate their career post-displacement. These questions touch on the extent to which the documented increase of labor volatility post-displacement is a result of the treatment explored in this paper.

Figure 12 shows the effect of displacement on the probability of changing jobs. Beyond three years after the displacement, task switchers are significantly less likely to switch jobs relative to task stayers.

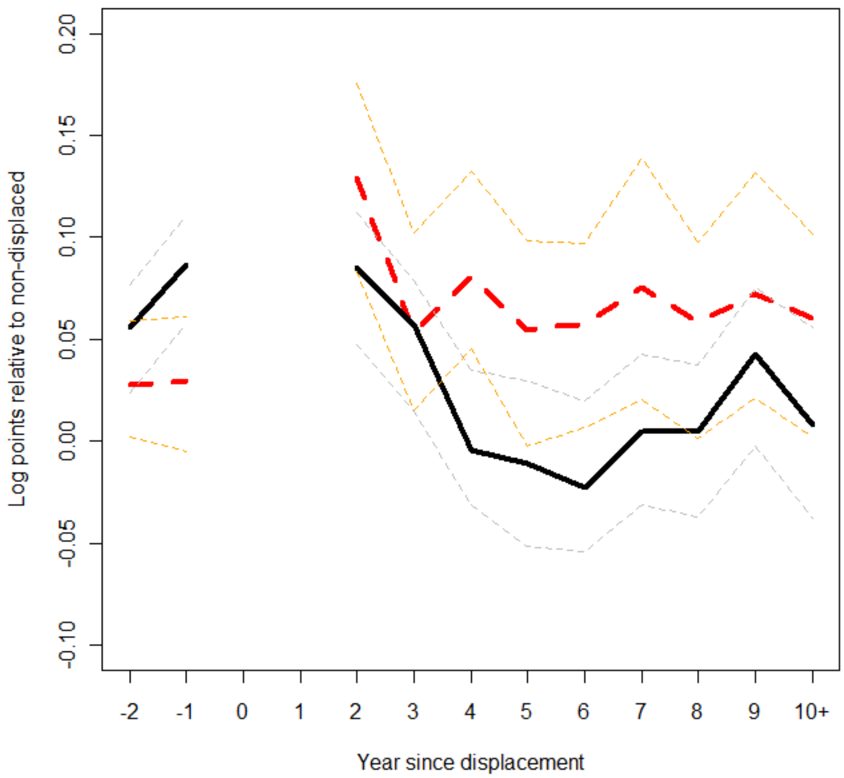

Figure 12: Effects of displacement for mean task switchers vs. non-switchers on the probability of changing jobs: Coefficients on displacement dummies from estimating a stratified version of equation 7 (stratification on task distance moved) with an indicator on whether or not the individual switched jobs in the last year as the dependent variable. This regression used the analysis sample which is described in section 3. An age quadratic is the only control. The thick red dotted line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Estimates near displacement are omitted to enhance readability. Lines representing the confidence intervals ($\pm 2*$S.E.) of these estimates are also plotted. Standard errors are panel robust.

Figure 13 shows the effect of displacement on the probability of changing occupations. Here the pattern reverses. Beyond three years after the displacement, task switchers are significantly more likely to switch occupations relative to task stayers.

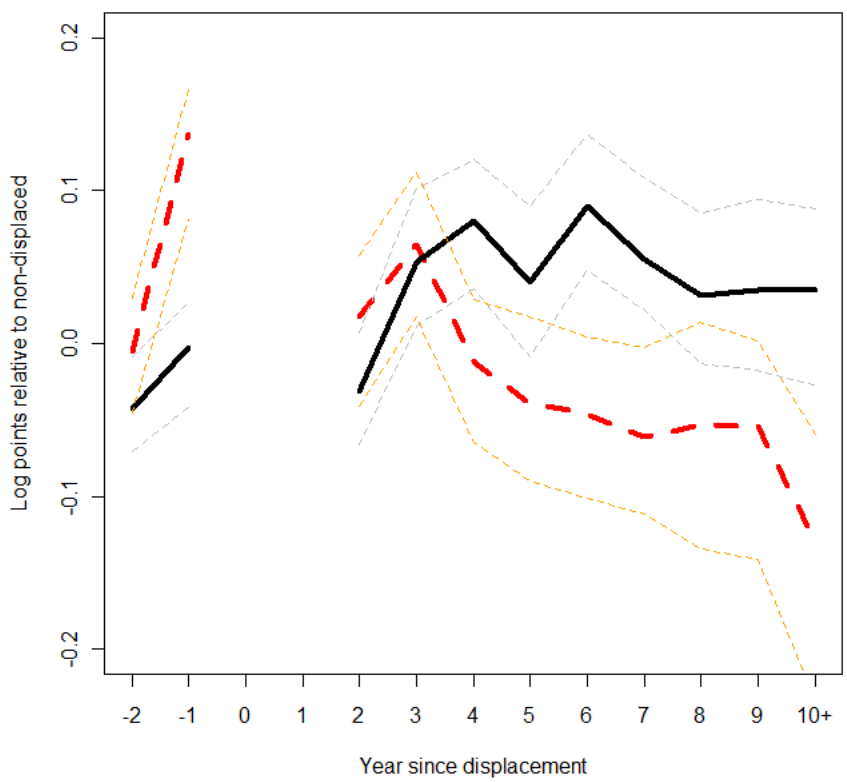

Figure 13: Effects of displacement for mean task switchers vs. non-switchers on the probability of changing occupations: Coefficients on displacement dummies from estimating a stratified version of equation 7 (stratification on task distance moved) with an indicator on whether or not the individual switched occupations in the previous year as the dependent variable. This regression used the analysis sample which is described in section 3. An age quadratic is the only control. The thick red dotted line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Estimates near displacement are omitted to enhance readability. Lines representing the confidence intervals (±2∗S.E.) of these estimates are also plotted. Standard errors are panel robust.

Figure 14 shows the effect of displacement on the probability of changing industries. At displacement, task switchers are less likely to switch industries but both the control and treatment groups are equally likely to switch industries in out years.

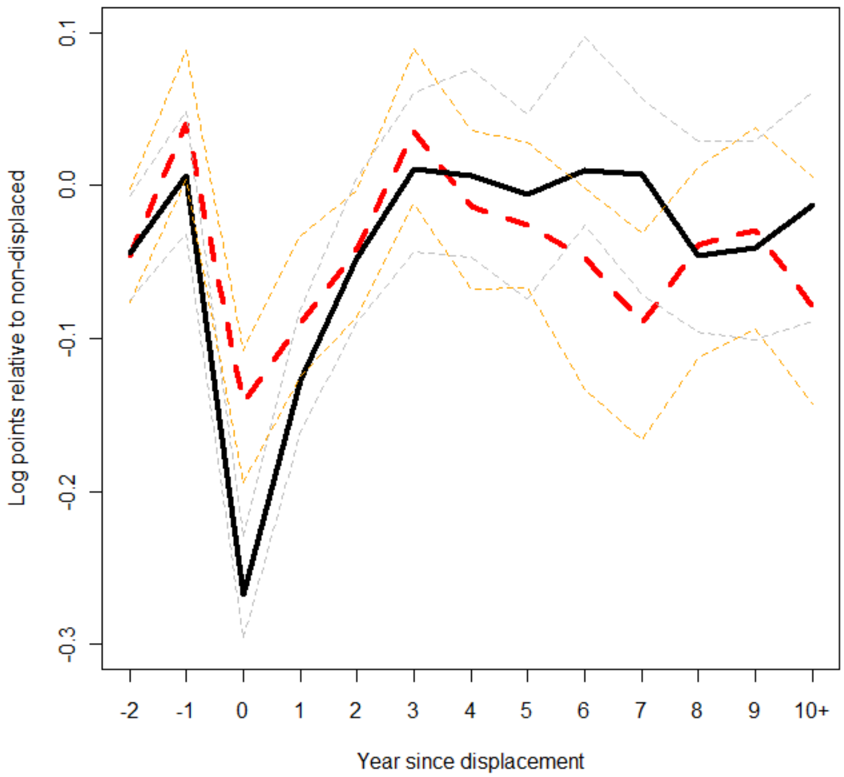

Figure 14: Effects of displacement for mean task switchers vs. non-switchers on the probability of changing industries: Coefficients on displacement dummies from estimating a stratified version of equation 7 (stratification on task distance moved) with an indicator on whether or not the individual switched industries in the previous year as the dependent variable. This regression used the analysis sample which is described in section 3. An age quadratic is the only control. The thick red dotted line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Estimates near displacement are omitted to enhance readability. Lines representing the confidence intervals ($\pm 2*$S.E.) of these estimates are also plotted. Standard errors are panel robust.

Another outcome is hourly wage. This variable is often thought to correspond to the theoretical construct in human capital theory, but unfortunately it is not directly reported in the PSID. I construct an hourly wage by dividing total yearly labor income by total hours worked. The results, shown in figure 15, roughly correspond to those with earnings. In terms of wages, task switchers see significantly higher costs of displacement than task stayers (p-stat=98% and q-stat=80%).

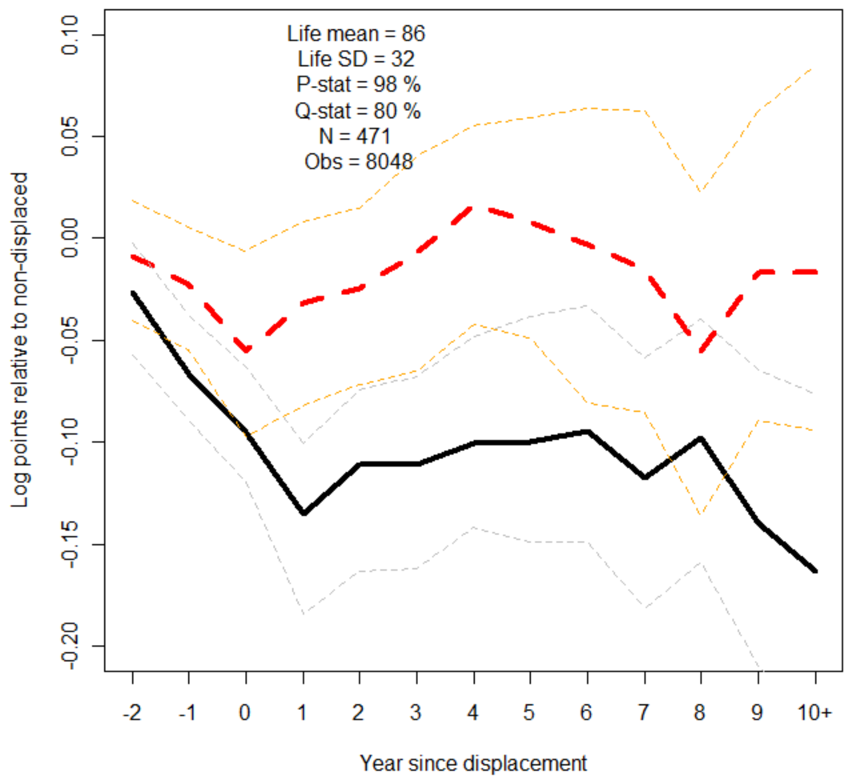

Figure 15: Effects of displacement on job task switchers (vs. non-switchers): Coefficients on displacement dummies from estimating equation 7 (stratification on task distance moved) with labor earnings per hour worked as the dependent variable. This regression used the analysis sample which is described in section 3. An age quadratic is the only control. The thick red (dotted) line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals ($\pm 2*$S.E.) of these estimates are also plotted. Standard errors are panel robust.

7.4 Alternative samples

The findings reported above are robust to a number of variations on the analysis sample. While some specifications lower statistical significance, all have qualitatively similar results. The number of individuals in each sample, the p-stat and the q-stat is reported in table 7.4. The table reports statistics corresponding to the OLS estimates of the task switch treatment. The first column reports the sub-sample and the second column the number of individuals. The third and fourth columns show statistics quantifying the effect size and the last two columns report the p- and q-stats. For example, the main results are reported in the first row. There are 11,868 individuals in that sample. At displacement, task stayers had earnings 17 log points below the counter-factual non-displaced group and task movers were 6 log points below that. On average post-displacement, task stayers were 13 log points below the counter-factual group and task movers were 23 log points below that group. In 98% of bootstrap simulations stayers had better lifetime outcomes than movers and they did better than movers in every year post-displacement 55% of the time. In the table, there are three results of note.

The first item in the table that stands out is that results are more stark for the "Square" sample (the sample that only contains individuals for which there is observations for every year) than for the analysis sample. As can be seen in figure 16, task stayers have effects of displacement statistically indistinguishable from zero and task switchers have significantly worse outcomes. This suggest, in this sample at least, that the only reason why there is costs to displacement is because workers switch the tasks they perform post displacement.

Table 3: Robustness of task switching results

Alternative sample	Obs	Effect size (100 * stayers, 100 * diff) @disp	Ave post-disp	p-stat	q-stat
Analysis sample	11868	-17, 6	-13, 10	98%	55%
Include zero earners	12125	-17, 10	-1, 15	90%	40%
Include women heads of households	17560	-15, 7	-12, 6	88%	21%
Include zero earners and women	18017	-12, 11	1, 17	93%	42%
Only include individuals with observations in every year	527	-10, 15	-2, 21	98%	60%
Workers displaced in counties with unemployment $>$ 9%	231	-24, 5	-11, 18	79%	48%
Workers displaced in counties with unemployment $<$ 9%	1606	-17, 5	-14, 7	92%	25%
Workers with task tenure below the median (and year $>$ 1975)	634	-27, -14	-15, -9	20%	1%
Workers with task tenure above the median (and year $>$ 1975)	484	-2, 33	-0, 34	100%	100%
Before and including 1984	5737	-24, 1	-19, 5	76%	18%
After 1972 and before and including 1989	6568	-23, 2	-18, 8	90%	24%
After 1977 and before and including 1994	9146	-17, 14	-11, 19	100%	90%
After 1983 and before and including 1999	9450	-14, 13	-13, 15	97%	62%
After 1984	9791	-11, 13	-11, 12	94%	52%
Occupation switchers	5169				
Industry switchers					

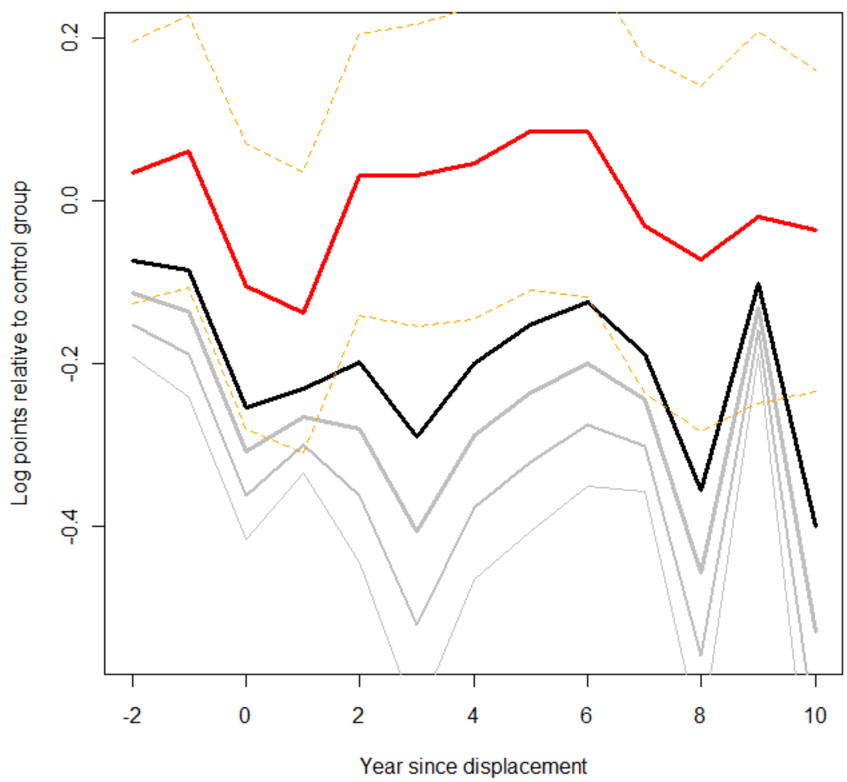

Figure 16: "Square" sample; effects of displacement on job task switchers (vs. non-switchers): Coefficients on displacement dummies from estimating a stratified version of equation 7 (stratification on task distance moved) with labor earnings as the dependent variable. This regression used a subset of the analysis sample — which is described in section 3 — which consists of individuals that have observations in every year. An age quadratic is the only control. The thick red (dotted) line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals ($\pm 2*$S.E.) of these estimates are also plotted. Standard errors are panel robust.

It is hard to interpret why this sample shows stronger results than the analysis sample because there are two effects at play. First, the members of this sample were all members of cohorts of similar ages and second, nobody in this sample dropped out of the survey. If the first effect predominates, then this suggests a strong cohort effect. Older cohorts have less mobile human capital. If the second effect predominates, then this suggests a sample attrition bias. Workers that eventually drop out of the survey decrease the estimates of earnings post-displacement. This suggests if there was no attrition, my estimates would be lower and the difference between treated and the untreated might narrow.

Second, task knowledge seems to have become more important over time. As figure 17 shows, a difference between movers and stayers doesn't emerge until the mid-1980s but that difference appears to have continuously widened over time.

The obvious interpretation of this result is the cost of displacement, in terms of task knowledge lost, has been increasing over time. However, this result could also be an artifact of the O*Net data. The abilities database was constructed around the turn of the century. This means the ability scores best match the abilities for occupations as they were performed nearest to us in time. Thirty or forty years ago, the ability scores may have been quite different for the "same" occupations. Similarly, the distance between occupations in task space may have changed over time. Thus, the increase in the cost of losing task knowledge over time is an artifact of the fact that we get better at measuring that loss over time.

A third notable result — perhaps the most striking result in this paper — is that task tenure before displacement seems to completely determine whether or not displacement has an impact on the worker. As can be seen from figure 18, if a worker has low task tenure, whether or not they switch tasks post-displacement, the impact of displacement on their earnings is statistically indistinguishable from zero. If anything, task stayers have worse outcomes than task movers when tenure is low (p-stat=20% and q-stat=1%).

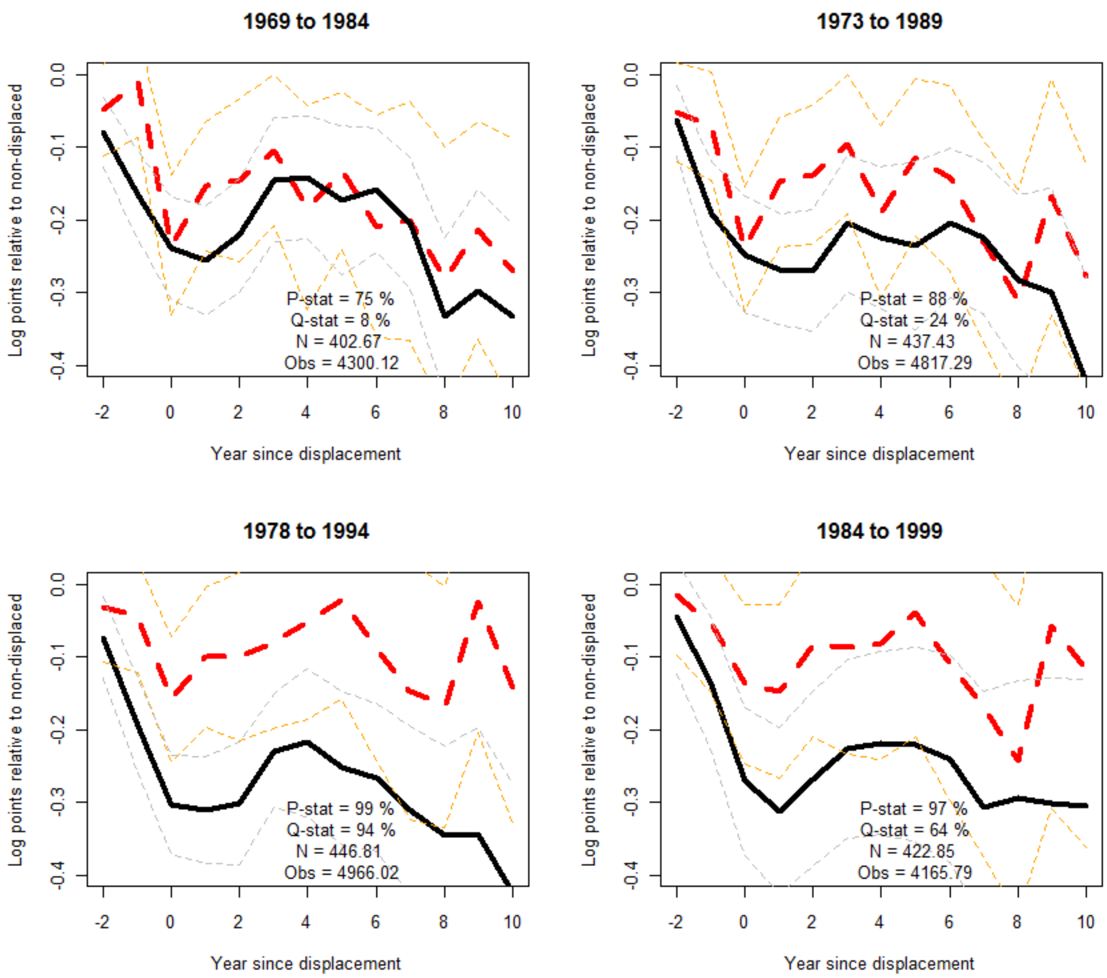

Figure 17: Time restricted samples; effects of displacement on job task switchers (vs. non-switchers): Coefficients on displacement dummies from estimating a stratified version of equation 7 (stratification on task distance moved) with labor earnings as the dependent variable. These regressions are on subsets of the analysis sample — which is described in section 3 — which consist of a moving window of 15 years. An age quadratic is the only control. The thick red (dotted) line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals ($\pm 2{*}$S.E.) of these estimates are also plotted. Standard errors are panel robust.

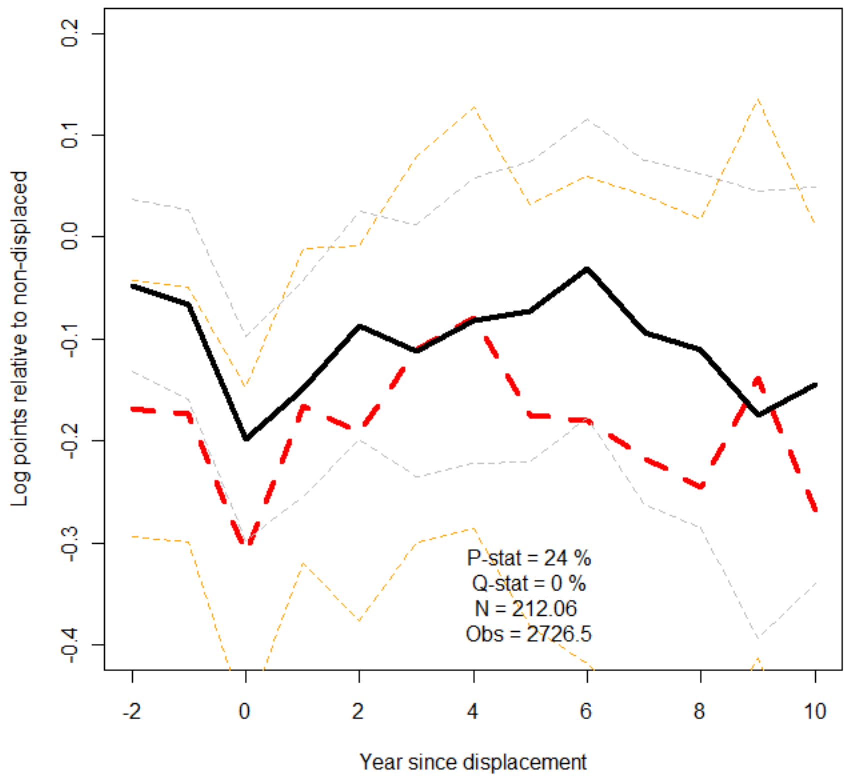

Figure 18: Low task tenure sample; effects of displacement on job task switchers (vs. non-switchers): Coefficients on displacement dummies from estimating a stratified version of equation 7 (stratification on task distance moved) with labor earnings as the dependent variable. This regression used a subset of the analysis sample — which is described in section 3 — which consists of individuals that had low task tenure before displacement. An age quadratic is the only control. The thick red (dotted) line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals ($\pm 2 *$S.E.) of these estimates are also plotted. Standard errors are panel robust.

As is plain in figure 19, there are much different results when task tenure is high. Outcomes for task stayers can not be distinguished from the control group while task movers see significant costs of displacement. In every bootstrap sample, the red line is completely above the black line (i.e. p-stat = q-stat = 100%). In fact, the error bars for the estimates only overlap at two points.

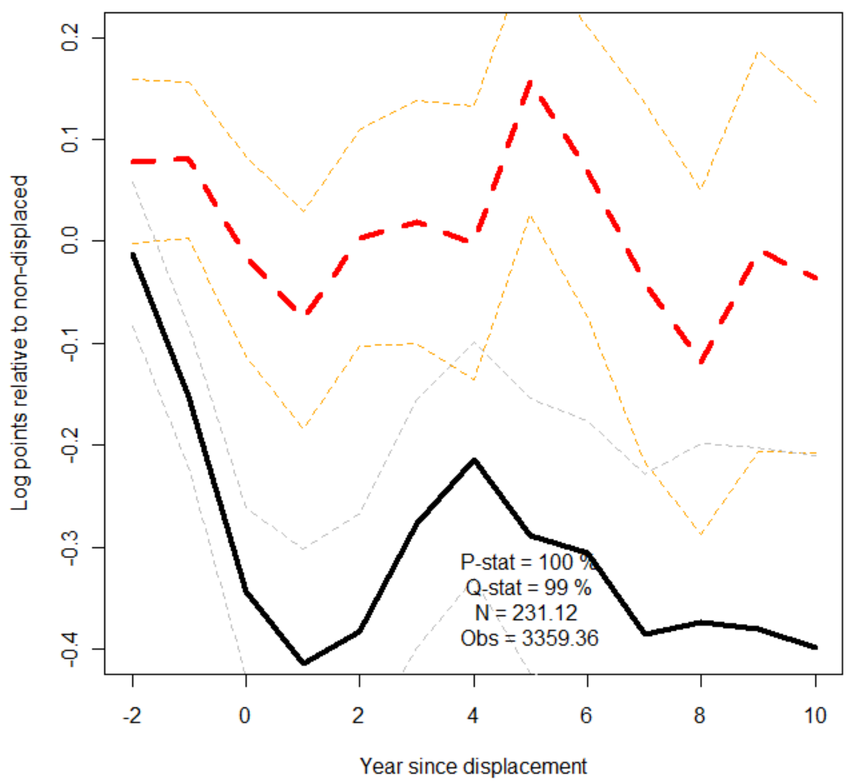

Figure 19: High task tenure sample; effects of displacement on job task switchers (vs. non-switchers): Coefficients on displacement dummies from estimating a stratified version of equation 7 (stratification on task distance moved) with labor earnings as the dependent variable. This regression used a subset of the analysis sample — which is described in section 3 — which consists of individuals that had high task tenure before displacement. An age quadratic is the only control. The thick red (dotted) line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals (±2∗S.E.) of these estimates are also plotted. Standard errors are panel robust.

This result is strong evidence for a learning-by-doing theory of task knowledge or any theory that predicts a strong relationship between task tenure and task knowledge.

7.5 Tasks versus occupation switches and industry switches

When a worker changes occupation or industry, they are also changing tasks, at least to some degree. It is possible, then, that occupation or industry switches are accounting for all the results reported above. Figure 20 shows the results of limiting the sample to occupation switchers, figure 21 shows the results for industry switchers and figure 22 shows results for those workers that switched both occupation and industry. For the occupation switcher sample, not surprisingly, the estimates for the treated group are almost identical to the main estimates (this sample contains most of the observations used to estimate those effects in the main results). The effects on the control group are less efficiently estimated (as would be expected given this sample throws out so much data that would otherwise be used to estimate these effects) but the pattern in the main results remain. In bootstrap simulations, stayers have better long-term outcomes than switchers 80% of the time. In the out years, the inefficiency of the control group estimates makes it hard to know how they compare to those for the treated group.

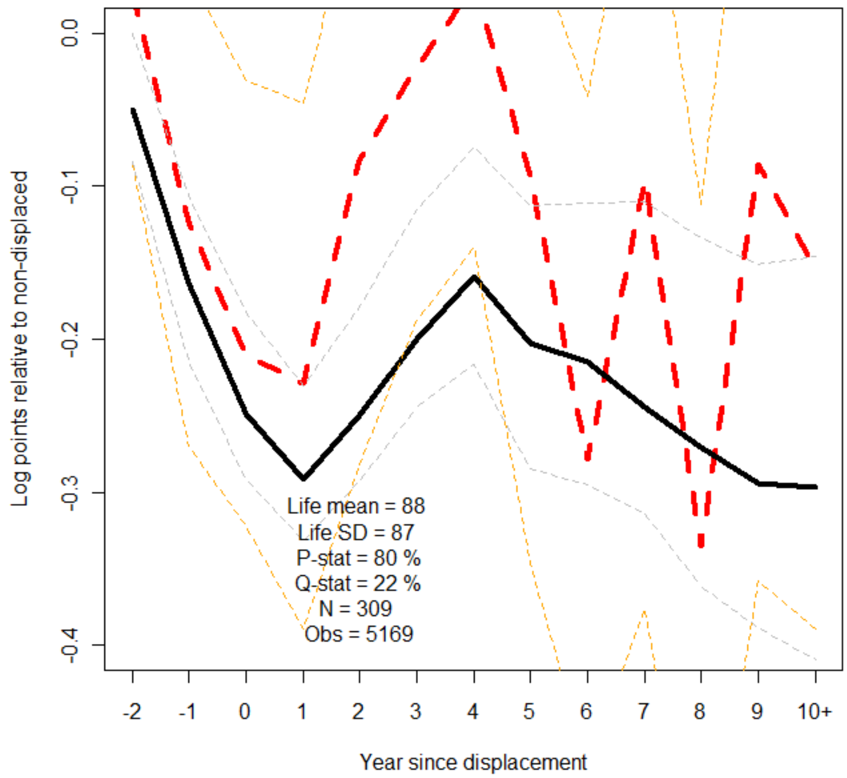

Figure 20: Occupation switchers sample; effects of displacement on job task switchers (vs. non-switchers): Coefficients on displacement dummies from estimating a stratified version of equation 7 (stratification on task distance moved) with labor earnings as the dependent variable. This regression used a subset of the analysis sample — which is described in section 3 — which consists of individuals that switched occupations at displacement. An age quadratic is the only control. The thick red (dotted) line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals (±2∗S.E.) of these estimates are also plotted. Standard errors are panel robust.

For industry switchers the results are more pronounced than in the main results. The estimates of the effects on the treated are, again, nearly identical to the main results, but the estimates for the control group are shifted towards zero. In simulations, the controls had better lifetime outcomes 100% of the time and they had better outcomes in every year post displacement 86% of the time. The point estimate for the lifetime difference in outcomes is 160 log points with a simulation standard deviation of 58. These statistics suggests that for workers that switch industries, switching tasks at displacement had a significant negative effect on yearly and lifetime earnings.

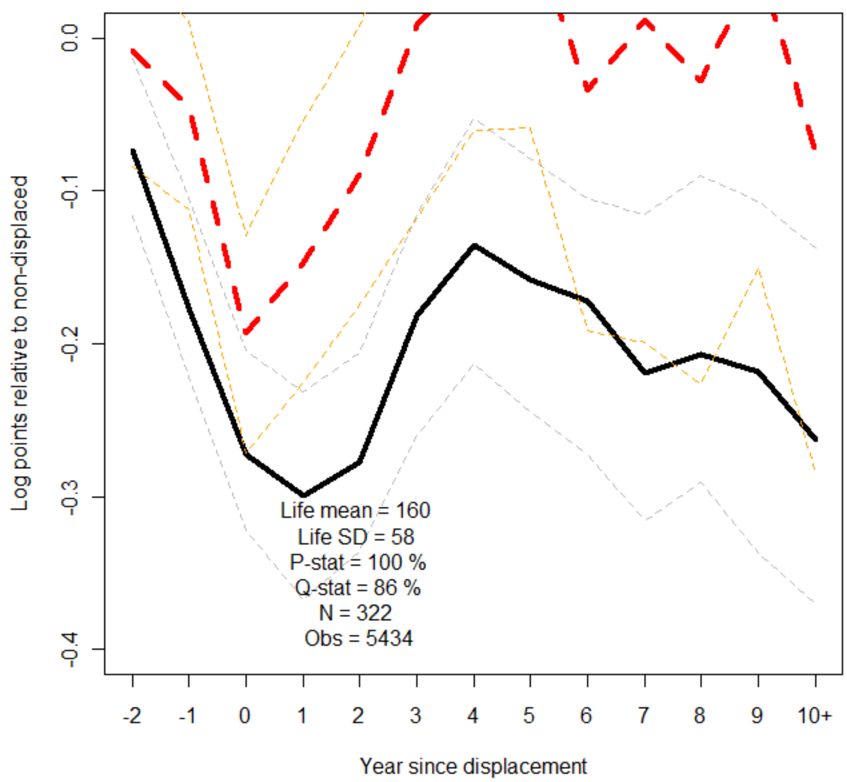

Figure 21: Industry switchers sample; effects of displacement on job task switchers (vs. non-switchers): Coefficients on displacement dummies from estimating a stratified version of equation 7 (stratification on task distance moved) with labor earnings as the dependent variable. This regression used a subset of the analysis sample — which is described in section 3 — which consists of individuals that switched industries at displacement. An age quadratic is the only control. The thick red (dotted) line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals ($\pm 2*$S.E.) of these estimates are also plotted. Standard errors are panel robust.

For industry and occupation switchers the estimates for the control group are very noisy. While the estimates for the treated group are consistent with the main results, I cannot reject the hypothesis that for this sample treatment had no effect (p-stat=62% and q-stat=2%). Similarly, for industry stayers and the combination of occupation switchers and industry stayers the estimates for both the control and treated groups are very noisy. This is most likely because the sample size is very small. Fewer than 31% of displaced workers stay in the same industry after displacement and only about 11% of displaced workers stay in their previous industry and switch occupations.

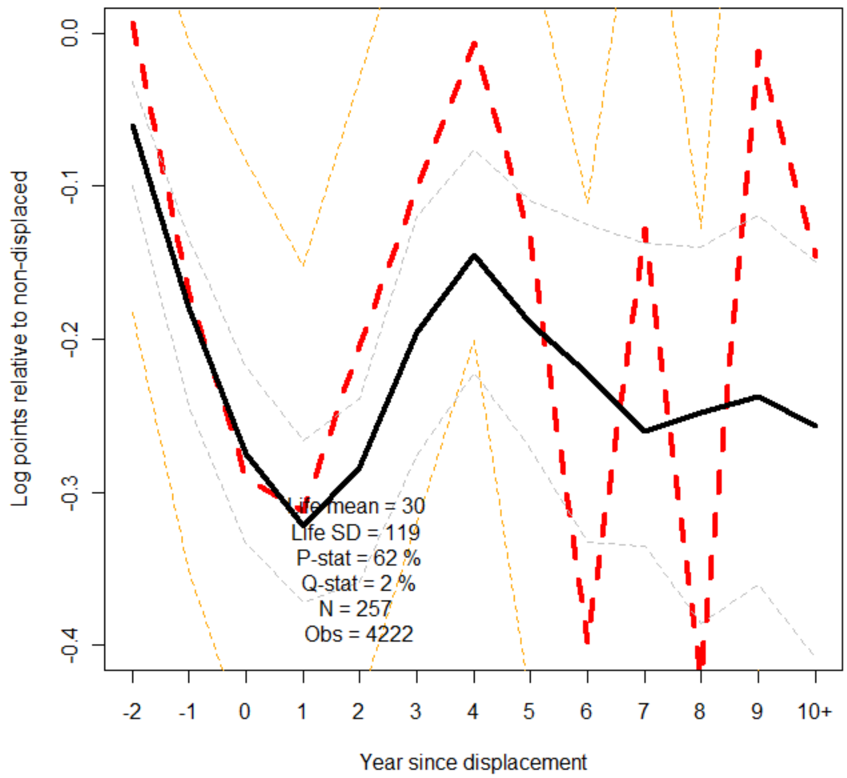

Figure 22: Industry and occupation switchers sample; effects of displacement on job task switchers (vs. non-switchers): Coefficients on displacement dummies from estimating a stratified version of equation 7 (stratification on task distance moved) with labor earnings as the dependent variable. This regression used a subset of the analysis sample — which is described in section 3 — which consists of individuals that switched industries and occupations at displacement. An age quadratic is the only control. The thick red (dotted) line is the marginal effects for non-task switchers and the thick black line is the marginal effects for task switchers who moved the mean distance in task space. Lines representing the confidence intervals (±2∗S.E.) of these estimates are also plotted. Standard errors are panel robust.

While the estimates for industry stayers is inconclusive and I cannot reject the null hypothesis for joint industry and occupation switchers, the evidence from industry and occupation switchers separately is suggestive. Task-specific knowledge appears to be important for the determination of wages in a way that is orthogonal to occupation- or industry-specific knowledge.

8 Conclusion

Domain specific human capital in the form of task-specific knowledge exists and it is transferable. This paper has shown that displaced workers that move far in the task space post-displacement relative to their pre-displacement job lose more human capital than those that do not move far in task space.

Alternatively, these results could be driven by as yet unobserved sub-task specific domains of knowledge, just as task-specific human capital is a sub-domain of occupation. Tasks, for example, can be cognitive, verbal, physical, routine, etc. The obvious next move in the literature is to find these sub-domains and to quantify their importance. The contribution of this paper, however, is not only to help refine what is meant by domain specific human capital, it is to bring into focus the idea that domain specific human capital is transferable.

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	OccSw	TaskSw	Iso	Iso (Sw)	IsoIV	Hours
S(-2)	-0.0433	-0.031	0.030	0.05947	0.0213	-0.0425
	(0.031)	(0.030)	(0.050)	(0.071)	(0.13)	(0.025)
S(-1)	-0.0033	-0.019	0.021	0.04668	0.0669	-0.0425
	(0.032)	(0.029)	(0.055)	(0.083)	(0.12)	(0.026)
S(0)	-0.1736*	-0.178*	-0.220*	-0.11517	-0.1836	-0.1095*
	(0.035)	(0.034)	(0.059)	(0.074)	(0.14)	(0.029)
S(1)	-0.1211*	-0.144*	-0.191*	-0.16499*	-0.1607	-0.0491
	(0.037)	(0.038)	(0.067)	(0.083)	(0.14)	(0.031)
S(2)	-0.1092*	-0.102*	-0.150*	-0.04994	-0.0832	-0.0298
	(0.042)	(0.043)	(0.071)	(0.079)	(0.12)	(0.033)
S(3)	-0.0713	-0.065	-0.178*	-0.04890	-0.0619	0.0135
	(0.044)	(0.041)	(0.077)	(0.088)	(0.13)	(0.039)
S(4)	-0.1138*	-0.089	-0.170*	-0.08060	-0.1154	-0.0075
	(0.051)	(0.046)	(0.076)	(0.093)	(0.13)	(0.036)
S(5)	-0.0298	-0.040	-0.114	0.00065	-0.0075	-0.0350
	(0.054)	(0.051)	(0.092)	(0.126)	(0.22)	(0.039)
S(6)	-0.0785	-0.116*	-0.135	-0.11222	-0.1511	-0.0063
	(0.059)	(0.059)	(0.100)	(0.129)	(0.18)	(0.042)
S(7)	-0.1713*	-0.161*	-0.128	-0.06666	0.0063	-0.0516
	(0.069)	(0.067)	(0.110)	(0.139)	(0.15)	(0.043)
S(8)	-0.1949*	-0.226*	-0.337*	-0.23926	-0.1654	-0.0659
	(0.069)	(0.067)	(0.098)	(0.135)	(0.13)	(0.043)
S(9)	-0.1301	-0.108	-0.062	0.01154	0.0858	-0.0451
	(0.077)	(0.073)	(0.117)	(0.165)	(0.16)	(0.047)
S(10)	-0.1671*	-0.150*	-0.306*	-0.08280	-0.0274	-0.0973
	(0.076)	(0.067)	(0.112)	(0.144)	(0.17)	(0.054)

M(-2)	-0.0448*	-0.057*	-0.037	-0.04829*	-0.0772	-0.0323*
	(0.021)	(0.024)	(0.020)	(0.024)	(0.11)	(0.016)
M(-1)	-0.1550*	-0.146*	-0.077*	-0.15597*	-0.1895	-0.0900*
	(0.025)	(0.028)	(0.021)	(0.031)	(0.11)	(0.019)
M(0)	-0.2521*	-0.235*	-0.221*	-0.23447*	-0.2291	-0.1644*
	(0.029)	(0.031)	(0.024)	(0.031)	(0.14)	(0.022)
M(1)	-0.2901*	-0.269*	-0.250*	-0.28073*	-0.2564	-0.1259*
	(0.032)	(0.032)	(0.027)	(0.034)	(0.14)	(0.027)
M(2)	-0.2420*	-0.236*	-0.208*	-0.23580*	-0.2453	-0.1117*
	(0.034)	(0.035)	(0.028)	(0.039)	(0.14)	(0.028)
M(3)	-0.1897*	-0.183*	-0.203*	-0.17821*	-0.1814	-0.0489
	(0.036)	(0.038)	(0.032)	(0.038)	(0.15)	(0.029)
M(4)	-0.1483*	-0.154*	-0.184*	-0.13844*	-0.1389	-0.0218
	(0.038)	(0.038)	(0.035)	(0.042)	(0.16)	(0.030)
M(5)	-0.1769*	-0.174*	-0.172*	-0.18076*	-0.1938	-0.0681*
	(0.043)	(0.043)	(0.037)	(0.048)	(0.22)	(0.034)
M(6)	-0.1938*	-0.177*	-0.220*	-0.19762*	-0.1636	-0.0645
	(0.044)	(0.044)	(0.041)	(0.051)	(0.18)	(0.036)
M(7)	-0.2243*	-0.231*	-0.254*	-0.22261*	-0.3258	-0.0827*
	(0.049)	(0.051)	(0.046)	(0.052)	(0.18)	(0.036)
M(8)	-0.2538*	-0.242*	-0.267*	-0.25589*	-0.2752	-0.1127*
	(0.053)	(0.050)	(0.048)	(0.060)	(0.16)	(0.037)
M(9)	-0.2573*	-0.278*	-0.259*	-0.26642*	-0.3826	-0.1022*
	(0.058)	(0.059)	(0.044)	(0.066)	(0.20)	(0.040)
M(10)	-0.2891*	-0.295*	-0.276*	-0.28862*	-0.3738*	-0.1060*
	(0.056)	(0.062)	(0.047)	(0.071)	(0.17)	(0.038)
Observations	8225.85	7996.11	10193.97	5160.09	8027.1	8083.76

Individuals	489.63	466.59	698.38	309.42	469.07	471.5

Table 4: Regressions: This table lists the estimates of effects of displacement from various models and samples. The rows labeled with 'S(y)' are estimates of the effect of displacement in year y on stayers and those labeled 'M(y)' are estimates of the effects of displacement on movers. Each column represents a different model. The column labeled 'OccSw' contains the estimates for occupation switchers and movers and corresponds to figure 3. Model 'TaskSw' is the main result of the paper and correspondes to figure 4. 'Iso', 'Iso(Sw)' and 'IsoIV' are the task isolation models where 'Sw' denotes just the task switchers subset and 'IV' are the IV estimates. Model 'Hours' is the same as 'TaskSw' but hours worked in the year are the dependent variable. All models include fixed time and individual effects and a quadratic in age. Stars, *, indicate the estimate is significantly different from zero at the 5% level(this is not, generally, the hypothosis being tested in this paper).