It's who you used to know: professional networks, heterogeneity, and inequality*

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Abstract

This paper studies the effect of an exogenous reduction in the size of a worker's professional network, due to the unexpected death of a past coworker. Using administrative data from Sweden and a matched event study, I find that unexpectedly losing a connection lowers employment by 0.4 percentage points after two years, with slightly larger effects on earnings. Workers are less likely to work in the deceased connection's past workplace post-treatment and treated unemployed workers, in particular, take longer to find a job. I explore heterogeneity in the empirical value of a connection, using the Generalised Random Forest. Valuable connections were older and higher-earning before their death and had a smaller set of connections competing to receive information from them. Using these estimates to quantify the total value of a worker's network I find that removing networks would increase inequality. Workers at the 25th percentile of earnings become 10 percentage points less likely to be employed after losing their network, while workers at the 75th percentile of earnings are not affected.

Keywords: networks, job search, referrals

JEL codes: J30, J20, J62

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1 Introduction

Understanding the sources of earnings inequalities between workers is one of the oldest questions in labour economics. A major source of earnings inequality not explained by observable differences between workers is the existence of frictions in the labour market. Search frictions lead firms to offer different wages to attract and retain workers (Burdett and Mortensen, 1998; Mortensen, 2003; Hornstein et al., 2011). Workers who face fewer search frictions extract a greater share of bilateral rents and experience faster wage growth (Postel-Vinay and Robin, 2002; Cahuc et al., 2006; Caldwell and Danieli, 2024). Information frictions can lead to involuntary unemployment when workers and firms form matches that are ex-post unprofitable (Jovanovic, 1979; Nagypál, 2007). To overcome frictions in the labour market, workers rely heavily on networks of informal contacts to find jobs. Across countries, 25 to 80 per cent of jobs are found through informal contacts, with a consensus that the majority of jobs are found through networks (Topa, 2019).

Because personal networks play an important role in overcoming labour market frictions, leading authors have argued that differences in social networks are an important source of economic inequality (e.g. Ioannides and Loury, 2004; Jackson et al., 2017). However, to evaluate the contribution of networks to inequality, we need a measure of the total effect of a given connection on employment and earnings. The total effect of a connection is a function not only of the direct effect of a connection on individual frictions, but also of how these frictions interact and of the endogenous responses of workers to the frictions they face. For example, connections are thought to reduce search frictions (Beaman, 2012; Calvó-Armengol and Jackson, 2004, 2007), raising the arrival rate of job offers for a given search effort, leading to higher employment and earnings. Connections are also thought to reduce information frictions (Dustmann et al., 2016; Montgomery, 1991; Simon and Warner, 1992), increasing offered wages, again increasing earnings. Depending on the relative strength of income and substitution effects, workers may respond to a greater arrival rate of offers or an improved wage offer distribution by increasing or decreasing their search effort, amplifying or attenuating the direct effect of a connection on earnings and employment.

In this paper, I estimate the total value of a connection, focusing on a layer of workers' networks that is likely to be particularly important for job search: past professional connections. The total effect of a connection cannot be recovered from the effect of a connection on individual frictions without assumptions on individual behaviour. However, the total effect of a connection does have a direct empirical counterpart, since it can be identified as the reduced-form effect of adding or removing a connection from a worker's network. I estimate this effect using the natural experiment created by the unexpected deaths of former coworkers.

Using population-level matched employer-employee data for Sweden, I define an individual's relevant professional contacts as people they shared a workplace with in the recent past, but with whom they no longer work. I focus on 4634 deaths of past coworkers during the period 1991–2010 where the cause of death was recorded as accidents or the intervention of other external forces. Even unexpected deaths may be predictable, in a statistical sense. For example, individuals with more past coworkers are more likely to be exposed to a past coworker death. I therefore employ a matching strategy, using an estimated propensity score to match each death in my sample with a set of placebo deaths, similar to approach proposed by Azoulay et al. (2010) and Jäger and Heining (2022). I conduct a matched event study to estimate the dynamic effect of a death on labour market outcomes, comparing individuals affected by the true death of a past coworker with individuals affected by the placebo death of a past coworker. I find that losing a past coworker has a negative average effect on workers' labour market outcomes. Earnings fall by around 1900 SEK after two years, or by 0.9 percent relative to earnings in the control group. This is largely due to a reduction in employment, which falls by around 0.4 percentage points after two years.

I next turn to studying how workers' job search is affected by the loss of a connection. The natural experiment I study potentially affects all frictions workers face. It therefore provides a unified empirical setting for establishing whether connections are more important in overcoming certain frictions. I provide evidence that points most clearly to increased search frictions after the loss of a connection. After treatment, unemployed workers are 1.2 percentage points less likely to find a job in the first two years of unemployment. In particular, they are less likely to find a job in a workplace employing a former coworker. However, there is also some evidence of workers facing increased information frictions. Treated individuals are 0.4 percentage points more likely to leave a new job for unemployment in the first year than control individuals, and they are offered jobs with marginally lower wages. Both these patterns are consistent with greater information frictions (Dustmann et al., 2016).

Finally, I study the relationship between connections and inequality. The use of job search networks may explain part of observed inequality if there is either differential exposure to high-quality potential connections, due e.g. to different forms of segregation (Miller and Schmutte, 2021; Willis, 2022), or differential effects of exposure to the same connection, due e.g. to homophily, the tendency to preferentially share information with similar connections (Currarini et al., 2009; Bayer et al., 2008; McPherson et al., 2001). Both differential exposure and differential effects are forms of heterogeneity in the total effect of a connection. I use a machine learning algorithm, the Generalised Random Forest (GRF, Athey et al., 2019) to empirically characterising heterogeneity in the effect of losing a connection. The GRF allows me to simultaneously consider the large number of possible sources of heterogeneity in the value of a connection that have been proposed in the literature without overfitting the data.

I find that the effect of losing a connection is largest when the deceased individual is connected to few treated individuals and when the treated individual is younger and less strongly attached to the labour market. Demographic characteristics are no particularly important predictors of the value of a connection. Workers more weakly attached to the labour market are therefore more harmed by the loss of a connection. To study the effect of networks on inequality, I use the output of the GRF to predict the employment effect of removing a randomly drawn individual from the network of all my sampled individuals. Again, the effects are largest for workers who have just entered the labour market. Demographic characteristics of the treated worker, such as gender or nativity, are of second-order importance. I conclude that the use of networks in job search is a substitute for observable human capital and therefore tends to reduce inequality, rather than increase it.

The major challenge to the approach I propose is that the death of a past coworker might also affect earnings and employment through non-network channels. However, the cumulative evidence suggests that the network channel is likely to be the main mechanism explaining my estimated effect. First, the effect of a lost coworker is negative and there is no effect on the probability of returning to work at the workplace the treated individual last shared with the deceased individual. These findings suggest that shocks to demand for the surviving worker's labour (e.g. Jäger and Heining, 2022) do not explain the effect, nor do changes in the treated individual's perceptions of the attractiveness of a particular types of workplace. The effect is also gradual, which is inconsistent with previous findings on the effect of grief on wages and employment (van den Berg et al., 2017). Finally, the largest negative effects are observed following the death of older, higher-earning individuals, who are more likely to have better information to pass on. This pattern suggests that the effect is unlikely to be due to changes in either the wage–amenity or labour–leisure tradeoff workers make as they update their perceptions of their own mortality risk.

My findings contribute to the empirical literature on the effects of professional connections on labour market outcomes. Most recent studies continue to focus on specific frictions in isolation, typically using within-firm designs to study a subset of job seekers or job switchers. They find that professional connections can reduce search frictions for employed workers (Caldwell and Harmon, 2019) and unemployed workers (Cingano and Rosolia, 2012; Glitz, 2017; Eliason et al., 2023) and reduce information frictions in the job matching process (Glitz and Vejlin, 2021; Hensvik and Skans, 2016). In contrast to those papers, I rely on a natural experiment, which is robust to unobserved heterogeneity in firms' demand for different types of workers as well the possibility that connections to firms are proxies for "typical" patterns of worker mobility for similar workers.[1] It also allows me to study the effect of a connection for a larger population. I find larger effects on earnings and employment than would be implied by previous findings on the effect of a connection on a single friction. My findings therefore suggest that interactions between frictions, and the endogenous responses of workers, may be important for understanding the effects of professional connections on job search.[2]

The second contribution of the paper is to the literature on heterogeneity in the value of social connections. Previous empirical work has generally considered the importance of individual characteristics, or some small subset of the set of possibly relevant characteristics. The

[1] In a related paper, Wang (2013) finds a substantial negative effect on earnings from the death of a father-in-law in China. However, the effect of this type of connection is not likely to be directly comparable to the effect of a typical past coworker and Wang argues that fathers-in-law in China affect wages through nepotism, rather than by reducing labour market frictions, as I argue here.

[2] Workers' endogenous responses to losing a connection will likely be different for workers at different points of the earnings distribution, due to different income effects. This would attenuate the direct effects of losing a coworker for high-earnings individuals and amplify them for low-income individuals, consistent with my findings.

role of race (Pedulla and Pager, 2019), nativity (Åslund et al., 2024), gender (Beaman et al., 2018), education levels (Hensvik and Skans, 2016), or age (Loury, 2006) have been highlighted. Homophily, or similarity between individuals has also been argued to be an important predictor of the value of a connection (Bayer et al., 2008; Currarini et al., 2009; McPherson et al., 2001). In contrast, I find that economic variables are more important predictors of the value of a connection than socio-demographic variables. Connections are particularly valuable for workers less attached to the labour market and valuable connections are those that are themselves well-placed in the labour market.

My findings also highlight the importance of network structure in shaping the value of a connection. Theoretical papers have argued that ties with whom workers share few common ties are particularly valuable, since these individuals are more likely to provide new information (Granovetter, 1973), or because we face less competition to receive information from these connections (Calvó-Armengol, 2004; Calvó-Armengol and Zenou, 2005). However, previous descriptive empirical evidence has suggested that a strong degree of overlap in two workers' connection set is in fact a strong predictor of the value of a tie (Gee et al., 2017a,b). In contrast, I find clear evidence of competition between connections. Valuable connections are those who have relatively few other workers to pass on job information to.

Finally, I contribute to our understanding of the contribution of professional networks to labour market inequality. The widespread use of social networks to overcome labour market frictions has led many authors to suggest that differences in workers' social networks could be an important contributor to differences in observed earnings (e.g. Calvó-Armengol and Jackson, 2004, 2007; Ioannides and Loury, 2004). However, my estimates of the total value of a worker's professional network suggest that lower-earning groups, and in particular younger workers, are most severely harmed by losing a connection. This finding is consistent with recent evidence showing that there is less sorting in network hires than non-network hires (Eliason et al., 2023), since networks are used to help workers and firms form matches that would not be made through the market. My findings also generalise prior findings on the larger importance of social networks in the job search of groups of workers who are more weakly attached to the labour market, including new immigrants (Beaman, 2012; Damm, 2009; Edin et al., 2003) or school leavers (Kramarz and Skans, 2014; San, 2022).

The paper proceeds as follows. I outline a conceptual framework of information sharing in a network that guides the empirical analysis in Section 2. In Section 3 I describe the data and sample used. In Section 4 I report event study estimates of the average effect of a lost connection on earnings and employment. In Section 5 I analyse the effect of the treatment on job search outcomes. In Section 6 I report and interpret GRF estimates of heterogeneity in the value of a connection before concluding in Section 7.

2 Theoretical framework

To understand what is identified by the death of a past coworker, and illustrate some of the sources of heterogeneity in the value of a lost connection, I outline a stylised theoretical model of information sharing in a social network, drawing on Calvó-Armengol (2004). The model abstracts from the provision of referrals and focuses on the role of connections in providing information about vacancies alone. A worker i is connected to a set of workers N_i, with cardinality n_i; the set of links between workers constitutes a undirected graph. Initially, workers are employed and face an exogenous separation probability of b.[3] Employed and unemployed workers receive a job offer with probability a. If unemployed, they take the offer, if employed, they pass it on to a worker drawn at random from their set of unemployed connections. The probability that individual i receives a job offer from their contacts is

$$P(N_i) = 1 - \prod_{j \in N_i} q(n_j), \tag{1}$$

where the probability that a worker i does not receive a job offer from connection j is

$$q(n_j) = 1 - a(1-b)\frac{1-(1-b)^{n_j}}{bn_j}. \tag{2}$$

Define the *marginal value of a link* as the change in the probability of receiving a job offer from the network, $P(N_i)$, when worker i becomes connected to worker j, keeping all other links

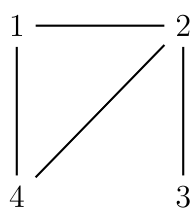
[3] The assumption that workers are initially employed corresponds to the empirical setting, where workers must be employed at some point in the network-building phase, in order to subsequently have former coworkers.

in the network constant. $P(N_i)$, is increasing in N_i, so the marginal value of a link is positive.[4] The marginal value of a link is also decreasing in the number of connections a worker already has, n_i. Furthermore, because information is rival in this setting, the probability of receiving a job offer from worker j, and hence the marginal value of a link to j, is decreasing in the number of j's connections, n_j.

In the empirical analysis below, I estimate the effect on earnings and employment of removing a worker from the network altogether. Define the *net value of connection j for worker i* as the change in the probability of receiving an offer from the network when worker j is removed from the network, i.e. all of j's links are deleted simultaneously. Removing j from the network will have a negative *direct* effect on the probability i receives a job offer, since the marginal value of a link is positive. The magnitude of the direct effect will vary with the marginal value for i of the link to j; the direct effect is decreasing, i.e. less negative, with both n_i and n_j. Removing worker j will also have a positive *indirect* effect. Decreased competition from j for job offers from i and j's remaining mutual connections, $j' \in N_i \cap N_j$, increases the marginal value to i of the remaining mutual links. The indirect effect is decreasing in $n_{j'}$ and n_i.

Calvó-Armengol and Zenou (2005) show, for the special case of symmetric networks, where $n_i = n_j = n$, $\forall i, j$, that there is a unique common degree $\bar{n}$ such that for $n > \bar{n}$ the probability of receiving an offer from the network is decreasing in n. In this case, the indirect effect dominates, so the net value of a connection is negative. More generally, the relative strength of the direct and indirect effects will be a complex function of the geometry of the network. For a given set of connections, N_i, the indirect effect will be relatively greater in magnitude the larger the share of i's connections who are also in the set of j's connections, N_j.

Figure 1: Network structure and the value of connections



Consider the example in Figure 1. Worker 3 is only connected to worker 2. Deleting worker

[4] For $N_i^1 \subseteq N_i^2$, $P(N_i^1) \leq P(N_i^2)$, keeping all other links in the network constant.

3 from the network has only a direct effect on worker 2, unambiguously reducing the probability that they receive a job offer from the network. On the other hand, worker 4 is connected to workers 1 and 2, who compete to receive job offers from 4. The direct effect of deleting worker 4 from the network will be to reduce the probability that worker 2 receives an offer from worker 4. The indirect effect will be to increase in the probability that worker 2 receives an offer from worker 1. The net effect for worker 2 in this simple case is still negative, however smaller in magnitude than the effect of deleting worker 3 from the network. Furthermore, deleting worker 4 from the network will decrease the probability that worker 1 receives a job offer by more than it decreases the probability that worker 2 receives an offer, since worker 2 has more remaining connections. See Appendix A for proofs.

In this simple model,where workers are ex-ante identical and choose a recipient for information at random, the structure of links alone leads to heterogeneity in the value of a connection. Making workers in the model heterogeneous ex-ante, and assuming that worker characteristics affect who receives information, will in general increase heterogeneity in the value of connections. For example, workers might differ in their wages, and job offers may also be drawn from a wage distribution. Workers pass on job offers they do not accept to an individual randomly drawn from the set of connections whose current wage is below the offered wage. In this model, high wage workers, who are less likely to accept an offer, are more valuable, particularly when they are connected to other high-wage workers, rather than low-wage workers, reducing competition for low-wage offers among their connections (Calvó-Armengol and Jackson, 2007; Beaman, 2012). Worker may also preferentially share information with socio-demographically similar individuals, in line with models of homophily (Jackson, 2019; McPherson et al., 2001), rather than select a recipient at random.

3 Deceased, placebo, treated, and control individuals

3.1 Data sources

The empirical analysis is based on administrative data for the entire Swedish population 16–64 years old during the period 1985–2018. With unique personal identity numbers, it is possible to link individual data from different registries (created by different government agencies) held at Statistics Sweden.[5] The data requirements for constructing the network variables, knowing the cause of death, and observing individuals for a sufficient window on either side of the death of a coworker mean that I analyse the effect of deaths occurring in the years 1991–2010.

Information on employment and earnings is drawn from an employment register which contains a link between gainfully employed persons and the firms and workplaces at which they work (*Registerbaserad arbetsmarknadsstatistik*, RAMS). Through statutory income statements, filed by employers to the Swedish Tax Agency, both the employee and the firm and establishment is identified. The employment register also includes information on job spells which I use to identify past coworkers, discussed in greater detail below.

I also make use of the Longitudinal integrated database for health insurance and labour market studies (LISA) from which I collect information on gender, birth year and level of education for the entire population. The county of birth for the Sweden-born population and the country of birth or a region of birth for the foreign-born population are collected from a separate table based on the Total population register (*Registret över totalbefolkningen*, RTB). The cause of death is recorded in the Cause of death register (*Dödsorsaksregistret*).

3.2 Defining the set of professional connections

To identify an individual's set of relevant job search contacts, I proceed in several steps. First, employment spells, which are reported in months, are collapsed to a single main job for each individual and year. This is defined as the workplace where the individual is employed in the month of December and from which the individual has the highest annual earnings. If an individual is not employed anywhere in the month of December, no main job is defined for that year. All subsequent analyses build on the annual panel of main jobs to construct firm characteristics, individual networks, etc. The only exception is individual annual earnings, which I calculate using all employment spells in a calendar year, deflate to 2018 Swedish crowns (SEK), and winsorise at the 99th percentile.

To identify individual's professional connections in year $t-1$, the year before they are potentially affected by the death of a connection, I first identify their workplaces during the

[5] In practice, when linking registers, a pseudonymised personal identification number is used.

past five years, i.e. in $[t-6, t-2]$. I then define an individual's connections as all coworkers from the past five year years who are no longer their coworkers in year $t-1$. A connection may therefore be created through the focal individual's job mobility, when they leave a past workplace, or through a past coworker's mobility. Given the definition of a connection I use, the size of an individual's professional network will be a complex function of the following factors:

1. Attachment to the labour market, measured as an individuals' employment rate during the network-building phase;

2. Worker mobility, measured as the number of job transitions during the network-building phase;

3. Average size of the workplace during the network-building phase.

An increase in any of these factors, for either a focal individual or their coworkers during the network-building phase, will in general increase the size of an individual's network.

I impose the restriction that the largest past workplace where an individual was employed during the network-building phase must have at most 100 employees. This restriction ensures that the focal individual likely had some interaction with their past colleagues. Prior evidence in the Swedish context has shown that the probability that a past coworker leads to a hire in their current firm is decreasing in past workplace size and falls to zero when the past workplace employed more than 100 workers (Eliason et al., 2023). Furthermore, the workplace identifier is not defined for certain large firms (municipal public firms, firms operating on multiple sites, e.g. firms in construction or cleaning services). This restriction implies that I drop individuals having worked in such firms. I do not place any further restrictions on either the focal individual or their connections' employment status in year $t-1$.

3.3 Deceased and treated individuals

Next, I identify unexpected coworker deaths. Treated individuals in year t are those affected by the death of any of their connections in calendar year t, where the set of connections was defined in year $t-1$. I impose the following restrictions on treated and potential control individuals:

1. they are aged 19–54 in year $t-1$;

2. their full set of professional connections is observed in year $t-1$;
3. they do not die themselves in year t;
4. they are affected by the death of at most one past coworker in year t;
   (a) each treated individual is affected by the death of exactly one past coworker, referred to as the deceased individual.
   (b) each control individual is not affected by the death of any past coworkers in t.

Deaths due to, e.g. chronic illness or suicide may, to some extent, be anticipated by a deceased individual's past coworkers. To prevent treated individuals from adjusting their job search behaviour before the death of their past coworker, I use information on the cause of death to restrict the sample of deceased individuals to those whose death was unexpected.[6] Even medically unexpected deaths may be predictable in a statistical sense. Observable characteristics, such as past earnings trajectory, gender, education or age may be associated with the risk of exposure to the unexpected death of a past coworker. Indeed, a larger set of connections implies a greater risk of one of those connections dying unexpectedly. To ensure that such associations do not bias the comparison of treated and control individuals, each deceased individual will be matched with ten placebo deaths, using a procedure described below. The past coworkers of the placebo death, who satisfy the set of restrictions above, are the control individuals. I impose the following restrictions on the included deceased and placebo deceased individuals:

1. they are aged 19–59 in year $t-1$;
2. their full set of professional connections is observed in year $t-1$.[7]

[6] Unexpected deaths are those due to transport accidents, accidental drowning, accidental poisoning, falls, fire and hot substances, and other external causes, corresponding to codes V01–V99, W65–W74, X40–X49, W00–W19, X00–X19, and W20–W64, W75–W99, X20–X39, or X50–X59 respectively in the ICD10 classification. These exclude deaths due to intentional self-harm or assault.

[7] The size restriction on the largest past workplace implies that knowing an individual's set of connections does not imply that their connections' connections are all known. A treated individual's set of past coworkers must be observed, however some of their past coworkers' sets of connections may not be observed, if their past coworker was employed at some point in a workplace with more than 100 employees.

3.4 Matched sample of placebos and control individuals

To identify a suitable set of control individuals, I employ a matching strategy, similar to Azoulay et al. (2010) and Jäger and Heining (2022). The matching takes place at the level of true/placebo deceased individuals. For all individuals satisfying the second set of restrictions above, I estimate a propensity score model, where the outcome is dying unexpectedly. The set of explanatory variables includes demographic and economic characteristics of the deceased/placebo individuals as well as average characteristics among their connections satisfying the first set of restrictions above (i.e. among the treated/control individuals).

The sample of true deaths and potential placebos is relatively large, ranging between 1.3–1.9 million observations annually, while the risk of an unexpected death in this sample is small, of the order of 0.01 per cent. Traditional estimation methods, such as OLS, will lead to a risk of overfitting in this setting. I therefore estimate a logit model of the propensity score via the lasso, one year at a time, including higher-order terms and interactions of the included characteristics. However, since the outcome is a very rare event, this procedure selects only a very limited set of predictors. Convergence rates for the lasso are slower than for traditional methods, so the lasso may suffer from finite-sample bias in this setting. Indeed, matching on the lasso-estimated propensity score leads to residual imbalance in the matching variables.

I therefore augment the lasso propensity score estimation as follows. I estimate a model of average earnings of included connections (treated/potential control individuals) in t, the year of death, on a one per cent sample of potential placebo individuals, via the lasso. I then estimate the propensity score on the full sample using a logit model to regress an indicator for dying in t on the union of the set of predictors selected by the original lasso propensity score estimation and those selected by the outcome model estimation. Each deceased individual is matched with ten placebo individuals via nearest neighbour matching on the estimated propensity score, without replacement. Finally, the connections of the placebo individuals are identified as the control observations.[8]

The procedure described above could be considered a propensity score matching analogue of

[8] A control individual may, in rare cases, be connected to more than one included placebo individual. In this case I treat each control-placebo pair as a separate observation. A treated individual is connected to exactly one deceased individual. Treated individuals may be selected as control observations in a year where they are not affected by death if they are connected to a selected placebo. Again, the control-placebo pair is treated as a separate observation to the treated-deceased pair.

the post-double selection procedure proposed by Belloni et al. (2013) for estimating treatment effects via linear regression with a high-dimensional control set. It is also similar in spirit to an approach used in the synthetic control literature. Synthetic control methods are normally used in situations where treatment is very rare, typically limited to a single treated unit, and treatment is assigned as an outcome of some unobserved (or at least unmodelled) process (e.g. a political process). To obtain a valid counterfactual, pre-treatment values of the outcome are usually given a large weight when constructing a synthetic control observation (Abadie, 2021). In my case, where treatment is also very rare, rather than explicitly using pre-treatment values of the outcome for matching, I select good predictors of the earnings in year t in the untreated group. Pre-treatment values of the earnings can then be used to asses the success of the procedure in identifying a valid comparison group. Further details on the propensity score estimation are provided in Appendix B.[9]

3.5 Analysis sample

The restrictions in Section 3.3 lead to a sample of 4634 unexpected deaths. The deceased individuals are connected to 76,351 included treated individuals. The 46,340 matched placebo deaths are connected to 760,449 included control individuals. Basic summary statistics for the treated and control individuals are reported in Table D.1. Summary statistics for the average characteristics of their connections are reported in Table D.2. The average sampled individual has 65 connections, though the distribution is skewed, the median is 55 connections. On average, a treated/control individual shares just over half of their set of connections (51 per cent), with the deceased/placebo individual. Treated/control individuals last worked with the deceased/placebo individual 2.9 years ago in the year before the death, $t-1$. The treated and control individuals earn on average around 10 per cent more than their connections and are five to six percentage points more likely to be employed. There are no significant differences between treated and control individuals in their observable characteristics or in the average characteristics

[9]Alternative matching procedures I have considered are (i) matching on the lasso-estimated propensity score within cells defined by combinations of gender, network size, or past employment; and (ii) matching on propensity scores estimated with a regression forest. Again, I do not use pre-treatment earnings in $[t-4, t-1]$ in either of these approaches. There are no pre-trends in earnings using either the alternative approaches or my proposed approach, which is the key identifying assumption in Section 4. However, both alternative approaches produce analysis samples with greater imbalance on socio-demographic characteristics, supporting the use of my proposed approach.

of their connections, with the exception of the share of connections female. Since the average characteristics of treated/control individuals' connections were not used in the matching, the lack of imbalance suggests that the procedure described above identifies a very similar set of control individuals.

I report summary information on the deceased and matched placebo individuals in Table D.3 as well as the distribution over industries of the workplace where the deceased-treated or the placebo-control pairs last interacted, in Figure D.1. The deceased and placebo individuals have on average 35 connections, just over half as many as the treated and control individuals; they also earn around 18 per cent less and are 12 percentage points less likely to be employed in the year before the death. There are some small imbalances between true deaths and placebos in industry of employment, which was not used in the matching, and demographic characteristics, in particular sex. The deceased are 9 percentage points less likely to be female than the matched placebos. These differences arise because the demographic characteristics are not strong predictors of either unexpected death or connections' subsequent earnings, conditional on economic variables such as earnings or network variables such as number of connections. The post-double selection procedure for estimating the propensity score therefore gives them little weight. Imbalances in deceased and placebo individuals' characteristics will not affect the estimation of the average effect of the death of a coworker, in Section 4, which relies on a parallel trends assumption. Deceased and placebo characteristics may, however be relevant come the estimation of heterogeneous treatment effects, a point I return to in Section 6.

4 The average value of a professional connection

4.1 Event study design

To estimate the average effect of the death of a past coworker on earnings and employment, I conduct an event study on the matched sample of treated and control individuals, similar to the strategy of Jäger and Heining (2022). The general empirical specification is summarised by the following equation:

$$y_{ijtk} = \alpha_{ijt} + \sum_{s=-5}^{6} \gamma_s \mathbf{1}(k = s) + \sum_{s\neq -1, s=-5}^{6} \beta_s \mathbf{1}(k = s, ijt = treated) + \epsilon_{ik}. \tag{3}$$

The outcome of interest (earnings or employment) is denoted y_{ijtk} for treated or control individual i, affected by the true or placebo death of j in calendar year t. The outcome is observed in event year k, where $k = 0$ corresponds to the year of death, t. Individuals affected by multiple true or placebo deaths in different years are considered distinct observations; ijt therefore indexes unique panel observations. α_{ijt} is an individual fixed effect. γ_s measure the effect of event time dummies, while β_s measure the dynamic treatment effect.

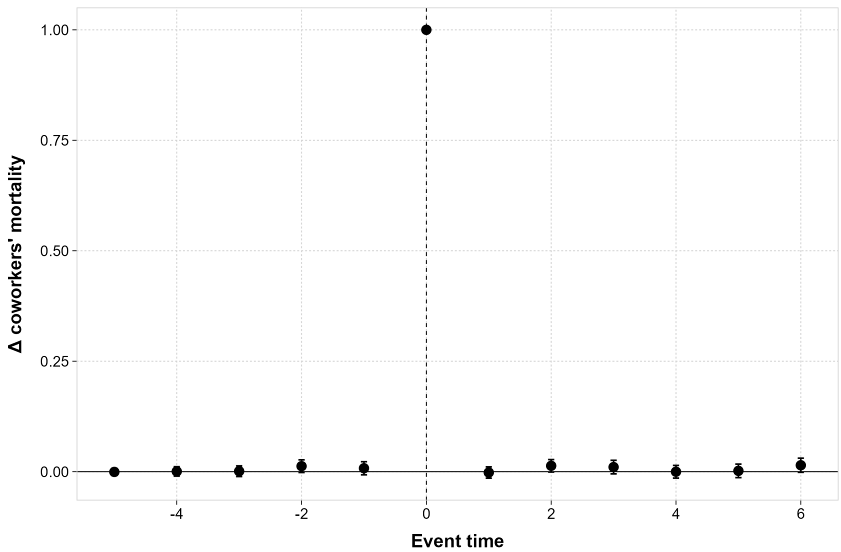
In Figure 2, I plot differences between treated and control individuals in total all-cause mortality in their set of connections, defined in $t-1$.[10] By construction, this difference is one in year t. There is no evidence of treated individuals' connections facing a higher risk of mortality in general, nor is there any evidence of mean-reversion in the relative mortality of control individuals' connections outside of the event year. Instead, the death of a past coworker is related one-to-one to changes in the size of a treated individual's professional network.

The event study specification will identify the reduced-form effect of the death of a past coworker on subsequent outcomes under a parallel trends assumption. Specifically, if the untreated potential outcomes follow a parallel trend across the treated and untreated individuals, conditional on the propensity score, then the coefficients on the post-treatment dummies β_s, $s \geq 0$, identify the ATT, the average effect of unexpectedly losing a past coworker on the treated.

Given an exclusion restriction, the ATT can be interpreted as the net value of losing a connection, defined in Section 2. The exclusion restriction requires that the death of a past coworker affects earnings and employment only through its effect on the size and composition of the job-search network. The exclusion restriction will hold if workers do not otherwise change their behaviour following the death of a past coworker, e.g. by voluntarily working fewer hours or exiting the labour force. I will discuss the plausibility of the exclusion restriction at various points throughout the presentation of the results. However, the maximum age restriction imposed on

[10]Mortality for either treated or control's connections may be positive for $k < 0$, since I do not impose the restriction that all past coworkers be alive in $t-1$.

Figure 2: Relationship between treatment and total past coworker mortality



Notes: Simple differences between treated and control in total deaths (all causes) among connections; connections are defined in $k = -1$. Standard errors are clustered by deceased-matched placebo strata, 95 per cent confidence intervals are shown.

the treated individuals implies that early retirement is not an option following the death of a past coworker, given pension eligibility requirements.

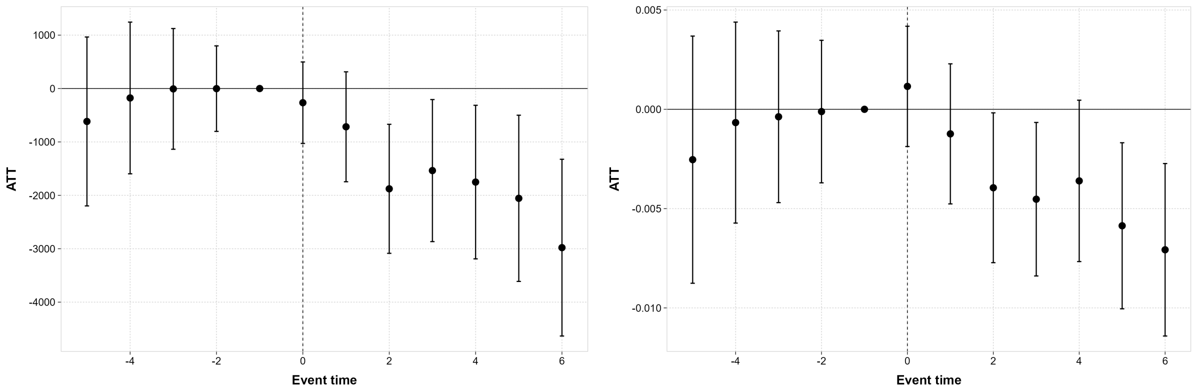
The panel is stacked, so it is balanced in event time; individuals not observed in the administrative registers in a given year are recorded as having zero earnings. Since the panel is balanced and adoption is not staggered in event time, Equation (3) can be estimated as a two-way fixed effects regression. Two-way fixed effects is, in this case, numerically equivalent to the difference-in-difference estimator of Callaway and Sant'Anna (2021). Treatment is defined at the level of the deceased or placebo individual j. Furthermore, in matched samples, treatment status is not independent within match strata (Abadie and Imbens, 2006). I therefore cluster standard errors at the level of the matched deceased-placebo stratum.

4.2 Average effect on earnings and employment

The average dynamic effect of the unexpected death of a past coworker on labour earnings is shown in Figure 3a. Affected individuals' earnings decline relative to the matched controls, however the effect is not immediate. Two years after the death, affected individuals' earnings are 1878 SEK lower than they would have been, which corresponds to 0.9 per cent of annual earnings in $t+2$ for the control group. In later years, the effect is approximately constant. Pre-treatment earnings of the treated individuals follows the same trend as the matched control individuals, though only average earnings in $t-5$ for those affected by the same true or placebo death were included in the estimation of the propensity score. Raw earnings for treated and control individuals are reported in Figure D.2.

Figure 3: Labour market effect of unexpected past coworker deaths

(a) Earnings (b) Employment



Note: The figures report event-study estimates the effect of unexpected deaths of past coworkers, occurring during 1991–2010, on real annual labour earnings, measured in 2018 Swedish crowns (Panel A) or a dummy for being employed at some point during the calendar year (Panel B). Coefficients are estimated on the matched sample, using Equation (3). Standard errors are clustered by matched deceased-placebo strata, 95 per cent confidence intervals are reported.

The average dynamic effect of the unexpected death of a past coworker on employment is shown in Figure 3b. The time pattern of effects is very similar to what was observed for earnings. Employment of affected individuals decreases gradually, relative to the control group. The effect becomes significant after two years and is relatively stable, with some weak evidence of further declines after $k = 4$. The treated are 0.4 percentage points less likely than the control individuals to be employed two or more years after the event, or 0.5 per cent of employment in the control group in $t + 2$.

The time pattern of effects lends preliminary support to the exclusion restriction, since a reduction in the flow of information from the network will have a gradual, cumulative effect on employment and earnings. In contrast, grief following the death of someone close has immediate effects on labour market outcomes (van den Berg et al., 2017). To quantify the magnitude of these effects, the deceased individuals are connected to 36 treated individuals on average.

This implies it would take seven such unexpected deaths ($\frac{1}{36\times 0.004} = 6.94$) for one extra treated individual to be unemployed at $t+2$.

In Figure D.3 I further probe whether the death of a past coworker has an effect on the labour demand facing the surviving coworker. First, I re-estimate Equation 3 using as an outcome an indicator for whether the workplace where the treated and deceased individuals most recently worked together continues to employ any workers in event year k. There are no significant differences between the past workplaces of treated and control individuals, implying that the treatment does not lead to differential exit of the past workplace. Second, I consider whether the treated or control individuals are working in the workplace they shared with the deceased or placebo individual. Again there is no effect. These results indicate the negative earnings and employment effects are not due to a reduction in labour demand from the past common workplace, or due to a reduction in the treated individuals' willingness to supply their labour to these workplaces.

5 Connections and job search

5.1 Job and non-employment spells

Professional networks have been shown empirically to (i) reduce search frictions in the process of finding a job for both the unemployed (Cingano and Rosolia, 2012; Glitz, 2017; Eliason et al., 2023) and the employed (Caldwell and Harmon, 2019); and (ii) reduce information asymmetries in the matching of workers and firms, improving match quality (Hensvik and Skans, 2016; Glitz and Vejlin, 2021). The negative earnings effect of losing a connection documented in Section 4 is likely due to treated individuals facing some combination of increased search and information frictions in the job market. To better understand which of these frictions is most affected by the loss of a connection, I now study the effect of treatment on the duration of job and non-employment spells and on wages in job spells.

The linked employer-employee data record employment spells at the monthly level. However, the start and end date of employment spells are plagued by measurement error, as firms report when wage payments were made, not the actual dates of employment. I therefore measure job and non-employment spells at an annual frequency comparing workers' main jobs in December, as defined in Section 3, in contiguous years. Wages are only available for a subset of workers, so I instead follow standard practice and calculate average monthly earnings in the main job and treat these as wages, censoring monthly earnings that are below the first percentile of the full-time wage distribution (e.g. Eliason et al., 2023).[11] However, this introduces substantial measurement error to wages, likely biasing estimated wage effects towards zero. Hours worked are not recorded in the RAMS data.

To study the relationship between losing a connection and information frictions, I estimate the effect of the treatment on the characteristics of different types of spells that start in years $[t, t+2]$, i.e. the year of the death and the two subsequent years. For robustness, I also report difference-in-difference estimates, using treatment-control differences in characteristics of spells that start in $[t-4, t-2]$ as the baseline.[12] To avoid contamination by the treatment, the duration of pre-treatment spells is censored at the end of $t-1$, affecting the precision of the difference-in-difference estimates of the effect of treatment on spell duration.[13]

5.2 Results

In Figure 5, I report differences between treated and control individuals in Kaplan Meier non-parametric hazard rates. If the death of a past coworker increases search frictions, treated individuals should become less likely to transition into a new job. Furthermore, if the treatment increases information friction in job matching, treated individuals should be more likely to leave a job early in their tenure, though not later in their job tenure (Dustmann et al., 2016).

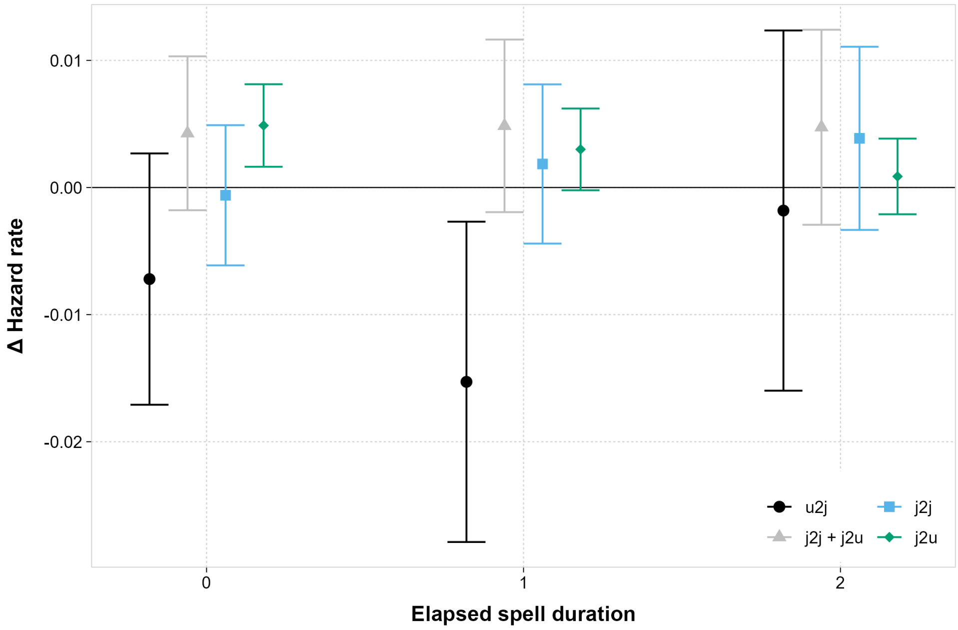
I find evidence of both these patterns. Unemployed workers in particular experience a large reduction in the exit rate from unemployment during the first two years of a spell, with no effect on the hazard rate conditional on having been unemployed two years. Pooling unemployment spells, the probability of exiting unemployment in the first two years is 1.2 percentage points lower in the treated group than in the control group (se = 0.5). Employed individuals, on the

[11] There are no significant differences between treated and control individuals in missing wages, nor are there changes pre- and post-treatment, see columns 1 and 2 of Table D.4.

[12] This analysis necessarily conditions starting a job or non-employment spell, itself an outcome of the treatment. A causal interpretation of the estimated treatment effects in this section is, therefore, subject to caution, as the treatment may affect individual selection into starting different types of spells.

[13] Censoring spell durations at the date of treatment implies that all spells starting in $t-1$ are censored at zero. These spells are therefore dropped from the analysis.

Figure 4: Effect of losing a connection on hazard rates



Note: Differences in Kaplan Meier nonparametric hazard rates between treated and control individuals for spells starting in $[t, t+2]$. Hazard rates for job spells (exit to job and exit to unemployment) are estimated under the assumption that competing risks are independent. Standard errors are clustered by matched deceased-placebo strata, 95 per cent confidence intervals are reported.

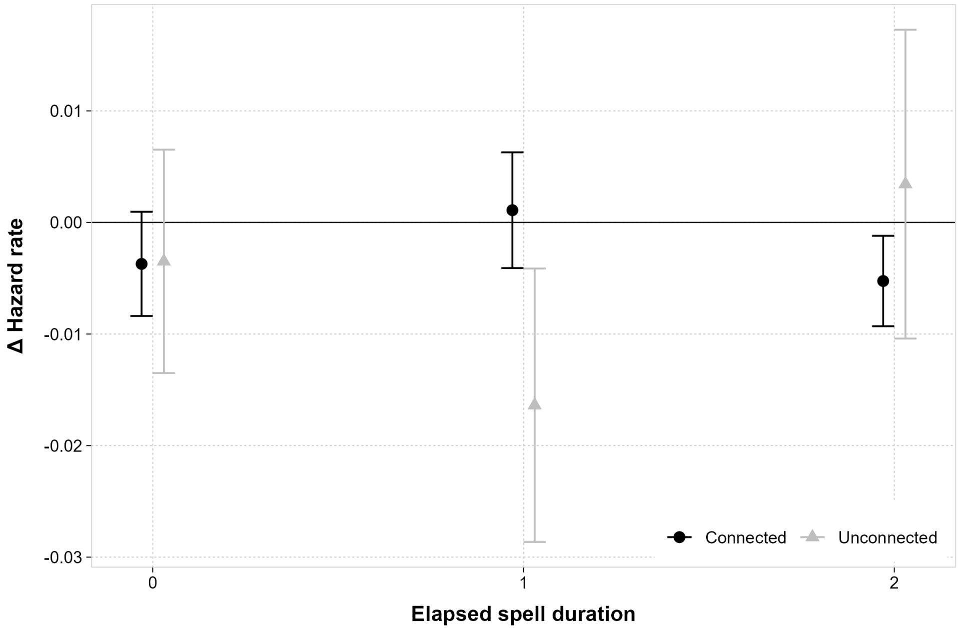
other hand, experience a small increase in the hazard rate. This is driven by an increase in the job separation rate in the first year of a job, which is around 0.3 percentage points higher for the treated in the first year of a job. Job-to-job transitions, on the other hand, do not appear to be significantly affected by the loss of a past coworker.

Difference-in-difference estimates of the effect of the treatment on the exit rate from unemployment, reported in Figure D.4, are qualitatively similar, though less precise. The probability of exiting unemployment in the first two years of a spell falls by 1.2 percentage points (se = 0.7) post treatment. Among employed individuals, the increase in the job separation rate appears more persistent over the job spell and there is more evidence of the treatment decreasing job-to-job transitions too.

To understand the role of connections in providing information to unemployed job seekers, I estimate separate hazard rates for exiting unemployment to a connected workplace, where a past co-worker was already working in the year before the treated/control individual joined the workplace, and to an unconnected workplace.[14] The total decrease in the probability of exit in

[14]Non-parametric (Kaplan Meyer) hazard rates are estimated under the assumption that competing

Figure 5: Types of exit from unemployment



Note: Differences in Kaplan Meier nonparametric hazard rates between treated and control individuals for spells starting in $[t, t+2]$. Hazard rates are estimated under the assumption that competing risks are independent. Standard errors are clustered by matched deceased-placebo strata, 95 per cent confidence intervals are reported.

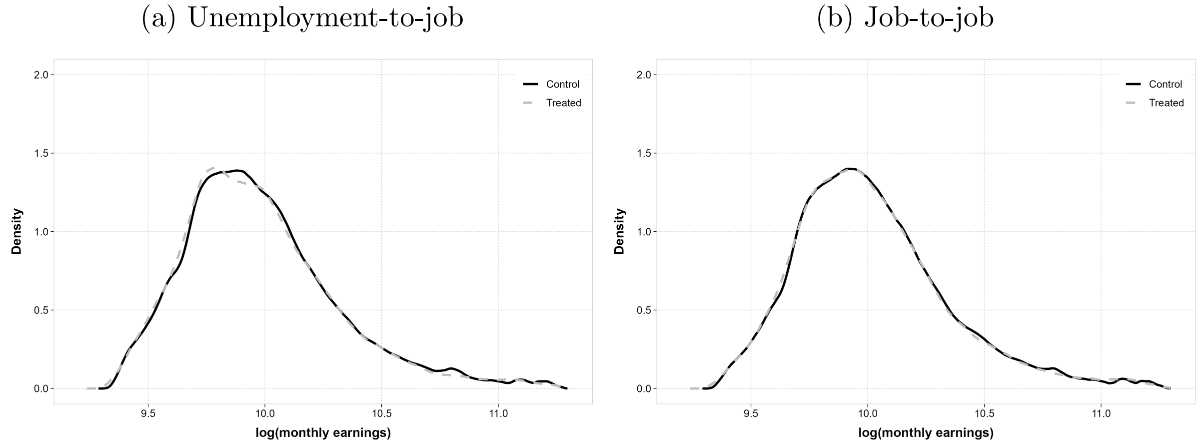
the first year of unemployment is similar when considering exits to connected and unconnected jobs. The large reduction in the exit probability in the second year of unemployment appears entirely due to a decrease in exit to unconnected jobs. However, these patterns mask the true importance of the decline in exit to connected jobs since (i) half of unemployment spells are shorter than a year; and (ii) the probability under no treatment that an unemployment spell ends in exit to a connected job in the first year (0.06) is much smaller than the probability of exit to an unconnected job in the first year (0.44). The probability of exit to a connected job in the first year of unemployment therefore declines by 5.9 per cent, versus an 0.8 per cent decline in exit to unconnected jobs. Conditional on exiting unemployment in the first year of a post-treatment spell, the probability of finding a connected job is 0.6 percentage points lower among the treated (se = 0.5).[15]

To further investigate the relationship between past connections and job match quality, I

risks are independent. Connected jobs are defined using the network in $t-1$, so a differences-in-differences analysis analogous to Figure D.4 is not feasible.

[15] Conditional on exiting unemployment in the first three years of a spell, the treated are 0.4 percentage points less likely to find a connected job (se = 0.4).

Figure 6: Effect of losing a connection on offered wages



Note: Wage offer distributions for jobs starting in $[t, t+3]$. Wages are measured as average monthly earnings. Kernel density estimates are calculated using the Epanechnikov kernel and a default bandwidth.

also plot the distribution of offered wages, i.e. wages at the start of a new job spell, in Figure 6. I again limit attention to job spells starting in $[t, t+2]$. There is evidence that the wage offer distribution for workers is shifted left for treated individuals entering a job from unemployment, though not for job-to-job switchers. There is no such difference in offered wages at the start of new employment spells between treated and control individuals in the pre-treatment period (see Figure D.5).

The difference between the treated and control wage offer distributions is significant in the post-treatment period (Kolmogorov-Smirnov p-value = 0.005). Losing a past connection lowers offered wages by approximately 1 percent on average, though the average difference is not significant, both when considering simple post-treatment treated-control differences, or difference-in-difference estimates (see columns 3 and 4 respectively of Table D.4). Previous research has found referrals from past coworkers raise wages around 4 per cent (Glitz and Vejlin, 2021; Hensvik and Skans, 2016). Since I find that new employment spells are 0.4 percentage points less likely to be in a connected workplace post-treatment, a 1 per cent wage effect of treatment is relatively large.

One reason for the larger wage effect found here, is that the reduced referral probability is only one effect of losing a connection. A smaller network may also reduce workers' bargaining power, lowering offered wages (Caldwell and Harmon, 2019). Workers may also respond to a lower arrival rate by endogenously lowering their reservation wage. Another reason for the larger effect is that previous research has used either within-firm or within-individual designs, which compare connected and unconnected jobs within the same firm or individual, including both unemployment-to-job and job-to-job transitions. My findings suggest that the referrals are especially valuable for workers coming from unemployment; this heterogeneity would be masked by studying the average value of a referral across all job transitions. Within-individual designs may also have low external validity, as they identify effects of connections from individuals who find jobs both through their connections and through the market. Individuals with high-value networks, however, may not to find jobs through the market.

On the other hand, I do not find any economically or statistically significant difference in wage growth between treated and untreated individuals, in columns 1 and 2 of Table D.5. If treated individuals are less likely to receive referrals, this could subsequently lead to higher wage growth (Dustmann et al., 2016), undoing any reduction in wage growth that might result from receiving fewer outside offers over time (Caldwell and Harmon, 2019). However, I do not find any evidence of the treatment affecting wage growth when separately considering connected and unconnected jobs (see columns 3 and 4 of Table D.5).

Summarising my findings on the effect of losing a connection on job search, I find that both search and information frictions are increased by the loss of a past coworker. However, this increase particularly affects unemployed individuals searching for work. The exit rate from unemployment declines substantially for these individuals and, conditional on being offered a job, wages in the job are lower than wages offered to untreated individuals.

6 Heterogeneous value of professional connections

An extensive theoretical and empirical literature studies heterogeneity in the value of different types of connections. Network topology has been highlighted as an important theoretical source of heterogeneity (Granovetter, 1973), including in job search (Calvó-Armengol, 2004; Calvó-Armengol and Zenou, 2005) as the network topology might either make some links redundant, or increase competition when information is rival, as the stylised model presented in Section 2 emphasises. Empirical research has, however, not found strong evidence of competition for or crowding out of job search information (Gee et al., 2017a,b). Economic factors, such as a connections' earnings or attachment to the labour market have also been highlighted theoretically (Calvó-Armengol and Jackson, 2004, 2007; Beaman, 2012) and empirically (Cingano and Rosolia, 2012; Glitz, 2017; Eliason et al., 2023). Different authors have also highlighted the role of demographic characteristics and homophily, suggesting that information from the network matters more for workers with different education levels (Hensvik and Skans, 2016) or gender (Beaman et al., 2018), or that information flows more readily between socio-demographically similar individuals (McPherson et al., 2001; Pedulla and Pager, 2019).

6.1 The Generalised Random Forest

Traditional heterogeneity analysis would involve estimating versions of Equation (3) where the treatment is interacted with covariates thought to predict heterogeneous treatment effects. Since there is a large number of potentially relevant covariates, the classical approach may lead to overfitting, identifying spurious associations between covariates and treatment effect size. The classical approach is particularly ill-suited to studying heterogeneity if, as the theory predicts, there are interactions between individual and network characteristics or if the relationship between treatment effect and some characteristics is non-linear.

To identify covariates that predict heterogeneous treatment effects, I therefore use a machine learning algorithm, the generalised random forest (GRF, Athey et al., 2019). The GRF estimates a series of causal trees (Athey and Imbens, 2016), where each tree is estimated on a random subset of the data and of the included covariates. A tree is a series of splits of the data. At each split the algorithm chooses a covariate and a value of the covariate on which to split so as to maximise treatment effect heterogeneity between the subsets of the data created by the split. Trees flexibly handle potential interactions and non-linearities in the relationship between the different covariates. For each observation, the tree produces a predicted conditional average treatment effect (CATE), given that observation's covariates. The GRF produces a predicted CATE for each observation, which is the average prediction produced by the trees that did not use the observation in question for estimation.

The causal effect for each terminal node of the causal tree, or leaf, is estimated as the average augmented inverse propensity weighted (AIPW) score for observations that fall in that leaf. These are valid causal estimates under two assumptions. The first is treatment *unconfounded-*

ness (Imbens and Rubin, 2015). Conditional on the included covariates, treatment assignment must be as good as random. The balance statistics shown in Tables D.1 and D.2 provide strong evidence that the treatment is randomly assigned within the matched sample, even unconditionally, as one would hope, given the unexpected nature of the deaths. To the extent that there is some residual selection on included observables, for example the deceased individual's characteristics where there is imbalance, the AIPW estimates will correct for such selection.[16] The second assumption is treatment propensity *overlap* between treated and control individuals (Imbens and Rubin, 2015). To calculate AIPW scores, in a first step the GRF algorithm estimates a propensity score via a random forest. The distribution of the estimated propensity scores is shown in Figure C.1. While the estimated treatment propensity is slightly higher in the treated group, there is good overlap and estimated treatment propensities fall in a relatively narrow range for both treated and control individuals.

To estimate the GRF, I use as an outcome employment status in $t+2$. This ensures, first of all, that heterogeneity in estimated treatment effects does not simply proxy for heterogeneity in human capital, which causes differences in baseline earnings. Using employment as the outcome is also justified by the fact that the results in Section 4 suggests that half to two-thirds of the earnings decline is due to reduced employment, a finding supported by the analysis of hazard rates in Section 5. Finally, when the variation in individual treatment effects is small relative to variation in the outcome under no-treatment, the GRF algorithm will tend to overstate the degree of heterogeneity in treatment effects if too few trees are estimated. The coefficient of variation of earnings in $t+2$ is 0.72, while it is 0.41 for employment, suggesting a lower risk of over-fitting treatment effects.[17] Further details on the implementation of the GRF, as well as the list of included covariates, are in Appendix C.

[16]Selection potential outcomes or on unobservables would bias the AIPW estimates. The unexpected nature of the included deaths makes such selection unlikely.

[17]Classical heterogeneity analyses of the earnings event study produce similar results to those found here using the GRF on employment. However, estimating the GRF on earnings fails to converge in reasonable time, suggesting there is too much noise in the outcome relative to the variation in individual treatment effects.

6.2 Heterogeneity in the value of a connection

6.2.1 Results

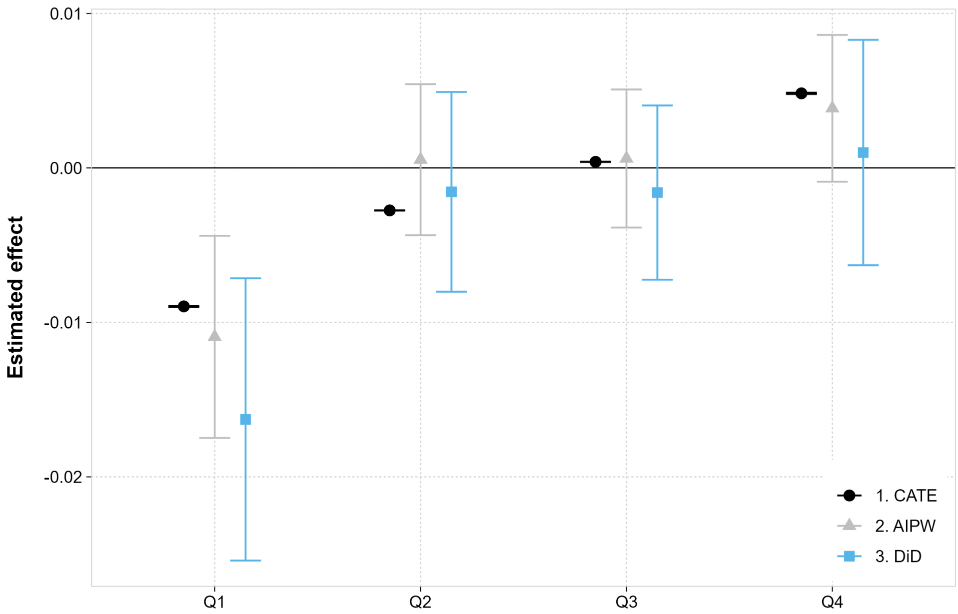
For each individual, the GRF estimates an individual-specific treatment effect, a conditional average treatment effect (CATE). In Figure 7, I rank observations by their estimated CATE and report the average CATE in each quartile.[18] There is evidence of non-trivial treatment effect heterogeneity. The average effect of losing a past coworker for individuals with the most negative effects is a 1 percentage point reduction in employment rates. For individuals in the top quartile of treatment effects, losing a past coworker is estimated to *increase* employment rates by 0.45 percentage points in $t + 2$. This pattern is consistent with the indirect effect of reduced competition for information dominating the direct effect of reduced information from the deceased individual for treated/control individuals in the top quartile. The tree-based estimate of the ATT is a 0.18 percentage point reduction in employment (SE = 0.17), smaller than the difference-in-differences estimate in Section 4.

The GRF algorithm will find heterogeneity in treatment effects by design, regardless of whether the true conditional average treatment effects vary across individuals. An implication of this is that the estimated CATEs increase monotonically over quartiles by construction. To validate the heterogeneity identified by the GRF, I re-estimate the effect of the treatment within each quartile, using both tree-based AIPW scores and a difference-in-differences with respect to employment in $t - 1$. If the GRF has identified real heterogeneity, the quartile-by-quartile estimates of the treatment effect should also be monotonically increasing across quartiles.

The results of the procedure are shown in Figure 7. The within-quartile AIPW estimates of the ATE are monotonically ranked and are very similar to the average CATE in each quartile. The difference-in-difference estimates of the ATT are also monotonically ranked, however, the estimated ATT in the bottom quartile are more negative, at 1.6 percentage points, while there is no evidence of a positive ATT in the top quartile. These two sets of estimates, relying on different identifying assumptions and identifying different parameters, together provide strong

[18]Formally, each tree of the GRF is estimated on a randomly sampled fold, where sampling is stratified at the level of deceased individual-matched placebos. I therefore independently rank observations on their predicted CATE for all observations in a stratum, referred to as a cluster in the machine learning literature, then combine the rankings. This ensures that no observations in the stratum were used to estimate the CATEs that are use to rank the observations, so the estimated ranking of CATEs is unbiased.

Figure 7: Estimated treatment effect heterogeneity



Note: GRF estimates of treatment effect heterogeneity (CATE) and a validation exercise, re-estimating treatment effect within each quartile of CATEs. The GRF is fitted on 5000 trees, imposing a minimum leaf size of 50. The outcome is an indicator for being employed in year $t + 2$. 95 per cent confidence intervals, clustered by match strata, are reported. For estimate details, see Appendix C.

evidence that the underlying individual treatment effects are heterogeneous across individuals, and that the GRF uncovers real patterns of heterogeneity. However, the difference-in-differences estimates of the ATT imply that caution is warranted regarding the finding of the GRF that treatment effects are positive for the top quartile.

6.2.2 Predictors of the value of a connection

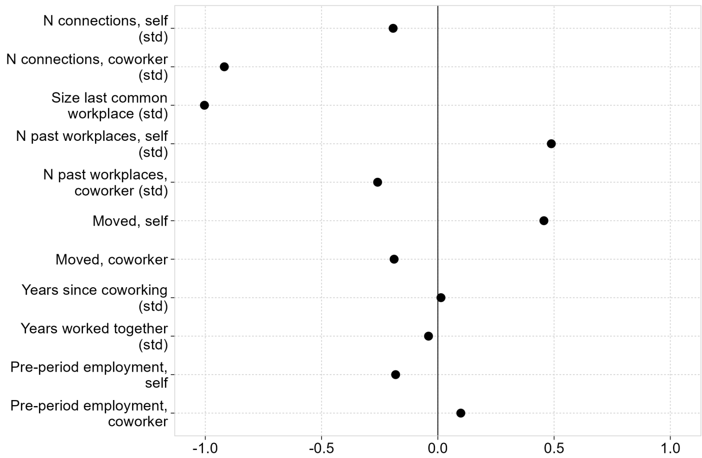
The GRF does not estimate the causal effect of a given covariate on the treatment effect, since covariates are not randomly assigned. Instead, as in traditional heterogeneity analysis, the GRF identifies covariates that are associated with larger treatment effects. To know which professional connections are the most valuable, and for whom, we would like to know which characteristics are associated with large negative predicted treatment effects.[19]

In Figure 8, I follow Athey et al. (2023) and report the difference between the average covariate for the lowest quartile of treatment effects and the highest quartile of treatment effects,

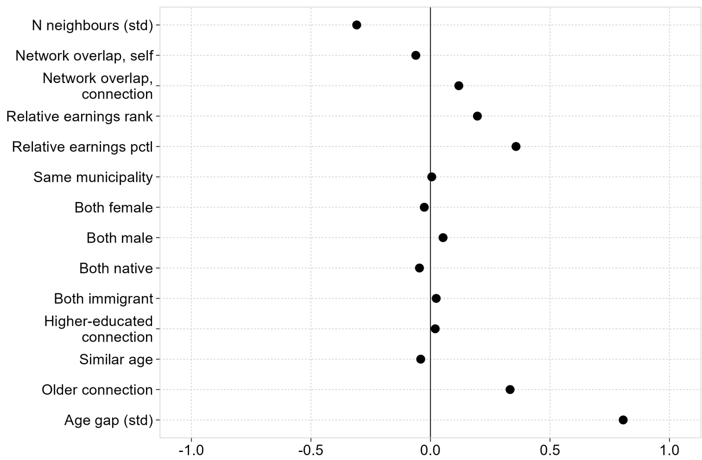
[19]Relevant characteristics may also be correlated with one another. If a given characteristic both causes larger treatment effects and is associated with another characteristic, the GRF cannot isolate which of these two characteristics causes the larger treatment effects.

Figure 8: Predictors of treatment effect size

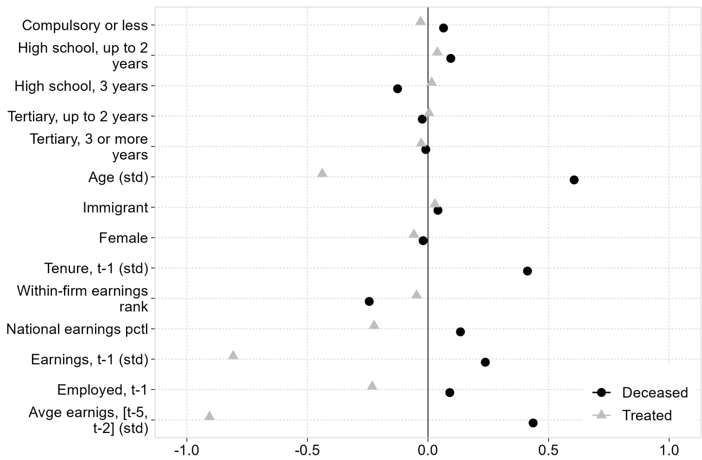
(a) Network size



(b) Tie characteristics



(c) Individual characteristics



Note: Differences in average covariates between the bottom quartile of predicted CATEs and the top quartile of predicted CATEs. Covariates denoted (std) have been normalised to have mean zero and standard deviation one. All other covariates take values on either $[0, 1]$, or $[-1, 1]$.

to identify important predictors of large (negative) treatment effects. I consider 53 potential sources of heterogeneity, though some are naturally correlated with one another.

Network size and its determinants. In Figure 8a, I consider how the size of either the treated or the deceased individual's network affects the treatment effect. Treated individuals in the bottom quartile (most negative) of treatment effects have 0.19 standard deviations fewer connections than those in the top quartile. However, this association is dwarfed in magnitude by the variation in the size of the deceased individual's network. The largest negative effects are associated with the death of past coworkers who have 0.92 standard deviations fewer connections. In the framework of Section 2, this pattern is consistent with a larger indirect effect of losing a connection, due to greater competition for information, when the deceased individual has more connections, that compensates for the direct effect of losing a connection. These countervailing effects have been highlighted theoretically (Calvó-Armengol, 2004; Calvó-Armengol and Zenou, 2005), though credible empirical evidence for them has been harder to come by. This pattern of heterogeneity is also consistent with a greater likelihood of the deceased individual actually having interacted in the past with treated individual when the deceased individual had fewer other connections.

Network size is a complex function of an individual's prior work history. To understand whether network size is a proxy for some feature of an individual's past work history, the rest of Figure 8a considers how various determinants of network size, such as job transitions, differences in past workplace size, or differences in attachment to the labour market during the network-building phase, are associated with the treatment effect. These analyses show that the variation in the number of connections of the deceased that is strongly associated with the treatment effect above all reflects variation in the size of the past workplace. To a lesser extent, it reflects fewer job transitions during the network-building phase for the deceased individual, it does not reflect reduced employment during the network-building phase.

Tie-level characteristics At the tie level, there is again evidence for indirect effects being an important component of the total value of a connection. The most negative treatment effects are associated with the deceased and treated individual having 0.31 standard deviations fewer common connections and with a 6.1 percentage point reduction in the share of the treated individuals' connections that are in common with the deceased individual. Both associations are again suggestive of valuable connections being those who compete less with the treated individual for information, although they also partly reflect the correlation between number of neighbours and number of connections for the deceased individual.

When focusing on different measures of similarity and dissimilarity between the treated and deceased individuals, two stand out. The most valuable connections were on average 20 ranks higher in the firm earnings ranking at the time of working together than the least valuable, or 36 percentiles in the national earnings distribution. Similarly, the most valuable connections are much more likely to be older than the treated individual; the age gap is 0.81 standard deviations larger in the case of the most negative treatment effects. These patterns suggest that valuable connections are those where a clear hierarchy exists between the connection and the treated individual. Homophily by gender or nativity and differences in education appear to be less important predictors of the value of a connection.

Individual characteristics The socio-demographic characteristics of both the treated individual and the deceased individual tend to confirm that hierarchy is an important predictor of which connections are valuable. The most negative treatment effects are associated with both younger treated individuals and older deceased individuals. The larger earnings differences between treated and deceased individuals in the case of the most negative treatment effects appear more strongly driven by treated individual who earn less, rather than deceased individual who earn more. However, when comparing earnings ranks within the firm at the time of working together, it is the earnings of the deceased individuals that vary more when comparing top and bottom treatment effect quantiles (by 24 ranks, versus 5 ranks for the earnings of the treated individual).[20] Labour market attachment also matters. Individuals with low tenure at their workplace are more strongly affected, as are individuals who are unemployed in $t-1$, while the loss of an individual with a long tenure is also associated with particularly negative treatment effects.

Taken together, the results in Figure 8 paint a coherent picture of when professional connections are most valuable. Valuable connections have fewer other connections competing to receive information from them, they are older, higher up the earnings distribution, particularly

[20]The highest-earning individual in a firm is ranked 0.01, the next highest 0.02, etc., such that earnings ranks within the firm are contained on $[0, 1]$. The highest national earnings percentile is 0.99, the next highest 0.98, etc.

within the firm where the connection was formed, and have a longer tenure at the time of their death. These connections are plausibly more likely to have and pass on relevant job search information, lending support to the exclusion restriction for the analyses in Section 4. My findings are consistent with previous empirical research documenting the association between network structure (Calvó-Armengol and Zenou, 2005) or age of the connection (Loury, 2006) and the value of connections. Connections are particularly valuable to younger individuals, individuals who are lower in the earnings distribution and who are more weakly attached to the labour market.

6.2.3 Heterogeneity across and within deaths

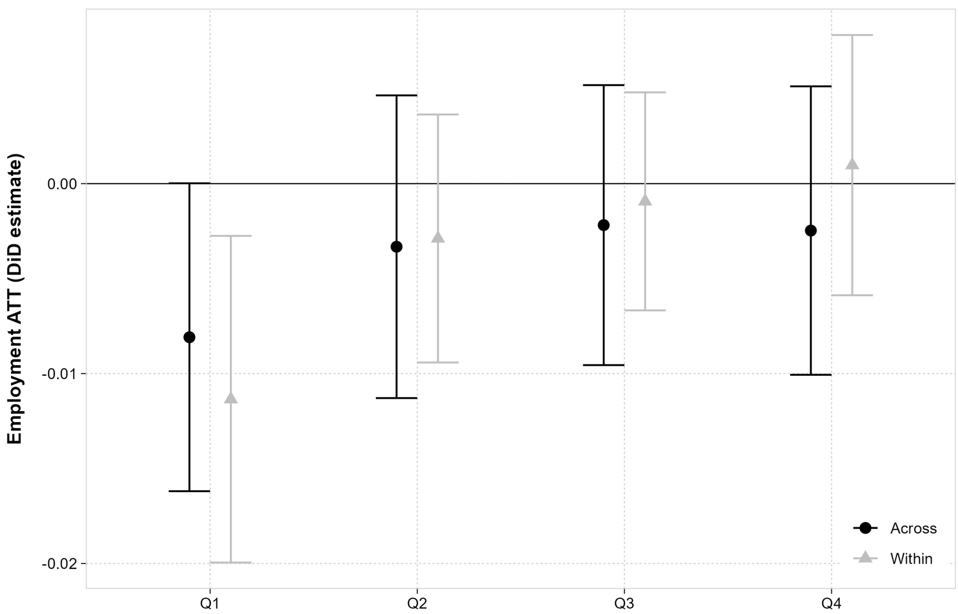
The heterogeneity analysis in the previous section showed that both the characteristics of the deceased and the treated individual are associated with heterogeneous treatment effects. To explore further whether the characteristics of one or other member of a dyad matter more, I decompose variation in treatment effects into variation *within* deceased individuals (across all treated individuals connected to the same deceased individual), and *across* deceased individuals.

To study variation within deceased individuals, for all deceased individuals connected to at least four individuals in my sample,[21] I divide individuals into within-death quartiles based on their estimated CATE. I then pool deaths and estimate the ATT quartile-by-quartile using differences in difference in employment between $t-1$ and $t+2$. To study variation across deceased individuals, for each individual I calculate the mean CATE for all other individuals affected by the same coworker death. I then pool observations and rank individuals by their leave-out mean CATE and again estimate the ATT within each quartile of the resulting ranking.[22]

The results of the decomposition exercise are shown in Figure 9. There is variation in the treatment effect both within and across deceased individuals' sets of connections. Large negative treatment effects are concentrated in a subset of deceased and treated individuals, rather than varying smoothly across either distribution. There is a core of very valuable connections, whose loss lowers employment by a whole percentage point, while losing a connection in the top three quartiles does not affect employment. Similarly, there is a subset of individuals who are more

[21]This corresponds to 98 per cent of the original sample.
[22]Rankings are again computed within cluster, to ensure unbiasedness.

Figure 9: Decomposition of variation in ATT



Note: ATTs for treated/control individuals ranked within deceased individual or across deceased individual CATE quartiles. Only deaths affecting at least four connected individuals who satisfy my sample restrictions are included. The ATT is estimated via differences-in-differences within each quartile in employed between $t-1$ and $t+2$. 95 per cent confidence intervals, clustered by match strata, are reported.

strongly affected by the loss of a given connection.

To quantify the relative importance of variation within and across deceased individuals, it is also possible to regress individual CATEs on a set of fixed effects for deceased individuals. The R^2 from such a regression is 0.46. The remaining variation in estimated CATEs is due to treatment heterogeneity that is associated either with the characteristics of the treated individual or with dyad-level characteristics.

6.3 Networks and inequality

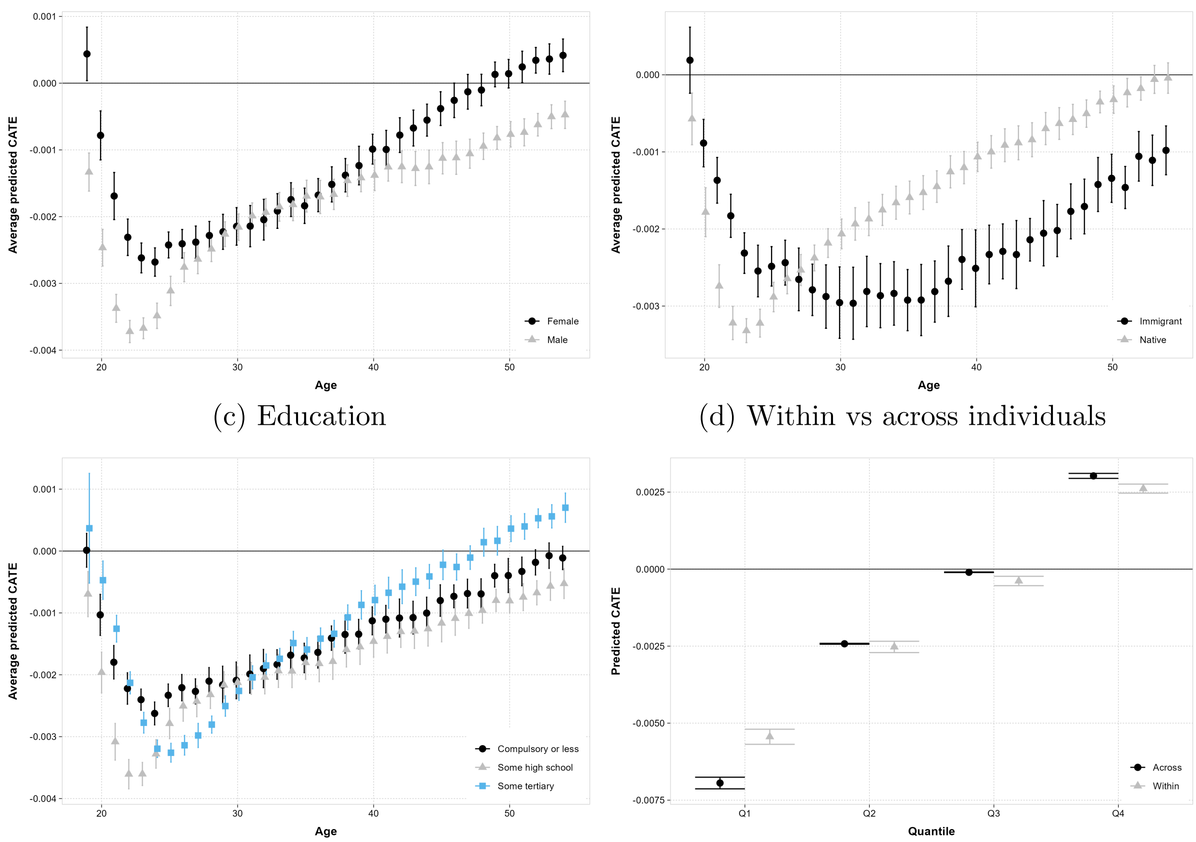
The output of the GRF can be used to predict the subjective value of any worker's connections, given the observed characteristics of the focal individual and the past coworker. To understand how the value of a worker's total network varies by socio-demographic group, I predict the CATEs for all connections of all workers in my sample. I append these predicted CATEs to the estimated CATEs for the workers' true or placebo deceased connections. I then calculate the average CATE across all focal workers' connections. This average can be interpreted as the expected reduction in employment at $t + 2$ from removing a uniformly randomly chosen individual from their network.

I show how the average CATE among coworkers varies with the focal worker's age and either gender, nativity, or education in Figures 10a–10c. The predicted CATE is missing for any coworkers who previously worked in a large workplace, or a workplace with a missing identifier. Since there is a declining trend over time in the share of missing workplace identifiers in the Swedish data, I cluster standard errors by the year in which the network is defined (the pre-event year).

Figure 10: Average CATEs across all connections

(a) Gender (b) Nativity

(c) Education (d) Within vs across individuals



Note: Average CATEs by demographic characteristic, as well as within- and across- individuals. 95 per cent confidence intervals, clustered by calendar year in which the network is measured, are reported.

Consistent with previous research showing that connections can be particularly important for young workers (Kramarz and Skans, 2014), connections are at their most valuable in a worker's twenties. The value of the average connection increases rapidly in all cases (the predicted employment effect becomes more negative) in the early twenties, reflecting the fact that very young workers had very few years' exposure to their past coworkers. From workers' mid-twenties, the value of a connection starts to fall. The value of male workers' connections is higher than for female workers in the extremes of the age distribution, in line with findings that men's networks can be more productive (Beaman et al., 2018). Networks have been shown to be important for immigrants' job search (Edin et al., 2003; Battisti et al., 2022; Åslund et al., 2024). For the foreign-born, the value of a connection increases for longer than for natives and is at its maximum in the mid-thirties. Finally, the value of a connection is largest a few years after completing education and the value of connections declines most rapidly for the most highly-educated. For this group, the value of a connection even becomes negative after age 50, suggesting competition within networks, either for information about jobs, or for the jobs themselves, becomes important at this age.

Differences in the effect of networks across demographic groups could reflect differential exposure to high-value connections or differential effects of similar connections. To understand the relative importance of these two channels, I first rank a worker's connections by predicted CATE, for all individuals with at least four connections. I also rank the focal workers by their average predicted CATE. I then calculate the average within each quartile of both distributions. The value of workers' connections varies both within and across individuals, shown in Figure 10d, suggesting that most workers have access to at least some valuable connections. I confirm this by reporting the predicted decrease in employment at $t + 2$ across different groups if they were to lose their most valuable connection; i.e. the connections with the most negative predicted CATE.[23] The results in Figures D.6a, D.6c, and D.6e show similar patterns to the effect of losing a randomly drawn connection. However, women in particular appear relatively more harmed by losing their most valuable connection, rather than a randomly chosen connection, than men.

Finally, I simulate the counterfactual effect of losing a randomly drawn connection for women, immigrants, or the low-educated, had they instead been men, natives, or high-educated; I report the results in Figures D.6b, D.6d, and D.6f. The only substantial differences are observed for women. This is consistent with my finding that network topology, age, and labour market attachment were more important than demographic characteristics for determining the value

[23]The correlation between the average CATE and the CATE of the most valuable coworker is 0.79.

of a connection. However, women, who earn less and are less likely to be employed than men, nevertheless do not benefit from their networks as much as their past labour market outcomes would predict, had they been men. I also show that the age pattern of changing network value is not explained by differences by age in missing connection characteristics (if, in particular, connections worked in past workplaces with over 100 coworkers), in Figure D.7b. The minimum CATE is slightly more robust than the average CATE to missing connection characteristics, as I show in Figure D.7a.

The results in this section again highlight that connections are more valuable to individuals who typically face larger frictions, such as younger workers or immigrants. Connections therefore appear to function as substitutes for labour market experience in job finding, leading me to conclude that networks will in general work to reduce inequality in the labour market. However, the differential productivity of women's networks may contribute to explaining some inequality by gender.

7 Conclusion

In this paper I investigated the effect of losing a past coworker on an individual's earnings, employment, and job search. I found that the loss of a past coworker lowers earnings by an average of around 1900 SEK two years after the death and lowers the probability of employment by 0.4 percentage points. The effect of losing a connection is particularly large for those who become unemployed, who then face higher search frictions and are worse-matched to jobs when they exit unemployment.

My findings on the effects of professional connections on job search could be extended in multiple ways. My estimates of the total effect of a connection on earnings or employment are somewhat larger than would be implied by existing findings in the literature. This suggests that it is important to understand the endogenous responses of workers to being better- or worse-connected. Future work using data from job search platforms linked with administrative data could trace out how the availability of connections affects job seekers' behaviour at each step of the search process, from job application to acceptance, via the interview offer, call-back, job offer, and wage negotiation, using observed outside options. The relative importance of changes in the direction of job search, search intensity, and reservation wages in response to different network qualities could then be explored.

I found that the value of a lost connection is heterogeneous. Older, more economically successful connections, both absolutely and relative to the treated individual, are particularly valuable, as are connections with a small set of connections of their own, highlighting the importance of competition for rival job search information. Because connections are more valuable for individuals who are more weakly connected to the labour market, the contribution of professional networks to labour market inequality is limited and such connections may even work to reduce inequality on average. However, this does not imply that differential access to or productivity of connections formed in other settings, such as schools or neighbourhoods may not contribute to inequality, particularly as such connections may affect important life decisions, such as whether to attend university. The approach developed in this paper, where I comprehensively study heterogeneous effects of quasi-experimental variation in exposure to some component of one's network, could productively be extended to other settings to understand how other layers of individual's networks may contribute to inequality at different points in the life-cycle.

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A Effect of deleting a connection

Consider the network structure represented in Figure 1. The net reduction in probability that worker 2 receives a job offer from the network when removing worker 3 from the network is

$$\Delta_{P_2,3} = a(1-b)\left[1 - a(1-b)\frac{1-(1-b)^2}{2b}\right]^2, \tag{A.1}$$

which is positive for $(a, b) \in (0, 1) \times (0, 1)$. The net reduction in the probability worker 2 receives an offer from their contacts when removing worker 4 from the network is

$$\Delta_{P_2,4} = [1 - a(1-b)]\{1 - a(1-b) - \left[1 - a(1-b)\frac{1-(1-b)^2}{2b}\right]^2\} \tag{A.2}$$

Let $\alpha = a(1-b) \in (0, 1)$ and $B = \frac{1-(1-b)^2}{2b} = 1 - \frac{b}{2}$, $B \in (\frac{1}{2}, 1)$. Then we may write

$$\begin{aligned}
\Delta_{P_2,4} &= (1-\alpha)[1 - \alpha - (1-\alpha B)^2] \\
&= (1-\alpha)[1 - \alpha - (1 - 2\alpha B + \alpha^2 B^2)] \\
&= (1-\alpha)[\alpha(2B-1) + \alpha^2 B^2].
\end{aligned}$$

Since $1 - \alpha > 0$, we have

$$\begin{aligned}
\Delta_{P_2,4} \leq 0 &\iff \alpha(2B-1) + \alpha^2 B^2 \leq 0 \\
&\iff 2B \leq 1 - \alpha B^2.
\end{aligned}$$

Note, however, that $2B \in (1, 2)$, while $1-\alpha B^2 < 1$, a contradiction. We conclude that $\Delta_{P_2,4} > 0$. To show that $\Delta_{P_2,3} > \Delta_{P_2,4}$, simply note that

$$\begin{aligned}
\Delta_{P_2,4} &= \Delta_{P_2,3} + [1 - a(1-b)]^2 - \left[1 - a(1-b)\frac{1-(1-b)^2}{2b}\right]^2 \\
&= \Delta_{P_2,3} + [1 - a(1-b)]^2 - [1 - a(1-b)B]^2 .
\end{aligned}$$

It follows from $B \in (\frac{1}{2}, 1)$ that $a(1-b) > a(1-b)B$, therefore $\Delta_{P_2,3} > \Delta_{P_2,4}$.

Finally, the reduction in the probability that worker 1 receives a job offer from the network if we delete connection 4 is

$$\Delta_{P_1,4} = a(1-b)\frac{1-(1-b)^3}{3b}\left(1 - a(1-b)\frac{1-(1-b)^2}{2b}\right). \tag{A.3}$$

To show that $\Delta_{P_1,4} > \Delta_{P_2,4}$, rewrite the inequality as

$$(1-\alpha B)\alpha\frac{1-(1-b)^3}{3b} > (1-\alpha)[1-\alpha-(1-\alpha B)^2]$$

Note that $1-\alpha B > 1-\alpha > 0$ and $\frac{1-(1-b)^3}{3b} > B^2$. If $\Delta_{P_2,4} \geq \Delta_{P_1,4}$, then

$$\begin{aligned} 1-\alpha-(1-\alpha B)^2 &\geq \alpha\frac{1-(1-b)^3}{3b} \\ &> \alpha B^2, \end{aligned}$$

which, when rearranged, is equivalent to

$$B(2-(1+\alpha)B) > 1.$$

However, $B \in (\frac{1}{2}, 1)$, so this inequality implies that $2-(1+\alpha)B > 1$. Substituting the definitions of α and B into the inequality, we have

$$\begin{aligned} 2-[1+a(1-b)](1-\frac{b}{2}) &> 1 \\ -\frac{ab^2}{2} + \frac{3ab}{2} - a &> 0. \end{aligned}$$

It can readily be shown that $\sup_{(a,b)\in(0,1)^2}[-\frac{ab^2}{2} + \frac{3ab}{2} - a] = 0$, a contradiction. We therefore conclude that $\Delta_{P_1,4} > \Delta_{P_2,4}$.

B Propensity score estimation details

The post double-selection propensity score matching procedure described in Section 3.4 identifies matched placebo deaths for true deaths in year t, using the connections of the deceased/placebo individual in year $t-1$. The estimation of the propensity score and outcome model (average earnings among treated/control workers in t) uses the following features as inputs:

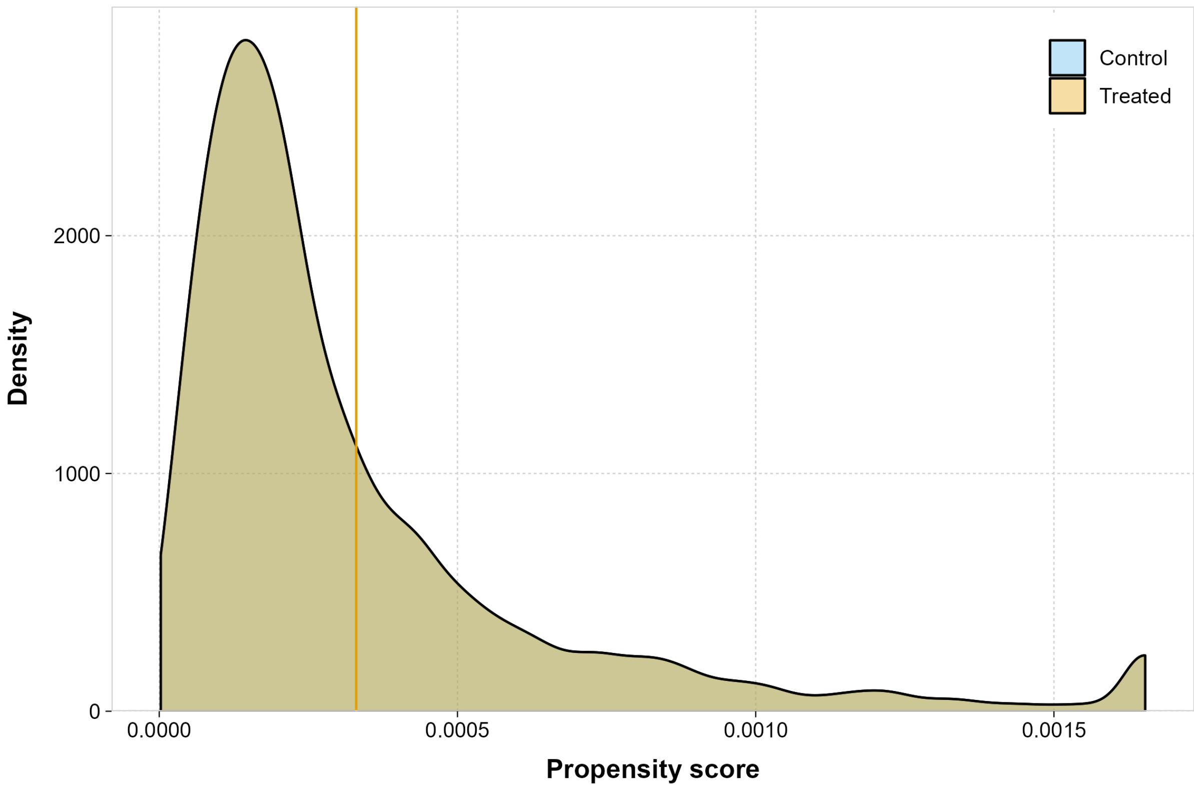
- *Deceased/placebo individuals*: number of workplaces in the network-building phase, $[t-6, t-2]$, tenure in workplace in $t-1$ (zero if unemployed, one if $t-1$ is the first year in the current workplace), wage earnings in $t-1, \ldots, t-5$, employment status in $t-1 \ldots, t-5$, number of connections in $t-1$, fraction of connections whose connections are observed, age, sex, nativity, completed education (Compulsory schooling or less, High school graduation, Tertiary education or higher).

- *Treated/control individuals connected to a given deceased/placebo individual*: fraction of treated/control who have left the workplace they shared with the deceased/placebo worker, fraction of treated/control where the deceased/placebo worker has left the workplace they shared with the treated/control worker, fraction of deceased-treated or placebo-control pairs where both members have left the workplace where they were employed together, average overlap between the the deceased/placebo's set of connections and the treated/control's set of connections, share female, share immigrant, share low-, medium-, or high-educated, average years since deceased-treated or placebo-control pair worked together, average number of connections for the treated/control workers, average number of workplaces in the network-building phase, $[t-6, t-2]$, employment rate among treated/control in $t-1, \ldots, t-5$, and average earnings in $t-5$ (but not in $t-1, \ldots, t-4$).

Average characteristics are calculated over those individuals satisfying the restrictions in Section 3.2 and who are, furthermore, not missing information for any of the included characteristics. Since treated/control earnings in $[t-4, t-1]$ are excluded from the set of included features, these can be used to asses the quality of the match on the estimated propensity score.

The lasso estimation of both the propensity score and the outcome model includes all features listed above, the square of all listed continuous variables, all two-way interactions between features listed above, and all interactions between the squared terms and included categorical

Figure B.1: Estimated propensity score for dying unexpectedly



Notes: Distribution of propensity scores estimated using the post-double selection procedure described in Section 3.4. Propensity scores reported for deceased individuals and ten matched placebo for each event. The estimated propensity scores have been winsorised at the 99th percentile, for visual clarity.

variables. There are 40 basic features included, leading to approximately 1550 constructed features. The predictors for the propensity score are selected to maximise the Area Under Curve (AUC) criterion via five-fold cross validation. The predictors for the outcome model are selected to minimise the prediction mean squared error, again via five-fold cross validation. The estimated propensity scores for the deceased individuals and matched controls are shown in Figure B.1. The two distributions are indistinguishable from one another.

C GRF estimation details

To estimate the GRF, I randomly split the data into five folds, stratifying the data on matched deceased-placebo groups. The predicted CATEs for each observation are the average of trees estimated using the remaining four folds. This ensures that other individuals affected by the same coworker death (or for that matter affected by a placebo death matched to the same coworker death) are not used to estimate an individual's CATE, which would otherwise lead to overfitting, if workers affected by the same event are similar. This approach is known as *clustering* in machine learning. Clustering by folds means that CATEs can be ranked within folds, since none of the observations within the fold were used to estimate forests that are averaged to produce the CATE. Within an estimation sample, the data are split into subsets, one to identify the folds, another to estimate the treatment effects within terminal nodes, an approach known as *honesty* (Athey and Imbens, 2016), before the roles are reversed and the two estimate averaged, an approach known as *cross-fitting*.

I use the following covariates to estimates CATEs:

1. *Network size and determinants*: Number of connections of the treated/control individual in $t-1$, number of connections of the deceased/placebo individual in $t-1$, Number of employees in the workplace where the connection was formed in the last year working together, number of workplaces of the treated/control individual during the network-building phase, number f workplaces of the deceased/placebo individual during the network-building phase, whether the treated/control individual has left the past workplace in $t-1$, whether the deceased/placebo individual has left the past workplace in $t-1$, years since the two last worked together, years spent working together during the network-building phase, employment rate of the treated/control individual during the network-building phase, employment rate of the deceased/placebo individual during the network-building phase.

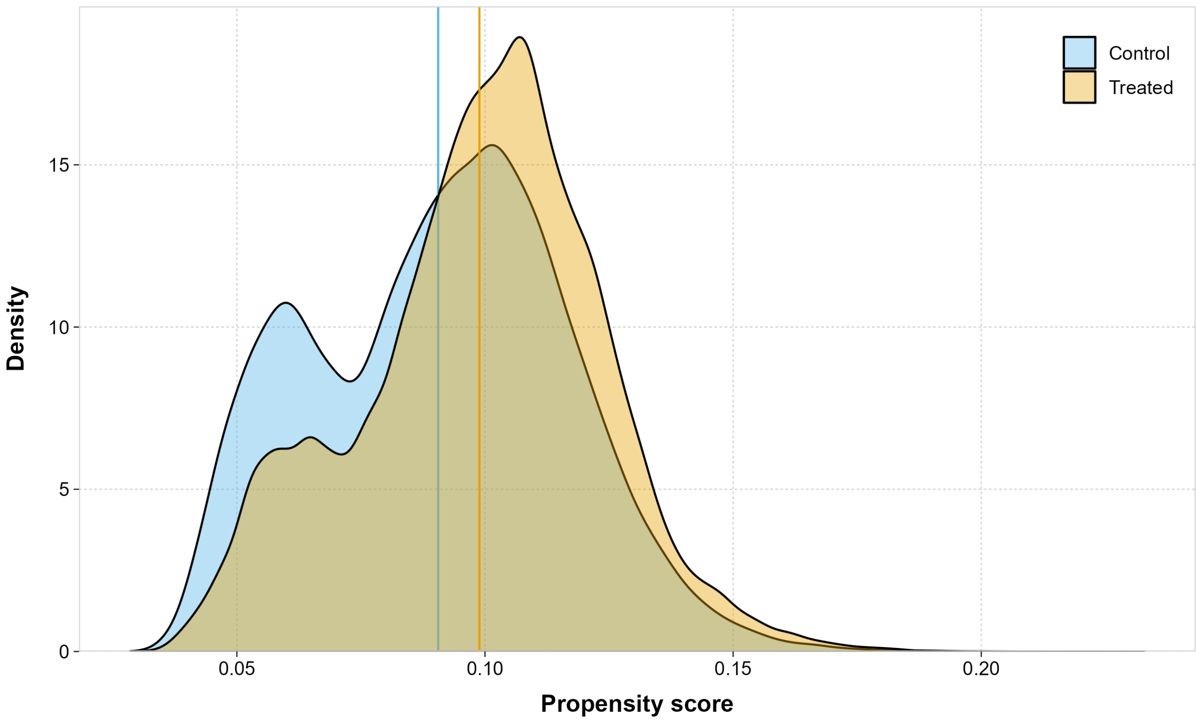
2. *Tie-level characteristics*: Number of neighbours (i.e. common connections of the treated-deceased/control-placebo tie), share of the treated/control's connections who are also connections of the deceased/placebo, share of the deceased/placebo's connections who are also connections of the treated/control, the difference in earnings rank in the firm

between treated-deceased/control-placebo in the last year working together (where a positive difference means the connection earned more than the treated/control individual), the difference in national earnings percentile between treated-deceased/control-placebo in the last year working together, indicators for living in the same municipality in $t-1$, both being female, both being male, both being Sweden-born, both being foreign-born, the deceased/placebo have an equal or higher level of education than the treated/control, both aged within five years of each other, the deceased/placebo being five or more years older than the treated/control, the age gap in years between the treated-deceased/control-placebo.

3. *Demographic characteristics*: For both treated/control and deceased/placebo individual, I include five educational categories, age in years, immigrant status, tenure in the workplace in $t-1$ (equal to zero if unemployed, 1 in the first year of employment, etc., and winsorised at 6), earnings rank in the firm in the last year working together (where the highest-earning individual is ranked 0.01, the next highest 0.02, etc.), earnings percentile in the national distribution in the last year working together (measured on [0, 1]) total earnings in $t-1$ in 2018 SEK, employment status in $t-1$, average earnings over the period $[t-5, t-2]$. I also include an indicator for whether the deceased/placebo individual was self-employed in $t-1$, and the year of the death.

To estimate the forest, I impose a minimum terminal node size of 50 observations in each tree and estimate 5000 trees. As an input to the estimation of the causal forest, the GRF first estimates an outcome model and a propensity score, using a regression forest. The estimated propensity scores for the included treated/control individuals are shown in Figure C.1.

Figure C.1: Estimated propensity score for being exposed to an unexpected death



Notes: Propensity scores estimated via a random forest by the GRF algorithm. Estimated propensity scores are reported for for the treated and control individuals, average propensity scores are reported as vertical lines..

D Supplementary tables and figures

Table D.1: Treated and control individuals

	Control	Treated	P-value
Age	35.2	35.36	0.14
Female	0.36	0.36	0.93
Immigrant	0.1	0.1	0.99
Compulsory educ.	0.19	0.19	0.66
High School	0.72	0.72	0.53
Tertiary	0.09	0.09	0.83
Employed, $t-1$	0.84	0.84	0.53
Earnings, $t-1$	181,400	180,068	0.42
Time since working together	2.89	2.92	0.32
Treated/control moved	0.67	0.67	0.81
Deceased/placebo moved	0.8	0.8	0.73
N connections	65.41	65.37	0.96
Share common connections	0.51	0.51	0.47

Notes: Summary statistics for 76,351 treated individuals and 760,449 control individuals. Earnings are deflated to 2018 Swedish crowns. P-values are reported for the t-test of differences in means; standard errors of the mean are clustered by deceased/placebo individual.

Table D.2: Treated and control individuals' connections

	Control	Treated	P-value
Mean moved	0.67	0.67	0.93
Mean connection moved	0.78	0.78	0.29
Mean N neighbours	30.75	30.56	0.66
Share female	0.38	0.37	0.02
Share immigrant	0.1	0.11	0.56
Mean earnings, $t-1$	163,276	162,422	0.52
Mean employment, $t-1$	0.79	0.78	0.31
Share compulsory educ.	0.21	0.22	0.03
Share high school	0.69	0.68	0.19
Share tertiary	0.1	0.1	0.37
Mean age	36.76	36.94	0.12
Mean years since interacted	2.91	2.9	0.52
Mean N connections	69.38	69.71	0.62

Notes: Average characteristics among all connections for 76,351 treated individuals and 760,449 control individuals. Earnings are deflated to 2018 Swedish crowns. P-values are reported for the t-test of differences in means; standard errors of the mean are clustered by deceased/placebo individual.

Table D.3: Deceased and placebo individuals

	Placebo	Deceased	P-value
Age	38.38	39.31	0
Female	0.28	0.19	0
Immigrant	0.11	0.11	0.53
Compulsory educ.	0.25	0.29	0
High school	0.68	0.64	0
Tertiary	0.08	0.06	0
Employed, $t-1$	0.72	0.72	0.97
Earnings, $t-1$	148,175	147,051	0.61
N connections	35.41	36.22	0.15

Notes: Average characteristics among all connections for 4634 deceased individuals and 46,340 placebo control individuals. Earnings are deflated to 2018 Swedish crowns. Mean N neighbours and Mean N connections are only calculated using the subset of the treated/control individuals' connections whose own set of connections is observed. P-values are reported for the t-test of differences in means; robust standard errors are used.

Table D.4: Difference in offered wages, u2j

Group	Missing wage		log(offered wage)	
Model:	(1)	(2)	(3)	(4)
treated × post = TRUE	0.0065	-0.0038	-0.0092	-0.0102
	(0.0054)	(0.0058)	(0.0064)	(0.0066)
post	Yes	Yes	Yes	Yes
treated		Yes		Yes
Observations	769,032	769,032	283,477	283,477
R^2	0.00049	0.00049	0.01027	0.01027

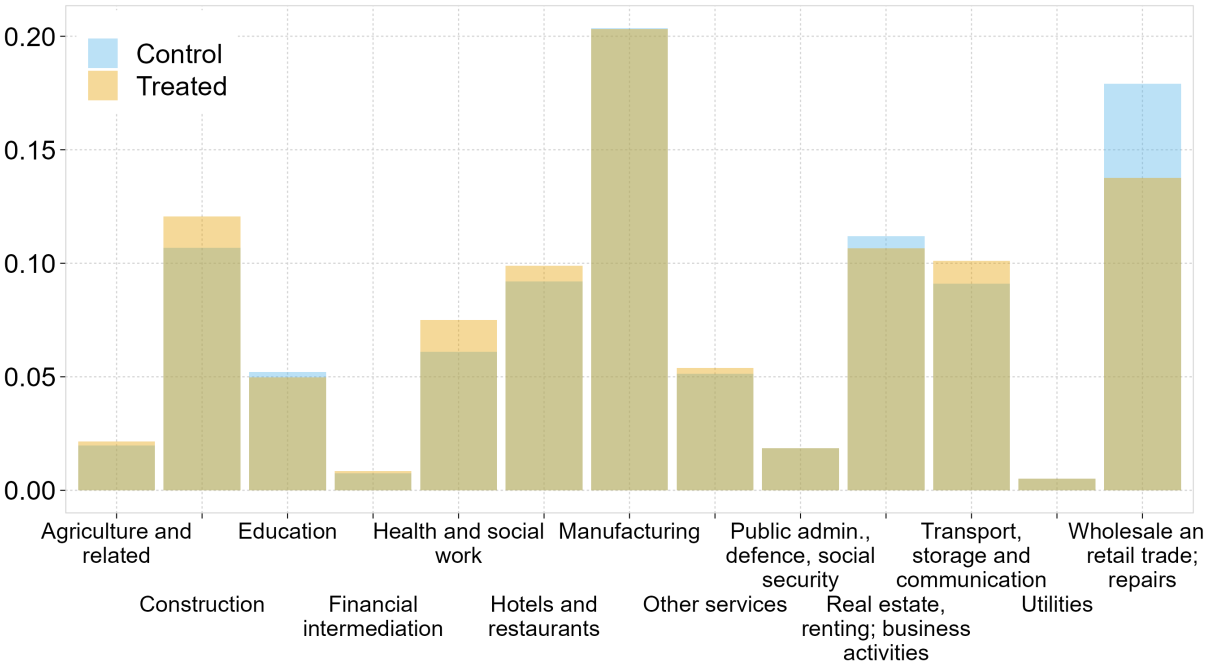
Notes: Effect of treatment on an indicator for missing wages (columns 1 and 2) and offered wages following an unemployment to job transition (columns 3 and 4). Wages are measured as log monthly earnings and censored if below the first percentile of the national wage distribution. Standard errors clustered by matched deceased-placebo strata are reported.

Table D.5: Difference in wage growth, all workplace spells

Group	All		Connected	Unconnected
Model:	(1)	(2)	(3)	(4)
treated × post = TRUE	0.0004	-0.0002	0.0018	0.0001
	(0.0013)	(0.0026)	(0.0032)	(0.0014)
post	Yes	Yes	Yes	Yes
treated		Yes		
Observations	333,168	333,168	141,187	191,981
R^2	0.00201	0.00201	0.00195	0.00015

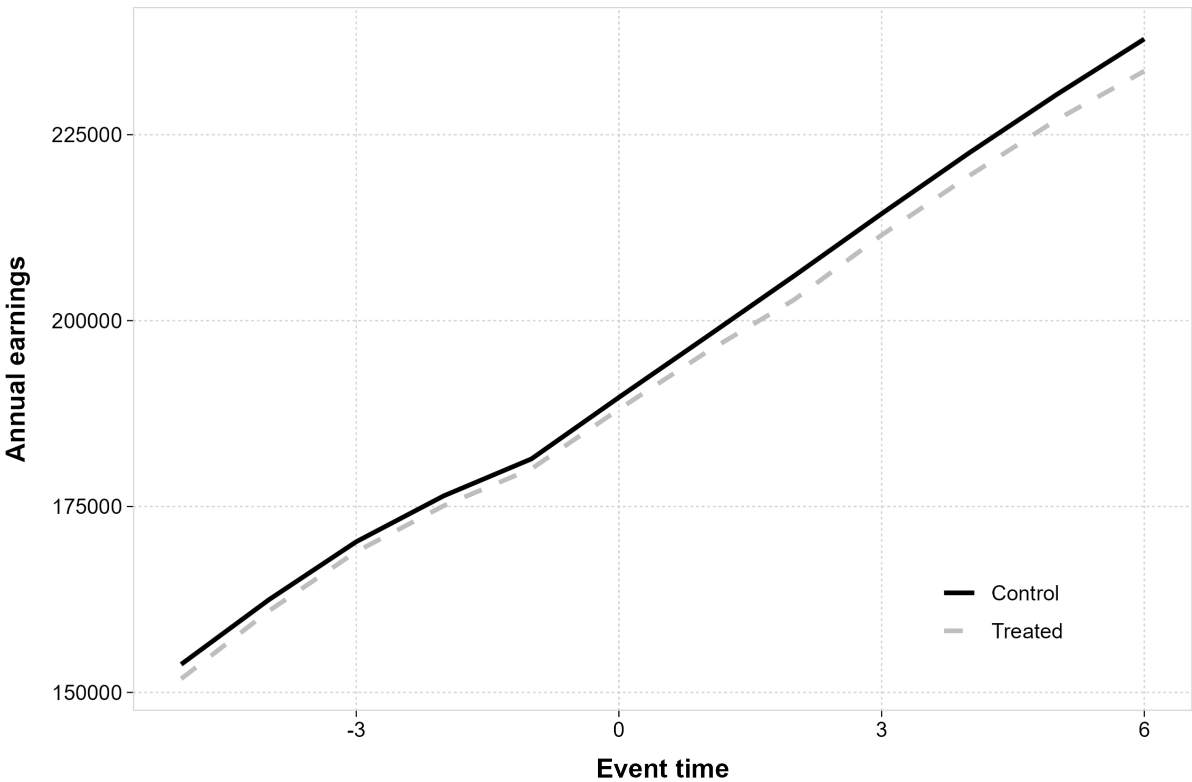
Notes: Effect of treatment on wage growth, measured as the difference in log monthly earnings from one year to the next. Standard errors clustered by matched deceased-placebo strata are reported.

Figure D.1: Shared past industry



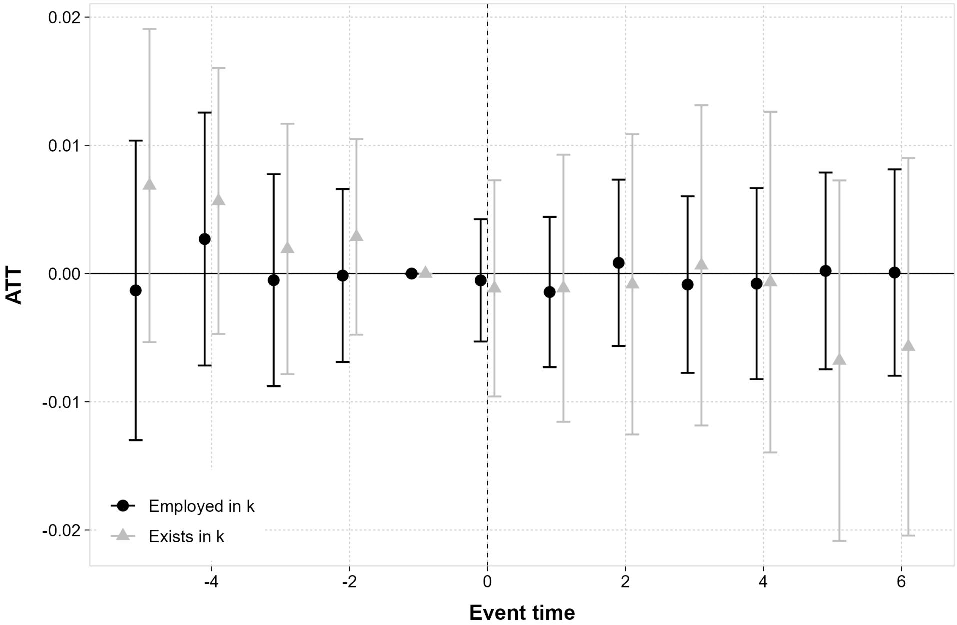
Notes: Distribution over industries of the workplace where the treated/control individuals last worked with the deceased/placebo individual, according to the NACE Rev. 1 classification. Mining and quarrying, Extra-territorial organisations, and Private households have been omitted due to too few cases.

Figure D.2: Unexpected death of a connection and earnings



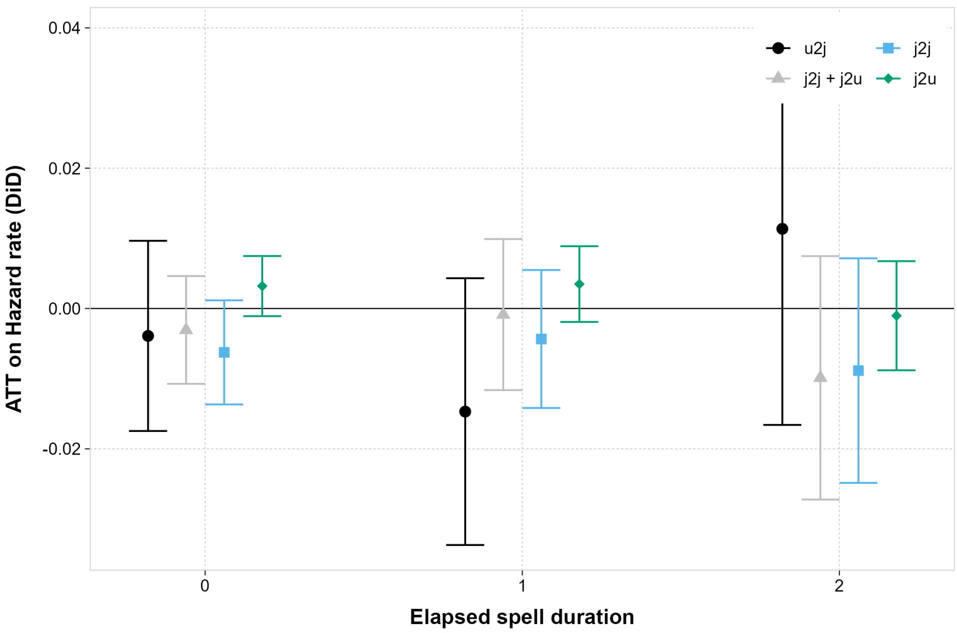
Note: Real labour earnings, measured in 2018 Swedish crowns of treated and control individuals affected by the death of a past coworkers in 1991–2010.

Figure D.3: Most recent joint workplace



Note: The figure reports event-study estimates of the effect of unexpected deaths of past coworkers, occurring during 1991–2010, on differences in the relationship to the most recent past workplace. Standard errors are clustered by matched deceased-placebo strata, 95 per cent confidence intervals are reported.

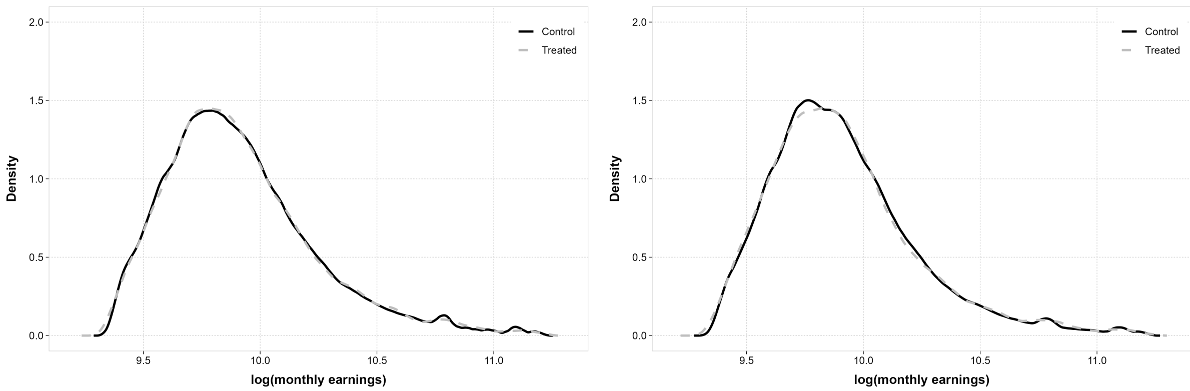
Figure D.4: Effect of losing a connection on hazard rates



Note: Difference-in-differences in the Kaplan Meier non-parametric hazard rates between spells starting in $[t, t+3]$ and spells starting in $[t-5, t-2]$. Pre-treatment spells are censored at the treatment date. Hazard rates for job spells (exit to job and exit to unemployment) are estimated under the assumption that competing risks are independent. Standard errors are clustered by matched deceased-placebo strata, 95 per cent confidence intervals are reported.

Figure D.5: Effect of losing a connection on offered wages, pre-treatment

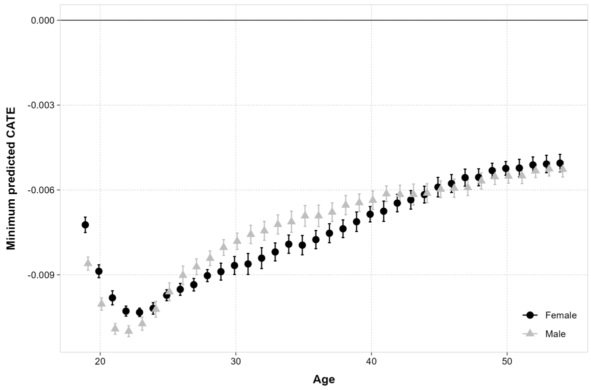
(a) Unemployment-to-job (b) Job-to-job



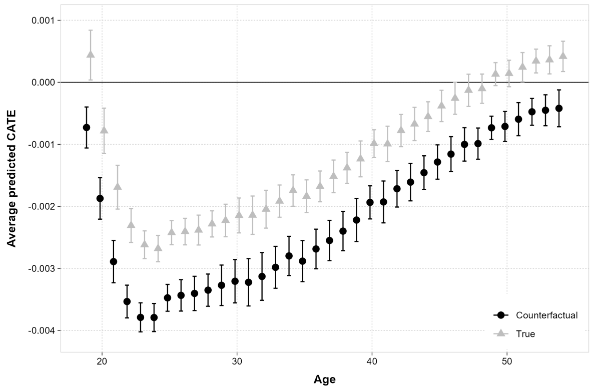
Note: Wage offer distributions for jobs starting in $[t-4, t-2]$. Wages are measured as monthly earnings. Kernel density estimates are calculated using the Epanechnikov kernel and a default bandwidth.

Figure D.6: Alternative CATEs

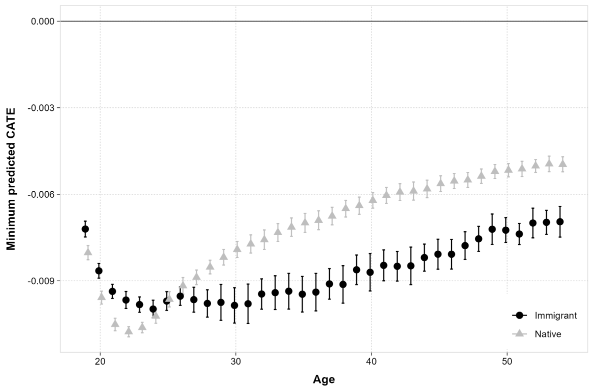
(a) Minimum CATE, gender



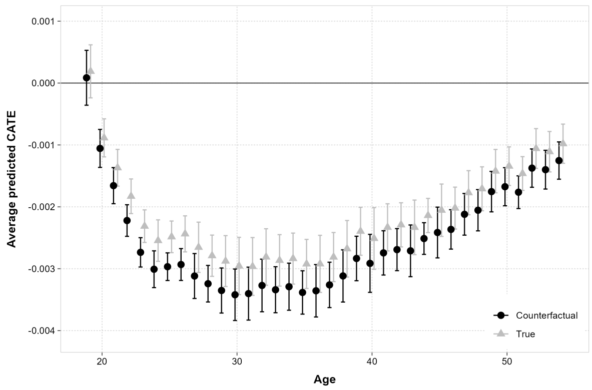
(b) Average CATE, Women



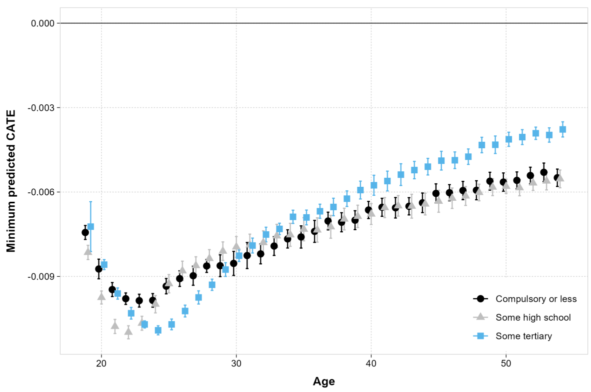
(c) Minimum CATE, nativity



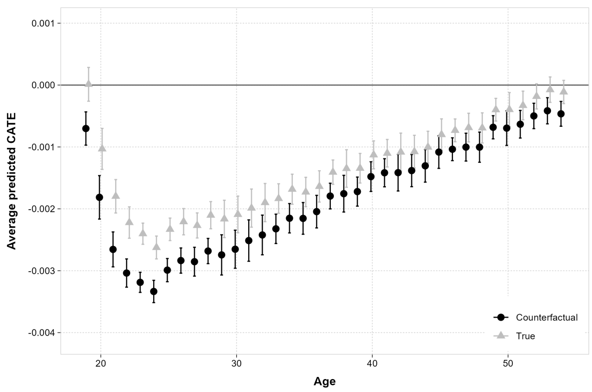
(d) Average CATE, immigrants



(e) Minimum CATE, education



(f) Average CATE, low-educated

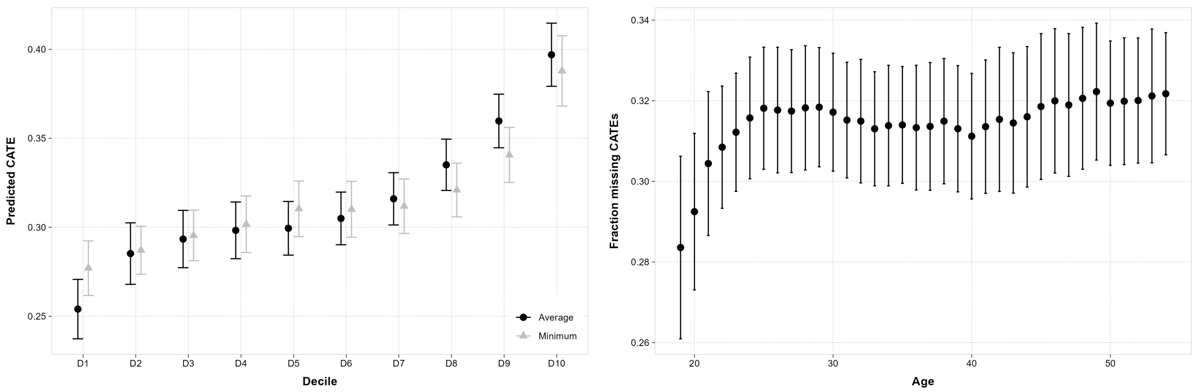


Note: Column 1 shows the effect of removing the most valuable connection, i.e. the one with the most negative CATE, by demographic characteristic and age. Columns 2 shows counterfactual average predicted CATEs, replacing a given demographic characteristics with another for the focal individual, keeping their set of contacts constant. 95 per cent confidence intervals, clustered by calendar year in which the network is measured, are reported.

Figure D.7: Fraction of missing CATEs

(a) By CATE deciles

(b) By age



Note: Average share of missing CATEs per worker within decile of either the within-worker average CATE or minimum CATE, or by age. 95 per cent confidence intervals, clustered by calendar year in which the network is measured, are reported.