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What makes a good caseworker?[a]

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Abstract

How do caseworkers affect job finding and what characterizes a productive caseworker? To answer these questions we exploit variation coming from the fact that many local employment offices in Sweden assign job seekers to caseworkers based on their date of birth. We couple this identification strategy with fine-grained administrative data on both caseworkers and job seekers. Estimation of caseworker fixed effects reveals sizable variation in overall caseworker value-added. Female caseworkers perform better than male caseworkers and caseworkers with two years of experience outperform caseworkers with less experience. Cognitive ability and personal experience of unemployment are not related to caseworker performance. Based on the actions taken by the caseworkers we show that caseworker strategies are important. Analyses of caseworker–job seeker matching show that matching based on previous labor market experiences or gender leads to better outcomes.

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1 Introduction

Countries around the world make extensive use of active labor market policies (ALMPs) during both booms and busts. Job-search assistance, training, monitoring and other labor market programs are frequently used to try to bring unemployed workers back to work. By now, there is extensive evidence on these policies, and surveys of the literature provide policy relevant insights (see, e.g., Card et al., 2010, 2017). However, an often overlooked aspect of labor market policies is the caseworkers that are responsible for implementing the labor market policies.

Caseworkers at local employment agencies (public or private) typically provide counseling and job-search assistance to job seekers. Since previous evidence show that such services affect employment (e.g., Graversen and van Ours, 2008; Crépon et al., 2013), the quality and the quantity of the counseling and the job-search assistance provided by the caseworkers should affect job finding. Caseworkers can also refer job seekers to relevant job openings, suggesting that we need caseworkers who understand the job seekers' labor market opportunities. They may also use their networks to help job seekers find suitable jobs, which may be important considering the empirical evidence on the importance of informal hiring channels (see, e.g., Hensvik and Nordström Skans, 2016; Dustmann et al., 2015). Moreover, caseworkers typically assign job seekers to labor market programs and targeting the right programs to the right job seekers have important employment effects, both via locking-in and post-program effects. All this suggests that caseworkers are important, and thus, understanding what makes a good caseworker can help us to improve labor market services.

Despite their importance, the evidence on the performance of caseworkers is scarce. One reason is that, in most cases, there is non-random sorting of job seekers to caseworkers. Moreover, data do typically not link caseworkers to job seekers, and in the rare cases when such information is available, little is known about the caseworkers. Our paper overcomes these challenges and provides novel evidence on what makes a good caseworker. Two features of our analyses are key. First, we use rich data on caseworkers and link caseworkers to job seekers. Our data contain information on caseworker demographics, cognitive and non-cognitive ability as well as measures of labor market history, such as caseworker experience, previous occupations, and personal experience of unemployment. This fine-grained data is used to study what characteristics and labor market experiences make a good caseworker. Combining information on caseworkers with administrative data on job seekers also allow us to study matching of caseworkers and job seekers in detail.

Second, we break the caseworker–job seeker sorting by exploiting that many local employment offices in Sweden use date-of-birth-rules to allocate job seekers to caseworkers. That is, within an office one caseworker is, for example, responsible for all job seekers born on the 1st-5th each month, another caseworker is responsible for job seekers born on the 6th - 10th, and so on. Since the exact birth date is unrelated to both observed and unobserved job seeker characteristics, this creates as-if random allocation. To handle occasional exemptions from the date-of-birth-allocation, we use an IV-framework based on the caseworker each job seeker would have had if they had been allocated using the date-of-birth-rule.

The random allocation and the fine-grained data provide an ideal setting to study what characterizes a productive caseworker. In the first part of the paper we analyze how observed caseworker characteristics are related to caseworker performance as measured by the unemployment duration among their job seekers. One finding is that female caseworkers perform better: job seekers with female caseworkers have 3.1% shorter unemployment durations than those with a male caseworker. We also see that caseworkers with at least two years of experience outperform recently hired caseworkers, but beyond that additional experience does not matter. Many other caseworker characteristics, such as type and level of education, previous occupations and personal experience of unemployment are not related to caseworker performance. There is also no evidence that caseworkers with higher cognitive and non-cognitive ability have better outcomes than low-ability caseworkers.[1]

Based on the actions taken by the caseworkers we examine caseworkers' strategies. Inspired by Arni et al. (2020) we define "supportive" caseworkers as those who more often use supportive policies, such as sending their job seekers to labor market training, whereas "restrictive" caseworkers are those who more often use restrictive policies such as workfare programs. We also exploit that our detailed data include information on all meetings between caseworkers and job seekers, and label "active" caseworkers as those who more frequently meet with their job seekers. This type of strategy that has not been studied before but can be very important considering that meeting with job seekers is a core task for caseworkers. Our results show that restrictive caseworkers appear to perform better than non-restrictive caseworkers, but we find no evidence of any positive impact of supportive caseworkers. We also see that active caseworker perform better than other caseworkers. The fact that strategies matter is good news from a policy perspective as employment agencies may use on-the-job training and a good agency culture to promote certain strategies.

[1] The fact that many observed caseworker characteristics do not predict caseworker performance is consistent with results from the teacher literature, which finds little evidence of a relationship between teacher quality and observed teacher characteristics, see e.g., Rockoff (2004), Rivkin et al. (2005) and Rockoff et al. (2011) for some early studies on this topic.

The second part examines the overall value-added of caseworkers by estimating caseworker fixed effects. This takes both differences due to observed and unobserved caseworker characteristics into account. We find that a one standard deviation increase in caseworker value-added increases the job-finding rate among job seekers by around 0.05-0.08 standard deviations. We conclude that the differences between caseworkers are economically important, but note that these estimates are smaller than those found for teacher value-added, which typically range from 0.10 to 0.30 standard deviations (Rockoff, 2004; Rivkin et al., 2005; Hanushek and Rivkin, 2010; Rothstein, 2010; Jackson et al., 2014; Chetty et al., 2014a).[2]

In the third part of the paper we examine caseworker–job seeker matching. Using our fine-grained data we show that matching job seekers to caseworkers with similar labor market experiences leads to substantially shorter unemployment durations: having a caseworker with work experience from a similar industry increases the 180-days job-finding rate by 0.8 percentage points and shortens the unemployment duration by 3.2%. One explanation could be that experience from working in a similar industry as the job seeker enables caseworkers to better understand the individual-specific labor market opportunities. Another explanation, supported by previous evidence on the importance of informal hiring channels, is that caseworkers use networks from previous job to help the job seekers. We also see that gender matching matters. These results show that systematic matching of job seekers to caseworkers lead to better results than random allocation via date-of-birth-rules.[3]

Our paper relates to a relatively small, but growing, literature on caseworkers. In a recent paper, Schiprowski (2020) studies the overall impact of a meeting with a caseworker using unplanned absences among caseworkers as exogenous variation. Job seekers who lose out on a meeting stay, on average, unemployed 5% longer and the bulk of the effect is driven by caseworkers in the upper half of the productivity distribution. Other studies have focused on a comparison of caseworkers who prefer supportive or restrictive polices (Behncke et al., 2010b; Huber et al., 2017; Arni et al., 2020). The only previous study on caseworker-job seeker matching is Behncke et al. (2010a), who study similarity between caseworker and job seeker in four dimensions at the same time (age, gender, education and nationality). Some studies have examined how caseworkers assign job seekers to labor market programs and its impact on job finding (Lechner and Smith, 2007; Staghøj et al., 2010; Knaus et

[2]Our results on caseworker value-added also relates to studies documenting that managers matter for firm policies and firm performance (Bertrand and Schoar, 2002; Bloom et al., 2013; Lazear et al., 2015).

[3]Studies on the matching of teachers and students have found that similar ethnic background improves student outcomes (Dee, 2004), while there are mixed results for having the same gender (Neumark and Gardecki, 1998; Bettinger and Long, 2005; Dee, 2007; Hilmer and Hilmer, 2007).

al., 2020)[4], and Schmieder and Trenkle (2020) study how caseworkers respond to differences in unemployment insurance eligibility.

Using the date-of-birth-allocation and our detailed data we add to this literature in several ways. With the exception of Schiprowski (2020), previous studies are mainly based on conditional independence assumptions, assuming that the allocation of job seekers to caseworkers is random conditional on observed job seeker characteristics (see, e.g., Lechner and Smith, 2007; Behncke et al., 2010a,b; Arni et al., 2020). Compared to previous studies we can also provide more comprehensive evidence on caseworker performance. It includes estimation of caseworker value-added, opening the black box of what characterizes a successful caseworker, and providing a deeper understanding of caseworker–job seeker matching, for instance, by showing that matching job seekers to caseworkers with similar labor market experiences are important.

The outline of the paper is as follows. Background and institutional details are given in Section 2. Section 3 describes our data and Section 4 presents identification strategy based on the date-of-birth-allocation. The three empirical parts are then presented in three separate sections: the impact of caseworker characteristics in Section 5, caseworker value-added in Section 6, and the effects of caseworker-job seeker matching in Section 7. Section 8 concludes.

2 Background: Caseworkers

Caseworkers play an important role for labor market policies around the world. In the U.S., caseworkers all over the country support job seekers at around 2,500 American Job Centers (AJCs). The idea is that the AJCs should be "one-stop" resources, providing comprehensive services to job seekers. Services include: 1) core services, which include staff-assisted job search, placement information and counseling; 2) intensive services with more comprehensive assessment and counseling, and career planning; and 3) training services, such as on-the-job training and training programs (see, e.g, Brown and Holcomb, 2018, for more detailed information.). As expected, there are local and state variation in the implementation of these services, but, generally, caseworkers at the job centers help job seekers search for jobs, offer career planning, and provide basic and intensive counseling. Since all intensified counseling

[4] Lechner and Smith (2007) and Staghøj et al. (2010) study assignment to training and compare the actual assignment by caseworkers, random assignment, and assignment based on estimated treatment effects. One conclusion is that caseworkers add little to the effectiveness of training programs. In a related recent paper, Knaus et al. (2020) use machine learning techniques to estimate heterogeneous effects of a job-search program, and identify easy-to-implement program assignment rules.

and training services have to be approved, caseworkers also play an important role for assignment to these kind of services. Previous evidence show that job-search assistance and counseling services have important employment effects (e.g., Graversen and van Ours, 2008; Crépon et al., 2013), and that training and similar intensive services may have positive long-run employment effects but also cause locking-in effects (e.g., Lechner et al., 2011; van den Berg and Vikström, 2019). It suggests that meeting with a caseworker that provides high-quality counseling or being assigned to training by a highly qualified caseworker may have important implications for your chances of finding a job. Thus, understanding what makes a good caseworker may help us to improve labor market services.

In the early 2000s – the period studied in this paper – most labor market policies in Sweden were organized by the Swedish Public Employment Service (PES). The services provided by the PES are organized by local offices, which provide services to all job seekers in the local area. At the time, there were around 300 local offices and 6,000 caseworkers. Almost all job seekers register at the local PES office, since it is a requirement for obtaining unemployment insurance benefits and receiving support from the PES. Once job seekers register, they are assigned a caseworker at the local office.

Similar to the U.S., caseworkers in Sweden have a wide range of tools at their disposal. They decide how frequently to meet with each job seeker, what kind of labor market programs (e.g., training and work practice programs) that are suitable for each job seeker, and they can use their formal or informal connections with employers to refer job seekers to relevant job openings. There are also differences compared to the U.S., however. One is that Swedish caseworkers are also responsible for monitoring job seekers search behavior with respect to unemployment insurance (UI) requirements. Another difference is that caseworkers in Sweden – at least for the period studied here – are explicitly responsible for giving support to the job seeker throughout the unemployment spell.

In providing the services, Swedish caseworkers have guidelines, recommendations, and laws to follow. However, survey evidence in Lundin (2004) and Lagerström (2011) and reveals that caseworkers have a substantial degree of discretion when deciding which programs and services job seekers should get.[5] One reason is that the guidelines and recommendations give caseworkers a great deal of leeway.

[5]Besides providing survey evidence on caseworker discretion, Lagerström (2011) studies caseworker performance using Swedish data. He uses a selected sample of offices based on a survey conducted in the fall of 2002. With our data we can see that there is strong non-random sorting of job seekers to caseworkers at many of the selected offices, and thus it is unclear if the results on caseworker performance reflect sorting or actual caseworker performance.

Another reason is that caseworkers typically are evaluated based on specific goals (e.g., number of job seekers who find a job) and not on the programs and strategies they use (Lundin, 2004). Altogether, it means that caseworkers have substantial discretion when choosing and implementing a range of key services to their job seekers.

There is no specific education for becoming a caseworker in Sweden. In fact, the PES have tried to attract individuals with different types of education and background, with the argument that different skills and experiences are needed to provide the best support to different types of job seekers. This has led to a diverse group of caseworkers in Sweden, with people from different backgrounds and with different prior experiences. For the time period we are studying, there were only two formal criteria for becoming a caseworker: at least an upper secondary education degree, and at least three years of work experience.

The local PES offices are supposed to adjust the activities and organization to local needs (Lundin and Thelander, 2012). Among other things, the local offices are free to decide how to allocate job seekers to caseworkers. Some offices try to match job seekers to the caseworker they think can give the best support. Other offices have caseworkers who specialize in job seekers from certain industries (e.g., construction) or from certain groups (e.g., immigrants and disabled workers). Many offices use simple date-of-birth-rules to allocate job seekers to caseworkers (described in detail in Section 4). Correspondence with managers at local offices reveal that such date-of-birth-rules are viewed as a transparent and easy way to equalize workload across caseworkers, and to monitor performance.

3 Data

We have detailed data on both caseworkers and job seekers that are linked using unique caseworker identifiers, including information on the job seekers' exact date of birth, which is key when we exploit the date-of-birth-rules.[6]

Using the staff records at the PES we have retrieved the social security number for each caseworker. Since the same social security number is used in all administrative records in Sweden we can add information from various records with information on demographics, employment history, ability scores, etc. We use caseworker data for the period 2003–2010, because caseworker identifiers are not available in data prior to 2003, and after 2010 fewer offices use date-of-birth-rules.

[6]Each caseworker has a unique five-letter signature that is used for all documentation at the PES, for instance, when documenting meetings between caseworkers and job seekers.

We use the population register Louise from Statistics Sweden to add information on *demographics* (e.g., age, gender, and country of origin) and *education* (level and field). We add information on the universe of employer-employee matches between 1993 and 2010, using employment records from Statistics Sweden (RAMS). Since we study caseworkers during 2003–2010, it means that we can construct detailed measures of *employment history* for each caseworker during the last 10 years which, for instance, are used to examine whether experience from a similar industry as the job seeker matters for caseworker performance. Using these employment records we also measure *caseworker experience*, defined as the number of years employed as caseworker at the PES.

Unemployment registers at the PES provide measures of *unemployment history* at the daily level. Together with information from the staff records it allows us to identify whether a caseworker was recruited directly from unemployment (being registered as unemployed at the PES at least one day during the two weeks prior to being hired as caseworker). The records at the PES also contain information on all actions taken by the caseworkers, including job search support and assignment of job seekers to various training programs. This is used to characterize caseworker *strategies*. As explained in detail in Section 5, we characterize caseworker as "supportive", "restrictive" and/or "active".

We also have access to measures of *cognitive* and *non-cognitive* abilities from military enlistment tests for a large share of the male caseworkers (60.5%). Essentially, all men born between 1951 and 1981 were obliged to participate in an enlistment process at the age of 18, which included ability tests. The measure of cognitive ability is an index incorporating problem solving, induction capacity, and numerical, verbal and spatial comprehension. The non-cognitive ability is assessed by a certified psychologist. Both ability measures are cohort standardized normalized, and range from 1 (worst) to 9 (best), with mean 5.[7]

The same administrative registers are used to construct background characteristics and employment outcomes for the job seekers. It gives us information on demographics, education, as well as employment and unemployment history. To create employment outcomes, we use that the PES records include day-by-day information on unemployment status. Our main outcomes are indicators for leaving unemployment within 90 and 180 days, and log unemployment duration. Since bringing the unemployed quickly back to work is the main goal for the caseworkers, these job seekers outcomes should reflect differences in caseworker performance.

[7]The quality of the Swedish enlistment tests is considered high, and the measures have a strong predictive power on future earnings. See Lindqvist and Vestman (2011) for a more detailed description of the enlistment tests and the measures of cognitive and non-cognitive abilities.

We sample all job seekers who start an unemployment spell during 2003–2010, but apply some sampling restrictions. We exclude public employment offices with less than 200 registered job seekers per year, and caseworkers with fewer than 30 job seekers per year. We also exclude job seekers with an unemployment spell in the year before the current spell as they are often exempted from the date-of-birth-allocation. The final data set consists of 2,217,863 unemployment spells, 1,600,132 unique job seekers, 6,812 caseworkers, and 252 offices (see Table 1). The statistics in the two first columns of Panel A of Table 1 show that the average job seeker is almost 32 years old, about half of them are females, 86% are Swedish, 24% are married, 36% have children, 30% have only primary school education, and two thirds are eligible for UI. The statistics in the other four columns in Panel A are discussed in Section 4.

3.1 Caseworker descriptives

Caseworkers in our data are on average about 47 years old, almost two thirds are women, and 9 out of 10 are born in Sweden (Panel B, Table 1). Almost all caseworkers have completed at least upper secondary education (as expected according to the rules for becoming a caseworker), and two out of three have a university degree. The two most common fields of education are social and business studies, together accounting for almost half of the caseworkers.

Table 1 also shows that 12% of the caseworkers have less than two years of caseworker experience, while more than half of the caseworkers in our sample have 10 or more years of experience. We have also examined what types of jobs the caseworkers had before they became caseworkers. One type of measure is the share of caseworkers with experience from the private sector, defined by manufacturing, construction, retail or hotel/restaurant industries. This share is rather low, but note that because of the data that we have we can only study experience in the last ten years, and roughly half of the caseworkers have worked at the PES for more than ten years. We also see that 41% of the caseworkers were recruited directly from unemployment. That is, when the PES recruits new caseworkers they often do so from the pool of unemployed job seekers.

Figures 1 (a) and (b) show the distribution of cognitive and non-cognitive ability scores from enlistment tests for the male caseworkers. As comparison, we include the corresponding figures for caseworkers at similar public sector authorities (National social insurance board and National tax audit office), and the full population of males. As by design, the ability scores for the population follow a normal distribution with mean 5. Caseworkers at the PES and caseworkers at the other public authorities have, on average, higher scores (both cognitive and non-cognitive) than the population. This comes as no surprise, as the population includes both employed and non-employed workers, and the employed typically have higher ability than the non-employed. We also see that, on average, caseworkers at the PES have somewhat lower cognitive scores than the caseworkers at the other public authorities, but for the non-cognitive scores we see almost no differences between the two groups.

4 Identification using date-of-birth-rules

Evaluating caseworker performance is complicated by the fact that job seekers are typically systematically assigned to caseworkers. For instance, below we show that disadvantaged job seekers in Sweden often are assigned to more experienced and more highly educated caseworkers. To break this non-random sorting we exploit the fact that some local PES offices use job seekers' date of birth (day of the month) to assign them to caseworkers. As the day in the month you are born (1st to 31st) is uncorrelated with individual characteristics this creates as-if random allocation of job seekers to caseworkers.

Figure 2 illustrates the date-of-birth-rules. It shows the distribution of the job seekers' date of birth for caseworkers at two offices in our sample. Figure 2 (a) depicts an office that uses a date-of-birth-rule: caseworker 1 is responsible for job seekers born on the 23rd–31st of each month, caseworker 3 for the 16th–22th, and so on. The office in Figure 2 (b) does not use a date-of-birth-rule, leading to a uniform distribution of the dates of birth across caseworkers. These offices without date-of-birth-rules use different allocation rules, such as trying to match productive caseworkers to the most disadvantaged job seekers or letting caseworkers specialize in different occupational groups. In both cases this creates non-random sorting.

Figure 2 (a) also shows that offices with a date-of-birth-rule occasionally make exemptions, however. For instance, caseworker 4, with job seekers predominately born on dates 1st–8th, also have some job seekers born on other dates. Reasons for such exemptions may include temporarily high caseloads and/or that job seekers with special needs occasionally are exempted from the date-of-birth-rule. Even though the exemptions are rather rare, they could still create non-random sorting. We therefore use an IV framework, where the caseworker that a job seeker would have been assigned according to the date-of-birth-rule (predicted caseworker) is used as an instrument for the actual caseworker. If many job seekers born on the same day also have the same caseworker (as in Figure 2(a)), there will be a close connection between the predicted caseworker and the actual caseworker, leading to a strong instrument. Moreover, since the predicted caseworker is based only on date of birth, the instrument is as-if random.

Optimally, this procedure is supported by complete information on the offices that use a date-of-birth-rule as well as information on which days of the month each caseworker is responsible for. The latter is relevant, since caseworkers at some larger offices have 3–4 days of the month and caseworkers at smaller offices may have 7–8 days. Unfortunately, the PES never collected this type of information. However, as apparent from Figure 2, data often immediately reveal if an office uses a date-of-birth-rule. One way to show this, is to perform F-tests for whether the job seekers' date of birth are evenly distributed across caseworkers within offices.[8] The distribution of the resulting F-tests in Figure A-1 in Appendix A (with truncation at 200) show that many offices clearly use a date-of-birth-rule (high F-values), but also that many offices do not (low F-values).

Since we also lack institutional information on which days of the month each caseworker is responsible for, data is also used to construct information on the predicted caseworker. For each office, year and day of the month, we let the predicted caseworker be the caseworker with the largest number of job seekers born on a specific day of the month. That is, the caseworker with the largest number of job seekers born on the e.g. the 10th of any month, will become the predicted caseworker for all job seekers born on the 10th for that office. For many offices, including the date-of-birth-office in Figure 2 (a), this procedure will capture the actual date-of-birth-rule very well.

This strategy is applicable for all offices, even for offices without a date-of-birth-rule. Therefore, we do not restrict the sample to certain offices. The only difference is that the predicted caseworker is a strong instrument for the actual caseworker at the offices with date-of-birth-rules, but not for offices without a date-of-birth-rule, since for them the predicted caseworker is uncorrelated with the actual caseworker. However, note that this only weakens the overall first stage: the predicted caseworker instrument is always random, as it is based solely on the date of birth. In sensitivity analyses we also restrict the sample to offices with a distinctive date-of-birth-rule, but this does not change the results.[9]

[8] Specifically, we regress the job seekers' date of birth (1–31) on caseworker dummies (within office and year) and examine the joint F-statistic.

[9] By construction, we estimate the local average treatment effect (LATE) for the complier population, consisting of job seekers at offices with a date-of-birth-rule that are assigned according to the rule. To illustrate the complier population the third and fourth column of Panel A of Table 1 show summary statistics for the offices that we label as date-of-birth-offices, defined by $F > 200$ for the above discussed test whether the job seekers' date of birth are evenly distributed

Finally, we note that it is rather common for offices to use special date-of-birth-rules for youths (aged 24 or younger). One example of such an office is shown in Figure A-2 in Appendix A. We therefore assign the predicted caseworker separately for youths and non-youths.

5 Caseworker characteristics and strategies

The date-of-birth-strategy is used in all three empirical sections of the paper. In this section we exploit our fine-grained data to examine if caseworkers' demographics (e.g., gender, age), labor market experiences, abilities and/or strategies are related to caseworker performance.

5.1 Empirical strategy

Caseworker performance is measured using employment outcomes (y) for the job seekers. For job seeker i in office k in year t in age group g with caseworker j our model is:

$$y_{ijktg} = \alpha + \beta CW_{jt}^{X} + (\phi_k \times \gamma_t \times \theta_g) + \eta_{ijktg}. \tag{1}$$

We instrument the characteristics of the actual caseworker, CW_{jt}^{X}, with the same characteristics of the predicted caseworker using 2SLS. Since the date-of-birth-rules create as-if random allocation within offices and the rules may change over time we include a full set of office $\times$ year fixed effects (ϕ_k and γ_t). Some offices use a separate date-of-birth-rule for youths, which is why we also interact with an age group dummy for being younger than 25 (θ_g). We cluster standard errors at the caseworker level.

Relevance We first examine the first-stage and show that our instruments (characteristics of the predicted caseworker) are correlated with the endogenous variables (characteristics of the actual caseworker). Since the identifying variation is across caseworkers within each office, year, and age group, these first-stage models also include a full set of office×year×youth fixed effects. The first-stage estimates for caseworker experience in years (column 1) and caseworker education (column 2) in Table 2 show that we have a strong first-stage, with first-stage F-statistics of 1,145

across caseworkers within the office. It shows that these offices are larger in terms of the number of caseworkers and job seekers, but otherwise the characteristics of the job seekers and the caseworkers are similar to those of the full population. We also characterize the complier population using the methods in Abadie et al. (2002). Here, the last two columns in Panel A show that compliers have somewhat shorter unemployment durations but otherwise they are similar to the full population.

and 1,077. The joint F-statistic for these two variables is also high (1,134). The full first-stage estimates for all caseworker characteristics reported in Table A-1 in Appendix A show that we also have a strong first-stage for all variables.[10] Table 2 shows that the date-of-birth-rules correctly predict the actual caseworkers for 44.1% of the job seekers.

Randomization Since our predicted-caseworker instruments are based on date of birth they should be as good as randomly assigned. To show this we use the predicted unemployment duration for each job seeker as one unified measure of job seeker quality.[11] Figure 3 (a) plots predicted unemployment duration against experience in years for the actual caseworker, revealing a striking selection pattern as more experienced caseworkers are assigned job seekers with weaker attachment to the labor market (longer predicted unemployment durations). Since we use the full sample of offices, this captures sorting at offices without a date-of-birth-rule as well as sorting due to the exemptions at the offices that use a date-of-birth-rule. However, as expected, Figure 3 (b) shows that this sorting vanishes when we use the experience of the predicted caseworker. That is, experience of the predicted caseworker is completely unrelated to the predicted unemployment duration.

These patterns are confirmed by the regression estimates in Table 3. Column 1 shows that more experienced caseworkers and caseworkers with a university education are paired with job seekers with worse characteristics (longer predicted unemployment durations). Column 2, on the other hand, shows that experience and education of the predicted caseworker are uncorrelated with job seeker characteristics. For instance, caseworkers with a university degree have, on average, job seekers with 12.5 days longer predicted unemployment duration. The corresponding number for the predicted caseworker is close to zero (0.5 days) and insignificant.[12]

The selective assignment of job seekers to caseworkers at the offices without

[10]Note that each first-stage equation includes all instruments, but for each caseworker characteristic the most relevant instrument is the predicted caseworker equivalent. Moreover, the joint F-test for all instruments are high and well above the conventional rule-of-thumb, i.e. there is no problem with weak instruments.

[11]Specifically, we use the predicted unemployment duration for each job seeker from an OLS regression using the duration of the last unemployment spell, welfare benefits in the last year, regional unemployment rate, age, age squared and dummies for UI eligibility, disability, immigrant status, female and level of education (6 levels) as covariates.

[12]We obtain similar evidence in favor of independence for all other caseworker characteristics. We have also correlated the instruments with each separate job seeker characteristic. Table A-2 shows that job seeker characteristics such as disability, education and prior earnings are highly predictive of actual caseworker experience, but these correlations disappear once we use the predicted caseworker (all coefficients get much smaller, and all but one are insignificant at the 5% level).

date-of-birth-rules is an interesting result in itself. We see that more disadvantaged job seekers are systematically assigned to caseworkers with longer experience and higher education. This is most likely based on the belief that more experienced and highly educated caseworkers are more productive. That is, we document assignment patterns suggesting that the PES aims to help more disadvantaged job seekers by paring them with presumably more productive caseworkers.

One potential threat to causal identification is that immigrants whose birth date is unknown upon arrival to Sweden are registered as being born on specific dates, such as the 1st, 5th, and 10th. Another potential concern is that even if job seekers are assigned using their date of birth, local offices may vary the number of days each caseworker is responsible for, for instance, because they have to allocate an odd number of days (31) to the caseworkers. Sensitivity analyses in Section 5.5 show that these two potential threats do not affect our results.

Exclusion restriction The exclusion restriction will be violated if there are important caseworker characteristics that we cannot observe with our detailed data that are correlated with the characteristics used in the analyses. We cannot formally test for this, but note that we have rich information on key caseworker demographics, labor market experiences, and cognitive and non-cognitive ability.

Monotonicity The monotonicity assumption implies that job seekers with an experienced predicted caseworker should not be assigned a less experienced actual caseworker than if they had a predicted caseworker with less experience. Since no job seeker can be assigned to two different caseworkers at the same time we cannot verify this assumption, However, one testable implication of monotonicity is that the first-stage estimates should go in the same direction for all sub-samples (see, e.g., Bhuller et al., 2020). If this is not the case we can reject the monotonicity assumption. Table A-3 shows that the first-stage estimates for various sub-samples all are positive and significant. This lends some support of the monotonicity assumption.

5.2 Demographics and education

To study caseworker demographics and education we use information on age, gender, immigrant status, level of education (compulsory, upper secondary, and university), and the two most common fields of education (business and social science).

The results in Table 4 show that the gender of the caseworker is an important indicator of caseworker performance. Being assigned a female caseworker shortens job seeker's unemployment duration by, on average, 3.1% (column 3), and increases the probability to leave unemployment within 90 and 180 days by 1 and 1.1 percentage points, respectively (columns 1–2). These estimates are comparable to Cheung et al. (2019) who study a job-search assistance program that increased the number of meetings from 2 to 5 in the first quarter of the unemployment, and find that the program increased the 90-days job-finding rate by 3.9 percentage points. Thus, the effect of having a female caseworker instead of a male caseworker is roughly one fourth of the effect of this program.

The other observed caseworker characteristics in Table 4 are unrelated to caseworker performance. Older or native caseworkers do not perform better than younger caseworkers or immigrants. Moreover, even though a higher level of education, in general, is related to higher ability and better skills, it is not predictive of what makes a good caseworker. Whether the caseworker has a degree in business or social science, which include human resource management, also makes little difference. The latter suggests that general knowledge of the recruitment process is of minor importance for caseworker performance.

5.3 Experiences and abilities

Studies on other occupations suggest that experience matters for productivity and performance (e.g., Shaw and Lazear, 2008; Haggag et al., 2017). Table 1 also revealed a great deal of heterogeneity in caseworker experience. To study if caseworker experience matters we divide caseworkers into groups, with [0–2), [2–4), [4–6), [6–8), [8–10), and 10 or more years of experience, respectively. The results in Panel A of Table 5 show that caseworkers with at least two years of experience perform better than caseworkers with 0–2 years of experience (reference category). The probability of leaving unemployment after 180 days increases with 1.3–1.5 percentage points (column 2) and unemployment durations are about 2.8% shorter (column 3). These effects are of about the same magnitude as the difference between female and male caseworkers. Higher levels of experience (4–6 years of experience and beyond) have a similar impact as 2–4 years of experience. The fact that the first years of experience are important while additional years are not in line with results from the teacher literature, where, for instance Rivkin et al. (2005), find that improvements in teaching skills only matter during the first 3—5 years in the classroom.[13]

These patterns may reflect learning but it may also reflect dynamic selection. Caseworkers who perform poorly in the beginning of their may career leave the

[13]We also see that caseworkers experience have no significant impact on finding a job within 90 days, which suggests that caseworkers experience matter more for long-term unemployed than for short-term unemployed.

caseworker profession, alternatively productive caseworkers may move on to higher paid occupations. In both cases the composition of caseworkers changes with experience, but a priori the direction of the selection is unknown. To explore this we quantify early performance (value-added) in the first two years and examine if early performance predicts who stay as a caseworker for more than 2 and 4 years.[14] The results in Table A-4 in the appendix provide some evidence that the most productive caseworkers are less likely to continue working as a caseworker (all coefficients are in that direction, but only one out of six estimates are significant). If anything, this suggests that we underestimate the importance of experience, implying that our experience estimates reflect learning effects rather than selection.

Around 40% of the caseworkers are hired from the pool of unemployed (Table 1). This raises the question if own unemployment is a good or bad experience? On the one hand, it may give caseworkers some insights into the practical problems associated with job search, but on the other hand, it may correlate with less favorable unobserved caseworker-attributes. In Panel B of Table 5, we show that caseworkers hired form unemployment do not perform differently compared to other caseworkers.[15] Thus, it seems that own personal experience of unemployment is irrelevant for the caseworkers, or that unobserved caseworker attributes perfectly cancel the benefits of unemployment experience.

Next, we exploit the information on all previous occupations held by the caseworkers in the last 10 years, and examine if caseworkers with certain occupational experiences perform better than others. A starting point is that personal experience from the private sector may be important when providing job-search counseling. This is examined in Panel B of Table 5, which reveals no significant impact of this kind of previous labor market experience, at least not on average. However, it may be the case that previous occupational experiences only are important for certain groups of job seekers. For instance, caseworkers who previously worked in the private sector may provide better counseling to job seekers searching for private sector jobs, highlighting the caseworker–job seeker matching questions studied in Section 7.

Section 3.1 revealed quite large heterogeneity for caseworkers' cognitive and non-cognitive abilities, raising the question if these abilities matter for caseworker performance. The results in Panel C of Table 5 indicate that both cognitive and

[14] As measure of early performance we use estimated value-added in the first two years using the procedure described in Section 6 below, and classify caseworkers by below or above median value-added among all caseworkers during their first two years.

[15] We have also estimated the effects of different lengths of own unemployment experience, but these analyses reveal no differences between caseworkers with different unemployment histories.

non-cognitive ability are relatively unimportant: the estimates are insignificant and close to zero. The reported estimates are for standardized scores (mean zero and a standard deviation of one), but we find similar results with more flexible specifications using the 1 to 9 score-scale. For policy, one conclusion could be that the frequently used ability tests seem to be an inefficient way to screen and recruit caseworkers.

5.4 Strategies

We next examine if caseworkers' strategies matter. Initially, we follow Arni et al. (2020) and label caseworkers as "supportive" if they are more prone to use supportive policies (training or intensified job search assistance), and "restrictive" if they more often use restrictive policies (workfare).[16] The idea is that training and job search assistance focus on improving the skills of the job seekers, i.e. promote employment through increased *support*, whereas workfare, which typically is used as a tax on leisure and to test whether the job seeker is ready to take a job, promotes employment through pressure and *restrictions*.[17]

To these two strategies we add a third one. Using our fine-grained data with information on all meetings between caseworkers and job seekers we label caseworkers as "active" if they more frequently meet with their assigned job seekers. In Sweden, there are no regulations for how often caseworkers should meet job seekers. The frequency of meetings therefore reveals how intensively caseworkers choose to interact with job seekers, or in other words, how much time and effort they put in. That is, how "active" they are. Since meetings are core tasks for many caseworkers, this may capture an important dimension of caseworker strategies not studied before.

To define the strategies, we calculate each caseworker's propensity to use each policy. Supportiveness is based on the fraction of job seekers assigned to training or the intensified job-search assistance, restrictiveness is the fraction sent to workfare. Similarly, activeness is based on the meeting intensity for each caseworker, defined as the average number of realized meetings with job seekers per month in unemployment. To deal with the fact that longer unemployment spells mechanically have more meetings and programs, we normalize each propensity measure by the

[16]When defining caseworker strategies based on actions taken by caseworkers, we are not restricted to having accesses to caseworker characteristics. Hence, we are able to define strategies for 24 additional caseworkers for whom one or more of the other characteristics are missing.

[17]Labor market training (*Arbetsmarknadsutbildning*) and intensified job search assistance (*Aktiviteter inom vägledning och platsförmedling*), define "supportive" caseworkers. Labor market training typically lasts for six months and intensified job search assistance typically lasts for three months. Work practice (*Arbetspraktik*) defines "restrictive" caseworkers, and involves 3–6 months of practice at a public or private firm.

job seeker's time spent in unemployment or until program start (as in Schmieder and Trenkle, 2020; Schiprowski, 2020). To avoid using the job seekers own meeting and program rate, we calculate a leave-one-out mean for each caseworker, where the individual itself is excluded. This gives continuous measures for each caseworker, but for ease of presentation we then label "supportive" caseworkers as those with a supportiveness measure above the median rate among all caseworkers, and define restrictive and active caseworkers in the same way.

In the same way as before, these strategies are instrumented by the corresponding strategies for the predicted caseworker, calculated in a similar way as the strategies for the actual caseworker. The only difference is that we now calculate the leave-one-out mean over the job seekers with same predicted caseworker. Since the predicted caseworker is solely based on date of birth it is as-if random, and we have a strong first-stage (see Table 6).

The results in Table 6 reveal no significant impact from having a supportive caseworker, while "restrictive" caseworkers appear to perform better than non-restrictive caseworkers. Being assigned a restrictive caseworker increases the 90-day job finding rate by 3.1 percentage points. We also see that "activeness" seems to be a successful strategy. Being assigned an active caseworker increases the 90-days and 180-days job-finding rates by 2.6 and 4.2 percentage points, and the unemployment duration is shortened by on average 7.6%. We note that these effects for restrictive and active caseworkers are larger than those for most caseworker demographics. One policy implication could be that employment agencies should focus on caseworkers strategies, i.e. how the caseworkers approach their work and profession. For instance, promoting certain ways of working via on-the-job training and by building a good agency culture. This seems to be more important than trying to find caseworkers with a specific background.

Our results add to existing literature, which has been inconclusive on similar topics. Arni et al. (2020) find that caseworkers that place more emphasis on support have better outcomes, while Behncke et al. (2010b) and Huber et al. (2017) show that tougher caseworkers are more successful than supportive ones.

5.5 Sensitivity analysis

Table 7 reports several sensitivity checks. To reduce the number of estimates, we show results for the effect of female caseworkers and the activeness strategy (all other estimates are available upon request). For comparison, the baseline estimates are reported in column 1.

One potential threat to our identification strategy is that immigrants whose birth date is unknown upon arrival to Sweden are registered as being born on specific dates, such as the 1st, 5th, and 10th, leading to a correlation between registered date of birth and job seeker characteristics. To test for this, we control for immigrant status (column 2) and include day-of-the-month fixed-effects (column 3). The latter removes any selectivity induced by registration of immigrants to certain days of the month. Note that our model is still identified, since there is variation in predicted caseworkers across offices for individuals born on the same day of the month. In both cases, our results are virtually unchanged.

In Tables 4–6 we examined separate blocks of covariates at a time. In column 4 of Table 7 we instead include all caseworker characteristics (with the exception of ability for which we have a much smaller sample) from these tables at the same time, but this does not change our results.

Since the identifying variation comes from offices with a date-of-birth-rule, column 5 examines what happens if we only use data from office with a distinctive date-of-birth-rule, as defined below in Section 6. Here, the estimated coefficients get somewhat smaller, but the changes are relatively small.

Finally, we examine if differences in caseload affect our results. Caseload difference may, for instance, because the offices have to allocate an odd number of days (31) to the caseworkers, or because certain types of caseworkers are given larger responsibility. In column 6 of Table 7 we therefore include caseload (number of job seekers per caseworker) as an additional characteristic. Using similar reasoning as above, this actual caseload for each caseworker is instrumented using the caseload of the predicted caseworker. This does not change our estimates, and we also note that the effect of caseload is close to zero and insignificant (not reported).

6 Caseworker value-added

We now examine the overall value-added of caseworkers and estimate caseworker fixed effects. The idea is to compare the impact of caseworkers when moving along the distribution of the fixed effects. That is, to compare the importance of having a caseworker in the upper part of the distribution instead of in the lower part, taking both differences due to observed and unobserved caseworker characteristics into account.

6.1 Empirical strategy

The starting point is to estimate caseworker fixed effects, μ_j, using a similar model as above:

$$y_{ijktg} = \alpha + \mu_j + (\phi_k \times \gamma_t \times \theta_g) + e_{ijktg}. \tag{2}$$

As above we adjust for the interaction between office, year and age group (dummy for under age 25) fixed effects. Previously, we used the characteristics of the predicted caseworker as instruments for the characteristics of the actual caseworker. Here, we use a similar approach with indicators for each predicted caseworker as instruments for the caseworker fixed effects. That is, for each endogenous variable (caseworker fixed effect) we have one instrument (indicator for the predicted caseworker).

There are some estimation issues to take into account. First, for the observed caseworker characteristics we used all offices, noting that the complier population is based on the offices with a date-of-birth-rule. Since we have no relevant instruments for the caseworkers at the offices without a date-of-birth-rule we therefore restrict the analysis to offices with a distinctive date-of-birth-rule.[18] For these offices, there is a close connection between the actual and the predicted caseworker, leading to a strong instrument for each caseworker fixed effect (F-statistics in Figure A-3). As before, the independence condition holds because identification is based on date of birth.

Second, the caseworker fixed effects from equation (2) gives unbiased value-added estimates for each caseworker, but by construction, these caseworker fixed effects are estimated with sampling error. If not accounted for, the sampling error will inflate the overall variance of the caseworker fixed effects, and thus exaggerate the overall importance of caseworkers. We therefore follow practices from the teacher literature and use multiple years of caseworker data to estimate the "true" variance of caseworker value-added (as in e.g., Kane and Staiger, 2008); a more detailed description is given in Appendix B. Briefly, we use equation (2) to estimate separate caseworker fixed effects for each year. Then, under the assumption that caseworker value-added is constant over time, the covariance between the fixed effects for the same caseworker across years equals the variance of the "true" caseworker value-added. This is similar to (Chetty et al., 2014a,b) with the only difference being that Chetty et al. also relax the assumption of constant effects over time by allowing for drift.

Third, following Kane and Staiger (2008) we compute value-added estimates for

[18] Specifically, we test for an even distribution of dates of birth across caseworkers within offices, and select offices with a F-value larger than 200. We have tried other cut-offs, and reach similar results. Sample statistics for these date-of-birth-offices are shown in Table 1.

each caseworker. The procedure "shrinks" each estimated caseworker fixed effect, which consists of both the true value added and sampling error, using a shrinkage factor that depends on the estimated "true" variance from above and the estimated sampling error for each fixed effect. Intuitively, caseworker effects with large sampling error are disproportionately "shrunk" toward zero, reflecting the fact that noisy caseworker fixed effect estimates carry less information about the "true" caseworker value added.

6.2 Results

The estimated caseworker fixed effects summarized in Table 8 reveal sizable differences in caseworker performance. By analogy to the teacher literature, we are interested in the distribution of the caseworker fixed effects and the impact of a one standard deviation increase in caseworker value-added on job seeker outcomes. We see that a one standard deviation better caseworker increases the probability of leaving unemployment within 180 days by 3.0 percentage points (column 1). Put differently, it implies that a one standard deviation increase in caseworker "value-added" increases the job seekers job finding rate by around 0.063 standard deviations (column 2). The estimates for the log unemployment duration shows that moving one standard deviation in the distribution of the caseworker fixed effects changes the unemployment duration by 11% or by 0.085 standard deviations.

Our results can be compared to the teacher literature, where the estimated impact of a one standard deviation increase in teacher "value-added" on student outcomes typically range from 0.10 to 0.30 standard deviations (Rockoff, 2004; Rivkin et al., 2005; Hanushek and Rivkin, 2010; Rothstein, 2010; Jackson et al., 2014; Chetty et al., 2014a). That is, the relation between caseworker fixed effects and job seeker outcomes are smaller than the estimates for teacher value-added. This is to be expected as it is natural to believe that teachers are relatively more important for students than caseworkers are for job seekers. We can also compare with Schiprowski (2020), who finds that job seekers who lose a meeting with their caseworker stay unemployed about 5% longer. Compared to our estimates for log unemployment duration this is roughly half of the impact of a standard deviation better caseworker, but note that we capture all actions taken by the caseworker, such as differences in the number of meetings, quality of the meetings as well as the quality of all other caseworker actions.

This was the distribution of the adjusted caseworker fixed effects using the "shrinkage" procedure in Appendix B. For comparison, column 3 of Table 8 reports the distribution of the unadjusted fixed effects. They are larger than the adjusted ones, which confirms that it is important to adjust for sampling error and failing to do so would overstate the importance of caseworkers.

We can also examine the variation in caseworker value added more closely. Figure 4 shows the full distribution of the caseworker value-added estimates when log unemployment duration is used as the job seeker outcome. Note that a log duration outcome means that above-average performing caseworkers have negative value-added measures as this implies shorter durations. The figure shows that a substantial share of the caseworkers are in the middle of the performance distribution, within the -0.05 to 0.05 span. Since the outcome is the log unemployment duration, this implies that most caseworkers are less than 5-percent better or worse than the "average" caseworker. However, a non-negligible share of the caseworkers also perform substantially better or worse than the average caseworker. There are somewhat more very good caseworkers than very bad caseworkers (high-performing caseworkers are in the left-tail of the distribution).

Next, we study for whom caseworker performance matter the most. It may be that advantaged job seekers are more able to find jobs on their own, while more disadvantaged job seekers gain more from a productive caseworker. To answer this question we use the adjusted caseworker value-added measures and distinguish between high and low performing caseworkers (above and below median value-added), and study the impact of a high performing caseworker for advantaged and disadvantaged job seekers (four groups by quartile of the predicted unemployment duration).[19]

Figure 5 shows that caseworkers are important for all job seekers, irrespective of their connection to the labor market. The outcome is the log unemployment duration, which means that negative estimates correspond to improved outcomes. Job seekers are divided by quartiles of the predicted unemployment duration, implying that group 4 is the most disadvantaged group. Although, the effects for advantaged and disadvantaged job seekers are not significantly different from each other, there is some indication that disadvantaged job seekers gain the most from a high performing caseworkers. There may, however, be more heterogeneous gains from matching on more specific characteristics. This is studied in the next section.

[19] We want to study value-added of the actual caseworker. To do so, we instrument the dummy for a high-performing caseworker with the corresponding dummy for the predicted caseworker. Even if we have unbiased value-added estimates we still need to instrument because of the selective exemptions. We also include interacted office, year and youth fixed effects.

7 Caseworker–job seeker matching

A literature in sociology has shown that individuals with the same gender or ethnicity (or other social attributes) behave differently towards one another than towards individuals of the other gender or other ethnic groups. For instance, sharing the same social identity could enhance communication and trust (see, e.g., Sherif et al., 1961). This argument suggests that caseworker–job seeker similarity with respect to gender and ethnic background is argued to matter for job seeker outcomes (Behncke et al., 2010a). Similar labor market experiences may also be relevant. Besides promoting communication and trust, it may enable caseworkers to better understand individual-specific labor market opportunities and therefore provide more adequate counseling and support. Caseworkers may also be able to use their networks from previous jobs to more effectively refer job seekers to suitable workplaces, and to better promote informal hiring channels. This is supported by evidence showing that informal hiring channels are important (see, e.g., Hensvik and Nordström Skans, 2016; Dustmann et al., 2015). Using our fine-grained data we can study all these dimensions of caseworker–job seeker matching

7.1 Empirical strategy

We explain our empirical strategy with gender matching. We create a variable, $Match_{ijt}^{X}$, that takes the value one if both the caseworker and the job seeker are males or if both are females, and zero otherwise. Since, the model also includes the main effects of the gender of the caseworker and the job seeker, this allows us to study if matching on gender is important. The resulting model includes the match effect for characteristic X, ($Match_{ijt}^{X}$), the direct effect of having a caseworker with characteristic X, (CW_{jt}^{X}), as well as the same characteristic for the job seeker, $Jobseeker_{i}^{X}$:

$$y_{ijktg} = \alpha + \delta Match_{ijt}^{X} + \beta CW_{jt}^{X} + \lambda Jobseeker_{i}^{X} + (\phi_j \times \gamma_t \times \theta_g) + \eta_{ijktg}. \quad (3)$$

As before, we use the corresponding variables for the predicted caseworker to instrument the actual caseworker-variables. For instance, the actual same-gender match variable is instrumented by the corresponding gender match variable for the predicted caseworker.

We proceed in the same way with ethnicity, ability, educational background and occupational background. We construct match-variables that takes the value one if job seekers and caseworkers are similar in the specific dimension, and zero otherwise.

To avoid overfitting we study matching on one characteristic at the time.

7.2 Results

Panel A of Table 9 shows results for gender matching using job-finding within 180 days as the outcome. It shows that that gender matching helps the job seekers. The positive and significant match-effect for gender-similarity means that job seekers who are assigned a caseworker with the same gender have a 0.4 percentage points, or 0.6%, higher likelihood of finding a job within 180 days. As above, we also see that female caseworkers perform better than male caseworkers. Interestingly, columns 2–3 show that both males and females are better off with a female caseworker. In particular, female job seekers benefit from being assigned a female caseworker (column 2), both because of the positive match effect and because the general positive effect of female caseworkers. For male job seekers (column 3), the gender of the caseworker is less important, because from a similarity perspective a male caseworker is preferred, but this is counteracted by the fact that female caseworkers perform better than male caseworkers.[20]

Matching caseworkers and job seekers based on immigrant status appears to be unimportant (Panel B of Tables 9 and A-5). Non-native caseworkers do not provide better support to non-native job seekers than native caseworkers. Next, we examine ability-similarity, and study matching of caseworkers and job seekers with above/below median ability.[21] Since we lack data on ability measures for female caseworkers, this analyses is restricted to male caseworkers. The results in Panel C of Tables 9 and A-5 provide no evidence that matching caseworkers and job seekers based on ability is important.

We now turn to caseworker–job seeker matching based on labor market experiences. We use previous occupational experiences and educational background as two measures of labor market experience. Similar educational background may give the caseworker a broader understanding of the relevant occupations and job-markets, while similar occupational experiences may also matter because of caseworkers' access to networks from previous jobs. As above, we define a match variable that takes the value one if the caseworker and job seeker share the same experience. For educational background we compare caseworkers and job seekers with and without

[20] The results in Table A-5 in the appendix for the log duration show similar results.

[21] Unfortunately, we do not have access to cognitive ability measures for job seekers. As shown in Figure 1, we do have data on cognitive and non-cognitive ability for a large part of the male population. However, these data cannot be linked to our job seeker data. Instead we use the predicted unemployment duration as a proxy for general ability, and define ability-similarity based on ability above/below the median for job seekers and caseworkers.

university education. For occupational experience, the match variable takes the value one if the caseworker has some personal experience from the private sector in the last 10 years, and if the job seeker worked in the private sector just prior to becoming unemployed, or if both of them have no such experience.

The results in Table 10 show that having a caseworker with similar labor market experience matters. Sharing the same occupational experience increases the 180-day job-finding rate by 0.8 percentage points (Panel A), and sharing the same educational background increases the 180-days job finding rate by 0.5 percentage points (Panel B). Both effects are significant at the 5-percent level. The results in Table A-6 in the appendix using the log duration as outcome confirm that having a caseworker with similar labor market experience and educational background shortens the unemployment duration, by 2.7% and 1.6%, respectively. The results for this outcome also suggest that caseworker's labor market experiences matter the most for job seekers from the private sector and for job seekers without university education.

In sum, we conclude that allocating job seekers to caseworkers based on previous labor market experiences improves job finding. Simply, caseworkers are more successful when they mediate jobs to job seekers for whom they can use their own previous labor market experiences. One explanation is that caseworkers better understand the relevant job-market opportunities if they have similar labor market experiences as the job seeker. It may also allow caseworkers to better use their networks from previous jobs when mediating jobs. It shows that careful matching of caseworkers and job seekers leads to more efficient employment policies.

These findings add to the evidence in Behncke et al. (2010a), who also show that similarity matters, but only when it comes to sharing characteristics in four dimensions simultaneously (age, gender, nationality, and education). Our results on labor market experience matching provide a deeper understanding of key aspects of caseworker-job seeker matching. Our results also relate to the teacher literature, which has found mixed evidence on the effects on student outcomes for teacher–student gender-similarity (Neumark and Gardecki, 1998; Bettinger and Long, 2005; Dee, 2004; Hilmer and Hilmer, 2007).

8 Conclusions

This paper has provided comprehensive evidence on caseworker performance, using an identification approach based on date-of-birth-allocation of job seekers to caseworkers and uniquely fine-grained data on caseworkers. By estimating caseworker fixed effects we examine the variation in caseworker value-added, quantifying both observed and unobserved differences between caseworkers. Our analysis reveals that caseworkers are important for the job seekers' chances of finding a job: a one standard deviation increase in the distribution of caseworker fixed effects increases the job-finding rate by around 0.05-0.08 standard deviations. This was expected since caseworkers play an important role for the implementation of labor market policies. Besides providing various forms of counseling and job-search assistance to job seekers, caseworkers can also refer job seekers to relevant job openings and assign them to more intensive services, such as training programs. Considering that previous research have found important employment effects of these policies, it is not surprising that caseworkers are important.

Using fine-grained caseworker data we relate caseworker performance to observed caseworker characteristics. It reveals substantial gender differences: job seekers assigned a female caseworker have shorter unemployment duration than those assigned a male caseworker. We also see that caseworkers with at least two years of experience perform better than those newly employed, but we find no differences between caseworkers with 2–4 years of experience and caseworkers with longer experience. It means that it takes some time for caseworkers to master the profession, but this is only relevant during the first years. Several other caseworker characteristics, such as level and field of education and cognitive and non-cognitive ability, are not related to caseworker performance, at least not in terms of their job seekers' employment outcomes.

We also examine the relationship between the actions caseworkers take and their performance. Following the previous literature we initially distinguish between caseworkers that focus on "supportive" and "restrictive" policy measures. We also define caseworkers as being "active" if they frequently meet with their job seekers. We find no evidence of any impact of having a supportive caseworker, while having a restrictive and/or active caseworker increases reemployment rates. Our results show that caseworker's actions and strategies are important, and that there are more relevant dimensions than the ones commonly discussed in the literature ("carrots" and "sticks"). It also have implications for policy since caseworkers actions and strategies could potentially be affected by policy.

We show that the matching of caseworkers to job seekers matters. In particular, job seekers who are paired with a caseworker with work experience from the same industry as the job seeker find jobs faster. If the caseworker and the job seeker have similar educational background this also have positive employment effects. Two possible explanations for these findings are that similar labor market experiences help the caseworker to understand the relevant job-market opportunities and that it allows caseworkers to better use their networks. We also see that job seekers do better when assigned a caseworker with the same gender. This is especially the case for female job seekers who finds jobs substantially faster if they are assigned to a female caseworker. Taken together, it means that employment policies will be more efficient if caseworkers and job seekers are matched in a optimal way.

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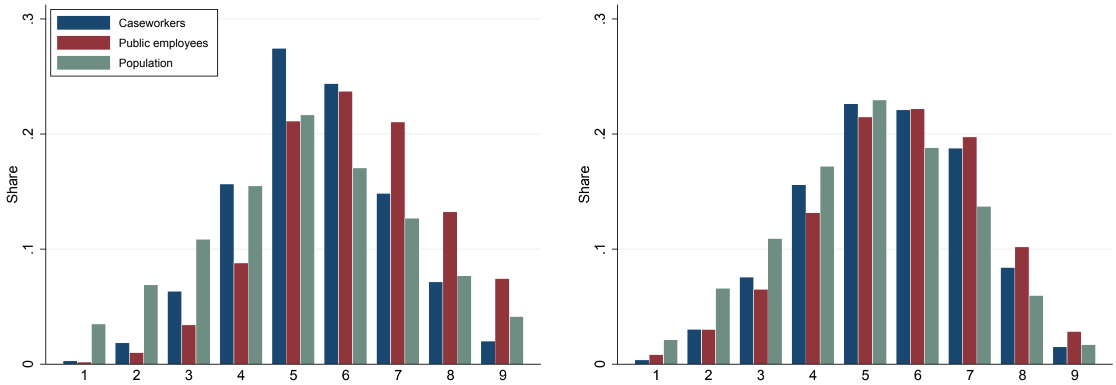
Figures and Tables

Figure 1: Ability scores for caseworkers, other public employees, and the population

(a) Cognitive score

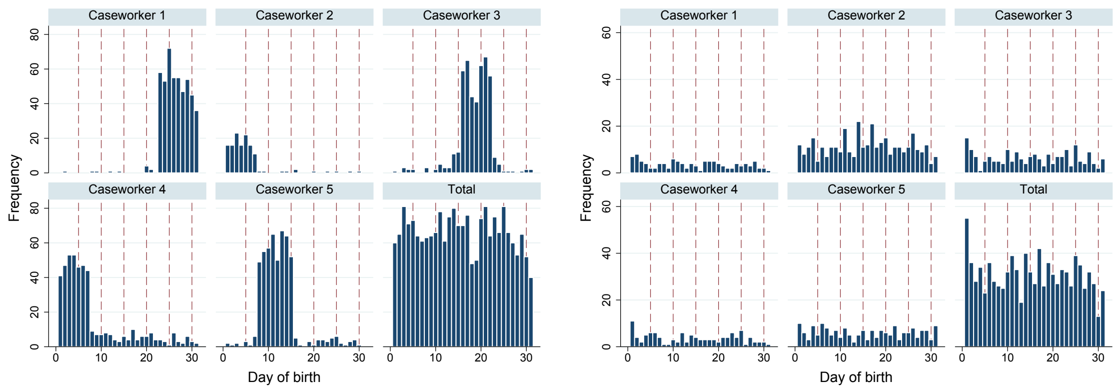
(b) Non-cognitive score



Notes: Statistics for 2005 of the distributions of cognitive and non-cognitive scores from military enlistment. Public employees includes caseworkers at similar public sector authorities (National social insurance board and National tax audit office), and the full population is everyone in ages 20–65.

Figure 2: Allocation of job seekers to caseworkers over day of birth (1–31), at two local offices

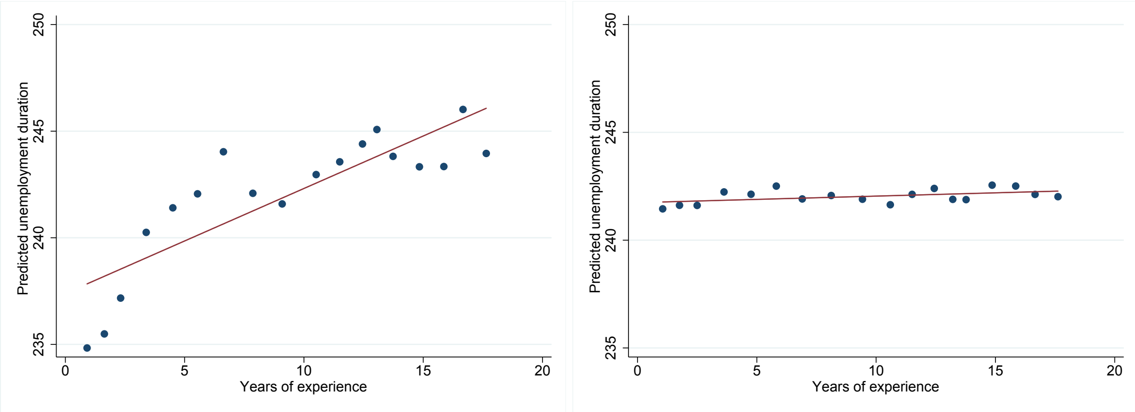
(a) Office #1: date-of-birth-rule (b) Office #2: no date-of-birth-rule



Notes: Number of job seekers born on each day-in-month per caseworker.

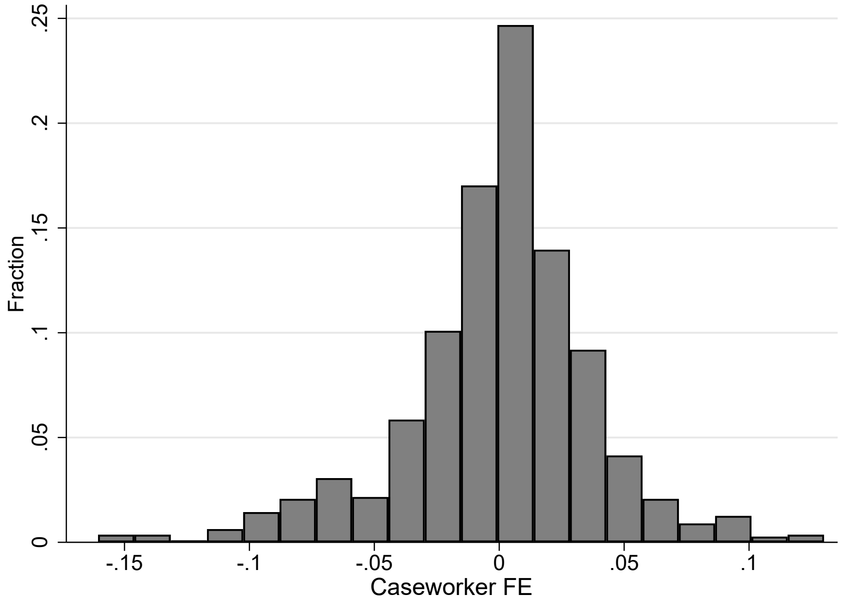
Figure 3: Predicted unemployment duration versus actual (a) and predicted (b) caseworker experience

(a) Actual caseworker (b) Predicted caseworker



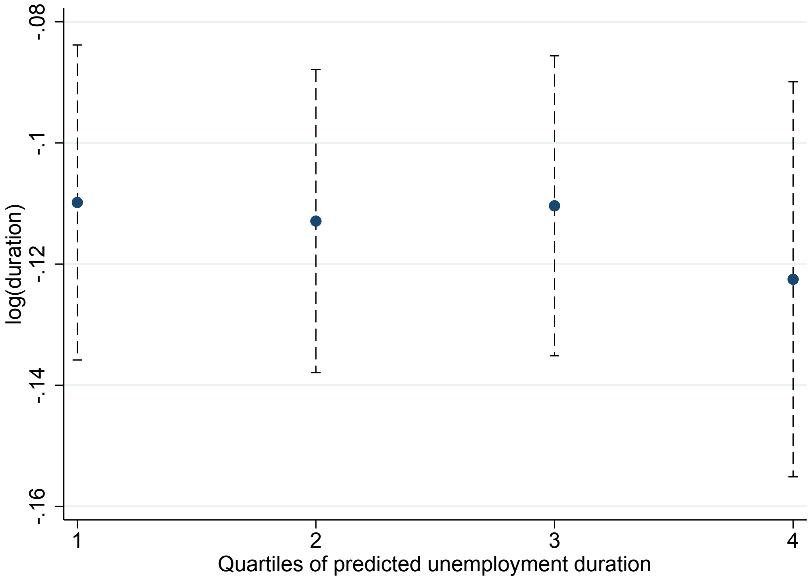
Notes: The figure plots job seekers' predicted unemployment duration and years of experience of a) actual caseworker and b) predicted caseworker. The sample include the inflow of job seekers 2003–2010. Each point are averages in bins of equal size with fitted linear regression lines. Predicted unemployment durations are generated by taking the fitted values from a regression of actual unemployment duration on duration of last unemployment spell, amount of welfare benefits last year, regional unemployment rate, age, age squared and dummies for UI eligibility, disability, immigrant, female and 6 levels of education, after adjusting for the interaction of office, year and above/below age 25 fixed effects.

Figure 4: Histogram for estimated caseworker fixed effects (log unemployment duration)



Notes: IV-estimates of caseworker fixed effects, adjusted using the procedure described in Appendix B. The sample is all offices with date-of-birth assignment as defined in section 4.

Figure 5: Caseworker value-added by quartiles of predicted unemployment duration



Notes: The figure shows IV-estimates of the effect on log unemployment duration of being assigned a caseworker above the median in the value added distribution by quartiles of job seekers predicted unemployment duration. The dashed lines are 95 percent confidence intervals where standard errors are clustered at the caseworker level. The model include interacted year fixed effects, office fixed effects, and a dummy for age being greater or equal to 25 and the sample is all offices with date-of-birth assignment as defined in section 4.

Table 1: Descriptive statistics

	Job seeker characteristics					
Panel A	All offices		Date of birth offices		Compliers	
	Mean	SD	Mean	SD	Mean	SD
Age	31.82	12.24	31.39	11.98	30.35	12.16
Female	0.47	0.50	0.48	0.50	0.47	0.50
Swedish	0.86	0.34	0.87	0.34	0.88	0.32
Married	0.24	0.43	0.23	0.42	0.21	0.41
Children	0.36	0.48	0.35	0.48	0.36	0.48
Earnings (t-1)	95758.99	120035.72	93169.92	115928.82	92850.59	116943.14
Disabled	0.04	0.19	0.04	0.19	0.02	0.15
Eligible UI	0.66	0.47	0.67	0.47	0.64	0.48
Days unemployed	281.61	488.04	280.13	486.08	243.20	429.12
Compulsory school	0.30	0.46	0.28	0.45	0.33	0.47
Upper secondary school	0.53	0.50	0.53	0.50	0.52	0.50
University degree	0.17	0.38	0.19	0.39	0.15	0.36
# observations (unique)	1,600,132		425,120		711,099	
# observations	2,217,863		587,523		985,836	

	Caseworker characteristics			
Panel B	All offices		Date of birth offices	
	Mean	SD	Mean	SD
Age	47.06	10.18	46.96	10.25
Female	0.62	0.49	0.62	0.49
Swedish	0.88	0.32	0.87	0.33
Experience				
0 − 2 years	0.12	0.32	0.13	0.33
2 − 4 years	0.10	0.30	0.10	0.30
4 − 6 years	0.09	0.29	0.09	0.29
6 − 8 years	0.08	0.27	0.08	0.27
8 − 10 years	0.08	0.26	0.08	0.28
10+ years	0.53	0.50	0.51	0.50
Recruited from unemployment	0.41	0.49	0.43	0.50
Experience private sector	0.07	0.25	0.06	0.24
Primary school	0.03	0.17	0.03	0.18
Upper secondary school	0.33	0.47	0.31	0.46
University degree	0.64	0.48	0.66	0.47
Business degree	0.32	0.47	0.32	0.47
Social degree	0.15	0.36	0.16	0.37
Cognitive ability	5.43	1.55	5.38	1.53
Non-cognitive ability	5.42	1.64	5.26	1.68
# clients	97.08	107.28	114.05	111.35
# observations (unique)	6,812		1,564	
# observations	22,962		5,175	

	Office characteristics			
Panel C	All offices		Date of birth offices	
	Mean	SD	Mean	SD
# caseworkers	12.61	10.87	16.32	11.38
# job-seekers	1217.94	1281.46	1853.38	1600.21
# observations (unique)	252		51	
# observations	1,821		317	

Notes: Sample statistics for job seekers, caseworkers and local offices in Sweden in 2003–2010. Earnings are in SEK. Date-of-birth offices are offices in column 3–4 are defined in section 4. Statistics for the complier population in Panel A is complied using the procedure in Abadie et al. (2002). Cognitive and non-cognitive ability scores are for a sample (60%) of male caseworkers for whom we have enlistment tests scores.

Table 2: First-stage regressions of actual caseworker characteristics on the date-of-birth-predicted caseworker characteristic

	Caseworker experience (1)	Caseworker univeristy education (2)
Instruments		
Predicted caseworker experience	0.324***	0.001*
	(0.007)	(0.001)
Predicted caseworker university education	0.014	0.344***
	(0.053)	(0.008)
Share correct predictions	.441	
Joint F-statistic	1,134	
F-statistic	1,145	1,077
# clusters	6,812	6,812
# observations	2,217,863	2,217,863

Notes: The sample consists of job seekers in Sweden 2003–2010. Actual caseworker characteristics have been regressed on predicted caseworker characteristics. For details on how predicted caseworker is defined, see section 4. All models include interacted year fixed effects, office fixed effects, and a dummy for age being less than 25. Joint F-statistic is from the joint test that all coefficients are equal to zero. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Table 3: Randomization tests: date-of-birth rules and allocation of job seekers to caseworkers

	Dependent variable: Predicted unemployment duration in days	
	Actual caseworker (1)	Predicted caseworker (2)
Caseworker experience	1.049***	0.070
	(0.160)	(0.053)
Caseworker university education	12.515***	0.486
	(1.689)	(0.562)
Mean outcome	242.03	242.03
F-statistic	32.970	1.002
p-value	0.0000	0.3672
# clusters	6,812	6,812
# observations	2,217,863	2,217,863

Notes: OLS regressions for job seekers' predicted unemployment duration on actual/predicted caseworker characteristics. The sample consists of job seekers in Sweden in 2003–2010. All models include interacted year fixed effects, office fixed effects, and a dummy for age being less than 25. F-statistic is for a joint test that all coefficients are equal to zero. Predicted unemployment durations are generated by taking the fitted values from a regression of actual unemployment duration on duration of last unemployment spell, amount of welfare benefits last year, regional unemployment rate, age, age squared and dummies for UI eligibility, disability, immigrant, female and 6 levels of education. Standard errors in parentheses clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Table 4: Caseworker demographics, caseworker education and job seeker outcomes

	Leave unemployment within		log(duration)
	90 days	180 days	
	(1)	(2)	(3)
Caseworker demographics			
Age	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)
Female	0.010***	0.011***	-0.031***
	(0.003)	(0.003)	(0.009)
Native	0.006	0.006	-0.009
	(0.005)	(0.005)	(0.015)
Caseworker level of education			
Upper secondary	-0.002	0.006	0.003
	(0.008)	(0.008)	(0.023)
University degree	-0.007	0.002	0.028
	(0.008)	(0.008)	(0.023)
Caseworker field of education			
Business degree	0.001	-0.000	0.001
	(0.003)	(0.004)	(0.010)
Social degree	0.005	-0.000	-0.009
	(0.005)	(0.005)	(0.014)
Mean outcome	0.423	0.634	4.769
First stage F-statistic	133	133	133
# clusters	6,812	6,812	6,812
# observations	2,217,863	2,217,863	2,217,863

Notes: IV estimates where each characteristic of the actual caseworker is instrumented with the corresponding characteristic of the predicted caseworker. All models include interacted year fixed effects, office fixed effects, and a dummy for age being less than 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Table 5: Caseworker experiences, labor market experience, abilities and job seeker outcomes

	Leave unemployment within		log(duration)
	90 days	180 days	
	(1)	(2)	(3)
Panel A: Caseworker experience			
2-4 years	0.009	0.015**	-0.028*
	(0.006)	(0.006)	(0.017)
4-6 years	0.010	0.013**	-0.028
	(0.007)	(0.007)	(0.019)
6-8 years	0.003	0.013*	-0.018
	(0.007)	(0.007)	(0.019)
8-10 years	0.005	0.010	-0.021
	(0.007)	(0.007)	(0.019)
10+ years	0.006	0.013**	-0.028*
	(0.005)	(0.005)	(0.015)
Mean outcome	0.423	0.634	4.769
First Stage F-statistic	341	341	341
# observations	2,217,863	2,217,863	2,217,863
Panel B: Caseworker labor market experience			
From registered unemp.	0.000	-0.002	0.007
	(0.003)	(0.003)	(0.009)
Experience from private sector	0.001	-0.002	-0.014
	(0.006)	(0.006)	(0.017)
Mean outcome	0.423	0.634	4.769
First Stage F-statistic	290	290	290
# observations	2,217,863	2,217,863	2,217,863
Panel C: Caseworker abilities			
Cognitive	-0.002	0.003	0.002
	(0.005)	(0.005)	(0.014)
Non-Cognitive	0.008	0.000	-0.010
	(0.005)	(0.005)	(0.014)
Mean outcome	0.444	0.656	4.694
First stage F-statistic	204	204	204
# observations	254,165	254,165	254,165

Notes: IV estimates where each characteristic of the actual caseworker is instrumented with the corresponding characteristic of the predicted caseworker. Tenure as caseworker at the PES in years. Wages based on staff records in SEK 1000. Own unemployment is an indicator for more than 30 days of unemployment in the last 10 years. Experience from manufacturing or retail is an indicator from working in these sectors in the last 10 years. Abilities on a scale from 1 to 9 standardized to have a mean of zero and a standard deviation of one. All models include interacted year fixed effects, office fixed effects, and a dummy for age being less than 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Table 6: Caseworker strategies on job seeker outcomes

	Leave unemployment within		log(duration)
	90 days	180 days	
	(1)	(2)	(3)
Supportive	0.004	0.007	-0.038
	(0.010)	(0.010)	(0.028)
Restrictive	0.031***	0.018*	-0.050*
	(0.011)	(0.011)	(0.030)
Active	0.026**	0.042***	-0.076**
	(0.012)	(0.011)	(0.032)
Mean outcome	0.423	0.635	4.766
First stage F-statistic	153	153	153
# clusters	7,002	7,002	7,002
# observations	2,278,293	2,278,293	2,278,293

Notes: IV estimates where each characteristic of the actual caseworker is instrumented with the corresponding characteristic of the predicted caseworker. All strategies are indicators for above median propensity to assign to training (supportive), assign to work practice (restrictive) and to have meeting with their job seekers (active). All models include interacted year fixed effects, office fixed effects, and a dummy for age being less than 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Table 7: Sensitivity check

	Main estimate (1)	Immigrant control (2)	Date-of-birth fixed effects (3)	Caseworker characteristics (4)	Date-of-birth offices (5)	Caseload control (6)
	Panel A: Caseworker demographics					
Female	0.011***	0.010***	0.010***	0.009***	0.008**	0.011***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
# observations	2,217,863	2,217,863	2,217,863	2,217,863	588,945	2,217,863
	Panel B: Caseworker strategy					
Active	0.042***	0.042***	0.042***	0.039***	0.028**	0.042***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)	(0.011)
# observations	2,278,293	2,278,293	2,278,293	2,217,863	602,850	2,278,293

Notes: Column 1 reproduced our baseline estimates. Column 2 controls for immigrant status and column 3 includes date-of-birth fixed effects. In column 4 we include all caseworker characteristics included in Table 4 and 5. Column 5 restricts the analysis to date-of-birth-offices (as defined in Section 4). Column 6 hold constant caseworker caseload. All models include interacted year fixed effects, office fixed effects, and a dummy for age being less than 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the office level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Table 8: Distribution of caseworker fixed effects

	Adjusted		Unadjusted
	Level	Standard deviations	Level
	(1)	(2)	(3)
Leave unemployment within			
90 days	0.026	0.052	0.172
180 days	0.030	0.063	0.159
Log unemployment duration	0.110	0.085	0.471

Notes: The table reports estimates of the effect of moving one standard deviation in the estimated caseworker fixed effects on job seeker outcomes. IV-estimates using indicators for the predicted caseworker as instruments for the actual caseworker. The sample is all offices with date-of-birth assignment as defined in section 4). Column 1–2 are based adjusted fixed effects (see Appendix B), and Column 3 on unadjusted fixed effect estimates. All models include interacted year fixed effects, office fixed effects, and a dummy for age being less than 25.

Table 9: Caseworker and job seeker similarity: demographics and ability

	(1) All	(2) Job seeker characteristic	(3)
Panel A : Gender similarity		Female job seeker	Male job seeker
Match effect	0.004**		
	(0.002)		
Female caseworker	0.011***	0.015***	0.006*
	(0.003)	(0.004)	(0.004)
Mean outcome	0.634	0.643	0.626
First stage F-statistic	1,269	1,269	1,269
# observations	2,217,863	1,040,284	1,177,579
Panel B : Immigrant similarity		Native job seeker	Foreign born job seeker
Match effect	-0.003		
	(0.004)		
Native caseworker	0.007	0.004	0.010
	(0.006)	(0.005)	(0.008)
Mean outcome	0.634	0.664	0.540
First stage F-statistic	370	370	370
# observations	2,217,863	1,674,099	543,764
Panel C : Ability similarity		High ability job seeker	Low ability job seeker
Match effect	0.001		
	(0.004)		
High ability caseworker	0.005	0.006	0.004
	(0.008)	(0.008)	(0.010)
Mean outcome	0.655	0.738	0.560
First stage F-statistic	418	418	418
# observations	280,104	148,745	131,359

Notes: IV estimates where each match-effect and main caseworker effects is instrumented with the corresponding variable for the predicted caseworker. High ability caseworker is above median caseworker cognitive ability, and high ability for the job seeker is above median predicted unemployment duration. All models include interacted year fixed effects, office fixed effects, and a dummy for age being less than 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

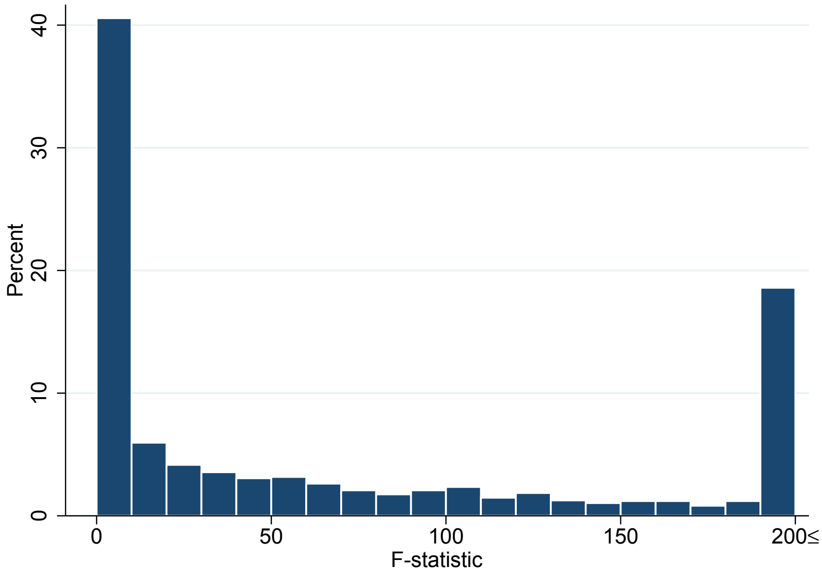
Table 10: Caseworker and job seeker similarity: experience and education

	(1) All	(2) Job seeker characteristic	(3)
Panel A: Experience from private sector		Private sector job seeker	Other sector job seeker
Match effect	0.008**		
	(0.004)		
Caseworker w. private sector	0.000	0.008	-0.008
	(0.006)	(0.007)	(0.007)
Mean outcome	0.653	0.648	0.657
First stage F-statistic	280	280	280
# observations	1,973,798	823,938	1,149,860
Panel B: University degree		University edu. job seeker	No university edu. job seeker
Match effect	0.005**		
	(0.003)		
University degree caseworker	0.000	0.005	-0.005
	(0.004)	(0.006)	(0.003)
Mean outcome	0.634	0.644	0.632
First stage F-statistic	1080	1080	1080
# observations	2,217,863	370,395	1,847,468

Notes: IV estimates where each match-effect and main caseworker effects is instrumented with the corresponding variable for the predicted caseworker. Caseworker has experience from the private sector if having ever worked manufacturing, construction, retail, hotel and restaurant within the last ten years. For job seekers experience from the private sector is based on the last job just prior to becoming unemployed. All models include interacted year fixed effects, office fixed effects, and a dummy for age being less than 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

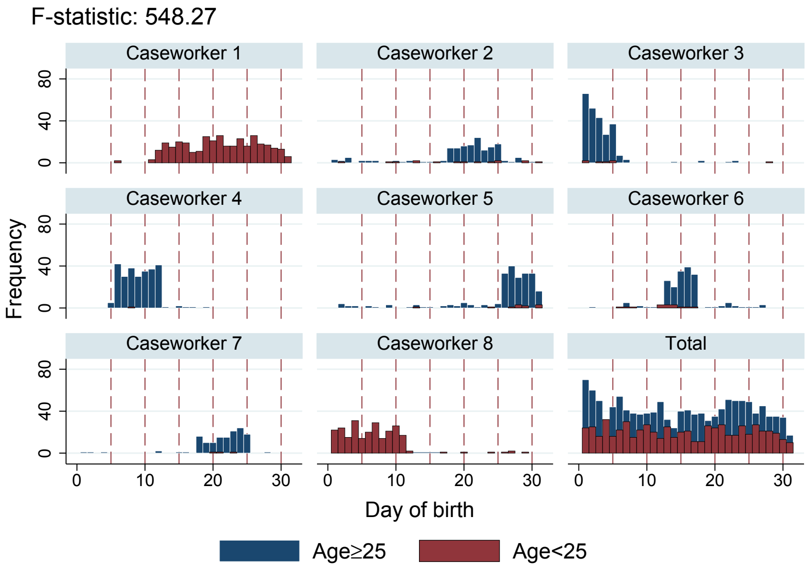
Appendix A: Additional Tables and Figures

Figure A-1: Prevalence of date-of-birth-rules



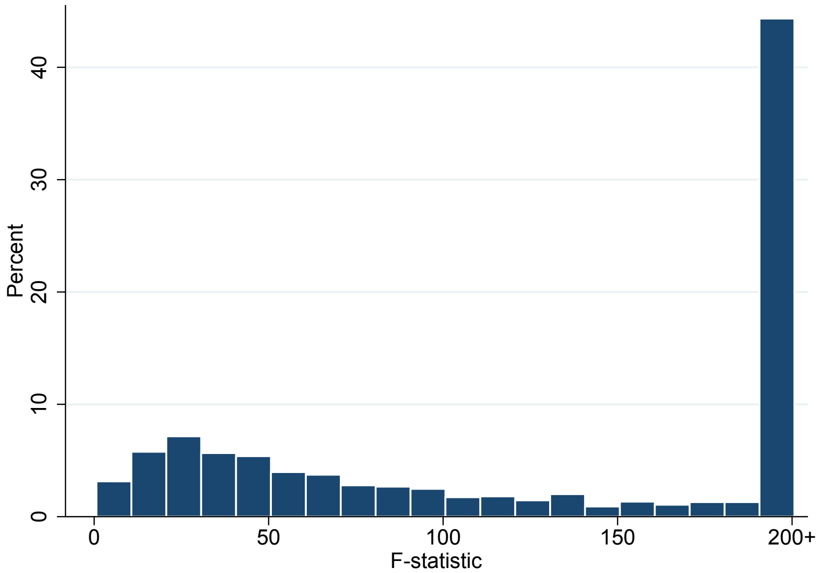
Note: The distribution of F-statistics from regressions of job seekers' date of birth (1–31) on caseworker dummies (within office and year). A low F-value indicates no date-of-birth-rule (an even distribution of dates of birth over caseworkers), and a high F-value indicates a date-of-birth-rule (an un-even distribution of date-of-birth over caseworkers).

Figure A-2: An example of an office with a separate date-of-birth-rule for youths



Notes: Number of job seekers (above/below 25 years of age) born on each day-in-month per caseworker at one office, in 2003.

Figure A-3: Strength of predicted caseworker instrument



Notes: The figure show separate first stage F-statistics where a dummy for the actual caseworker has been regressed on a set of dummies of predicted caseworker within and office and year.

Table A-1: First Stages : Caseworker demographics and caseworker education

	Dependent variable: Actual caseworker characteristic						
	Age	Female	Swedish	Secondary degree	University degree	Business degree	Social degree
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Predicted caseworker characteristic							
Age	0.403***	-0.000	-0.000	-0.000	0.000	-0.000	0.000
	(0.008)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female	-0.125	0.378***	0.006	0.002	-0.002	-0.000	-0.002
	(0.119)	(0.007)	(0.004)	(0.006)	(0.006)	(0.006)	(0.004)
Native	-0.031	0.020**	0.377***	-0.002	0.001	-0.008	-0.001
	(0.176)	(0.009)	(0.013)	(0.008)	(0.008)	(0.009)	(0.007)
Upper secondary	-0.300	0.001	-0.003	0.393***	0.023	0.031**	0.009
	(0.453)	(0.019)	(0.012)	(0.017)	(0.025)	(0.015)	(0.009)
University degree	-0.147	0.002	-0.000	0.007	0.408***	0.017	0.007
	(0.451)	(0.019)	(0.012)	(0.015)	(0.024)	(0.014)	(0.009)
Business degree	-0.062	0.001	-0.006	0.015**	-0.014*	0.384***	-0.001
	(0.128)	(0.007)	(0.004)	(0.007)	(0.007)	(0.008)	(0.003)
Social degree	0.137	-0.007	-0.004	0.011	-0.009	0.006	0.377***
	(0.190)	(0.009)	(0.006)	(0.007)	(0.008)	(0.007)	(0.012)
F-statistic	395	422	117	339	374	389	144
# offices	6812	6812	6812	6812	6812	6812	6812
# observations	2,217,867	2,217,867	2,217,867	2,217,867	2,217,867	2,217,867	2,217,867

Notes: The table shows first stage estimates, where we have regressed each actual caseworker characteristic (13 different regressions) on the full set of instruments, i.e. the 13 predicted caseworker characteristics. The most predictive instrument is the one corresponding to the actual caseworker characteristic (see diagonal). All models include interacted year fixed effects, office fixed effects, and a dummy for age being less than 25. Standard errors are clustered at the caseworker level and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Table A-2: Date-of-birth rules and random assignment of job seekers to caseworkers

	Independent variables:			
	Experience actual caseworker		Experience predicted caseworker	
	Coef. Est.	Std. Err.	Coef. Est.	Std. Err.
Demographics				
Male	0.000993***	(0.000351)	0.000098	(0.000134)
Disabled	0.001030***	(0.000181)	0.000033	(0.000054)
Native	0.000191	(0.000265)	-0.000032	(0.000089)
Age	0.176114***	(0.027857)	0.005892***	(0.001818)
Unemployment and earnings history				
Earnings (t-1)	866.902766***	(166.457698)	-10.263959	(30.832128)
Employed (t-1)	0.003549***	(0.000601)	-0.000056	(0.000115)
Welfare (t-1)	-0.000296	(0.000212)	0.000016	(0.000080)
Level of education				
Primary school < 9 years	0.000452***	(0.000081)	0.000026	(0.000039)
Compulsory school 9 years	-0.003704***	(0.000593)	-0.000158	(0.000097)
Upper secondary school 2 years	0.002109***	(0.000274)	0.000165	(0.000111)
Upper secondary school 3 years	0.000084	(0.000093)	-0.000073	(0.000045)
University < 3 years	0.001204***	(0.000419)	0.000052	(0.000092)
University $\geq$ 3 years	0.000014	(0.000021)	-0.000011	(0.000016)
# observations	2,217,863		2,217,863	

Notes: The table shows separate OLS estimates for each job seeker characteristic on years of experience of the actual (column 1) and the rules-predicted caseworker (column 2). All models include interacted year fixed effects, office fixed effects, and a dummy for age being less than 25. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Table A-3: Test of monotonicity assumption

Panel A	Quartile rank of predicted unemployment duration 1^{st}	2^{nd}	3^{rd}	4^{th}
Predicted caseworker experience	0.398***	0.354***	0.314***	0.254***
	(0.011)	(0.008)	(0.007)	(0.007)
# clusters	6,232	6,617	6,775	6,788
# observations	543,510	542,050	541,056	543,049
Panel B	Quartile rank of job seeker age 1^{st}	2^{nd}	3^{rd}	4^{th}
Predicted caseworker experience	0.416***	0.313***	0.292***	0.285***
	(0.012)	(0.007)	(0.008)	(0.007)
# clusters	6,082	6,735	6,541	6,504
# observations	584,445	531,837	573,013	528,564

Notes: First-stage estimates separately by quartiles of job seekers' predicted unemployment (panel A) and quartiles of job seekers' age (panel B). For details on how predicted caseworker is defined see section 4. All models include interacted year fixed effects, office fixed effects, and a dummy for age being less than 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Table A-4: Early performance and the likelihood to stay as caseworker

	Stay more than 2 years (1)	Stay more than 4 years (2)
Early VA leave unemployment within 180 days	-0.158 **	-0.106
	(0.073)	(0.070)
Early VA leave unemployment within 90 days	-0.093	-0.061
	(0.074)	(0.070)
Early VA log unemployment duration	0.104	0.069
	(0.074)	(0.070)
Mean outcome	0.52	0.33

Notes: Early VA are indicators for VA during years 1–2 of the career being above the median among all early career caseworkers. VA estimated using the method described in Section 6.1. Stay more than 2 or 4 years are indicators for continuing working as caseworker for more than 2 or 4 years, respectively. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Table A-5: Caseworker and job seeker similarity: demographics and ability on log(duration)

	All job seekers	Job seeker characteristic	
	(1)	(2)	(3)
Panel A : Gender similarity		Female job seeker	Male job seeker
Match effect	-0.009		
	(0.005)		
Female caseworker	-0.032***	-0.040***	-0.023**
	(0.009)	(0.011)	(0.010)
Mean outcome	4.769	4.730	4.803
First stage F-statistic	1269	1269	1269
# observations	2,217,863	1,040,284	1,177,579
Panel B : Immigrant similarity		Native job seeker	Foreign born job seeker
Match effect	0.012		
	(0.011)		
Native caseworker	-0.015	-0.003	-0.028
	(0.015)	(0.015)	(0.023)
Mean outcome	4.769	4.677	5.051
First stage F-statistic	370	370	370
# observations	2,217,863	1,674,099	543,764
Panel C : Ability similarity		High ability job seeker	Low ability job seeker
Match effect	-0.005		
	(0.012)		
High ability caseworker	-0.014	-0.019	-0.009
	(0.023)	(0.023)	(0.028)
Mean outcome	4.700	4.446	4.986
First stage F-statistic	418	418	418
# observations	280,104	148,745	131,359

Notes: IV estimates where each match-effect and main caseworker effects is instrumented with the corresponding variable for the predicted caseworker. High ability caseworker is above median caseworker cognitive ability, and high ability for the job seeker is above median predicted unemployment duration. All models include interacted year fixed effects, office fixed effects, and a dummy for age being less than 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Table A-6: Caseworker and job seeker similarity: experience and education on log(duration)

	All job seekers	Job seeker characteristic	
	(1)	(2)	(3)
Panel A: Experience from private sector		Private sector job seeker	Other sector job seeker
Match effect	-0.027***		
	(0.010)		
Caseworker w. private sector	-0.014	-0.041**	0.013
	(0.016)	(0.019)	(0.018)
Mean outcome	4.710	4.725	4.699
First stage F-statistic	280	280	280
# observations	1,973,798	823,938	1,149,860
Panel B : University degree		University edu. job seeker	No university edu. job seeker
Match effect	-0.016**		
	(0.008)		
University degree caseworker	0.013	-0.004	0.029***
	(0.011)	(0.016)	(0.009)
Mean outcome	4.769	4.726	4.778
First stage F-statistic	1080	1080	1080
# observations	2,217,863	370,395	1,847,468

Notes: IV estimates where each match-effect and main caseworker effects is instrumented with the corresponding variable for the predicted caseworker. Caseworker has experience from the private sector if having ever worked manufacturing, construction, retail, hotel and restaurant within the last ten years. For job seekers experience from the private sector is based on the last job just prior to becoming unemployed. All models include interacted year fixed effects, office fixed effects, and a dummy for age being less than 25. First-stage F-statistic is a joint test for all instruments. Standard errors in parentheses are clustered at the caseworker level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Appendix B: Estimation of caseworker value added

This appendix provides details for the estimation of caseworker value added outlined in Section 6.1. It is based on the empirical Bayes approach in Kane and Staiger (2008), which analyzes teacher value-added using student outcomes.

We first estimate the variance of the caseworker value-added, μ_j, from equation (2). In Kane and Staiger (2008), the covariance between the average student outcomes (residuals after covariate adjustment) for a teacher's class in year t and year $t-1$ is used as an estimate of the variance of the teacher component. This covariance calculation is weighted by the number of students for each teacher. It assumes that the student outcomes for each teacher are independent across years, so that the covariance across years captures the variance in "true" teacher value added. In our caseworker setting with an empirical strategy based on the date-of-birth-variation, these yearly average outcomes are obtained from IV-estimation of separate caseworker fixed effects for each year using equation (2). Denote these yearly caseworker fixed effect estimates by $\bar{v_{jt}}$. Then, following Kane and Staiger (2008) we obtain the variance of caseworker value-added, μ_j, from the covariance of $\bar{v_{jt}}$ for all t and $t-1$:

$$\hat{\sigma}^2_{\mu_j} = Cov(\bar{v_{jt}}, \bar{v_{jt-1}}). \tag{B-1}$$

Instead of weighting with the number of observations, the weighting is based on the precision (the inverse of the variance) of the year-by-year caseworker fixed effects. This gives the variance of the "true" caseworkers effects.

The second step of the Kane and Staiger (2008) procedure is to form a weighted average of the yearly average student outcomes, weighting each yearly average by its precision. We proceed in the same way and weight the yearly caseworker estimates, $\bar{v_{jt}}$, by the inverse of the variance of each estimate:

$$\bar{v_j} = \sum_t w_{jt}\bar{v_{jt}}, \tag{B-2}$$

where

$$w_{jt} = \frac{h_{jt}}{\sum_t h_{jt}} \tag{B-3}$$

$$h_{jt} = \frac{1}{Var(\bar{v_{jt}})}, \tag{B-4}$$

and $Var(\bar{v_{jt}})$ is the variance from the IV-estimation of the yearly caseworker effect.

The third step "shrinks" these estimated caseworker effects to obtain value-added estimates for each caseworker. Following Kane and Staiger (2008) we construct empirical Bayes estimates for each caseworker's true value added (VA_j) by multiplying the weighted caseworker estimates, $\bar{v_j}$, by an estimate of its reliability:

$$VA_j = \bar{v_j}\frac{\hat{\sigma}^2_{\mu_j}}{Var(\bar{v_j})}, \tag{B-5}$$

where

$$Var(\bar{v_j}) = \hat{\sigma}^2_{\mu_j} + (\sum_t h_{jt})^{-1}. \tag{B-6}$$

Specifically, $\frac{\hat{\sigma}^2_{\mu_j}}{Var(\bar{v_j})}$ is the shrinkage factor that reflects the reliability of $\bar{v_j}$ as an estimate of caseworker value-added, where the reliability depends on the variance of the "true" caseworker effects and the total variance of $\bar{v_j}$. Here, the total variance is the sum of the variance of each $\bar{v_j}$, i.e. $(\sum_t h_{jt})^{-1}$.