Involuntary Unemployment and the Labor Market Returns to Interim Jobs*

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Abstract

I investigate the wage and employment effects of taking up interim jobs after displacement. Proposing a novel approach, I decompose the wage losses along the distribution into a channel due to the take up of interim employment and a channel which accounts for all other factors. My results show that being employed in an interim job has negative effects on future wages over the whole distribution. This is due to a trade-off between the probability of finding stable employment and higher wages. Assessing the sensitivity of my results, I show that they are robust to failures of my identifying assumptions.

Keywords: Job Loss, Unemployment, Interim Job, Wage Ladder, Wage Loss, Quantile Treatment Effects, Direct Effect, Indirect Effect, Causal Channels

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1 Introduction

During the latest "Great Recession" millions of workers were involuntary displaced and there is doubt that the labor market has fully recovered yet (Farber, 2015). At the same time there has been a substantial rise in the share of workers engaged in alternative and non-standard working arrangements. Alone in the US, it is estimated that more than 15% of all workers are employed in such arrangements (Katz & Krueger, 2016). A similar development can be observed for most European countries (European Comission, 2016). Even though this relationship is driven by many factors, a common response to a job loss is to take up non-standard interim employment (Farber, 1999). Given the increasing number of workers employed in non-standard interim arrangements over the last years and continuing prevailing economic uncertainty investigating this relation is of particular interest.

The association between displacement and long-lasting earnings losses is now well explored in the literature. Starting with the seminal work of Jacobsen, LaLonde & Sullivan (1993), who showed that displaced workers suffer from long-lasting earnings losses, subsequent research has tried to determine the factors which can explain the long-run wage losses of displaced workers. These existing papers have mostly focused on certain subgroups or characteristics, such as industry switchers (Neal, 1995), repeated job displacement (Stevens, 1997), re-training (Jacobsen, LaLonde & Sullivan, 2005), periods of non-employment (Hijzen, Upward & Wright, 2010), and local labor market conditions (Couch & Placzek, 2010). Despite a large part of the literature assessing the consequences of an involuntary job loss on future earnings, not much is known about the interconnection between involuntary displacement, interim jobs, and the future careers of displaced workers.

In this work, I assess the impact of taking up non-standard interim employment immediately after an involuntary job loss on wages and employment. I decompose the treatment effect of an involuntary job loss on wages into an "indirect" effect working through the take up of an interim job immediately after displacement (mediator) and a "direct" effect which accounts for all remaining channels over the whole distribution. Estimating the indirect effect in this way allows me to quantify the wage ladder effect of an interim job.

From a theoretical point of view the effect of taking up interim employment after displacement on the future wages and employment of works is ambiguous. Displaced individuals face the choice of staying unemployed and looking for high paying work or to accept initially lower paying jobs with the possibility of a steeper wage trajectory and/or stable employment later on. This can be the result of the arrival of better job offers and more efficient search once employed and the slow down of human capital depreciation. (see, for example, Acemoglu (1995) and Faberman, Mueller, Sahin & Topa (2017)). Taking up interim jobs can, in addition, isolate the worker from repeated displacement risk which can have detrimental future effects on wages (e.g. Stevens (1997), Jarosch (2015), and Krolikowski (2017)). It is also possible that the take up of interim employment and transition into secure jobs comes at the cost of a lower wage trajectory. This happens, for example, if workers face a trade-off between employment probabilities and wages.[1]

My analysis complements and extends the existing findings in the displacement literature in at least two ways. First, I am interested in the effect of a particular form of employment, interim jobs, taken up within a short time period after displacement on medium-run wages and examine whether these jobs help displaced workers to "climb the wage ladder". My decomposition gives valuable insights into how interim jobs affect future wages and help to identify particularly affected groups. This would be missed by concentrating on mean effects. The approach does not rely on a structural model or functional form assumption and identification is achieved through a sequential conditional independence assumption similar to those imposed in the program evaluation literature (see, for example, Imbens & Wooldridge (2009)). This gives arguably more robust decomposition results.

Second, by exploiting the decomposition over the whole distribution I am able to investigate the link between wage effects of interim jobs and the possibility to transit into

[1] Delacroix & Shi (2006) propose a directed on-the-job search model which generates this type of outcomes.

stable employment. Farber (1999) provides evidence that taking up non-standard working arrangements, such as working as a freelancer, after a job loss is part of a transition process toward stable employment. My decomposition allows me to assess how and if taking up interim jobs is connected to future employment stability and if this process is associated with higher wages.

From a methodological point of view, my paper contributes to the literature on mediation analysis. Since the pioneering work by Baron & Kenny (1986), the estimation of direct and indirect effects has moved towards a more flexible approach (e.g. Robins (2003), Flores & Flores-Lagunes (2009), Tchetgen Tchetgen & Shipster (2012), and Huber, Lechner & Mellace (2016)). Most of these approaches concentrate on mean outcomes despite the importance and relevance of quantile effects.

I propose to estimate direct and indirect unconditional quantile effects by means of a flexible weighting approach using two different propensity scores. This method extends the estimator proposed by Firpo (2007) to mediation analysis.[2] To the best of my knowledge, Shen, Chou, Pentz & Berhande (2014) and Imai, Keele & Tingley (2010) are the only two papers besides mine which propose estimating mediating effects on quantiles under sequential conditional independence assumption.

Using a large administrative data set from Austria and exploiting the timing of displacement, I show that holding an interim job has a negative effect on wages earned one year after the displacement along the whole distribution. My indirect effect can explain a substantial amount of the total loss which accounts for up to 30% of the overall estimated wage loss. I show that this negative impact is partly due to a trade-off between future employment probabilities and wage growth, especially in the upper part of the wage distribution. While holding an interim job increases future employment days in general, I find that workers for whom the relative effect of holding an interim job is large are up to 36 days more employed in the year after the displacement compared to those workers for whom the impact is relatively low. My findings suggest that the wage ladder is

[2]Using a weighting-type estimator to obtain direct and indirect effects but concentrating on mean outcomes are used in, for example, Hong (2012), and Huber (2014). Frölich & Huber (2017) propose non-parametric identification when endogeneity is present in the mediator and/or treatment. Their estimators are also based on a weighting approach.

not necessarily intertwined with future employment, at least during a short time horizon around the lay-off. They highlight the delicate task for policy makers of balancing wage growth and stable re-employment after an involuntary job loss.

This paper proceeds by first describing the data, the definition of my treatment and control group, as well as interim employment as mediator. In Section 3, I discuss my decomposition approach. The selection process into treatment and the mediator is described in Section 4. The results of the decomposition are presented in Section 5. In Section 6, I discuss and review the career path of displaced workers with interim employment. I assess the sensitivity of my results by considering specific failures of my identifying assumptions in Section 7. Section 8 concludes.

2 Data and Selection Process

My analysis is based on the Austrian Social Security Data, an administrative data set which comprises of the whole universe of Austrian workers employed in the private sector. A key feature of the Austrian Social Security Date is that I have access to information about daily labor market spells, the exact employment category of the spell, and various background characteristics. Due to a unique person identifier I can link every individual to firms. Zweimüller, Winter-Ebmer, Lalive, Kuhn, Wuellrich, Ruf & Büchi (2009) provide a detailed overview of this data set.

2.1 Treatment and Control Group

I use a firm closure as a quasi-experimental setting for an involuntary job loss and concentrate in my analysis on young male workers between 31 and 35 years old. Concentrating on the impact of an involuntary job loss and the effect of interim jobs for younger male workers is particularly interesting as they are at the beginning of their working life.[3] In

[3]There is evidence that women (but not men) at this age group are more concerned with employability after an employment shocks (Del Bono, Weber & Winter-Ebmer, 2012). I also find in my data that females at age 31 to 35 are positively selected, especially when comparing to women displaced at an older age. The results for women are qualitative similar but less precisely estimated over most part of the distribution and available upon request.

addition, benefits legislation is fairly homogeneous for this age group and is based on previous employment rather than age at displacement. [4] In the appendix, I also show that my results do not depend on the specific age interval used.

As the disappearance of a firm identifier in the data does not necessarily imply the closure of the firm, identifying plant closures is based on a worker flow approach. In particular, I follow the definition of Fink, Kalkbrenner, Weber & Zulehner (2010), see also Del Bono et al. (2012), and regard the disappearance of a firm identifier as a plant closure while excluding firms where more than 50% of the workforce jointly transits to the same new employer in the last year of the existence. I consider all plant closures during the years 1995 until 2005 and define the quarter of exit as the closing date.

A natural way to obtaining my estimates would be to compare workers who experienced a plant closure to those who have not. One concern with this approach is, however, that workers might sort themselves into closing firms or interim employment on unobservable characteristics. In this case, results from my decomposition are likely to deliver biased results. In addition, when following this strategy it might be difficult to find appropriate control observations once the common support is taken into account.

For my main analysis, I pursue a different route and compare the outcome of workers who experienced a plant closure and define the treatment and control group in terms of the age at the actual lay-off; see also Fradkin, Panier & Tojerow (2017) for a similar strategy. I consider all workers who were employed in a closing firm up to one year before the exit date and were, at the time of the exit, between 31 and 35 years old as treated and assign the actual closing date or the date when they were last employed by the closing firm, whatever comes first, as the reference quarter.

To define my control group, I select all workers who were displaced at age 36 to 39 but use their labor market outcomes four years *before* the actual event. For example, for a worker who was displaced in the second Quarter of 2000 and was, at the time of the

[4] In the Austrian benefits system, workers who were displaced after the age of 40 can receive an extension of the duration of benefits payments based on the age at displacement.

displacement 38 years old, I use the second Quarter of 1996 as the reference date for his labor market outcomes.

The assumption behind this strategy is that workers in my sample are similar in terms of unobservable characteristics and preferences, and only distinguish themselves in the age at the lay-off. My estimates might still be biased, however, if workers in my control group anticipate the future lay-off. In Appendix E, I show that anticipation of a future plant closure is likely of no concern over my evaluation period. In addition, I provide additional estimation results with an alternative control group.

In my analysis, I concentrate on established male workers who had a minimum of 2 years of labor market experience of which at least 1 year at the current firm before the reference date and/ or were employed in companies with less than 3 or more than 350 employees. The first restriction ensures that my workers have a strong attachment to the labor market and have the right to claim unemployment benefits for at least 20 weeks. Notice that this restriction also imposes that individuals in my sample did not have any unemployment spell in the 2 years before the reference date. I impose the second restriction as firm closures with less than 3 employees are difficult to identify with the worker flow approach, while closures of larger firms is an extremely rare result.[5]

From this final sample, I exclude all individuals who did not hold paid employment the year after the reference date, that is 5 to 8 quarters afterward. These restrictions are similar but slightly weaker compared to previous studies on this topic like Jacobsen et al. (1993) and Couch & Placzek (2010).[6]

I calculate all pre-treatment variables until the beginning of the reference quarter, while all post-treatment variables are defined from the start of the reference quarter onward. This calculation is straightforward for all employment outcomes, as the data

[5] I start with a total of 24,512 individuals with 2 years of labor market experience and lose 2,385 individuals because of the tenure restriction and 2,360 individuals because of the firm size restriction. Imposing these restrictions do not alter the conclusions derived in this work, but improve balancing. Less restrictive selection criteria, for example requiring only 1 year of labor market experience or including larger firms in the analysis, give similar results. These are available upon request.

[6] Both Jacobsen et al. (1993) and Couch & Placzek (2010) are interested in long-run effects and include workers only if they report positive earnings in every year after the job loss. I concentrate only on the year after the displacement. This restriction excludes 1,357 individuals additionally from my analysis.

consists of daily labor market spells. However, the ASSD only contains yearly wages for an individual employed in a given firm.

To obtain wages which are measured over the same period as my pre- and post-treatment labor market outcomes some adjustment is needed. I do so by weighting the yearly wage with the days worked between the reference dates. Consider the worker from the previous example; I calculate the wage in the year directly preceding the reference date by first obtaining the total employment days in a given firm i in 1995 and 1996 respectively, $total_j^{i,1995}$ and $total_j^{i,1996}$. I then calculate for this individual the days employed between the beginning of the 2nd Quarter in 1995 until the end of 1995 in firm i, $duration_j^{i,2Q1995}$, and the days employed between the beginning of the year 1996 and the 2nd Quarter in 1996, $duration_j^{i,2Q1997}$. Finally, the yearly wage is then obtained as a weighted sum $wage_j(t-1) = \sum_{i \in F_{1995}} wage_j^{i,1995} \cdot \frac{duration_j^{i,2Q1995}}{total_j^{i,1995}} + \sum_{i \in F_{1996}} wage_j^{i,1996} \cdot \frac{duration_j^{i,2Q1996}}{total_j^{i,1996}}$, where $wage_j^{i,t}$ is the yearly wage rate in firm i and F_t is the set of all firm in which the individual was employed in at year t. This procedure ensures that all my variables are measured over the same time span.

2.2 Interim Employment & Outcome

There is no official definition of what is regarded as alternative working arrangement or interim job. Farber (1999) considers an individual employed in an alternative working arrangement if she is working as an independent contractor, consultant or freelancer, is otherwise self-employed, or has only temporary work, on-call work, and contract work. On one side, these type of jobs can be described by both a large degree of flexibility with respect to working hours and place, and likely lower job security compared to standard working arrangements.[7] On the other side, interim employment might be a stepping stone to permanent employment (Farber, 1999; Ichino, Mealli & Nannicini, 2008). In addition,

[7] There is evidence that some workers might generate a higher level of utility from flexible type of works (see, for example, Mas & Pallais (2017)).

being employed regardless of the job characteristics might lead to more efficient search for other jobs (e.g. Faberman et al. (2017)). [8]

In this work, I consider as an interim job those employments which are categorized as marginal employment in my data, freelancer agreement and/or if an individual holds more than one job at once. These definitions are similar to the ones of Farber (1999). The monthly wage of a marginal employed worker is bounded above by around 400 Euros per month, but the workers have in general the same rights as those with higher earnings. A freelancer is employed in a company usually only over a pre-specified period. Furthermore, I consider employees working in two or more parallel jobs as being employed in an interim job as a proxy for holding part-time work.[9]

I define the mediator as a binary variable which takes the value of one if I observe an interim job during the first three months after the reference date, and it takes a value of zero otherwise. Notice that this is a very short time period between the actual treatment and the time I measure my mediator which ensures that selectivity is less of an issue and is, for example, not driven by the end of benefit payments. All individuals in my sample are eligible for at least 20 weeks of unemployment benefits, or around 4.5 months, which is substantially longer than the time period during which I measure my mediator.

The outcome of interest is the wage rate in the year *after* the reference date, i.e. the wage earned 5 to 8 Quarters after the reference date. This period is of particular interest as previous research on the topic suggests that until the end of the second year after the unemployment spell a majority of the wage growth has taken place (see, for example, Couch & Placzek (2010) and Jacobsen et al. (1993)). As stated above, I only have information about yearly earnings but I know for each worker the firms these earnings are coming from, and the time worked in that respective firms. Hence, my outcome variable is adjusted in a similar way as described in the previous section.

[8] In addition, individuals might opt to take up interim employment instead of staying unemployed in order to contribute to pension payments and to other social security coverage.

[9] I do not observe hours worked in my data and can therefore not base the definition on this measure.

2.3 Summary of Sample

Table 1 presents descriptive statistics by treatment status. Looking at personal characteristics, one can see that displaced workers are slightly younger and are more likely to have no children. They are also more likely to be Austrian and to hold a university degree. With the exception of educational attainment, these differences, although significant, are quite small in magnitude.

I come to a similar conclusion when looking at the difference in terms of previous labor market experience. Displaced workers spend on average 1.16 fewer days in employment and 0.72 more days in unemployment which is significant but rather small. It is unlikely that such a difference is driving my results. I do not find any difference in pre-displacement wages. If at all, the summary statistics indicate that displaced workers seem to earn slightly more than individuals in my control group.

Firm characteristics seem to matter the most when explaining treatment status. Closing firms are on average significantly smaller and younger, but I do not find that they have a lower wage level on average. Furthermore, closing firms are more likely to operate in the construction, tourism, and financial service sectors.

One can also observe a large difference in the mediator. Around 10% of displaced workers have taken up interim employment within 3 month after the plant closure, compared to around 1.9% in the control group. This pattern is in line with the findings of Farber (1999) for the US and shows that taking up non-standard working arrangements is a common response to displacement in Austria.

Table 2 presents descriptive statistics by mediator status as well. As before, the observed differences in personal and labor market characteristics are small and they are mostly insignificant. The only exception is the pre-displacement wage rate. Workers with interim employment earned previous to displacement on average 1,420 Euros less than those without this type of employment.

I also observe significant differences in firm characteristics. Those who take up interim employment are more likely to be previously employed in younger and smaller

Table 1: Summary Statistics by Treatment Status

	Treatment Group $T = 1$	Control Group $T = 0$	Difference
Personal Characteristics			
Age	32.86	32.98	-0.12 ***
Non-Austrian	13.48%	14.02%	-0.54
Children	55.63%	57.48%	-1.84 *
University Degree	9.75%	6.90%	2.84***
Labor Market Characteristics			
Av. Wage (1'000 Euros)	31.71	31.52	0.19
Av. Employment (Days)	358.31	359.47	-1.16 ***
Av. Unemployment (Days)	2.97	2.26	0.72***
Local Unemployment Rate	9.19	9.19	0.00
Firm & Sector Characteristics			
Firm Characteristics			
Firm Age	13.45	16.35	-2.91 ***
Av. Firm Size	36.20	64.46	-28.26 ***
Av. Firm Wage (Euros per Day)	66.03	66.08	-0.05
Local Area			
Vienna	31.69%	29.35%	2.34***
North-East	18.27%	18.94%	-0.67
South-East	11.05%	11.41%	-0.36
North-Mid	15.59%	17.75%	-2.17 ***
Mid	7.74%	7.47%	0.27
West	9.77%	9.47%	0.30
South	5.89%	5.61%	0.28
Sector			
Agriculture & Mining	1.44%	1.29%	0.15
Production	27.69%	31.54%	-3.85 ***
Construction	20.87%	19.62%	1.26*
Commerce	21.40%	22.63%	-1.23 *
Tourism	6.08%	5.17%	0.91**
Transport	6.89%	6.96%	-0.07
Others	15.45%	12.50%	2.96***
Mediator			
Interim Employment	10.15%	1.87%	8.27***
Observations	8,270	10,140	

*,**,*** indicate a significance difference at a 10%, 5% and 1% level. The treatment group consists of all workers displaced during the age 30 to 35. The control group consists of all workers displaced at age 36 to 39. Interim Employment (First Quarter) and Interim Employment (First & Second Quarter) are dummy variables which take values of one if the worker had at least one parallel employment spell or held marginal and/or freelancing employment during the first 91 days and 182 days after the reference date respectively. Average Wage, Employment, and Unemployment were measured using the 3 years preceding the reference quarter. The Local Unemployment Rate was measured the year before the reference date.

firms. This might be the due to network effects if larger firms provide a better networks through which individuals can obtain information about better employment opportunities (e.g. Cavalo-Armengol & Jackson (2004)).

Figure 1 provides evidence that displaced individuals hold interim employment indeed only for a limited period of time. It depicts for Quarters 2 to 8 after the reference date the shares of individuals who hold an interim employment in a given quarter (solid line) conditional on being displaced and holding interim employment at the reference quarter. In addition, I also plot the share of workers who were at least 85 days employed subjected to social security payment in a given quarter for the same group (dashed line)

As one can see the share of individuals holding interim employment substantially drops from 19% in the quarter after the reference date to around 4.5% after three quarters after the displacement. After that, the share of workers in interim employment remains stable at around 4%. At the same time, one can observe a substantial rise in the share of individuals working at least 85 days in a quarter. It increases from 65% in the quarter after the reference date to around 84% after three quarters and remains relatively stable afterward. I discuss the connection between interim jobs and future employment in more detail in Section 6.

The mean differences presented in this section are without controlling for any background characteristics. A formal assessment of the balancing properties in my sample are provided in Section 4. Next, I describe my decomposition approach.

3 Econometric Framework

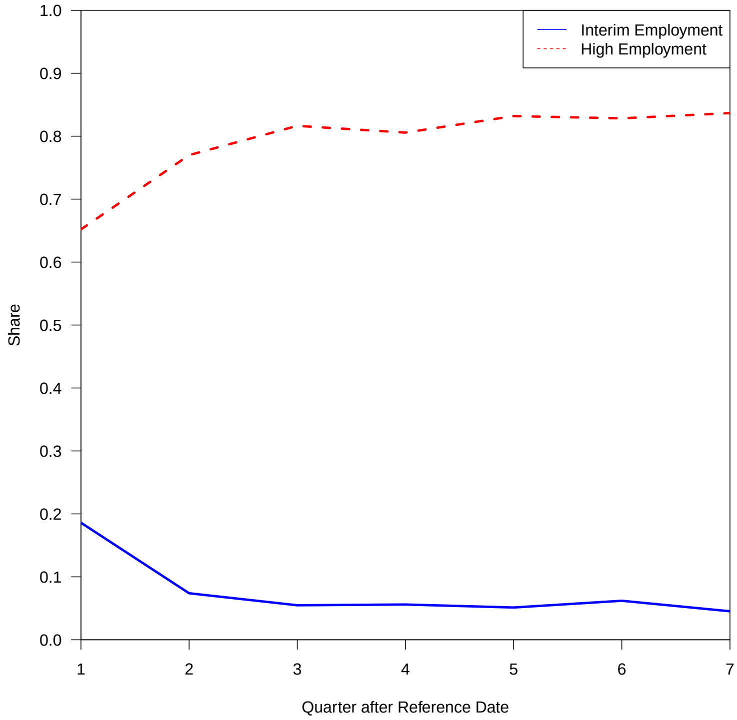
I am interested in decomposing the total effect of an involuntary job loss, the treatment, on future earnings into an "indirect effect" or wage ladder effect which operates through holding an interim job shortly after the displacement (mediator), and a "direct effect", which accounts for all other channels. The evaluation of these effects is called mediation analysis (see, for example, Baron & Kenny (1986) for an earlier work). It has been widely

Table 2: Summary Statistics by Mediator Status

	Interim Employment $M = 1$	No Interim Employment $M = 0$	Difference
Personal Characteristics			
Age	32.85	32.93	-0.08
Non-Austrian	15.74%	13.66%	2.08
Children	57.05%	56.63%	0.42
University Degree	7.97%	8.19%	-0.22
Labor Market Characteristics			
Av. Wage (1'000 Euros)	30.27	31.69	-1.42 ***
Av. Employment (Days)	358.34	358.99	-0.65
Av. Unemployment (Days)	2.89	2.56	0.33
Local Unemployment Rate	9.22	9.19	0.03
Firm & Sector Characteristics			
Firm Characteristics			
Firm Age	13.64	15.13	-1.49 ***
Av. Firm Size	47.46	52.02	-4.56 **
Av. Firm Wage (Euros per Day)	64.22	66.17	-1.95 ***
Local Area			
Vienna	27.02%	30.60%	-3.59 *
North-East	15.74%	18.81%	-3.07 **
South-East	19.34%	10.77%	8.57***
North-Mid	16.52%	16.79%	-0.27
Mid	6.71%	7.64%	-0.93
West	8.75%	9.65%	-0.91
South	5.93%	5.72%	0.20
Sector			
Agriculture & Mining	1.26%	1.36%	-0.10
Production	32.65%	29.64%	3.01*
Construction	20.12%	20.18%	-0.07
Commerce	18.85%	22.27%	-3.42 **
Tourism	5.54%	5.58%	-0.04
Transport	7.87%	6.88%	1.00
Others	13.51%	13.84%	-0.33
Treatment			
Displaced at Age 31-35	81.54%	42.75%	38.78***
Observations	1,029	17,381	

*,**,*** indicate a significance difference at a 10%, 5% and 1% level. Interim Employment is a dummy variable which takes a value of one if the worker had at least one parallel employment spell or held marginal and/or freelancing employment during the first 91 days after the reference date. The treatment group consists of all workers displaced during the age 31 to 35. The control group consists of all workers displaced at age 36 to 39. Average Wage, Employment, and Unemployment were measured using the 3 years preceding the reference quarter. The Local Unemployment Rate was measured the year before the reference date.

Figure 1: Share of Jobs and Employment by Distance to Reference Quarter



The solid line depicts the share of displaced individuals who held interim employment and the dashed red line depicts the share of individuals who were employment at least 85 days in a given quarter subject to social security contribution in the Quarters 2-8. Both shares were calculated conditional on holding an interim job in the reference quarter.

used in epidemiology, statistics and political science, and it is becoming increasingly popular in economics (see, for example, Flores & Flores-Lagunes (2009), Heckman, Pinto & Savelyev (2013), Huber (2014), Heckman & Pinto (2015), Huber et al. (2016), and Chen, Chen & Liu (2017)).

Most of the previous literature in mediation analysis has concentrated on decomposing the mean effect. The wage ladder effect of holding an interim job might differ across the wage distribution however. It is possible that workers in jobs with relatively low payment benefit from rapid re-employment by staying active regardless of the quality of the employment taken. If interim jobs lead to faster re-employment, stable employment trajectory, and the avoidance of repeated job loss in the future then it is also possible that individuals at the upper part of the wage distribution benefit more from taking up interim employment. These workers have the most to lose from repeated unemployment spells and unstable employment trajectories. Concentrating on mean effects can miss these important points.

In this work, I propose to decompose the treatment effect across the distribution of the outcome and concentrate on the treated sub-population, a quantity of special interest for policy makers.[10] My approach has the advantage of recovering the unconditional effects which are often the quantity of interest. This is done without facing the difficulties of integrating over the distribution of covariates as in Imai, Keele & Tingley (2010).

To define my parameters of interest, the direct and indirect quantile effects, sometimes referred to as pure direct and indirect effects (Robins & Greenland, 1992) or natural direct and indirect effects (Pearl, 2001), I make use of the potential outcome framework also used, for example, by Albert (2008) and Huber et al. (2016). Define the binary treatment indicator of an involuntary job loss as T, my outcome of interest by Y, and the mediator by M.

[10] The majority of the literature on mediation analysis concentrates on the entire population, with a few exceptions such as Huber et al. (2016) and Vansteelandt & VanderWeele (2012).

Denote by $Y(t)$ and $M(t)$ the potential outcome and mediator under treatment $t \in \{0,1\}$ respectively. I can express the Quantile Treatment Effect on the Treated, $\Delta_{\tau,T=1}$, for a given quantile τ as defined in Firpo (2007):

$$\Delta_{\tau,T=1} = q_{1,\tau|T=1} - q_{0,\tau|T=1} \tag{1}$$

where $q_{t,\tau|T=1} \equiv \inf_q Pr\left[Y(t) \leq q|T=1\right] \geq \tau$ and $t \in \{0,1\}$. This effect is the object of interest in the standard evaluation literature, but it does not allow to determine any underlying causal mechanisms.

To see how the decomposition works and to identify the underlying causal channels rewrite the potential treatment as a function of the mediator $Y(t) = Y(t, M(t))$. Equation (1) can be expressed as

$$\begin{aligned}
\Delta_{\tau,T=1} = \quad & q_{1,\tau|T=1} - q_{0,\tau|T=1} \\
= \quad & \left(q_{(1,M(1)),\tau|T=1} - q_{(0,M(1)),\tau|T=1}\right) \\
& + \left(q_{(0,M(1)),\tau|T=1} - q_{(0,M(0)),\tau|T=1}\right) \\
= \quad & \theta_{\tau,T=1} + \delta_{\tau,T=1}
\end{aligned} \tag{2}$$

where now $q_{(t,M(j)),\tau|T=1} \equiv \inf_q Pr\left[Y(t, M(j)) \leq q|T=1\right] \geq \tau$ and $t, j \in \{0,1\}$.

$\delta_{\tau,T=1}$, the indirect effect, is the object of main interest in this work and captures the effect of the mediator on the outcome at a given percentile τ. More precisely, it gives an estimate of the change in the future wage at percentile τ among displaced workers corresponding to a change in the mediator from the value what would be observed under control, $M(0)$, to the value that is observed under treatment, $M(1)$, while holding the treatment constant at $T = 0$. By keeping the treatment status fixed and varying the mediator over different treatment states, I can isolate the wage ladder effect through which interim employment affects future wages. The direct effect $\theta_{\tau,T=1}$ accounts for all other causal channels which are not attributable to the mediator by holding the mediator status constant at $M(1)$ and varying the treatment over different levels.

Notice that the direct and indirect effect is defined over opposite treatment states. In the above decomposition, I follow Vansteelandt & VanderWeele (2012) and define the direct effect using the potential mediator under treatment, $M(1)$, which is also used in Huber et al. (2016). Vansteelandt & VanderWeele (2012) argue that this constitutes a natural reference level for treated subjects as it corresponds to the direct effect under the actual choice of the treated.

Under the identification assumptions discussed in Appendix A, the counterfactual quantiles can be estimated using a two-step procedure. In a first step, I estimate $\pi(X) = Pr(T = 1|X)$ and $\pi(X, M) = Pr(T = 1|X, M)$. Here $\pi(X)$ is the propensity score as defined in the standard treatment effects literature and $\pi(X, M)$ is the propensity score including the mediator. The propensity scores are then used to construct weights $\omega_{(0,M(1))}$, $\omega_{(0,M(0))}$ and $\omega_{(1,M(1))}$. For person i these weights are defined as follows:

$$\begin{aligned}
\omega_{i,(0,M(1))} &= \frac{(1-T_i)}{\pi} \cdot \frac{\pi_i\,(X, M)}{(1-\pi_i\,(X, M))} \\
\omega_{i,(0,M(0))} &= \frac{(1-T_i)}{\pi} \cdot \frac{\pi_i\,(X)}{(1-\pi_i\,(X))} \\
\omega_{i,(1,M(1))} &= \frac{T_i}{\pi}
\end{aligned}$$

In the second step, I estimate $\Delta_{\tau,T=1}$, $\theta_{\tau,T=1}$ and $\delta_{\tau,T=1}$ as defined in Equation (2). Every component of my decomposition can be obtained via a weighted version of the quantile regression proposed by Koenker & Basset (1978):

$$\begin{aligned}
q_{(0,M(1)),\tau|T=1} &= argmin_q \frac{1}{N}\sum_{i}^{N} \omega_{i,(0,M(1))} \cdot \rho_\tau(Y_i - q) \\
q_{(0,M(0)),\tau|T=1} &= argmin_q \frac{1}{N}\sum_{i}^{N} \omega_{i,(0,M(0))} \cdot \rho_\tau(Y_i - q) \\
q_{(1,M(1)),\tau|T=1} &= argmin_q \frac{1}{N}\sum_{i}^{N} \omega_{i,(1,M(1))} \cdot \rho_\tau(Y_i - q)
\end{aligned}$$

where $\rho_\tau(p) = p \cdot (\tau - \mathbb{1}\{p < 0\})$ is the check function.

The proposed approach has two advantages compared to existing quantile meditation models such as Imai, Keele & Tingley (2010) and Shen et al. (2014). First, it allows me to obtain unconditional direct and indirect effects, while Imai, Keele & Tingley (2010) estimate conditional-on-covariate versions. How to obtain the unconditional effects in Imai, Keele & Tingley (2010) is not immediately clear. Furthermore, unconditional effects are usually of primary interest, and are more important for decision makers. Second, it does not rely on an additive outcome model as Imai, Keele & Tingley (2010) and Shen et al. (2014). Compared to the causal step method in Shen et al. (2014), the identification of the direct and indirect effects are formally derived. The approach presented here imposes less functional form restriction and more flexible. It complements and extends these existing ones.[11]

It should be mentioned that the decomposition in Equation (2) evaluates the current status quo. It quantifies how much of the total wage loss can be explained by taking up interim employment and how much can be attributed to other mechanisms. The decomposition does not allow to evaluate the efficient assignment of interim employment to displaced workers in order to improve the status quo.

4 Selection Process

Central to my identification is that I am able to account for all confounding factors which might lead to an involuntary job-loss due to a plant closure but might also affect earnings and the mediator. Furthermore, I need to be able to control for all factors which jointly affect earnings and the mediator given the treatment status, and to ensure there is no confounding after the treatment.[12]

[11] My method also differs in important ways from the approach suggested by Machado & Mata (2005). First, the decomposition in my paper is explicitly based on a causal model with the goal to obtain counterfactual estimates. This stands in contrast to Machado & Mata (2005) who decompose the effect along the distribution without causality as the primary objective in mind, although they do not claim to provide a causal decomposition. Second, my approach does neither rely on simulation methods nor additivity of the underlying model and therefore offers greater flexibility.

[12] Overlap can be imposed using trimming methods and monotonicity of the quantiles can be obtained by re-arrangement.

The probability of experiencing a plant closure depends first and foremost on employer and sector characteristics. Firms which operate in competitive sectors or those who are less established in their markets might be more prone to failure. Moreover, higher historical employee fluctuations might be a sign of firm quality. In my analysis, I include various firm characteristics to account for these confounding factors. In addition, I control for worker characteristics which might be related to sorting into specific firms, such as education, age, and previous unemployment spells. Region dummies and past sectoral unemployment rates capture regional differences and sectoral shocks.[13]

Selection into the mediator most likely depends on the same individual and firm characteristics as the selection into treatment. Being employed in specific sectors or having a certain educational level might have an effect on future employment probabilities. Moreover, financial needs can create pressure on individuals to take up employment faster. I do not have personal wealth in my data but I can proxy for it by using pre-displacement annual wage rates. Furthermore, I include various covariates in my analysis which account for past labor market history of an individual. For example, individuals with higher labor market experience are entitled to collect unemployment benefits for a longer time period and might be more reluctant to compromise on job quality.

In order to conserve space, I briefly discuss the results from my propensity score estimations here while presenting detailed results in Appendix B. The propensity scores were estimated using a series-logit estimator with the total number of series terms being 3 for the baseline sample. The results from the estimation of $\pi(X)$ confirm the conjectures drawn from the summary statistics. Firm- and sector characteristics are strong and highly relevant predictors for being observed in the treatment group. Being previously employed in less established firms and smaller companies is related to a higher displacement risk. The significance of the included higher order terms points toward important nonlinearities with respect to these variables. Similarly, high educational attainment is associated with a higher probability of getting displaced. I do not find that previous labor market characteristics matter once I control for all other background variables. This supports

[13] I do not include current unemployment rate as it is most likely affected by the treatment, invalidating Assumption 1.

my strategy of exploiting the timing of the displacement rather than to use non-displaced workers as control observations.

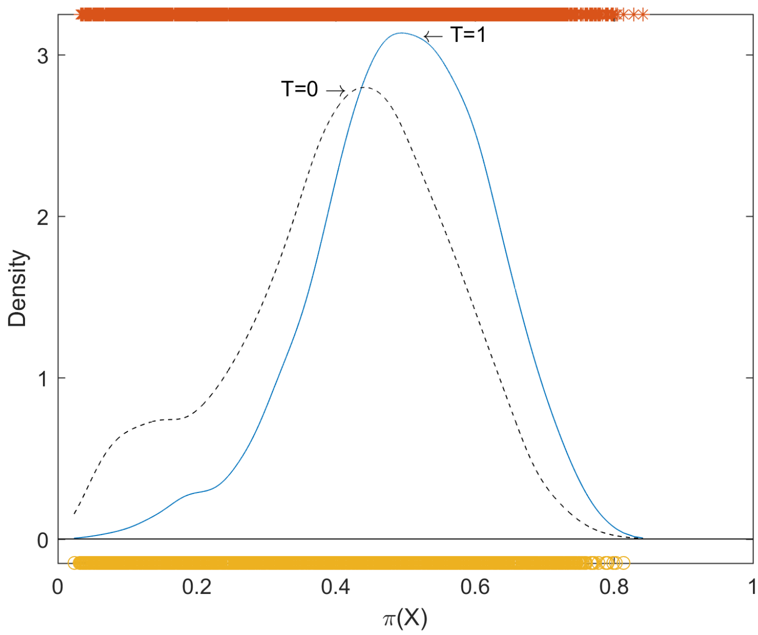
Including the indicator if an individual has taken up interim employment within three month after the reference date, my mediator, in the estimation of the propensity score barely changes the coefficients on the background variables and their significance level. The stability is re-assuring and shows that my mediator is unlikely to pick up other unobserved components related to the treatment. The estimated coefficient on the mediator is large and highly significant.

Figure 2 shows the distribution of the estimated propensity scores. Panel a. and b. contain the estimated density with and without the mediator by treatment status. From the figure it is clear that the overlap between both groups is good in general. There might be, however, only a few comparable control observations at the upper part. To account for this possible violation in the overlap, I will apply the trimming method as outlined in Section A in the Appendix. All estimation results reported in the main part of the paper are based on the trimmed samples. Results based on the untrimmed samples are virtually identical and are presented in Appendix C.

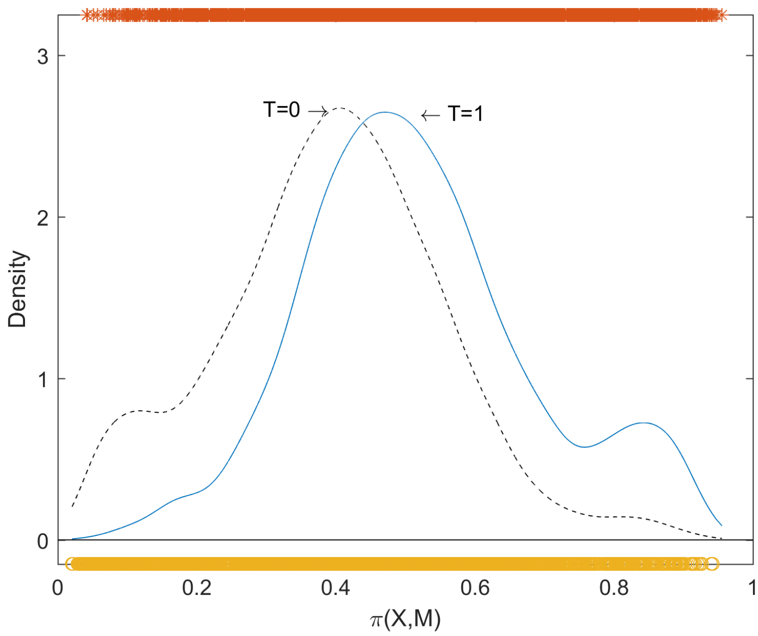
In order to assess whether my propensity score models are misspecified or the conditional independence assumption is violated, I conduct a set of balancing tests. The results are presented in Figure 3. The depicted box plots show the distribution of the Standardized Difference in Means (Rosenbaum & Rubin, 1985) and t-values for differences in means, with the lower and upper ends of the box representing the 25th and 75th percentile respectively. The vertical lines (whiskers) show the 5th and 95h percentile, and the stars outliers.

Balancing works reasonably well in my sample with and without the mediator. Once I adjust for observable covariates, both the maximal Standardized Difference in Means and t-value is around 5 and 1.4 respectively. These values are below any level which would give rise to concern.

Figure 2: Distribution of Propensity Scores



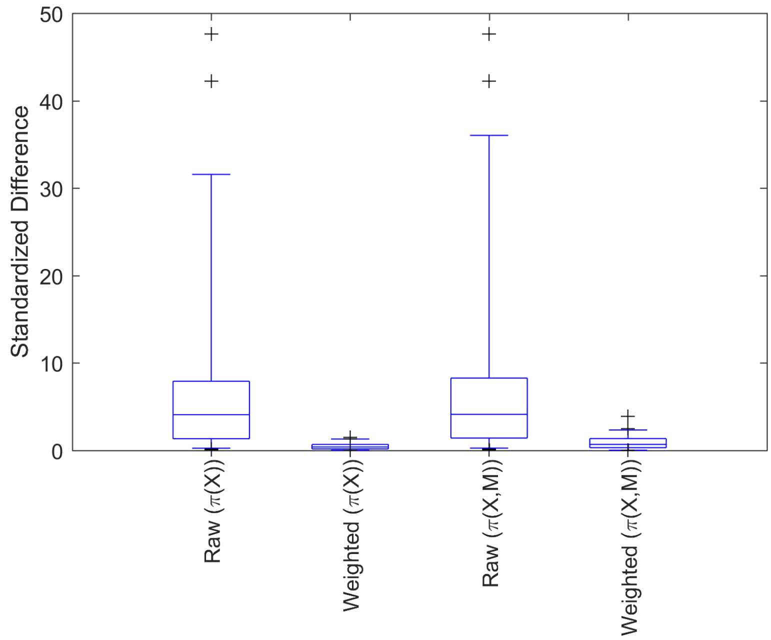
a. Distribution without Mediator



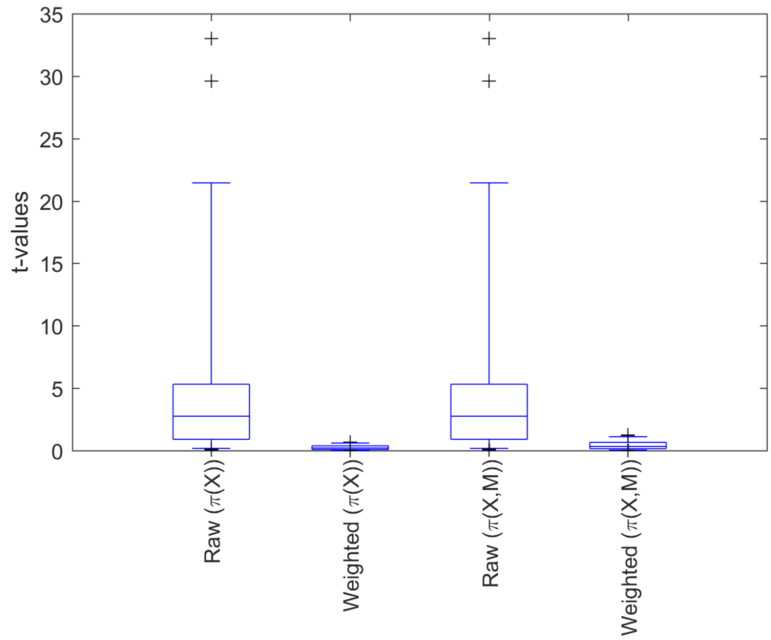
b. Distribution with Mediator

The upper panel depicts the densities of the propensity scores when the mediator is not included, the lower panel the densities when the mediator is included in the first step estimation. The solid line is a kernel density estimate of the conditional density of the propensity score among treated individuals, the dashed line a kernel density estimate of the conditional density of the propensity score among control individuals. Stars at the top of the figure represent the propensity score values for treated individuals, and circles at the bottom of the figure the propensity score values for control individuals.

Figure 3: Balancing Properties of the Propensity Scores



a. Standardized Differences in Means



b. t-values

The graph shows box-plots for the absolute Standardized Differences in Means (Rosenbaum & Rubin, 1985) in Panel a. and t-values for differences in means in Panel b. $\pi(X)$ refers to the estimates of the propensity score without the mediator, $\pi(X, M)$ to the estimates where the mediator was included. The vertical line in the middle of the box corresponds to the median. The lower and upper ends of the box represent the 25th and 75th percentile. The vertical lines (whiskers) extend to the 95th and 5th percentile respectively. The stars represent extreme observations. Weighted t-values are adjusted for the first step estimation of the propensity score using the method outlined in Newey & McFadden (1994).

Having established the balancing properties of $\pi(X)$ and $\pi(X, M)$, I present the results of my decomposition analysis next.

5 Results of Decomposition Analysis

5.1 Main Results

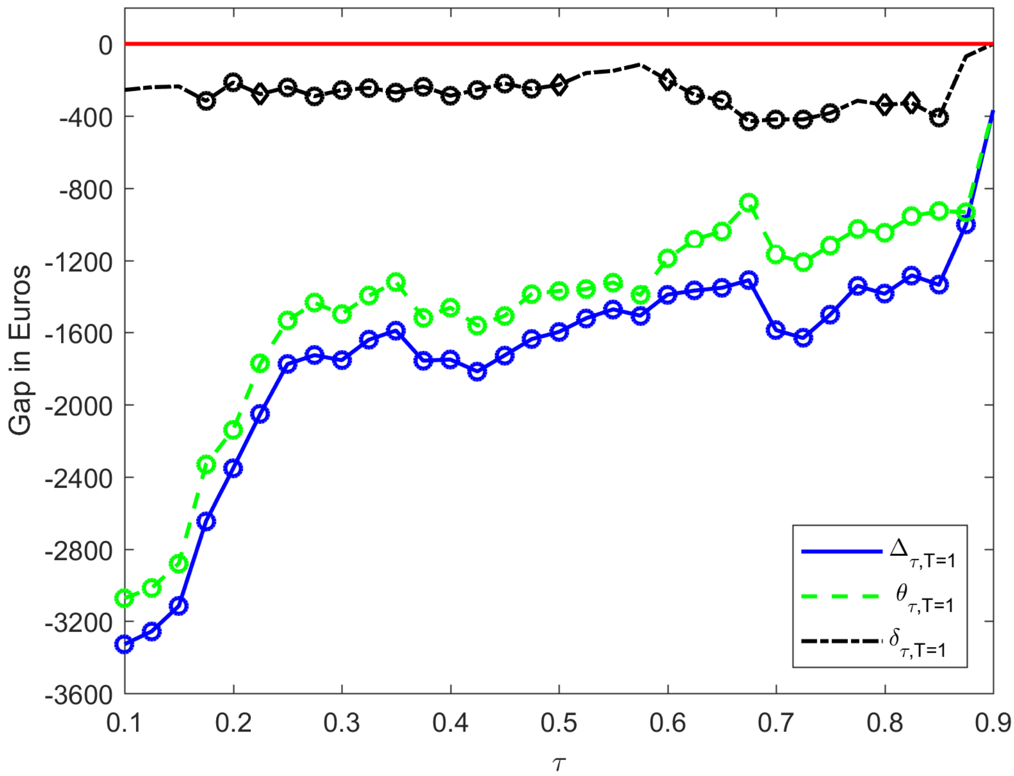
Figure 4 depicts my decomposition results from Equation (2) for the overall (trimmed) sample. I report the estimates for $\tau \in [.10, .90]$ at .025 unit intervals. The results without trimming which are virtually identical can be found in Appendix C.

Involuntary unemployment is associated with a substantial and significant wage loss over the whole earnings distribution. The treatment effect is, however, larger at the lower part of the distribution and almost monotonically decreasing with the quantile index. Displaced men at the the 10th quartile earn 3,200 Euros less per year than the counterfactual outcome. This treatment effect is more than twice as large as the loss for individuals in the middle of the wage distribution, and eight times as high as my estimates at the 90th percentile. These results are in line with Korkeamäki & Kyyrä (2014) who find that individuals at lower percentiles are more severely affected by job displacements.

Comparing my estimates to those previously obtained in the literature, they are smaller along the distribution than those reported for the US. Adding the quarterly estimates of the treatment effect reported by Couch & Placzek (2010), the wage loss is around \$7,800 in their sample. For the UK, the figures in Hijzen et al. (2010) indicate that, depending on sample and method, displaced worker suffer a wage loss of around 35% in the year after the displacement. This loss is comparable to my estimates at the lower part of the distribution.

Turning to the wage ladder effect of holding an interim job as estimated by $\delta_{\tau,T=1}$, one can see that the effect is uniformly negative across the distribution and over large parts of the distribution precisely estimated. The indirect effect is quite substantial in magnitude. At the lower quartile, between 300 and 180 Euros of the total loss is attributable to holding an interim job for individuals which translates into 7% to 14% of the total wage loss. The effect remains at around 250 Euros at the second quartile but, as the overall impact of the job loss is decreasing, it explains now roughly 15% of the total loss. Individuals between the 65th and 85th percentile seem to be the most affected, both in absolute and relative terms. Here the loss lies between 429 Euros and 315 Euros and $\delta_{\tau,T=1}$ can explain between 23% and 33% of the total wage loss.

Figure 4: Results of Decomposition - Overall Sample



All effects are reported for $\tau \in [.10, .90]$ at .025 unit intervals. The total effect $\Delta_{\tau,T=1}$ is depicted by the solid, the direct effect $\theta_{\tau,T=1}$ by the dashed, and the indirect effect $\delta_{\tau,T=1}$ by the dotted-dashed line. The markers show the significance levels: a circle significance on a 5%-level and a diamond significance on a 10%-level. Results are obtained by using the trimming approach described in Appendix A. Standard errors are obtained using 999 bootstrap replications.

An interesting feature of the estimates of $\delta_{\tau,T=1}$ is that it is negative across the entire earnings distribution. Interim employment does not help to climb the wage ladder faster after an involuntary job loss but rather leads to a lower wage trajectory.[14] Another interesting feature of the estimates are the non-constant effects across the wage distribution, especially at the upper part of the distribution. The estimates of $\delta_{\tau,T=1}$ are first decreasing in absolute terms until the 60th percentile. After that point, the indirect effect increases sharply until the 85th percentile after which it remains close to 0. Especially the drop around the 60th percentile could not have been uncovered when concentrating on the mean impact.[15]

5.2 Heterogeneous Results

Additional insight can be gained by conducting the analysis for specific subgroups. First, I investigate the role of the firm in explaining my results. As one could see from the summary statistics, workers previously employed in smaller and younger firms are both more likely to be displaced and more likely to take up interim employment after the job loss. This might be the result of network effects. Displaced workers formerly employed in larger firms might have access to a wider network which offers better information about employment opportunities (see, for example, Cavalo-Armengol & Jackson (2004)). In order to indirectly test this hypothesis, I select all workers who were, before the reference date, employed in firms with an average size of less than 21 people, which corresponds to the mean firm size in my data, for this analysis.

Second, I estimate the decomposition for individuals who were continuously employed over the 5 years before the reference date. These workers can be considered as highly productive with strong attachment to the labor market. They are eligible for 30 weeks of benefit payments which is 10 weeks longer than the base period. On the one hand, access to extended benefits can increase the selectivity during the job search and

[14] Another possibility might be that workers without interim employment and lower wage prospects are more likely to drop out of the labor force and therefore I would overestimate $\delta_{\tau,T=1}$. Examining the effect of holding interim employment on the decision to participate in the labor force, I do not find evidence for this hypothesis.

[15] Using a weighting approach but concentrating on the mean, I estimate an overall wage loss of 1350 Euros with a standard error of 181.97 and an indirect effect of 227 Euros with a standard error of 86.58 (results not reported in the figure). The overall mean effect is close to the my estimated loss at the upper part of the distribution while the indirect mean effect is similar to my estimates for the lower part of the distribution.

workers should be more willing to look for high paying jobs (Nekoei & Weber, 2017). Therefore, one would expect this group to climb the wage ladder faster and the effect of interim jobs to be less important. On the other hand, these workers can be regarded as highly productive and therefore might value future stable employment more than wage growth. If this is the case, one would expect this group to be relatively more affected by the take up of interim employment. In addition, it is highly likely that it depends on the individual rank in the distribution which one of these two forces are more important.

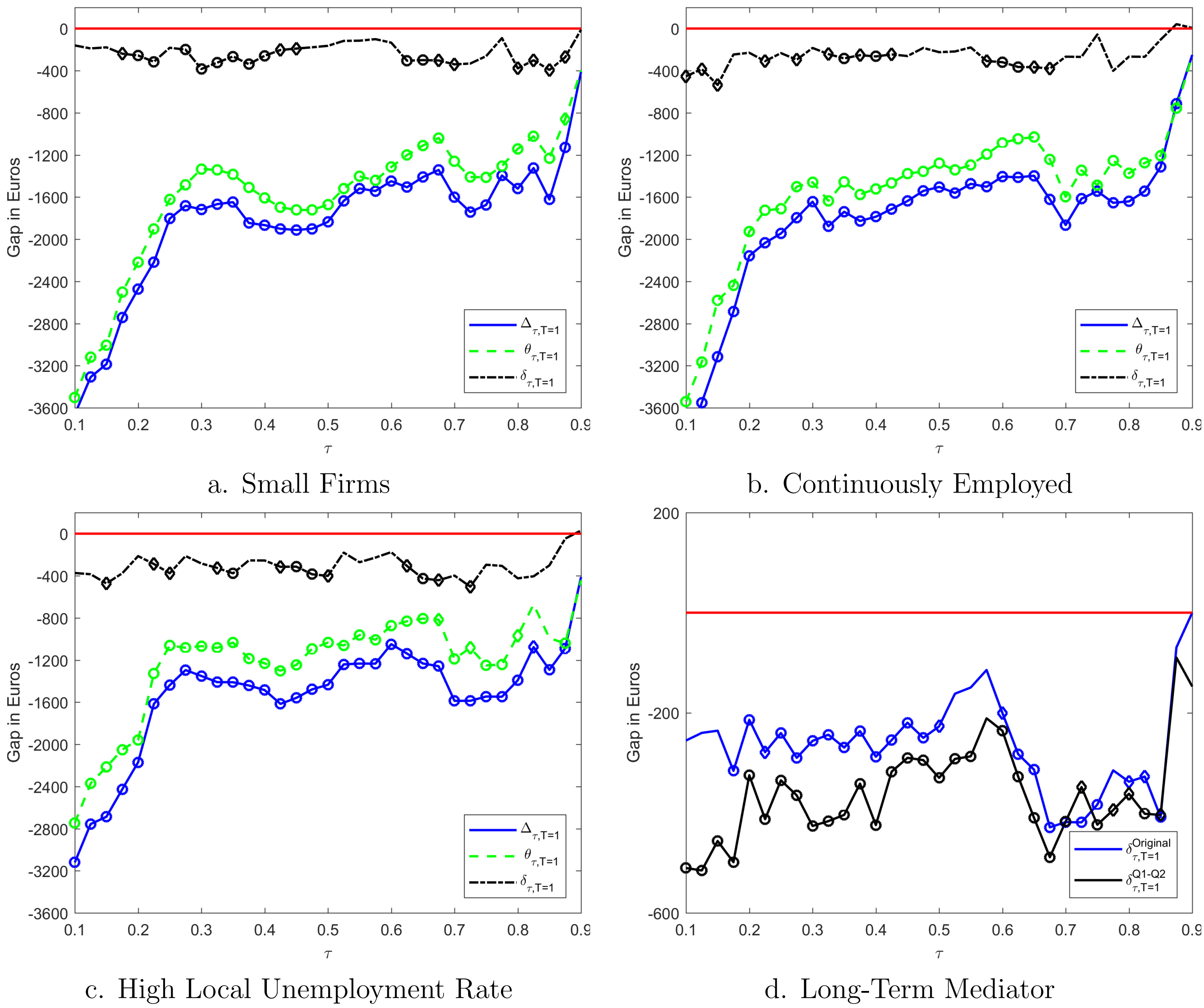
Third, I investigate how the local unemployment rate, a proxy for labor demand, is affecting my decomposition. Workers who are displaced during times of high local unemployment might be more willing to trade wage growth against job security. Hence, if I find that the overall wage loss for this sample is comparable to the results for my baseline sample but $\delta_{\tau,T=1}$ increases in magnitude this would point toward a trade-off between future employment probabilities and wage growth. I investigate this prediction using all workers employed in areas with an unemployment rate in the year of the plant closure above the median.

Fourth, I consider the impact of my mediator when measured over the first half year after the reference date instead of the first quarter. Workers might first search for traditional employment and take up interim employment later on if the search was unsuccessful or their savings are depleted. Therefore, my results might partly reflect biased beliefs about future employment opportunities (e.g. Spinnewijn (2015)). Allowing for longer time between the treatment and mediator might give some insights into how much biased beliefs might matter along the wage distribution.

Figure 5 shows the results for the specific subgroups for $\tau \in [.10, .90]$ at .025 unit intervals using the trimming approach. Results without trimming can be found in Appendix C. As before, the total effect $\Delta_{\tau,T=1}$ is depicted by the solid, the direct effect $\theta_{\tau,T=1}$ by the dashed, and the indirect effect $\delta_{\tau,T=1}$ by the dotted-dashed line.

Panel a. of Figure 5 shows the results of my decomposition for workers previously employed in small firms. Similar to my results in the previous section, wage losses are more

Figure 5: Results of Decomposition by Sub-Groups



a. Small Firms

b. Continuously Employed

c. High Local Unemployment Rate

d. Long-Term Mediator

All effects are reported for $\tau \in [.10, .90]$ at .025 unit intervals. The total effect $\Delta_{\tau,T=1}$ is depicted by the solid, the direct effect $\theta_{\tau,T=1}$ by the dashed, and the indirect effect $\delta_{\tau,T=1}$ by the dotted line. The markers show the significance levels: a circle significance on a 5%-level and a diamond significance on a 10%-level. Results are obtained by using the trimming approach described in Appendix A. Standard errors are obtained using 999 bootstrap replications. The *Small Firm Sample* consists of all workers who were previous to the reference date employed in firms with an average size of less than 21 people. The *Continuously Employed Sample* consists of workers who were all five years before the reference quarter continuously employed. The *High Unemployment Sample* consists of all workers who were employed in areas with an unemployment rate above the median in the year of the plant closure. The *Long-Term Mediator* sample measures take up of an interim job during the first half year after the reference date compared to one quarter in the original estimation. Notice that this Panel only depicts the indirect effect $\delta^{Q1-Q2}_{\tau,T=1}$ together with the original estimates $\delta^{Original}_{\tau,T=1}$

pronounced at the lower part of the distribution and the total effect of the involuntary job loss is almost monotonically decreasing in the quantile index for this group. The total losses suffered by these workers are, however, up to 20% higher. This points toward strong network effects in the referral of future job opportunities, but an in-depth analysis of these effects are out of the scope of this paper. For a detailed analysis of coworker-based networks on individual labor market outcomes see, for example, the work by Hellerstein, Kutzbach & Neumark (2015), and Glitz (2017).

My estimated for $\delta_{\tau,T=1}$ are similar to the baseline results reported in Figure 4 albeit less precisely estimated. They are somewhat smaller at the first quartile, ranging from 160 Euros to 315 Euros and higher in absolute terms at the second quartile staying at around 330 Euros before dropping to 160 Euros at the median. At the top of the distribution, my estimates for $\delta_{\tau,T=1}$ are lower compared to the baseline sample, but they can still explain between 6% and 24% of the overall loss. These results show that my baseline results are unlikely be driven by network effects and the transmission of employment opportunities. My estimates of $\delta_{\tau,T=1}$ for smaller firms are comparable to those obtained from the baseline sample over wide ranges of the wage distribution.

Panel b. of Figure 5 presents the results for continuously employed workers. The overall wage loss is remarkably similar to my previous estimates over large parts of the distribution. Only at the lower percentiles do experienced workers suffer larger overall losses. This finding stands in contrast to the one in Couch & Placzek (2010). They show that older workers suffer in general higher losses. Notice, however, that unlike Couch & Placzek (2010), I compare workers within a similar age range and tenure and do not use workers' age as proxy for experience. Comparing the effects over different age ranges, I also find that older workers experience larger losses, which can be seen from the results presented in the next section.

Looking at the estimates for $\delta_{\tau,T=1}$, one can see a similar pattern at the lower part of the distribution as those obtained for the baseline sample but the wage loss explained by taking up an interim job is now higher at the lower half of the distribution ranging from 184 Euros to 536 Euros or 10% to 15% of the overall loss. At the top of the distribution my estimates of $\delta_{\tau,T=1}$ are less pronounced and interim employment account for a smaller fraction of the total wage loss. They are, however, still quite substantial and explain between 4% and 26% of the total wage for $\tau \in [0.60, 0.85]$. The results imply that experienced workers at the lower part of the distribution might take up more stable but less risky jobs, while those at the upper part search, at least partly, for employment opportunities offering higher wage growth. To gain a deeper understanding between wage growth and employment possibilities, I investigate the effect in high unemployment areas next.

If individuals are confronted with higher job insecurity then they are likely to be more inclined to forgo higher wages and value stable employment more. The results of my decomposition for workers displaced in high unemployment areas are shown in Panel c. of Figure 5. What is striking from this subgroup analysis is that these workers bear, in general, lower wage losses compared to my baseline estimates. While this result seems to be surprising at first, notice that an unemployment spell in these areas is only a weak signal of a worker's productivity (see also the results in Kroft, Lange & Notowidigdo (2013)). Firms are not able to fully act on the signal and to offer lower wages to workers with an unemployment spell. The results also imply that firms impose stricter hiring standards in times of higher unemployment, similar as Wolthoff (2018).

My estimates for $\delta_{\tau,T=1}$ reveal that, despite a lower overall impact of displacement, interim employment is associated with large and significant wage losses over wide ranges of the wage distribution. The associated loss at the lower quartile is now between 213 and 473 Euros or between 10% and 26% of the overall loss. This is substantially higher than my estimates for the baseline sample. At the second quartile the effect varies between 212 Euros and 400 Euros. Here, my mediator can explain between 16% and 28% of the overall loss, which is again higher compared to my baseline estimates. Interestingly, I also estimate substantially stronger effects at the top of the wage distribution both in relative and absolute terms compared to my baseline samples. The estimates range now from 375 Euros to 500 Euros between the 70th and 85th percentile. At the 80th percentile, holding an interim job can explain 31% of the overall wage loss. These estimate are, however, mostly not statistically significant which can be traced back to the reduced sample size for this subgroup. Nevertheless, the results here support the conjecture that workers prefer stable employment to potential higher wage growth when insecurity is high. I provide further evidence for this in Section 6 where I show that displaced workers in high unemployment areas who are relatively more affected by taking up interim jobs have substantially more employment days the year after the displacement.

The last panel of Figure 5, Panel d., shows the result of my decomposition when considering a longer time span between treatment and measuring the mediator. Notice that the graph does not depict the total and indirect effect which is equal to the one shown in Panel a. Instead, I plot the original estimates of $\delta_{\tau,T=1}$ for comparison purposes.

As one can see from the figure, allowing for a longer time span over which I measure the mediator increases the estimates at the lower part of the distribution. Workers at the lower part might be overconfident and are reluctant to take up interim employment and look for traditional jobs first. When they do not succeed to find these jobs, they take up interim employment. In contrast, individuals at the upper part seem to take up interim employment relatively quickly after the job loss. My estimate for $\delta_{\tau,T=1}$ at the upper part of the distribution are remarkably similar to my previous ones. The results also provide suggestive evidence that, given wages reflect productivity, not only low productive workers are the ones who take up interim employment.

Up until now I have presented my decomposition results for individuals at age 31 to 35. One concern might be that this sample constitutes a specific selected age group. In Appendix D, I present the decomposition results for workers who were displaced in their 40s and show that my interpretations and conclusions are qualitatively similar.

6 Possible Explanations

In the previous section, I have shown that interim employment after involuntary displacement does not help to climb the wage ladder faster but, in contrast, can explain substantial amounts of the total wage loss arising from displacement along the distribution. Given the negative impact on future earnings one might wonder why displaced workers are willing to take up interim employment in the first place. I discuss possible explanations for my findings next. An excellent review on different theories why displacement leads to earnings losses is given by Carrington & Fallick (2017).

6.1 Employment Path of Displaced Workers

While theory does not give a unanimous prediction on the sign of the impact of interim jobs on future earnings, one might conjecture that workers trade wage levels for job security. This is especially true for those at the upper part of the wage distribution. Workers here suffer lower total losses but the relative share of $\delta_{\tau,T=1}$ is higher. These are also those workers who benefit the most from stable future employment and my results might be partly driven by a trade off the displaced worker face: taking up interim employment with higher prospects of stable employment in future or looking for high-wage - high-risk jobs. In this section, I provide evidence that workers for whom the relative effect of holding an interim job is high have better employment outcomes. This result shows that it is on one side rational and beneficial for a worker to take up this kind of employment in order to transit into stable employment. One the other side, this transition comes at the cost of lower wages.

I first estimate the effect on mean employment of holding an interim job on future employment day for individuals in my treatment gruop. If there is a general trade-off between future employment and wages, I should find positive effects here. Then, I do the same but for groups on different wage trajectories by computing

$$\Delta^{W}_{Group=g} = E\left[W(1) - W(0)|M = 1, T = 1, Group = g\right]$$

The group membership is defined by the different relative impact of the interim job on the total wage loss. The counterfacutal quantities are obtained by utilizing a a re-weighting approach using propensity score weighting (see also Imbens & Wooldridge

(2009)). This approach is similar to the one described in the previous section but now treating the mediator as a received treatment.

To provide evidence that workers face a trade-off between wage growth and employment possibilities, I first sort them into three groups according to the relative indirect effect on total wage loss. Low affected workers are those at wage percentiles where the indirect effect explains up to 10% of the total loss. The group of medium affected workers consist of those with a relative effect between 11% to 17%. Highly affected workers are those where the indirect effect explains 20% or more of the total loss.[16] For each of the three groups I calculate $\Delta^{W}_{Group=g}$ allowing the covariate effect to differ between categories. I follow a similar approach when determining the effect using the High Unemployment Sample.[17] For the year of the displacement, employment is measured only from Quarter 2 onward, that is, I exclude days worked during the same quarter I measure the mediator. Notice that group membership is not defined by the total wage loss. For example, members of the lowest group can be found both at the 10th and 90th percentile of the wage distribution corresponding to the highest and lowest absolute wage loss.

In order to compare the relative effect of $\delta_{\tau,T=1}$ and the effect on employment days for an observed quantile, I need to assume that there is no systematic changes in a worker's rank in the wage distribution after receiving the treatment or mediator. In other words, this means that changes in potential ranks solely happens trough random slippages such as, for example, luck. I formally test this assumption using the approach proposed by Dong & Shen (2017) and do not find evidence that this assumption is violated in my setting. All the obtained p-values are above any conventional level.[18]

Table 3 presents the mean effects and the results for the three group separately for the Baseline and High Unemployment Sample. The estimated effects support the conjecture from the previous section. In general, taking up interim employment immediately after

[16] These three categories occur naturally when sorting the relative effect with large jumps between two adjacent percentiles occurring at 10% and 20%.

[17] Here the jumps occurs at 14% and 20%

[18] The test of Dong & Shen (2017) is based on a discrete set of covariates. As I have many and mixed discrete-continuous covariates, I use groups defined by the estimated propensity scores: I first partition the observed distribution of the estimated propensity score into 10 different intervals and then determine if an individual's predicted propensity score falls within the respective boundaries. I then calculate the test statistic using this grouping. Results of the test were not sensitive to the exact partition used.

displacement is associated with more employment days. As one can see from the first row in Panel a. workers with interim jobs spend around 17 days more in employment during the year of the displacement and 14 days the year after that. Both effects are very precisely estimated.

The analysis also reveals interesting pattern by wage trajectories. Workers who are on a lower wage trajectory as a consequence of holding an interim job have more employment days in the year after the displacement, but I do not find any statistically significant impact for the year of displacement. Using the baseline sample (Panel a.), the estimated effect for individuals in the highest group is 12.55 days and for the second highest group 13.81 days. Both effects are statistically significant at a 5% level. In contrast, I find that members of the lowest group are actually negatively affected by holding interim employment, but the effect is not significantly estimated on any conventional levels.

The trade-off between future employment probabilities and lower wage trajectory becomes clearer looking at the results for the high unemployment sample at Panel b. In Section 5, I claimed that workers from this sample should be especially willing to trade wage growth with stable employment. The results from my analysis support this claim. The positive employment effect of interim employment for the two highest groups is now higher compared to the baseline sample but only significantly estimated for the highest group. I estimate a significant effect of almost 29 days in the year after displacement. My estimates for the second highest group are slightly higher but not significant. For Individuals where the indirect effect can only explain a relatively low share of the overall wage loss, my estimates indicate a negative albeit insignificant effect.

My findings suggest that interim employment is a stepping stone into full-time employment (see also Farber (1999)). These transitions are, however, not associated with a gain in wages, at least in the short-time considered in my analysis.

Table 3: Effect of Interim Employment on Future Employment Days

	Year of Displacement	Year after Displacement
Panel a.: *Effects by Relative Impact for Baseline*		
$\Delta^{W,Baseline}_{Mean}$	17.23***	14.11***
	(2.87)	(3.34)
$\Delta^{W,Baseline}_{Group=Low}$	−12.12	−6.38
	(16.98)	(17.81)
$\Delta^{W,Baseline}_{Group=Medium}$	12.48	13.81***
	(8.63)	(4.23)
$\Delta^{W,Baseline}_{Group=High}$	0.12	12.55**
	(10.75)	(5.18)
Panel b.: *Effects by Relative Impact for High UR Sample*		
$\Delta^{W,HighURRate}_{Mean}$	18.67***	21.39***
	(4.15)	(4.23)
$\Delta^{W,HighURRate}_{Group=Low}$	−15.04	−18.10
	(21.96)	(25.93)
$\Delta^{W,HighURRate}_{Group=Medium}$	29.99	29.49
	(29.87)	(32.76)
$\Delta^{W,HighURRate}_{Group=High}$	12.02	28.52***
	(19.17)	(9.26)

The sample consists of all displaced workers. The outcomes considered are total employment days in Quarter 2-4 (Year of Displacement) and Quarter 5-8 (Year after Displacement). The effect is calculated as the mean treatment effect of holding an interim job during the first 91 days after displacement using the approach as outlined in Section 6. The Baseline Sample consists of all workers and the High UE-Rate Sample consists of all workers who were displaced in areas with a local unemployment rate in the year of the plant closure above the median. Mean effects are calculated using the entire sample. Workers belong to the Low, Medium or High Group if their *relative* loss attributable to the mediator was below 10%, between 10% and 20%, or above 20%. The wage of low affected individuals correspond to $\tau \in \{0.10, 0.125, 0.15, 0.20, 0.55, 0.575, 0.875, 0.90\}$ in the wage distribution. The wage of medium affected individuals to $\tau \in \{0.175, 0.225, 0.25, 0.275, 0.30, 0.325, 0.35, 0.375, 0.40, 0.425, 0.45, 0.475, 0.50, 0.525, 0.60\}$, and the wage of highly affected individuals to $\tau \in \{0.625, 0.65, 0.675, 0.70, 0.725, 0.75, 0.775, 0.80, 0.825, 0.85\}$ in the wage distribution. A similar ordering was used for the High Unemployment Sample.
*,**,*** indicate a significance effect at a 10%, 5% and 1% level. Standard Errors are reported in parentheses and are based on 999 bootstrap replications.

6.2 Further Explanations

There are further possible explanations which can describe my estimation pattern others than a wage-employment trade off. I briefly discuss two alternatives, match quality/ productivity and signaling, next.

Match Quality/ Productivity: The negative impact of holding an interim job on wages might simply reflect poor match qualities and/or unobserved characteristics. Workers who find traditional employment quickly establish a good match with their firm and are in general more able than those who take up interim employment. Hence, my estimates would reflect these points rather than the employment-wage trade-off. While match quality likely plays a role in my results, I argue that it cannot be the sole explanations for at least two reasons.

First, if workers with interim job were of lower productivity or matches were of poor quality one should observe that firms are less inclined to hire them. As a consequence, future employment days should be lower for those individuals compared to workers without interim employment. As I have shown in the previous section, however, interim employment is actually associated with more employment days over the two years after the plant closure.

Second, if workers searched for traditional employment first and only unsuccessful, low productive individuals took up interim employment then one would expect the indirect effect to be substantially larger when allowing for a selection into the mediator with elapsed time since treatment. The results in Section 5 show that the estimates of the indirect effect remain relatively stable when changing the time span between displacement and measuring the mediator, especially at the upper part of the distribution.

Signaling: It might also be possible that prospective employers interpret an interim employment as a bad signal about a worker's productivity (e.g Ma & Weiss (1993)), Farber, Silverman & von Wachter (2017)). This explanation would imply that workers who take up interim employment are either unaware of the bad signaling or evaluate the prospective negative consequences less with the current financial necessity to find employment. This explanation is, however, not very likely to hold in my setting.

I showed previously that my results are similar for workers who are eligible for extended benefits and financial hardship is unlikely to explain my results. It is also not very likely that workers are completely unaware of the bad signal interim employment sends to prospective employers. In any case, workers could just not mention this type of work on their resume.

One remaining concern is that my results are driven by selection into the mediator. I will investigate the robustness of my results in the next section.

7 Sensitivity Analysis

The identification of my indirect effect is based on a sequential conditional independence assumption. Although the first-step results discussed in Section 4 do not indicate a strong evidence of selection into the mediator by background characteristics, one concern might be that unobserved confounders play an important role. In this section, I assess the sensitivity of my results when there is selection in the mediator following the approach suggested by Ichino et al. (2008). Sensitivity results for into treatment can be found in Appendix F.

Assume that the sequential conditional independence assumption as defined in Appendix A does only hold when controlling for an unobserved binary variable U, i.e. $Y(0,m) \perp\!\!\!\perp M|X=x, U=u, T=t$, for all x, u, t and m in the common support. U can be thought of as, for example, capturing unobserved skills or employment opportunities. For simplicity I will refer to it as "skills" in the remainder of this paper. Following Ichino et al. (2008), one can impose the values of the parameters which characterize the distribution of U, $p_{ij} = P(U=1|M=i, Y=j)$, for $i \in \{0,1\}$ and $j \in \{0,1,2,....\mathcal{Y}\}$. Assuming that $p_{0k} > p_{0l}$ for $k > l$ and that U and X is independent conditional on Y and M, I can simulate the unobserved confounder which has a positive impact on the relative outcome in the case of no mediation.[19] Therefore, under these assumptions and without explicitly modeling the dependence of my covariates the simulate confounders can be interpreted as having an increasing relative effect. In terms of my skill interpretation, that would mean that high skills are relatively more valuable at higher wages. Notice that unlike Ichino et al. (2008), I allow my outcome variable Y to take on arbitrary many discrete values.

Different chosen covariates, on which p_{ij} and the simulated confounding variable are based, will affect the selection into the mediator and the outcome differently. In order to quantify the effect of my simulated confounder, I estimate the selection effect ξ by the coefficient on U from a logistic regression of the mediator status on observable covariates and the simulated confounder. The effect of the confounding factor on the outcome χ is calculated at each percentile among the control group as the ratio of the quantile effects between those with $U = 1$ and those with $U = 0$. Both ξ and χ provide information about the influence of U. I base the simulation of U on a University-, Vienna Location-, and Below-Mean Firm Age- dummy.

In practice, I transform my outcome into a multi-valued discrete variable using cut-offs determined by 10 equally spaced points over the support of Y. The simulated decomposition is then estimated as follows: first, given a value of Y, M and values of p_{ij} I simulate U, where p_{ij} is chosen to follow the conditional distribution of a given discrete covariate. For example, using the dummy "University Degree", I can calculate $p_{k,1}$ by determining the empirical probability that someone with a wage falling in category k and no interim job holds at least a university degree. This probability determines the level of U. The simulated values are then used to determine $\pi(X, U)$ and $\pi(X, M, U)$, which are estimated in the same fashion as before but now the simulated U is included as an additional covariate. Having obtained the propensity scores, the procedure follows the same steps as outlined in Section 3. I repeat these steps 500 times and take the

[19] One can show that $p_{0k} > p_{0l}$ for $k > l$ implies $\frac{P(Y=k|M=0,U=1,X=x)}{P(Y=k|M=0,U=0,X=x)} > \frac{P(Y=l|M=0,U=1,X=x)}{P(Y=l|M=0,U=0,X=x)}$. In the case Y is binary, the result is the same as in Ichino et al. (2008) and the assumptions imply a positive effect of the confounder on the outcome in absolute terms. If Y is multi-valued, the interpretation of the sensitivity results in absolute terms is in general not feasible anymore.

average over all simulations to obtain the simulated effects. For inference, I bootstrap the procedure 999 times and apply the percentile method.[20]

Each row of Table 4 contains the simulated selection into the mediator and the simulated outcome effects for $\tau \in [.10, .25, .50, .75, 90]$.[21] As one can see from the table, my simulation is based on covariates which differently affect selection into the mediator and the outcome.[22] The first row uses the University dummy in order to simulate U. The outcome effect ξ for this confounder implies that once we control for observable "skills" have a positive and significant effect on wages in case of no treatment, but the effect is stronger at the lower part of the wage distribution. The selection effect into the mediator is positive with a simulated value of 0.10 but it is not significant at any conventional levels.

The second covariate I use to simulate U is based on the Vienna location dummy. Compared to before, the effect of the simulated confounder is now negative at the lower part of the wage distribution and positive and increasing at the upper part. It is largely significantly estimated. The simulated skill variable has now a negative effect on the selection into the mediator.

The third row is based on a dummy indicating if an individual was employed in a firm which was operative less than 15 years, the mean in my data. The simulated skill variable has a similar effect on the outcome as when I base my sensitivity analysis on the Vienna dummy. But, in contrast, the estimated selection into the mediator is now positive and significant. In general, the chosen covariates cover a wide range of different selection and outcome values, testing the robustness of my results.

Figure 6 depicts the results for $\delta_{\tau,T=1}$ under a deviation of the sequential conditional independence assumption with these characteristics . The dashed line represents the results based on the University dummy, the dashed-dotted line the effect for the simulation based on the Vienna dummy, and the dotted line the simulated values for $\delta_{\tau,T=1}$ based

[20]To be precises, during each bootstrap replication I simulate the effect 500 times.

[21]The coarse spacing was chosen for expositional reasons. Result for other values of τ are very similar and are available upon request.

[22]The results are very similar basing the simulation on other covariates than presented here.

Table 4: Influence of Simulated Confounder on Selection and Outcome

	Outcome Effect χ					Selection Effect ξ
	$\tau = .10$	$\tau = .25$	$\tau = .50$	$\tau = .75$	$\tau = .90$	
Confounder like:						
University Degree	1.13**	1.35***	1.47***	1.32***	1.06***	0.10
	(0.06)	(0.05)	(0.03)	(0.02)	(0.01)	(0.11)
Vienna Dummy	0.90***	0.99	1.07***	1.13***	1.03***	−0.15 **
	(0.02)	(0.01)	(0.01)	(0.01)	(0.00)	(0.07)
Firm Age Dummy	0.90***	0.98**	1.02***	1.06***	1.02***	0.32***
	(0.02)	(0.01)	(0.01)	(0.01)	(0.00)	(0.07)

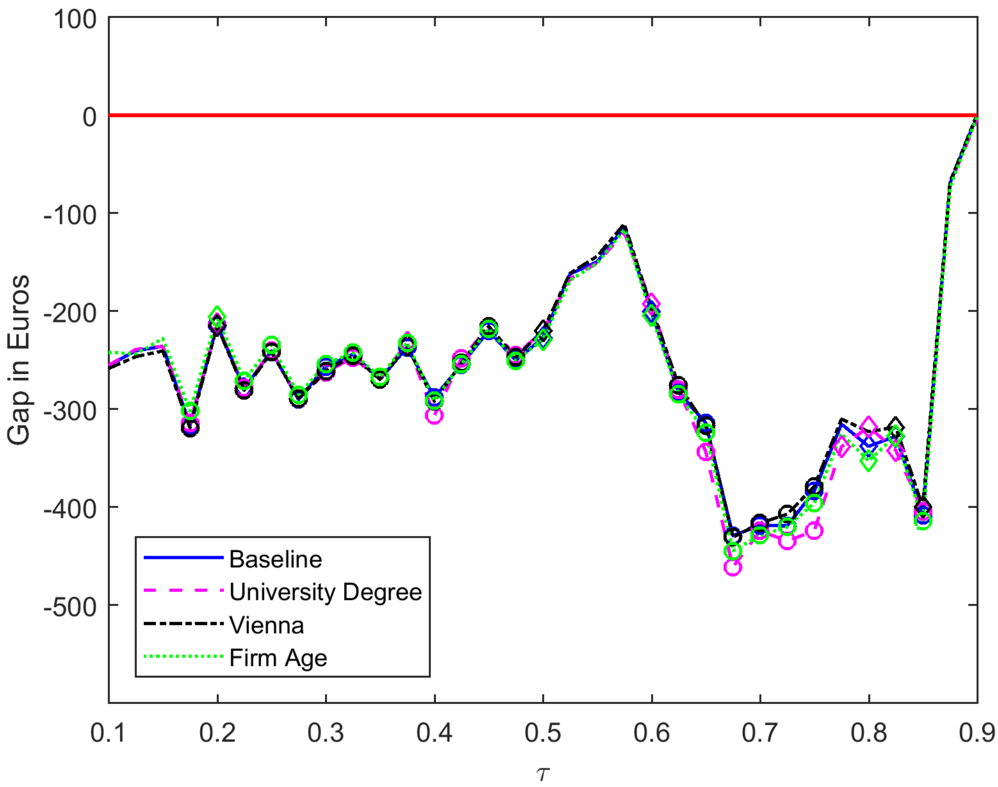
The table displays the effect of the simulated confounder U on the outcome and selection into the mediator. U is imputed based on a University degree, Vienna location, and Firm Age dummy as described in Section 7. The Selection Effect ξ is denoted by the coefficient on U from a logistic regression of the mediator status on observable covariates and the simulated confounder. The effect of U on the outcome is denoted by χ and calculated at each percentile among the control group as the ratio of the quantile effects between those with $U = 1$ and those with $U = 0$. The values are based on 500 simulations.
*,**,*** indicate a significance difference at a 10%, 5% and 1% level. Standard Errors are reported in parentheses and are based on 999 bootstrap replications.

on the Firm Age dummy. To facilitate comparison between the estimated and simulated effect, the figure also contains the baseline estimates.

The results show that my estimated indirect effect is robust to particular departures from the identifying assumptions. It is almost indistinguishable from the simulated ones, both in magnitude and significance. The results based on the Vienna and Firm Age dummies are virtually indistinguishable from my baseline estimates over the whole distribution. I estimate a maximal difference in absolute value of around 20 Euros. Looking at my results for the University dummy, one can see that the simulated estimates for $\delta_{\tau,T=1}$ are virtually identical to my baseline estimate at the lower part of the distribution, and slightly lower at the upper part. The maximal difference between my baseline results and the simulation is 40 Euros.

The sensitivity analysis presented in this section show that my results are robust to specific departure from my identifying assumptions. The difference between my simulated and original estimation results are small in magnitude and do not lead to different conclusions as drawn in Section 5. In light of these results, I am confident that my decomposition covers the causal impact of interim employment on wages.

Figure 6: Results of Sensitivity Analysis



All effects depicted is the indirect effect $\delta_{\tau,T=1}$ reported for $\tau \in [.10, .90]$ at .025 unit intervals. The continuous line refers to the baseline effect. The simulated effect using the binary University variable is depicted by the dashed, the effect using the Vienna location dummy is depicted by the dashed-dotted-line, and the effect basing the simulation on firm age by the dotted line. Effects are very similar at the lower part of the distribution and are therefore hard to distinguish in the graph. Simulated effects are based on 500 repetitions. The markers show the significance levels: a circle significance on a 5%-level and a diamond significance on a 10%-level. Results are obtained by using the trimming approach described in Appendix A. Standard errors are obtained using 999 bootstrap replications.

8 Conclusion

This work contributes to the discussion on the impact of an involuntary job loss on future wages, the effect of interim employment, and its impact on the future careers of displaced workers. I propose a new approach which allows to decompose a treatment effect into underlying channels over the whole wage distribution using a sequential conditional independence assumption. Under this assumption, I am able to disentangle the total wage loss after involuntary displacement into a wage ladder (indirect) effect which is due to holding an interim job from a direct effect which accounts for all remaining channels over the whole wage distribution.

Concentrating on a sample of young and established workers, I find that taking up interim employment shortly after an involuntary job loss does not help to climb the wage ladder faster. My decomposition results show considerable negative effects over the whole wage distribution with the indirect effect explaining up to 30% of the total wage loss. I also find substantial heterogeneity in my estimates which would not have been captured by concentrating on mean outcomes. Workers at the very lower and upper part of the distribution are less affected than those found in the middle. Interestingly, in times of low labor demand, the total wage loss after involuntary unemployment is smaller compared to my baseline estimates but the indirect effect can explain a larger fraction of it. My results are qualitatively similar when considering older workers. Assessing the sensitivity of my results, I show that they are robust to selective violations of my identifying assumptions.

I provide evidence that the results of my decomposition are a consequence of the trade-off between wage levels and future employment stability displaced workers face, especially at the upper part of the distribution. Estimating the impact of holding an interim job on future employment days by the relative magnitude of the indirect effect, I find that those relatively more affect also have more employment days during the year after the displacement. This is especially true when local labor demand is low and workers value employment stability more.

These findings together with the results from my decomposition analysis show the difficulty to balance rapid re-employment and higher wage levels. While interim employment facilitates the return into stable employment, there is the danger that those workers will lose out in terms of future wages. My results highlight the challenges policy maker are likely to face amid increasing economic uncertainty and the raise of alternative working arrangements. Given the increasing number of workers employed in non-standard interim arrangements over the last years and continuing economic uncertainty this is an important task.

Online Appendix for "Involuntary Unemployment and the Labor Market Returns to Interim Jobs"

BERNHARD SCHMIDPETER

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A Identification and Estimation of the Decomposition

A.1 Identification

In order to identify the direct and indirect effect in Equation 2, I need to impose some assumption on the treatment and mediator. I base my identification on a sequential conditional independence assumption which is, as I am interested in the effect on the treated sub-population, slightly weaker than the one commonly imposed in mediation analysis (see, for example, Imai, Keele & Yamamoto (2010)). Notice that the effect which involves only the treated sub-population is directly identified from the data. For identifying the quantities with counterfactual outcomes I need to impose the following assumptions which are also used, for example, in Huber et al. (2016):

Assumption 1.

$$\{Y(0,m), M(0)\} \perp\!\!\!\perp T|X = x \text{ for all x, m in the common support}$$

Assumption 2.

$$Y(0, m) \perp\!\!\!\perp M | X = x, T = t \text{ for all x, t and m in the common support}$$

Assumption 3.

$$Pr(T = 1 | X, M) < 1$$

Assumption 1 states that joint distributions of potential outcomes for any possible value m of the mediator and the mediator under non-treatment are independent of the treatment conditional on X. Hence, there exists no unobserved confounder which affects jointly treatment and the potential outcome or the mediator under non-treatment given my covariates. It would be violated if, for example, individuals in my control groups sort themselves into higher paying jobs and/or interim employment based on unobservable characteristics. Using plant closures as quasi-experimental setting for involuntary unemployment and exploiting the age at displacement while accounting for various firm- and worker characteristics ensures that this assumptions is likely to be met in my empirical analysis. Note that Assumption 1 implies that $Y(0) \perp\!\!\!\perp T | X = x$ and is therefore stronger than the conditional independence assumption commonly imposed in traditional policy evaluation.

Assumption 2 is similar to the first one. It states that my potential outcomes, for any possible value of my mediator under non-treatment, are independent of the mediator conditional on X and treatment status. It would be violated if, for example, if there was dynamic selection into the mediator and workers only take up interim employment after a certain time if they cannot find "standard" employment.

As I restrict my sample to workers who all experience plant closure but at different ages, consider only a short time span between treatment and the mediator outcome, and control for various background characteristics, Assumption 2 is likely to hold in my analysis. In addition, the evidence presented in Figure 1 in the main text shows that my mediator captures short-time interim employment which is taken up shortly after the reference date. It should be mentioned that Assumption 1 and 2 are untestable. In Section F, I investigate the sensitivity of my results to specific failures of them.

Assumption 3 is a common support assumption. It states that I can find suitable non-treated individuals for comparison. It implies $Pr(T = 1|X) < 1$, the assumption needed for identifying the total treatment effect. Assumption 3 can be imposed by concentrating on individuals within the common support using trimming methods.

Huber et al. (2016) show that under the aforementioned conditions one can identify the direct effect on the treated under treatment, $E[Y(1, M(1)) - Y(0, M(1))|T = 1]$, and the indirect effect on the treated under non-treatment $E[Y(0, M(1)) - Y(0, M(0))|T = 1)]$.[1] To identify the distributional direct and indirect effect among the treated an additional assumption on the quantiles is necessary. In particular, I need to impose a similar condition as Assumption 2 in Firpo (2007):

Assumption 4. For $t, j \in \{0, 1\}$, $Y(t, M(j))$ is a continuous random variable with support in $\mathbb{R}$ and there exist non-empty sets $\mathcal{Q}_{(0,0)}$, $\mathcal{Q}_{(0,1)}$ and $\mathcal{Q}_{(1,1)}$ such that $\forall c \in \mathbb{R}$ and $c > 0$

$$\begin{aligned}
\mathcal{Q}_{(0,0)} = \{\tau \in (0,1);\ & Pr\left[Y(0, M(0)) \leq q_{(0,M(0)),\tau|T=1} - c|T = 1\right] < \\
& Pr\left[Y(0, M(0)) \leq q_{(0,M(0)),\tau|T=1} + c|T = 1\right]\} \\
\mathcal{Q}_{(0,1)} = \{\tau \in (0,1);\ & Pr\left[Y(0, M(1)) \leq q_{(0,M(1)),\tau|T=1} - c|T = 1\right] < \\
& Pr\left[Y(0, M(1)) \leq q_{(0,M(1)),\tau|T=1} + c|T = 1\right]\} \\
\mathcal{Q}_{(1,1)} = \{\tau \in (0,1);\ & Pr\left[Y(1, M(1)) \leq q_{(1,M(1)),\tau|T=1} - c|T = 1\right] < \\
& Pr\left[Y(1, M(1)) \leq q_{(1,M(1)),\tau|T=1} + c|T = 1\right]\}
\end{aligned}$$

Assumption 4 states that the counterfactual quantiles in my decomposition are unique and well-behaved. In practice, one could impose monotonicity using an approach in line with Chernozhukov et al. (2010) to refine the estimates in finite samples.

[1]Under a slightly different set of assumption one could identify the direct effect on the treated under non-treatment, $E[Y(1, M(0)) - Y(0, M(0))|T = 1]$, and the indirect effect on the treated under treatment $E[Y(1, M(1)) - Y(1, M(0))|T = 1)]$. These effects are, however, of less interest in general (see also the argumentation of Vansteelandt & VanderWeele (2012)).

Under Assumptions 1-4 the quantiles of the decomposition in Equation (2) are implicit functions of the data:

Theorem 1. Under Assumption 1-4 all quantiles can be identified from the data:

$$\begin{aligned}
\text{i) } \tau &= E\left[\frac{T}{\pi}\mathbb{1}\{Y \leq q_{(1,M(1)),\tau|T=1}\}\right]; \forall\tau \in \mathcal{Q}_{(1,1)} \\
\text{ii) } \tau &= E\left[\frac{(1-T)}{\pi}\cdot\frac{\pi(X)}{(1-\pi(X))}\mathbb{1}\{Y \leq q_{(0,M(0)),\tau|T=1}\}\right]; \forall\tau \in \mathcal{Q}_{(0,0)} \\
\text{iii) } \tau &= E\left[\frac{(1-T)}{\pi}\cdot\frac{\pi(X,M)}{(1-\pi(X,M))}\mathbb{1}\{Y \leq q_{(0,M(1)),\tau|T=1}\}\right]; \forall\tau \in \mathcal{Q}_{(0,1)}
\end{aligned}$$

where $\pi = Pr(T = 1)$, $\pi(X) = Pr(T = 1|X)$ and $\pi(X, M) = Pr(T = 1|X, M)$. Here $\pi(X)$ is the propensity score as defined in the standard treatment effects literature and $\pi(X, M)$ is the propensity score including the mediator.

The results i) and ii) in Theorem 1 were proven in Firpo (2007), identification of $q_{(0,M(1))|T=1}$ via re-weighting is new to the literature. Part iii) can be proven using similar arguments as in Huber (2014). For notational simplicity write $q = q_{(0,M(1)),\tau|T=1}$ and define $\pi = Pr(T = 1)$, $\pi(X) = Pr(T = 1|X = x)$ and $\pi(X, M) = Pr(T = 1|X = x, M = m)$.

Proof:

$$\begin{aligned}
\tau &= Pr\left[Y(0, M(1)) \leq q|T = 1\right] \\
&= \int\left(\int\left(E\left[\mathbb{1}\left\{Y(0,m) \leq q\right\}|X = x, M(1) = m, T = 1\right]\right) dF_{M(1)|X=x,T=1}(m)\right) dF_{X|T=1}(x) \\
&= \int\left(\int\left(E\left[\mathbb{1}\left\{Y(0,m) \leq q\right\}|X = x, T = 1\right]\right) dF_{M|X=x,T=1}(m)\right) dF_{X|T=1}(x) \\
&= \int\left(\int\left(E\left[\frac{(1-T)}{(1-\pi(X,M))}\mathbb{1}\left\{Y(0,m) \leq q\right\}|X = x, M = m\right]\right)\frac{\pi(X,M)}{\pi(X)}\frac{dF_{M,X}}{dF_X}(m)\right)\frac{\pi(X)}{\pi}dF(x) \\
&= \int\left(\int\left(\int\frac{(1-T)}{(1-\pi(X,M))}\mathbb{1}\left\{Y(0,m) \leq q\right\}\frac{dF_{Y,M,X}}{dF_{M,X}}(y)\right)\frac{\pi(X,M)}{\pi(X)}\frac{dF_{M,X}}{dF_X}(m)\right)\frac{\pi(X)}{\pi}dF(x) \\
&= E\left[\frac{(1-T)}{\pi}\frac{\pi(X,M)}{(1-\pi(X,M))}\mathbb{1}\left\{Y \leq q\right\}\right]
\end{aligned}$$

The first line follows from the definition of q and Assumption 4. The second line is an application of the law of iterated expectations. The third line makes use of Assumption 2, that is the independence between counterfactual outcome and mediator given X and the treatment status. The fourth line uses Assumption 1 together with Bayes' Law and basic statistics. The fifth line is just a reformulation of line 3. The final results follows by simplifying, integrating out the distribution of M and X, and making use of the observation rule. □

An immediate implication from this theorem is that $\theta_{\tau,T=1}$ is identified for $\tau \in \mathcal{Q}_{(1,1)} \cap \mathcal{Q}_{(0,1)}$ and $\delta_{\tau,T=1}$ is identified for $\tau \in \mathcal{Q}_{(0,1)} \cap \mathcal{Q}_{(0,0)}$ (see also Firpo (2007)).

A.2 Estimation

In my analysis, I estimate $\pi(X)$ and $\pi(X, M)$ by series-logit estimation using power series, where the number of series terms K increases with the sample size (Hirano et al., 2003). This approach is an approximately nonparametric first-step and allows for a certain degree of flexibility. I choose K to be given by $K = [N^{1/8}]$ where N is the sample size and $[x]$ denotes the nearest integer of x.[2] Other possibilities for estimating the propensity scores are, for example, local logistic regression (Frölich, 2006) or the estimators proposed by Ichimura (1993), and Klein & Spady (1993). If I consider sub-samples in my analysis, I determine K and estimate $\pi(X)$ and $\pi(X, M)$ for each of these sub-samples anew.

Assumption 3 requires that I can find suitable non-treated individuals for each treated person. This can be easily violated in practice. Moreover, sufficient overlap seems crucial for a good performance of re-weighting estimators (Busso et al., 2014). To ensure that the overlap condition holds, I apply the trimming method suggested by Heckman et al. (1997) and Heckman, Ichimura, Smith & Todd (1998). In a first step I determine the common support as

$$\mathcal{S}_{\mathcal{C}} = \{\pi(X, M) : \hat{f}(\pi(X, M)|T = 1) > c \quad \hat{f}(\pi(X, M)|T = 0) > c\}$$

[2] As a tie-breaking rule, I choose the nearest lowest integer.

where c is an arbitrary constant and $\hat{f}(\cdot)$ is a kernel density estimate using a Gaussian Kernel and a bandwidth determined by Silverman's Rule (Silverman, 1998). In my analysis I set c to a small number, i.e. $c = 0.009$. In a second step I disregard all remaining individuals with an estimated density below a certain threshold c_p. The final set for my estimation is given by

$$\mathcal{S}_{\mathcal{F}} = \{\pi(X, M) \in \mathcal{S}_{\mathcal{C}} : \hat{f}(\pi(X, M)|T = 1) > c_p\}$$

where c_p is the value of $\pi(X, M) \in \mathcal{S}_{\mathcal{C}}$ corresponding to the pth percentile (Busso et al., 2008). In my analysis I chose p to be the 2nd percentile.

I base inference on the non-parametric bootstrap. That is, I draw 999 random sample and for each of the samples I estimate the total effect, the indirect effect, and the direct effect by applying my trimming procedure as outlined above. From the bootstrapped estimates I calculate then my standard errors.

B Propensity Scores Estimation

Table B.1 reports the estimated coefficient together with t- and p-values of my propensity score estimations. The left panel contains the results of the estimation including the covariates, $\pi(X) = P(T = 1|X)$, the right panel the results of the estimation including my covariates and the mediator, $\pi(X, M) = P(T = 1|X, M)$

Table B.1: Propensity Score Estimations

	Propensity Score without Mediator $\pi(X)$			Propensity Score with Mediator $\pi(X, M)$		
	Coeff.	Std. Error	p-value	Coeff.	Std. Error	p-value
Personal Characteristics						
Age	4.42	7.11	0.00	4.44	7.02	0.00
Age^2	−0.68	−7.22	0.00	−0.68	−7.12	0.00
Non-Austrian	−0.02	−0.39	0.70	−0.03	−0.65	0.52
Children	0.01	0.25	0.80	0.01	0.26	0.79
University Degree	0.17	2.72	0.01	0.16	2.57	0.01
Labor Market Characteristics						
Av. Wage (1'000 Euros)	0.00	−0.11	0.92	0.00	0.15	0.88
Av. Wage (1'000 Euros)^2	0.12	0.78	0.43	0.10	0.65	0.51
Av. Employment (Days)	0.03	1.25	0.21	0.03	1.26	0.21
Av. Employment(Days)^2	−0.02	−1.27	0.20	−0.02	−1.28	0.20
Av. Unemployment (Days)	0.00	1.22	0.22	0.00	1.08	0.28
Av. Unemployment (Days)^2	0.00	−0.31	0.76	0.00	−0.12	0.91
Local Unemployment Rate	−0.01	−0.30	0.77	−0.01	−0.20	0.84
Local Unemployment Rate^2	0.00	0.08	0.94	0.00	−0.02	0.98
Firm & Sector Characteristics						
Firm Characteristics						

Continued on next page

Table B.1 – continued from previous page

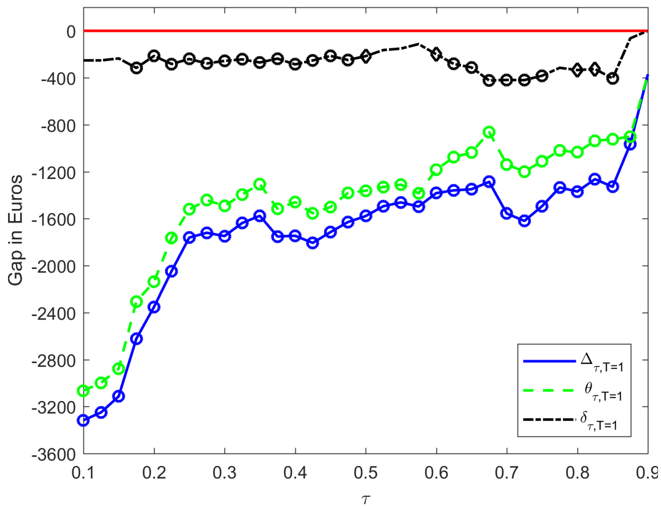
	Propensity Score without Mediator $\pi(X)$			Propensity Score with Mediator $\pi(X, M)$		
	Coeff.	t-value	p-value	Coeff.	t-value	p-value
Firm Wage	−0.02	−3.07	0.00	−0.02	−3.75	0.00
Firm Wage^2	0.01	2.74	0.01	0.02	3.41	0.00
Firm Size	−0.73	−8.72	0.00	−0.78	−9.15	0.00
Firm Size^2	−4.57	−1.22	0.22	−3.28	−0.86	0.39
Firm Age	−0.08	−10.56	0.00	−0.08	−10.71	0.00
Firm Age^2	0.02	7.59	0.00	0.02	3.41	0.00
Local Area						
Vienna	0.04	0.60	0.55	0.06	0.77	0.44
North-East	−0.04	−0.57	0.57	−0.03	−0.41	0.68
South-East	−0.04	−0.49	0.62	−0.11	−1.39	0.16
North-Mid	−0.06	−0.79	0.43	−0.06	−0.80	0.42
Mid	0.06	0.67	0.51	0.07	0.77	0.44
West	−0.01	−0.12	0.91	0.00	0.01	0.99
Sector						
Agriculture & Mining	0.21	1.51	0.13	0.22	1.53	0.12
Production	0.17	3.12	0.00	0.17	2.98	0.00
Construction	0.18	3.04	0.00	0.19	3.25	0.00
Commerce	−0.02	−0.40	0.69	−0.01	−0.13	0.90
Tourism	−0.04	−0.47	0.64	−0.04	−0.51	0.61
Transport	−0.03	−0.36	0.72	−0.03	−0.44	0.66
Mediator						
Interim Employment				1.93	22.42	0.00

Estimation includes a constant as well as year and quarter dummies. Average Wage, Employment, and Unemployment were measured using the 3 years before the reference quarter. The Local Unemployment Rate was measured the year before the reference date.

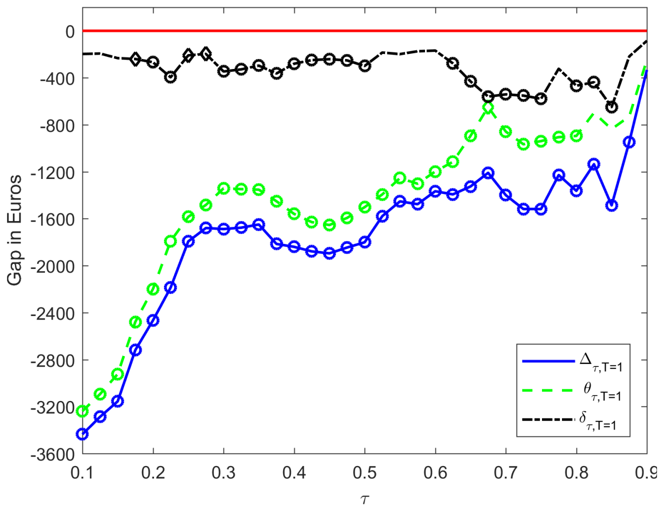
C Untrimmed Results

This appendix contains the result when the trimming procedure as outlined in Section 3 is not applied. Figure C.1 contains the results for workers displaced at age 31 to 39 which constitutes the main sample. Figure C.2 my estimates for workers displaced at age 41 to 49. As it its apparent from the figures, both the magnitude of the estimated effects and their precision are very similar to those presented in the main text. Hence, the conclusions derived in the main text do not depend on my trimming approach.

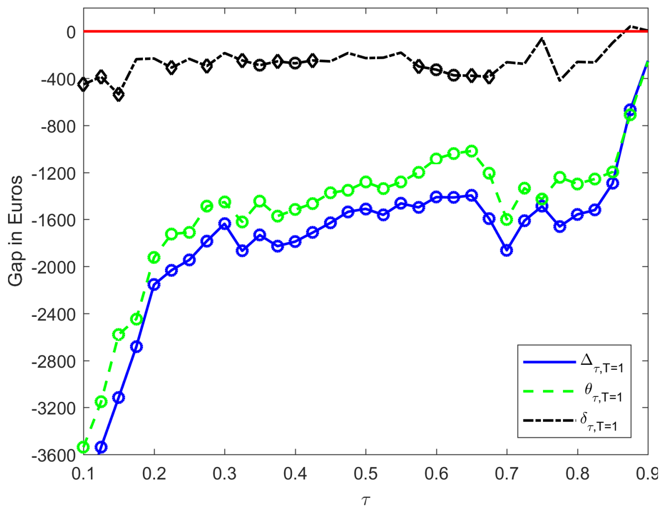
Figure C.1: Results of Decomposition - Untrimmed Sample



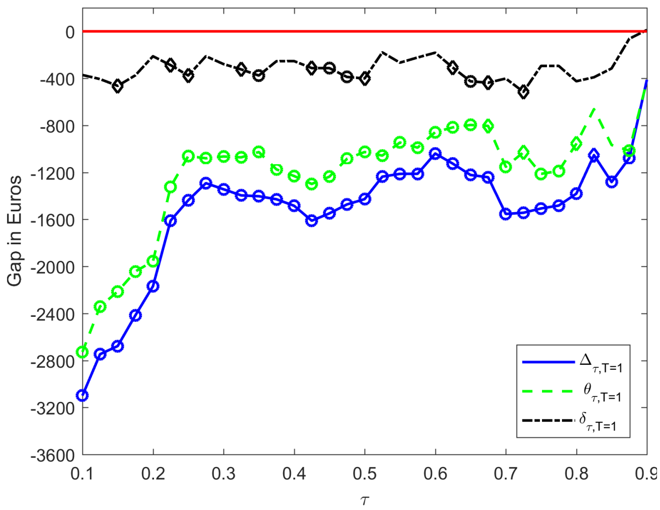
a. Overall Sample



b. Small Firms



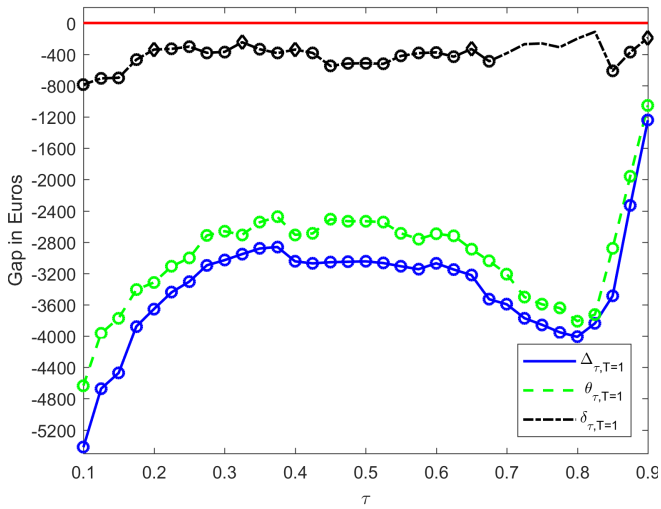
c. Continuously Employed



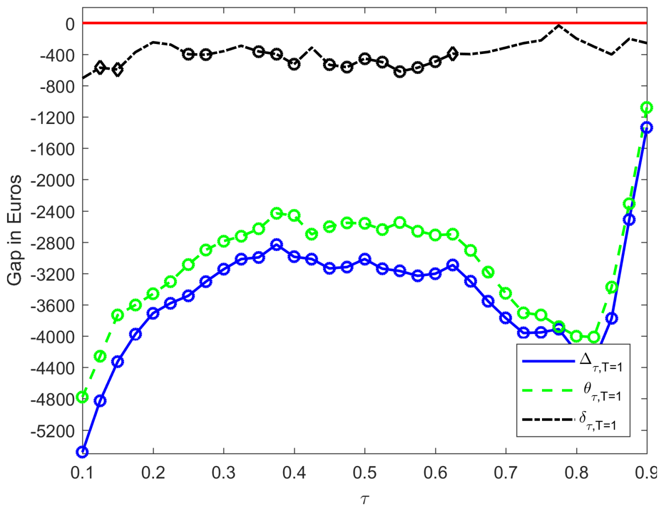
d. High Local Unemployment Rate

All effects are reported for $\tau \in [.10, .90]$ at .025 unit intervals. The sample consists of workers who were displaced at age 31 to 39, where the treatment group comprises of workers displaced at an earlier age, 31 to 35.The total effect $\Delta_{\tau,T=1}$ is depicted by the solid, the direct effect $\theta_{\tau,T=1}$ by the dashed, and the indirect effect $\delta_{\tau,T=1}$ by the dotted-dashed line. The markers show the significance levels: a circle significance on a 5%-level and a diamond significance on a 10%-level. Results are obtained by applying the estimator described in Appendix A. Standard errors are obtained using 999 bootstrap replications.

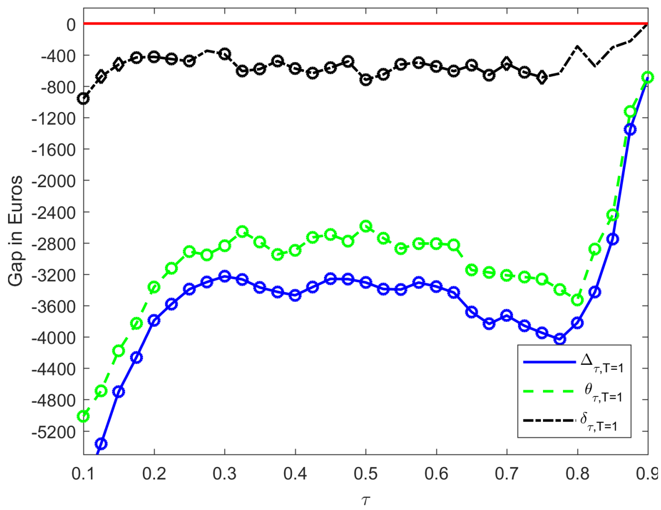
Figure C.2: Results of Decomposition for Older Workers - Untrimmed Sample



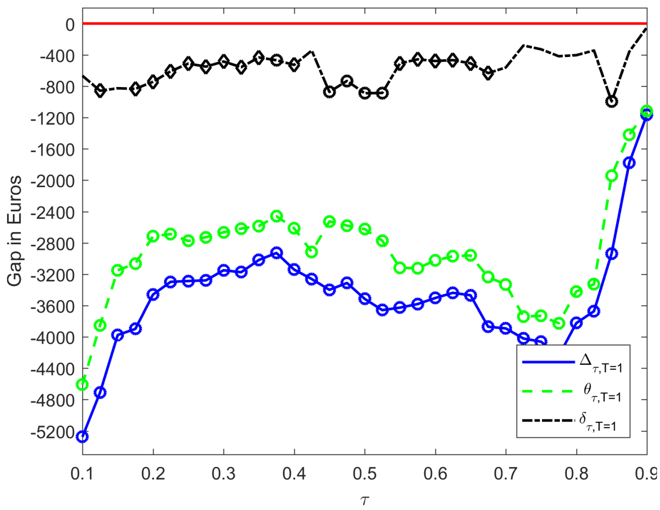
a. Overall Sample



b. Small Firms



c. Continuously Employed



d. High Local Unemployment Rate

All effects are reported for $\tau \in [.10, .90]$ at .025 unit intervals. The sample consists of workers who were displaced at age 41 to 49, where the treatment group comprises of workers displaced at an earlier age, 41 to 45. The total effect $\Delta_{\tau,T=1}$ is depicted by the solid, the direct effect $\theta_{\tau,T=1}$ by the dashed, and the indirect effect $\delta_{\tau,T=1}$ by the dotted-dashed line. The markers show the significance levels: a circle significance on a 5%-level and a diamond significance on a 10%-level. Results are obtained by applying the estimator described in Appendix A. Standard errors are obtained using 999 bootstrap replications.

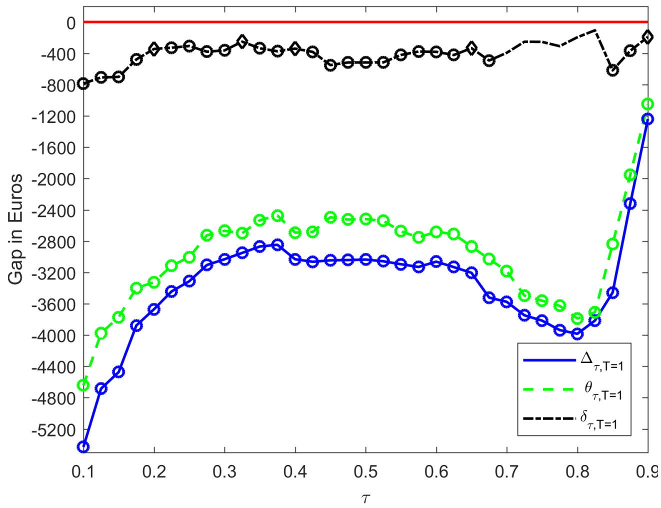
D Decomposition Results for Older Workers

In this section, I present the decomposition results for workers who were displaced while in their 40s. I apply a similar selection strategy as outlined in Section 2 and exploit the timing of the plant closure in order to define my treatment and control group. Workers displaced aged 41 to 45 are considered as treated, while workers displaced at age 46 to 49 are used as control observations. As before, I use as a reference date the same quarter but four years prior to the actual closing data.

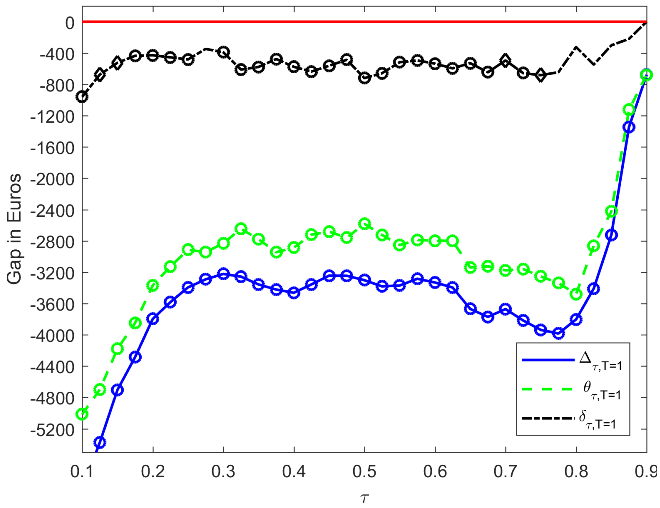
Figure D.1 depicts the decomposition results for this group. Panel a. shows the result for the overall sample and Panel b. - d. for the same type of subgroups considered in the previous section. In general, the effects are higher and more precisely estimated along the wage distribution compared to my younger age group. In terms of overall wage loss, I find that the effect of involuntary displacement is higher at the lower end of the distribution and decreasing afterward which is in line with my previous findings. But, unlike it was the case for the age 31-34 group, my estimates of $\Delta_{\tau,T=1}$ exhibit now an inverted U-shape pattern. Men at the 80th percentile have overall losses comparable to those at the 20th percentile. Similar as before, both workers with strong labor market attachment and those displaced during times of high unemployment have very similar overall losses compared to my baseline sample and are substantially lower at higher parts of the distribution.

Looking at my estimates for $\delta_{\tau,T=1}$, one can see that taking up interim jobs is associated with lower wages along the income distribution. Compared to my previous estimates $\delta_{\tau,T=1}$ is now larger in absolute terms and more precisely estimated. From Panel a., one can see that at the lower quartile my indirect effect accounts for 306 Euros to 787 Euros, or 9% to 15% of the overall loss. This is slightly higher compared to the age 31-34 sample. The effect remains higher in absolute terms at the second and third quartile compared to my younger group, lying between 250 Euros and 550 Euros. For this part of the distribution, the indirect effect explains between 7% and 18% of the overall loss which is lower compared to 20% to 31% estimated for my age 31-34 sample, but it is still

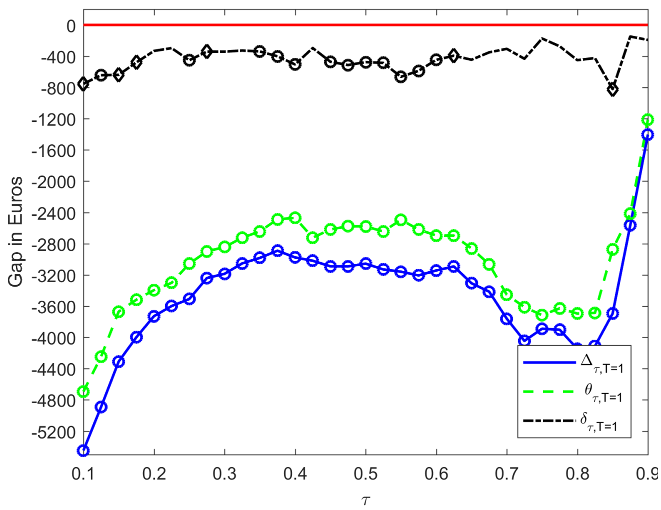
Figure D.1: Results of Decomposition - Workers Age 40 to 44



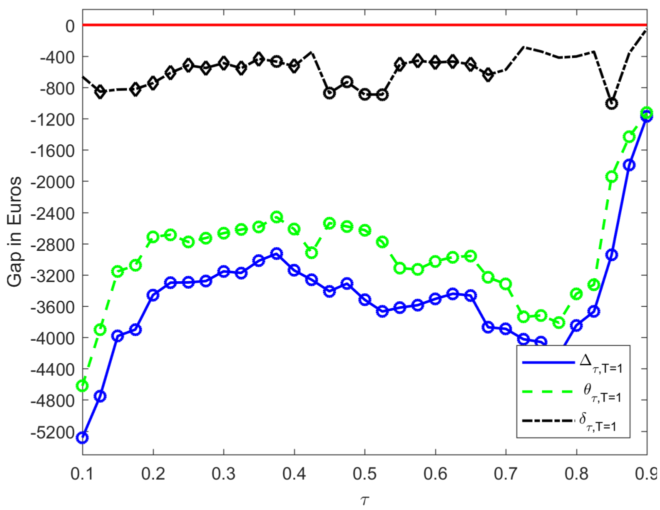
a. Overall Sample



b. Continuously Employed



c. Small Firms



d. High Local Unemployment Rate

All effects are reported for $\tau \in [.10, .90]$ at .025 unit intervals. The total effect $\Delta_{\tau,T=1}$ is depicted by the solid, the direct effect $\theta_{\tau,T=1}$ by the dashed, and the indirect effect $\delta_{\tau,T=1}$ by the dotted-dashed line. The markers show the significance levels: a circle significance on a 5%-level and a diamond significance on a 10%-level. Results are obtained by using the trimming approach described in Section A. Standard errors are obtained using 999 bootstrap replications. The treatment group consists of workers displaced at age 41 to 45, the control group of all workers displaced at age 46 to 49. The *Small Firm Sample* consists of all workers who were previous to the reference date employed in firms with an average size of less than 21 people. The *Continuously Employed Sample* consists of workers who were all five years before the reference quarter continuously employed. The *High Unemployment Sample* consists of all workers who were employed in areas with an unemployment rate above the median in the year of the plant closure.

quite substantial. At the top 75th to 90th percentile, $\delta_{\tau,T=1}$ is, with exception of the 85th percentile, lower as the results presented in Figure 4 and also less precisely estimated.

I come to a similar conclusion looking at the results for $\delta_{\tau,T=1}$ by subgroups, depicted in Panel b. to d. in Figure D.1. The indirect effect is now slightly higher in absolute terms at the bottom and less pronounced at the top. The general picture remains, however, unchanged and my conclusions drawn in the previous section remain valid. Interestingly, even for this sample I find that over parts of the wage distribution workers displaced in high unemployment areas suffer smaller total losses. My indirect effect can explain a larger share of the total loses for this subgroup pointing toward a similar wage growth - employment stability trade off as for my younger sample.

The results in this section show that interim employment has in general a negative effect on future wages along the distribution regardless of age at displacement. The next section shows that this can be partly explained by a trade-off between wage growth and stable employment.

E Additional Results

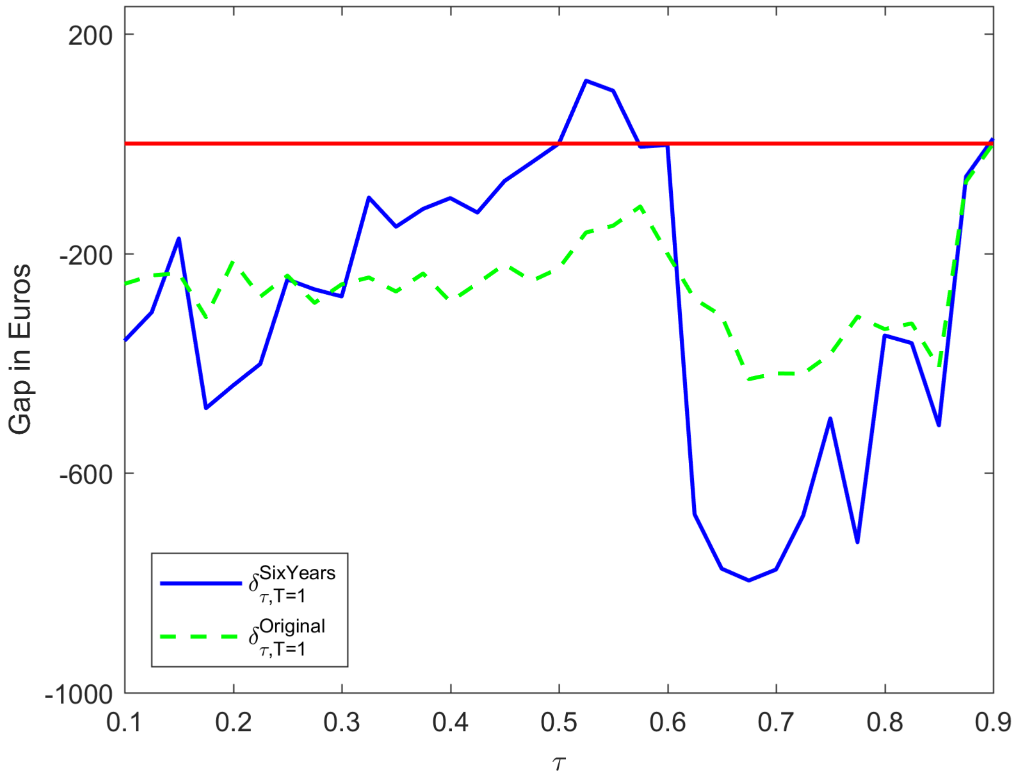
In this section, I present estimation results using an alternative control group. In addition, I show that anticipation of a future plant closure for individuals in my control group is unlikely to drive my main results.

E.1 Results with Alternative Control Group

I define the new control group applying the same criterion as in Section 2 in the main part of the paper but use the outcome six years *before* the actual event. The results the indirect effect, $\delta_{\tau,T=1}$, are presented in Figure E.1. In order to facilitate comparison, I also present the original estimates in the figure.

The estimates using the alternative control group are relatively similar to my original estimates at the lower part of the distribution, but diverge at the upper part. From the 60th percentile onward, the new estimates lie below the original ones, suggesting a likely

Figure E.1: Results Indirect Effect for Alternative Control Group



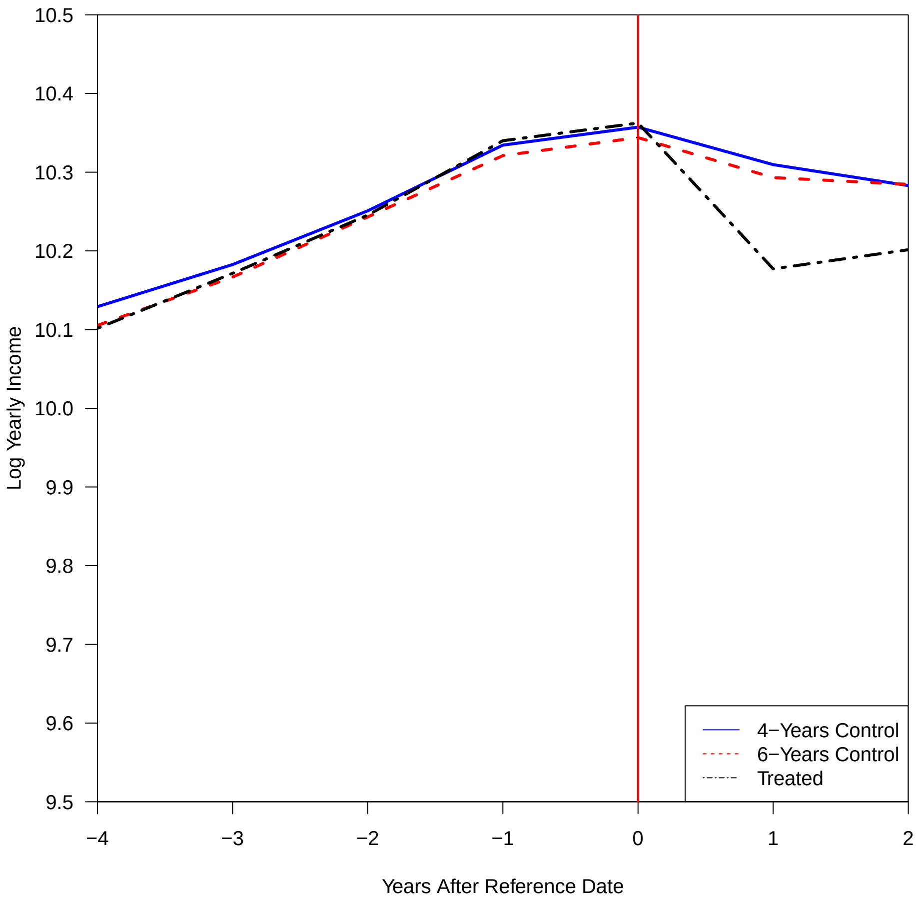
All effects are reported for $\tau \in [.10, .90]$ at .025 unit intervals. The solid line presents the estimates of $\delta_{\tau,T=1}$ for the alternative control group where the same criterion as in Section 2 in the main part of the paper are applied but the outcomes six years *before* the actual plant closure are used. The dashed line presents the original estimates of $\delta_{\tau,T=1}$.

positively selected control group when considering my alternative definition. The main conclusions from the main part of the paper are, however, unchanged even when using this positive selected control group and are likely to constitute a lower bound (in absolute terms) on the effect at the upper part of the distribution

E.2 Anticipatory Effects on Wages

One concern with my identification strategy might be that individuals in my control group can anticipate a future plant closure and change their behavior accordingly. I evaluate this possibility by looking at the average yearly log wage for individuals in my control groups from 4 years prior to the reference date until 2 years afterward.

Figure E.2: Log Yearly Income for Controls



The figure shows the average log yearly income for individuals in my control groups 4 years prior to the reference date until 2 years afterward. The solid line presents the results for the control group used in the main analysis. The dashed line presents the alternative control group used in Appendix E

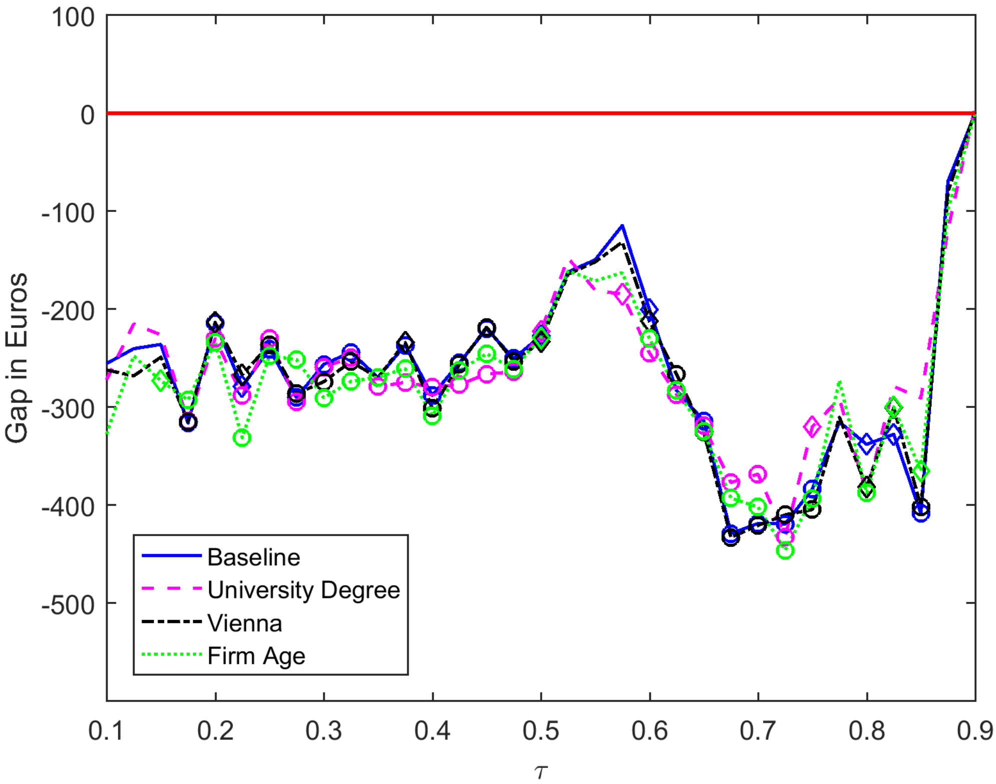
The results can be seen in Figure E.2. For comparison purposes, the figure also shows the average yearly log wage for individuals in my treatment group. The results do not indicate any sign that individuals in my control groups are anticipating a future plant closure. For both control groups, using a 4 and 6 years difference, annual wages do not exhibit any sudden changes. The wages are also similar to those observed for my treatment group prior to job displacement.

F Simulating Selection into Treatment

Section 7 presents the simulation results when U is based on mediator status. This appendix contains the sensitivity of my results when Assumption 1 is violated. In particular, U is now simulated using $p_{ij} = P(U = 1|T = i, Y = j)$, for $i \in \{0, 1\}$ and $j \in \{0, 1, 2, ...\mathcal{Y}\}$ and based on the University Degree, Vienna location, and Firm Age dummies. Like in Section 7 in the main text, the outcome variable is transformed into a multi-valued discrete variable using cut-offs determined by 10 equally spaced points over the support of Y. The results are based on 500 simulations and inference is done using 999 bootstrap replications.

Figure F.1 depicts the results from this exercise. Similar to my findings when assessing violations of the sequential ignorability assumption, the results are not sensitive to certain departures from Assumption 1. Both my baseline and simulated results are very close to each other and do not indicate any different conclusions as drawn in the main part of this work. The only notable difference is the lower (in absolute terms) estimated effect for $\delta_{\tau,T=1}$ at the 85th percentile when basing the simulation on the University dummy. Here, the difference is 117 Euros compared to the baseline. Over the rest of the distribution the absolute difference is rather small with a maximum of 90 Euros. Similarly, I do not find large divergence from my baseline results using the Vienna location and Firm Age dummy. Here the maximal absolute difference is 44 Euros and 72 Euros respectively.

Figure F.1: Results of Sensitivity Analysis - Selection into Treatment



All effects depicted is the indirect effect $\delta_{\tau,T=1}$ reported for $\tau \in [.10, .90]$ at .025 unit intervals. The continuous line refers to the baseline effect. The simulated effect using the binary University variable is depicted by the dashed, the effect using the Vienna location dummy is depicted by the dashed-dotted-line, and the effect basing the simulation on firm age by the dotted line. Effects are very similar at the lower part of the distribution and are therefore hard to distinguish in the graph. Simulated effects are based on 500 repetitions. The markers show the significance levels: a circle significance on a 5%-level and a diamond significance on a 10%-level. Results are obtained by using the trimming approach described in Appendix A. Standard errors are obtained using 999 bootstrap replications.

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