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Local labor demand and participation in social insurance programs

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ABSTRACT

Based on administrative data from Norway, we explore the "gray area" between the roles of unemployment- and temporary disability-insurances by examining how participation in these two program types is affected by local labor demand conditions. Local labor demand is identified by means of a shift-share instrumental variables strategy, where initial local industry-composition is interacted with subsequent national industry-specific employment fluctuations. Our results indicate that local labor demand has a large negative effect on the propensity to claim disability insurance, which, for some groups, is remarkably similar to its effect on the propensity to claim unemployment insurance. Based on this finding, we question whether it is meaningful to maintain a sharp distinction between these two programs.

1. Introduction

Social insurance programs are typically designed such that they distinguish sharply between unemployment and disability as the foundation for claims. Is this distinction meaningful? For the majority of shorter spells of unemployment or sickness, the answer is probably yes. Most unemployment insurance claims reflect labor market frictions – it simply takes some time for persons who have become unemployed to find a good job match. And most sickness insurance claims arise due to some short-term ailment with no consequences for future employment opportunities. However, as social insurance spells become longer, the ultimate causes behind the claims often become more ambiguous. A person may be unemployed because, e.g., a musculoskeletal disease or a light mental disorder makes it difficult to compete for jobs. And a person may be considered disabled because expected productivity is too low to ensure realistic job opportunities. Long-term social insurance claims may also result from a combination of several labor market barriers, and although a claimant is declared *either* unemployed or disabled, (s)he may in reality be unemployed with respect to one job, disabled with respect to another, and perhaps unwilling with respect to a third. Health problems may of course make it difficult to perform some kind of tasks, while being irrelevant for others.

Existing empirical evidence indicates a significant degree of substitution between unemployment- and disability-related social insurance program utilization (Black et al., 2002; Autor and Duggan, 2003; Rege et al., 2009; Bratsberg et al., 2013; Maestas et al., 2015, 2018; Charles et al., 2018), and points to a considerable remaining work capacity among marginal disability insurance claimants (Maestas et al., 2013; Kostøl and Mogstad, 2014; Borghans et al., 2014; French and Song, 2014). The probability of becoming a disability benefit claimant rises sharply in response to (exogenous) job loss. And although the positive relationship between layoff and disability risk to some extent reflects a genuine adverse health effect of job loss, the impacts identified in the empirical literature are simply too large to make this plausible as the *sole* explanation. In a recent US study, Maestas et al. (2018) estimate that 8.9% of all awards of Social Security Disability Insurance (SSDI) benefits during 2008–2012 was directly induced by the Great Recession. Based on Norwegian administrative data merged with records on mass layoffs identified from bankruptcy court proceedings, Bratsberg et al. (2013) estimate that men's risk of claiming permanent disability benefits over the next few years more than doubles in response to a job loss. And conditional on having been laid off, the probability of becoming a disability benefit claimant rises steeply with the local rate of unemployment.

Some of the effects identified in the literature are likely to be context-dependent. For example, as a result of individual job loss, it is probable that health problems that were tolerated within an existing employment relationship become a barrier in a search for new employment. As pointed out by Autor and Duggan (2003), job displacement can be viewed as a negative shock to the value of continued labor market participation. Empirical evidence from Norway also confirms that displacement leads to significant earnings losses (Huttunen et al., 2011). Hence,

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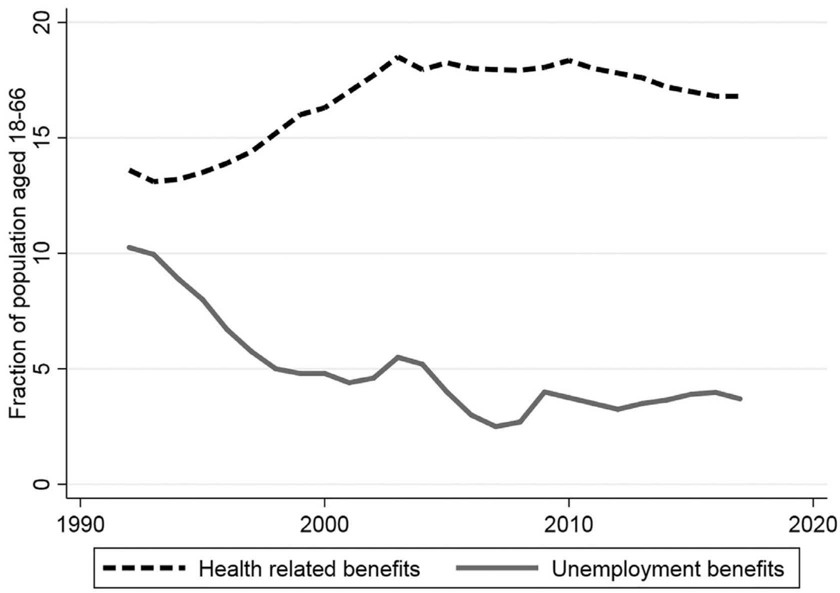

Fig. 1. Fraction of adult population with health-related and unemployment-related benefits by the end of each year 1992–2017. Source: Kann and Sutterud (2017, updated in 2018).

while an existing job is preferred over inactivity, it is possible that a disability benefit application is preferred over search for new employment.

In the present paper, we explore the gray area between unemployment and disability in more detail by examining how the participation in different types of social insurance programs and subsequent labor market outcomes are causally affected by local employment opportunities. Rather than focusing specifically on persons exposed to individual shocks, such as a job loss, we study the influence of labor demand on program participation propensities for all adult individuals. In addition, we examine how the sensitivity of program participation to local labor demand fluctuations varies with respect to initial labor market state.

Norway is a country with relatively large fractions on disability-related social insurance programs, but relatively few on unemployment-related programs. Over the past decades, there has also been a systematic shift in the caseload away from unemployment-programs toward disability-programs. These points are clearly illustrated in Fig. 1, which shows the fractions of the adult population in Norway claiming the two types of benefits year-by-year since 1992. Particularly during the 1990s, there was a considerable increase in disability-related social insurance claims accompanied by a decline in unemployment-related claims. And based on the most recent numbers, there are now more than four persons on disability insurance for each person on unemployment insurance.

Empirical evidence indicates that whether a given labor market problem is interpreted by the social insurer as a health problem or as an unemployment problem may have real consequences in terms of later labor market outcomes, as unemployment programs tend to be less generous and also much more activation-oriented than disability programs. For example, Schreiner (2019) shows that a local social insurance office's overall tendency to interpret youth problems as health-related rather than unemployment-related has a considerable negative impact on the youths' future labor market outcomes.

In the present paper, we use Norwegian administrative register data to empirically assess the influence of local labor demand conditions on unemployment- and disability-related social insurance claims, respectively. To do this, we divide the country into commuting zones, and examine how the caseloads of the two program types are associated with local labor demand conditions based on variation across commuting-zone-by-year cells. To represent a source of exogenous variation in local labor demand, we use a Bartik-type shift-share instrument that interacts the initial local structure of employment across industries at various points in time, with the subsequent national fluctuations in industry-specific employment. In relation to the existing literature, we make two novel contributions. The first is that we identify the influence of labor demand fluctuations for representative populations, without relying on large individual or aggregate economic shocks; hence our results should score high on external validity. The second is that we offer a direct comparison of the influence that labor demand exerts on the caseloads of disability- and unemployment-related social insurances. This gives us a natural scale against which the effects on disability insurance program participation can be measured.

Our findings confirm that there is indeed a considerable gray area between unemployment –and disability-related insurance claims. Although the impact of local labor demand conditions on the probability of claiming unemployment-related economic support is larger than the corresponding impact on the probability of claiming disability-related support, the latter is far from negligible, particularly when we take into account that transitions into disability insurance tend to be highly persistent. For example, considering the population of newly employed workers, we estimate that the fraction claiming an unemployment-related benefit three years later decreases with 0.83 percentage points for every demand-initiated percentage point increase in the overall local employment rate, while the fraction claiming a disability-related benefit decreases by 0.25 percentage points. Conversely, having already entered unemployment or disability insurance, the same one-percentage point increase in local labor demand is estimated to increase the fraction having returned to work after three years by 3.0 percentage points for unemployment entrants and by 1.9 percentage points for disability insurance entrants. Hence, the influence of labor demand is considerable for the caseloads of both programs.

2. Institutions

The Norwegian social insurance system makes a distinction between unemployment-related and health/disability-related needs for income support; see Table 1. Unemployed individuals may claim unemployment insurance (UI) if past earnings exceed a certain threshold, or means-tested social assistance (SA); in both cases conditional on active job search and willingness to accept any suitable job offer. If deemed to be in need of additional qualification or placement services for reasons other than a health problem, it is also possible to participate in active labor market programs (ALMP) or in a more comprehensive "qualification program" (QP) offering a fulltime activity with some income support. Unemployment insurance provides a replacement rate of 62.4% up to an earnings level corresponding to approximately 108% of average

Table 1
Income support programs targeted at unemployed job seekers and persons with disability or health problems in Norway.

	Main eligibility requirements	Replacement level	Maximum duration
Unemployment-related insurance programs			
Unemployment insurance (UI)	i) Labor earnings in the previous calendar year exceed 1.5 B (or 3 B over the past three years) ii) Willingness to search actively for new employment and accept suitable job offers	62.4% of previous labor earnings for previous annual earnings up to 6 B	2 years
Social assistance (SA)	i) Willingness to search actively for new employment and accept suitable job offers ii) No coverage by other social insurance programs	Coverage of basic needs only. Means-tested against household income and wealth.	No limit
Qualification program (QP)	i) Willingness to work or participate in a full-time program ii) Work capacity severely reduced by causes that do not qualify for disability insurance	2 B per year	2 years
Active labor market program (ALMP)	i) Willingness to work or participate in a full-time program ii) Participation in offered activity	Approximately 1 B per year (tax free), plus documented participation-related expenses	No specific limit
Health/disability-related insurance programs			
Temporary disability insurance (TDI)	i) Work capacity reduced by at least 50% due to health problems ii) Willingness to participate in programs aimed at rehabilitation	66% of previous earnings for previous earnings up to 6 B. Minimum benefit of 2 B.	3 years
Permanent disability insurance (PDI)	i) Work capacity reduced by at least 50% due to health problems ii) No realistic prospects for return to work	66% of previous earnings for previous earnings up to 6 B. Minimum benefit of 2 B.	No limit

Note: B is shortcut for the “Basic amount”, which is an important monetary parameter in the Norwegian social insurance system. In 2019 it is approximately NOK 100,000, which is around 18% of average full-time-full-year earnings in Norway. The table describes the current (2019) system. Some of the parameters have been subjected to changes during the period covered by this paper.

full-time-full-year earnings in Norway; see Table 1. UI benefits are conditioned on the unemployment spell being involuntary, however, and if a UI applicant quit a previous job voluntarily or was fired for cause, there is a 12-week embargo period on UI entitlements.

Persons who are in need of income support due to disability or other health problems may claim temporary or permanent disability insurance (DI). For employees, there is first a one-year entitlement to sick-pay (with 100% replacement), and during this period it is also illegal to fire the worker with reference to the sickness (employment protection regulations apply). After one year of absence, it is allowed to fire a worker who is unable to return to regular work due to sickness. It is then possible for the worker to apply for temporary or permanent disability benefits, with a typical replacement ratio around 66%. Persons who are not employed can apply for disability insurance directly, and there is no requirement of previous employment either. For persons without previous work experience, the benefit level is set to a fixed minimum level; see Table 1. For all applicants, the precondition is that the work capacity is reduced by at least 50% as a direct consequence of disability/impairment. This must be certified by an authorized physician, but the final decision is made by the social security administration (SSA). In most cases, DI claimants will first be enrolled into the temporary disability insurance program (TDI), which (currently) has a maximum duration of three years. During this period, various rehabilitation measures will be considered and possibly tried out. When TDI benefits are exhausted, many claimants move on to the permanent disability insurance (PDI) program, from which there is almost no prospects for returning fully to the labor market. For a more thorough description of the Norwegian DI system, see Fevang et al. (2017).

At the face of it, these insurances thus cover income losses caused by very different circumstances. However, with respect to the DI eligibility assessment of whether or not the work capacity is reduced by at least 50% due to health problems, the legislation allows the SSA to take the applicant's current realistic work opportunities into account. This represents a possible channel whereby labor demand conditions may influence the assessment of disability insurance eligibility. Schreiner (2019) presents evidence that there is considerable room for caseworker judgement, and that screening practices vary considerable across time and space.

It is notable that while eligibility to unemployment insurance is conditional on (and proportional to) previous labor earnings, disability insurance can be claimed even without previous work experience. For disability claimants, there is also a minimum benefit level, currently corresponding to approximately 36% of average full-time-full-year earnings in Norway. Given the apparent scope for physician and caseworker judgement regarding the assessment of the reduced work capacity, it appears plausible that the assignment of individuals to the different programs to some extent is influenced by the degree of economic coverage they provide.

3. Data and descriptive evidence

Our empirical analysis is based on administrative registers covering all residents in Norway over the period from 1999 through 2016. The primary purpose of our analysis is to identify and estimate the causal influence of labor demand conditions on the probability of claiming unemployment-related and disability-related social insurance benefits. In order to do that, we need exogenous variation in labor demand conditions. Such variation clearly exists across local labor markets as well as over time. However, it is not generally observed. Natural candidates for representing labor demand fluctuations in an empirical model are the local employment or unemployment rates (or other measures of labor market tightness). However, these are determined through the intersection of demand and supply; hence, they cannot be used directly as explanatory variables in a model intended to isolate the influence of labor demand. Across space and time, there will be a sort of mechanic relationship between the rates of social insurance program participation and the rate of employment (as these states to some extent are mutually exclusive); but without additional information we cannot identify the direction of causality. For example, if we observe that a local labor market at some point in time has a particularly low employment rate and a high rate of disability program participation, we still don't know whether it is the low employment rate that causes the high disability rate or vice versa.

To deal with this simultaneity problem, we will use a shift-share strategy in which we interact national industry-specific employment fluctuations with some initial spatial differences in industry composition. Instruments of this type have been used frequently in the literature; see, e.g., Bartik (1991), Blanchard and Katz (1992), Bound and Holzer (2000), Autor and Duggan (2003), and Bartik (2015). They isolate the employment fluctuations following directly from the national expansion and contraction of particular industries, caused by, say, changes in technology, trade liberalization (or exchange rate fluctuations), public expenditures, or consumer demand. The central identifying assumption is then that the initial industry shares do not predict future outcomes through other channels than those reflected in the national fluctuations; see Goldsmith-Pinkham et al. (2019). To operationalize this empirical strategy in the context of our data, we need to structure the data in terms of base-years (used to define the initial industry composition) and outcome-years. As explained in more detail below, we will in the main part of the analysis do this by allowing a three-year time-period between base-years and outcome-years. In addition, we need to define local labor markets. Based on Bhuller (2009), we divide the country into 46 such local markets, or commuting zones. The overall variation in labor demand exploited in our analysis thus comes from 690 combinations of 15 different base-years and 46 different commuting zones. We also need to assign all employees in Norway to particular industries. Such information is available in administrative registers, based on a five-digit industry code. In total, there are 648 different industries in Norway. However, many of these are very small, and perhaps located only in a few commuting zones, hence, we aggregate industries such that all industry-categories have at least 5,000 employees on average at an annual basis. This is done by first including all five-digit codes with at least 5,000 employees, then doing the same for four-digit codes, and so on. As a result, we end up with 171 unique industries. Fig. A1 in Appendix A provides a compact description of the longitudinal and cross-sectional variation in employment across industries in Norway. With a prominent exception for the oil and gas industry, there has been considerable decline in the employment share of production industries over the 1999–2016 period. As the importance of these industries also vary a lot across commuting zones, this is an important exogenous source of variation in labor demand conditions. The largest growth in employment has come in the construction industries and in the service sectors related to health and education. For these industries, the variation in employment shares is much smaller, yet far from negligible.

To study the influence of labor demand on social insurance program participation, we combine information from several administrative registers to assign unique monthly labor market states to all adult (age 18–61) residents in Norway. Our analysis focuses on four states; i.e., employment, participation in a disability-related social insurance program, participation in an unemployment-related program, and education, respectively. In order to characterize each person's main economic activity, the states are defined as mutually exclusive. In cases where people apparently have belonged to multiple states within the same month, uniqueness is achieved applying a hierarchy, which ranks states after their presumed distance to the labor market. This implies that health-related benefit claims are prioritized over unemployment, which is again prioritized over education, and finally employment. This hierarchy has the additional advantage of prioritizing data sources where the monthly information is considered most reliable. The definition of labor market states is described in more detail in Appendix B.

We construct two types of datasets. The first contains the complete stock of individuals, with a fine-grained record of labor market states by January each year. These data are again divided into different groups depending on initial state. The second type of dataset contains, for each year, new entrants into either employment, unemployment-related income support, or temporary disability insurance. A new entrant to a particular state is defined as being in that state in a given month, while not having belonged to that state during the previous three months. By focusing on entrants, we direct attention directly to those whose subsequent labor market performance presumably is most sensitive with respect to labor demand conditions.

The structure of the datasets is illustrated in Table 2. In total, there are almost 38 million observations divided between 3.6 million individuals, and 66% of these observations start out with employment in the base-year; see Column I. Among them, 87% are still in employment three years later, 2% have become unemployed, and 3% have become disabled; see Column IV. Employment is less stable for the newly employed (Column V), and their risk of becoming unemployed or disabled is also much higher.

Although the data contain information about individual outcomes, the variation in labor demand conditions comes from the 690 different combinations of base-years and commuting zones. Hence, most of the analysis can be done based on aggregates computed for each commuting-zone-by-year cell. Before we present our empirical model, we provide a descriptive picture of the relationship between fractions belonging to the three key states of employment, unemployment, and disability insurance in the respective base-years. For this purpose, we build on the data containing all adults described in Table 2, Column I. The upper two panels of Fig. 2 first show that there is a strong negative relationship between local employment rates and both unemployment- (panel (a)) and disability-related (panel (b)) insurance claims. This is not surprising, given that such claims by construction implies non-employment. The relationships are not entirely mechanic, though, as approximately 20% of the population does not belong to any of three states of unemployment, disability, and employment; see Table 2. The four lower panels illustrate that the cross-sectional variation (panels (c) and (d)) in all three rates are much larger than the longitudinal variation (panels (e) and (f)). They also indicate that while longitudinal variation in employment is most strongly (negatively) associated with participation in unemployment-related insurance programs, its cross-sectional variation is most strongly associated with participation in disability-related programs.

Panel (a) in Fig. 3 then focuses more directly on the relationship between the rates of unemployment-related and disability-related claims. At the face of it, there is a positive relationship between these two rates at the commuting-zone-by-year level; see the upwards-sloping stapled regression line. However, when we instead look at the relationships between the two caseloads among local areas with similar employment rates, a completely different pattern emerges. Then, there is a conspicuous negative relationship between the rates of unemployment and disability. Again, it is worth noting that this pattern is not purely mechanic. To illustrate this point, panels (b) and (c) in Fig. 3 show the corresponding relationships between the respective local fractions of social insurance program participation and the fraction belonging to "other" non-employment states, based on exactly the same grouping of employment rates as used in panel (a).[2] In these graphs, the systematic and tidy patterns displayed in panel (a) appear to be completely absent. Although this descriptive evidence is far from conclusive, it may point toward two suppositions; first, that unemployment and disability program participation are driven by some common determinant (e.g., cyclical fluctuations in the level of labor demand), and second, that, given the level of labor

[2] "Other" non-employment states include education, homemaking, periods spent outside the country, and inactivity.

Table 2
Data and descriptive statistics.

	The complete stock by January each year				Entrants to…		
	All I	Employed II	Unemployed III	Disabled IV	Employ-ment V	Unemployment VI	Temp. disability VII
N	37 939 858	25 554 728	1 280 145	3 791 759	4 469 054	1 919 349	688 913
Female	0.49	0.44	0.43	0.57	0.53	0.45	0.55
Age	38.2	40.6	35.5	44.2	29.5	33.6	39.7
Educational attainment							
Compulsory	0.36	0.30	0.60	0.66	0.33	0.50	0.54
High school	0.33	0.35	0.27	0.22	0.37	0.31	0.29
College	0.30	0.33	0.13	0.12	0.29	0.15	0.16
Labor earnings last year (B)	4.4	6.0	1.8	0.7	2.8	2.4	2.2
Immigrant low-income country	0.10	0.09	0.25	0.08	0.11	0.21	0.11
Immigrant high-income country	0.01	0.00	0.01	0.00	0.01	0.01	0.00
State in base-year							
Employed	0.66	1.00	0.00	0.00	1.00	0.00	0.00
Unemployed	0.04	0.00	1.00	0.00	0.00	1.00	0.00
Disability insurance	0.09	0.00	0.00	1.00	0.00	0.00	1.00
In education	0.15	0.00	0.00	0.00	0.00	0.00	0.00
State in outcome-year (three years later)							
Employed	0.69	0.87	0.48	0.10	0.70	0.59	0.28
Unemployed	0.03	0.02	0.23	0.02	0.05	0.16	0.04
Disability insurance	0.12	0.03	0.14	0.82	0.04	0.11	0.59
In education	0.10	0.05	0.06	0.01	0.15	0.07	0.02

Note: The entity of observation is person-year. All residents in Norway are included for each base-year 1999–2013, provided that they are between 18 and 58 years of age in the respective base-years (column I) and that they satisfy the initial state criteria indicated in the column heads (columns II-VII). In columns I-IV (the stock samples), base-year state is recorded in January each year. In columns V-VII (the entrant samples), base-year state is recorded in the month of entry. The state in the outcome-year is in both cases recorded exactly three years later. The unemployed and disability insurance states correspond to the categorization used in Table 1, such that unemployment comprises UI, SA, QP, and ALMP, and disability insurance comprises TDI and PDI.

demand, there is an important element of substitution between the two program types.

4. Empirical strategy

To establish more conclusive evidence regarding the “gray area” between unemployment and disability insurances, we now set up a more formal statistical model aimed at identifying and estimating the influence that labor demand actually has on the two caseloads. Using the seven different samples described in Table 2, we examine four different outcomes, all defined at the commuting-zone-by-year level: i) the fraction in employment, ii) the fraction in unemployment-related insurance, iii) the fraction in disability-related insurance, and iv) the fraction in education.

To describe our regression models, we need some notation. Let the subscript b indicate the base-year and let t indicate the outcome-year. In the stock sample, the base-year observations are defined in terms of the January records each year (1999–2013), whereas in the entrant samples it is defined in terms of records corresponding to the month of entry. The outcome-year observations are in the main part of our analysis measured exactly three years later (2002–2016). However, in Appendix D, we also present results for outcome-years measured from just one and up to seven years after the base-year. The subscript z indicates commuting zone, which always refers to the commuting zone occupied in the base-year.

Let y^s_{zt} be the fraction of the respective base-year population in commuting zone z that belongs to a state s in the outcome year t. Abstracting from the obvious problem that local labor demand is intrinsically unobserved, we would have liked to regress each outcome on the level of labor demand, while controlling for initial conditions and the composition of individuals under study; i.e.:

$$y^s_{zt} = \mathbf{y}'_{\mathbf{zb}}\boldsymbol{\pi} + \mathbf{x}'_{\mathbf{zb}}\boldsymbol{\gamma} + \beta LD_{zt} + \varepsilon_{zt}, \tag{1}$$

where LD_{zt} is a measure of local labor demand in commuting zone z in the outcome-year t, $\mathbf{y_{zb}}$ is a vector containing the state-specific population shares measured in the base-year, including the base-year value of the dependent variable (y^s_{zb}), and $\mathbf{x_{zb}}$ is a vector of average individual characteristics within the commuting zone's base-year sample (gender, age, education, immigrant status, and earnings; all measured in (or prior to) the base-year).[3]

Our primary interest lies in the impact of local labor demand; i.e., LD_{zt}. However, as pointed out above, labor demand is unobserved. A natural proxy for labor demand is the overall employment rate in the commuting zone at the time of outcome measurement. However, in order to isolate the exogenous fluctuations due to variations in labor demand, we need a valid instrument; i.e., we need a variable that affects the local employment rate through a channel of labor demand, but otherwise satisfies an exclusion restriction with respect to Eq. (1). We use a Bartik instrument of the following kind

$$z_{zt} = \frac{\sum_{j=1}^{J} w_{zbj}(L_{tj} - L_{bj})}{N_{zb}}, \tag{2}$$

where w_{zbj} is commuting zone z's fraction of employees within industry j in base-year b, $(L_{tj} - L_{bj})$ is the total change in the number of employees in industry j from the base-year to the outcome-year in the whole

[3] To control appropriately for variations in initial conditions across commuting-zone-by-year cells, the vector $\mathbf{y_{zb}}$ contains a more fine-grained state space than the outcome variables; i.e., the fractions belonging to i) full-time employment, ii) part-time employment, iii) self-employment, iv) parental leave, v) sick-pay, vi) unemployment insurance or participation in active labor market program, vii) social assistance or qualification program), viii) temporary disability insurance, and ix) permanent disability insurance(see the Appendix B for a more detailed description of the state-space). The vector $\mathbf{x_{zb}}$ contains the following variables: i) the fraction of females, ii) the fraction with high-school (upper secondary) education, iii) the fraction with college/university education, iv) the fractions belonging to different 5-year age intervals, v) the fraction of immigrants from low-income countries, vi) the fraction of immigrants from high-income countries, and vii) average labor earnings in the year prior to the base-year.

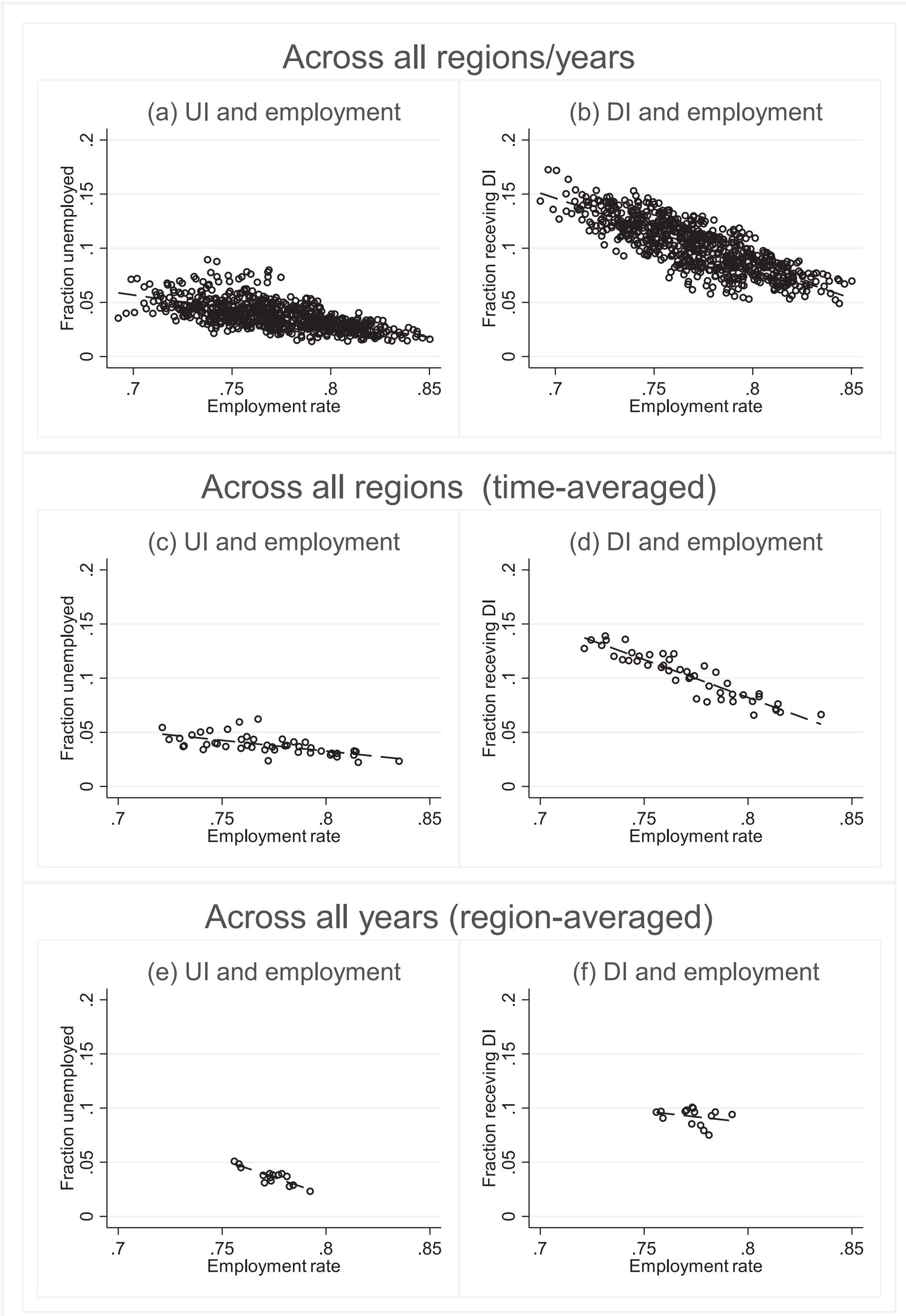

Fig. 2. Observed base-year employment rates, and rates of participation in unemployment-related and disability related insurance programs.
Note: Panels (a) and (b) contain 690 data-points showing the indicated rates across commuting-zones-by-year. Panels (c) and (d) contain 46 data-points showing the indicated year-averages across commuting zones. Panels (e) and (f) contain 15 data-points showing the indicating commuting-zone averages across baes-years (1999–2013). The dotted lines show (unweighted) linear regression lines through the respective data-points.

country, and $N_{zb}$is the size of the adult population in commuting zone z in the base-year.

The instrument$z_{zt}$is thus the predicted change in the local employment rate from the base-year to the outcome-year, based on the national changes in the industry-specific employment patterns only, and measured relative to the size of the base-year population. Taken at face value, the instrument in (2) also incorporates national changes in the overall employment rate, which may stem from fluctuations in labor supply as well as demand. Hence, to ensure that the identifying information provided by the instrument encompasses the idiosyncratic changes related to industry-composition only, we will control for outcome-year fixed effects. In addition, we control for commuting-zone fixed effects to ensure that any stable correlation between the initial industry structure and labor supply behavior across commuting zones is not picked up by the instrument. Finally, since the local employment rate instrumented by z_{zt}may deviate from the employment rate observed within the samples under study, we also control for the base-year value of the instrumented employment rate.

The baseline two-stage least squares (2SLS) models we estimate thus have the following form:

$$y_{zt} = \alpha_t + \delta_z + \mathbf{y}'_{\mathbf{zb}}\boldsymbol{\pi} + \lambda e_{zb} + \beta \hat{e}_{zt} + \mathbf{x}'_{\mathbf{zb}}\boldsymbol{\gamma} + \varepsilon_{zt}, \tag{3}$$

where$\alpha_t$are the year-fixed effects, δ_z are the commuting-zone-fixed effects and $e_{zb}$is the employment rate (age 25–60) in commuting zone z in

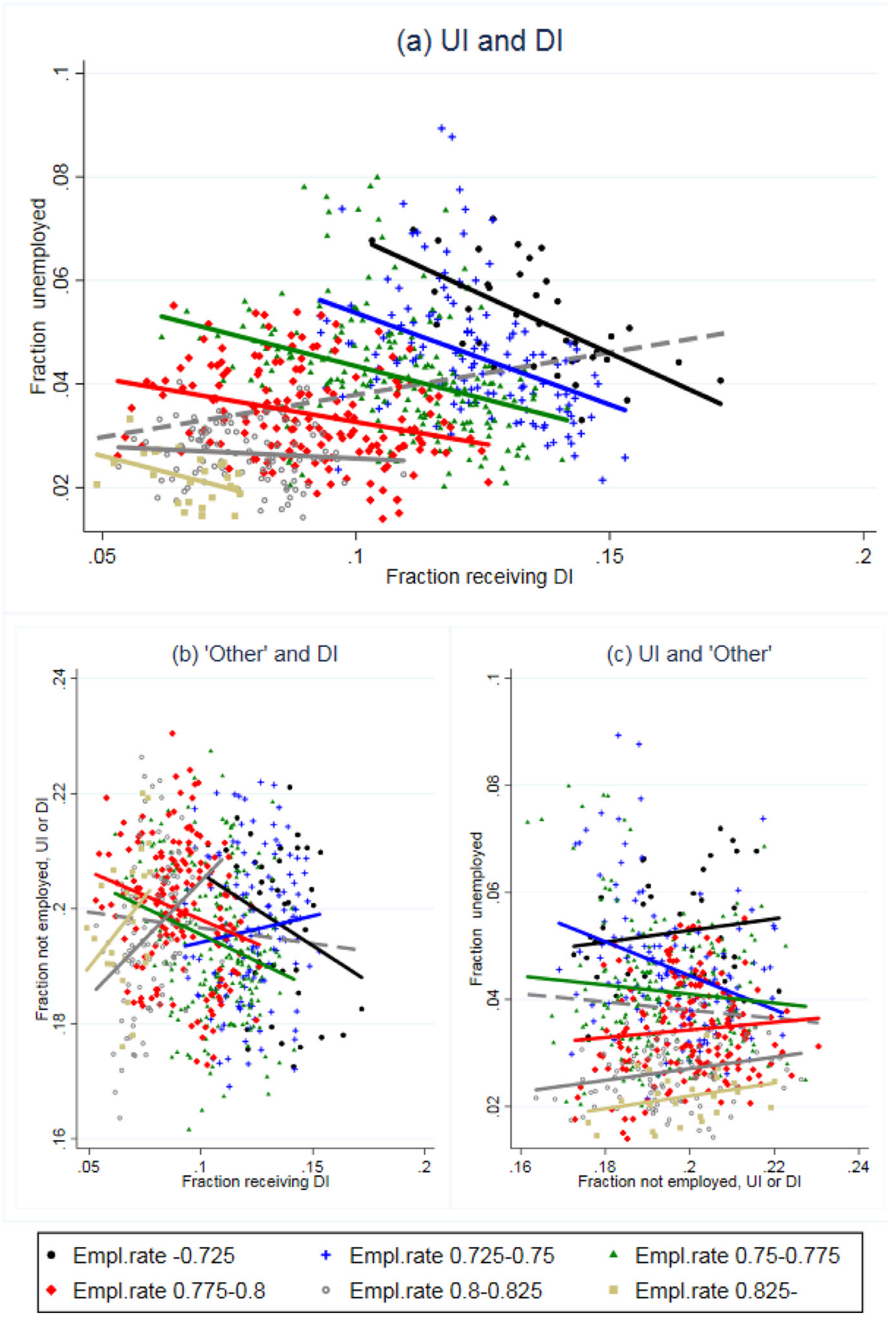

Fig. 3. Cross-plots of fractions belonging to different states in the base-year. Commuting-zone-by-year cells. Note: There are 690 data-points in each panel, and each data-point shows the indicated rates in a particular commuting zone in a particular year. The dashed regression lines show the linear regression through all points, whereas the solid lines show the (unweighted) linear regressions through points satisfying the employment conditions indicated in the legend.

the base-year.[4] and $\hat{e}_{zt}$is the corresponding predicted employment rate for the outcome-year based on the first stage equation

$$e_{zt} = \phi_t + \varphi_z + \mathbf{y}'_{\mathbf{zb}}\boldsymbol{\tau} + \theta e_{zb} + \mathbf{x}'_{\mathbf{zb}}\boldsymbol{\kappa} + \mu z_{zt} + \zeta_{zt}. \quad (4)$$

We estimate the model using the 690 commuting-zone-by-year observations, with weights reflecting the number of individual observations behind each data point.

While we build on this model in the presentation of results in the next section, we show in Section 6 and in Appendix E that the results are robust with respect to a number of alternative specifications. These include the use of alternative control variables (e.g., allowing for local linear time trends) and the use of a modified instrument where the influence of own commuting zone in national trends is removed (i.e., a "leave-out" Bartik instrument). They also include the use of individual data (instead of commuting-zone-by-year cells), which allows for the inclusion of individual-fixed effects. The robustness analysis also incorporates estimation of a different model, where the years used to compute initial industry weights are kept constant across different base-years, facilitating the inclusion of commuting-zone-by-weight-construction-year fixed effects. Finally, we present a "placebo" analysis where we use past instead of future outcomes as dependent variables in the baseline model.

5. Main results

A critical precondition for this empirical strategy to work is that there is a sufficiently strong first stage; i.e., that the national fluctuations in industry-composition really have a substantial impact on local employment patterns. Fig. 4, panel (a) first assesses this graphically, by plotting the realized change in local employment rate e_{zt}against its prediction

[4] Note that the base-year employment rate $e_{zb} = \frac{\sum_{j=1}^{J} w_{zbj} L_{bj}}{N_{zb}}$; conf. Eq. (2).

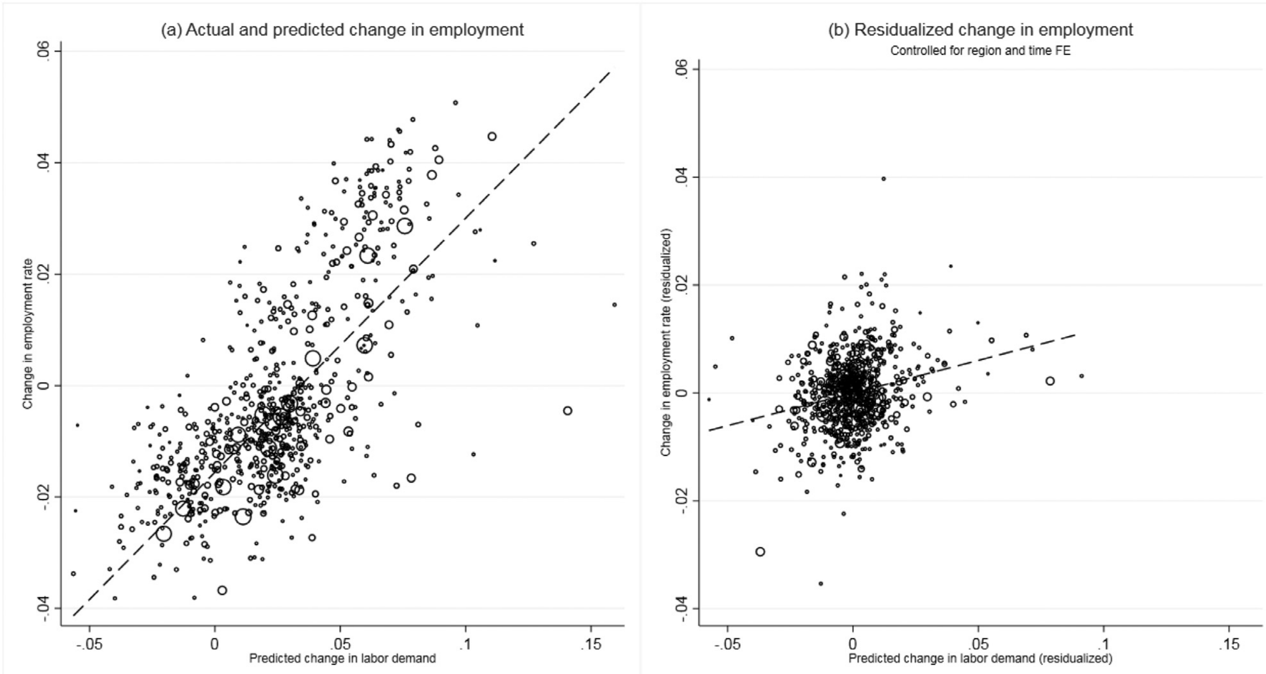

Fig. 4. Cross plots of predicted and observed change in local employment rates from the base-year to the outcome-year. Commuting-zone-by-year cells.
Note: Panel (a) shows predictions without any control variables, whereas panel (b) shows predictions with controls for region and year. Circle sizes are proportional to the number of observations behind each data-point.

Table 3
First stage estimates and F-tests.

	The complete stock by January each year				Entrants to…		
	All I	Employed II	Unem-ployed III	Disabled IV	Employment V	Unemployment VI	Temp. disability VII
Local employment rate	0.118***(0.032))	0.128***(0.037)	0.158***(0.041)	0.125***(0.038)	0.152***(0.041)	0.156***(0.041)	0.124***(0.035)
F-statistic excluded instrument	13.39	11.92	15.11	10.76	13.69	14.46	12.63
R sq. adj.	0.968	0.975	0.971	0.973	0.971	0.971	0.977
# Observations	690	690	690	690	690	690	690
# Person-years	37 939 858	25 554 728	1 280 145	3 791 759	4 469 054	1 919 349	688 913

Note: Each coefficient in this table is a result of a separate weighted first stage regression based on Eq. (4). Standard errors are clustered at commuting-zone. $^{*}/^{**}/^{***}$ indicates statistical significance at the 10/5/1 percent levels.

$\hat{e}_{zt}$. The relationship indeed appears strongly positive. However, as argued above, in the model we need to control for both year-fixed and commuting-zone fixed effects to ensure that we isolate the influences of labor demand; hence the variation actually exploited in the model is the variation remaining after having controlled for these factors. This is illustrated in Fig. 4, panel (b). The relationship then becomes considerably weaker, but still positive.

Table 3 presents the first stage estimation results from Eq. (4). They show that the instrument is sufficiently strong for valid statistical inference within all the samples described in Table 2. Having confirmed sufficient strength of the instrument, we now turn to the main results; see Table 4. For comparison, we present corresponding ordinary least squares (OLS) estimates in Appendix C. In most cases, the 2SLS estimates are a bit larger than the OLS estimates. There are two reasons why OLS and 2SLS estimates may differ. The first is directly related to the simultaneity problem discussed above, i.e. that the residual in Eq. (3) is correlated with the local employment rate. As a particularly high employment rate may indicate some favorable labor supply developments in the region, this is likely to exaggerate the influence of labor demand. The second reason is that the observed employment rate is an imperfect measure of labor demand, and thus subjected to measurement error. This will tend to bias the OLS estimates toward zero. In our case, it appears that the latter source of bias in most cases dominates the former.

Returning to the 2SLS estimates in Table 4, Column I first provides the results obtained for the full stock sample. As expected, the estimated effect on the employment rate in the full sample is approximately equal to 1. This particular result is almost tautological, as the population behind this estimate is almost the same as the population behind the first stage. However, the estimates regarding the states that the higher employment rate substitutes for are of more substantive interest. We note that a 1-percentage point demand-driven increase in the local employment rate reduces the local unemployment rate by 0.68 percentage points, and the rate of disability insurance program participation by 0.23 percentage points. The estimates also indicate a slight reduction in the probability of being in education, but this effect is not statistically significant.

It may also be of some interest to see how the effects reported in Column I vary across different demographic and educational groups. To shed light on this, we have estimated the model separately for 4 different age groups and for 12 different combinations of age and educational attainment. For ease of comparison, we present the results from this exercise graphically; see Fig. 5. It is clear that the effects of labor demand fluctuations are largest for the young, and among them, there is a ten-

Table 4
Second stage estimates: Effects of local labor demand on the fractions belonging to different states in outcome-year. By initial state.

	The complete stock by January each year				Entrants to…		
	All I	Employed II	Unemployed III	Disabled IV	Employment V	Unemployment VI	Temp. disability VII
Employment	1.057***	0.712***	3.519***	0.614**	1.659***	3.032***	1.911**
	(0.115)	(0.144)	(0.363)	(0.260)	(0.293)	(0.411)	(0.748)
Unemployment-related	−0.683***	−0.609***	−2.203***	0.056	−0.826***	−2.074***	−0.043
insurance	(0.096)	(0.090)	(0.544)	(0.089)	(0.152)	(0.482)	(0.212)
Disability insurance	−0.231***	−0.017	−0.858**	−1.028***	−0.248***	−0.853***	−2.127***
	(0.065)	(0.062)	(0.373)	(0.345)	(0.082)	(0.219)	(0.799)
Education	−0.147	−0.082	−0.045	0.038	−0.462	0.153	0.010
	(0.115)	(0.096)	(0.180)	(0.050)	(0.291)	(0.162)	(0.162)
# Observations	690	690	690	690	690	690	690
# Person-years	37 939 858	25 554 728	1 280 145	3 791 759	4 469 054	1 919 349	688 913

Note: Each coefficient in this table is a result of a separate weighted 2SLS regression based on Eqs. (3) and (4). Standard errors are clustered at commuting zone. */**/*** indicates statistical significance at the 10/5/1 percent levels.

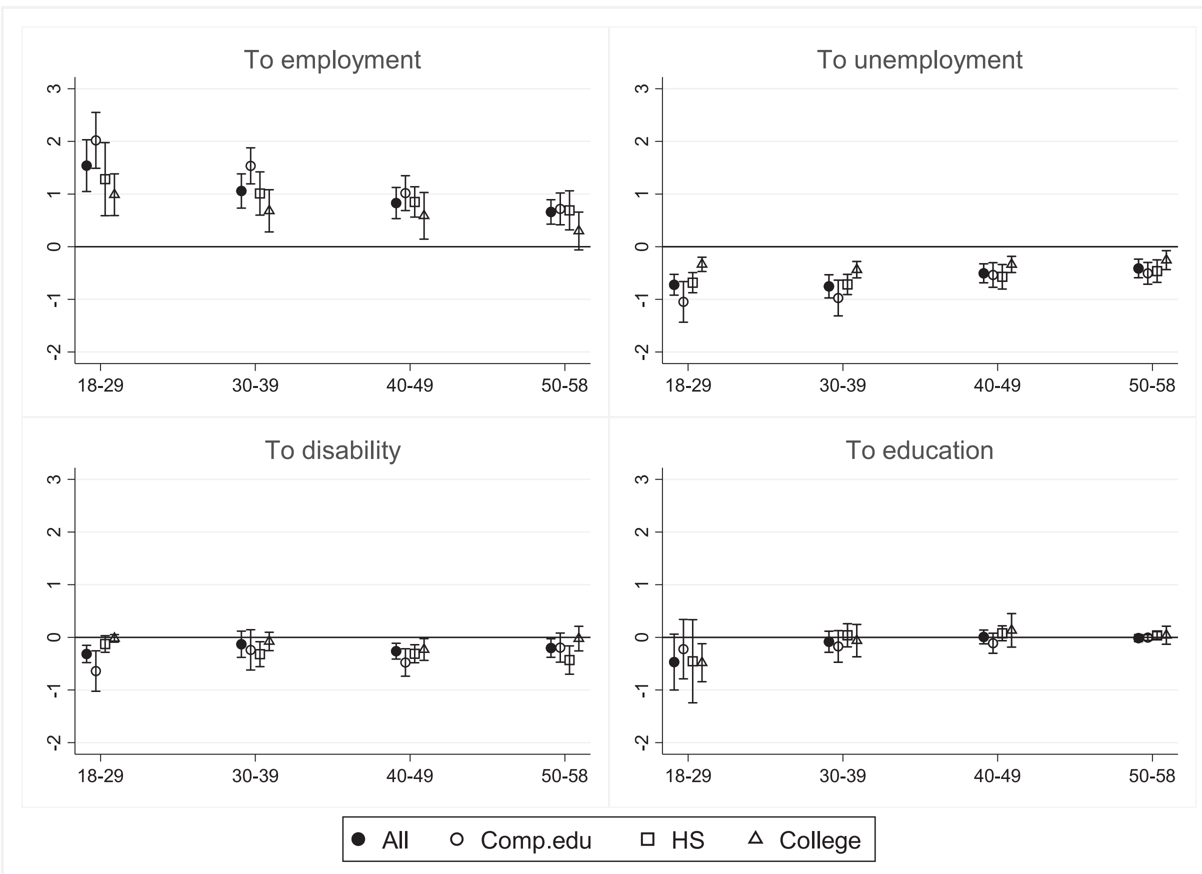

Fig. 5. Second stage estimates: Effects of local labor demand on the probability of belonging to different states in outcome-year. Complete stock sample by age and educational attainment.
Note: The age grouping is shown on the horizontal axis. The three educational groups indicated in the legend are defined as follows: Comp.edu: compulsory education only or incomplete high-school education; HS: high school (upper secondary) education; College: College/University degree. Each coefficient is a result of a separate weighted 2SLS regression based on Eqs. (3) and (4). Point estimates are shown with 95% confidence intervals.

dency that the effects are largest for those with least education. It is also for the uneducated young people that we see the strongest evidence that labor demand conditions influence disability program participation. Effects on continued education are almost exclusively concentrated among the young.

As it turns out, it is not primarily age or educational attainment *per se* that determines the size of labor demand effects, but rather the initial state, which is highly correlated with age and education. Moving on to the results that are conditioned on the initial state (Tables 3 and 4, columns II-IV), we note that the effects of labor demand are systematically larger for unemployed job seekers. For them, a 1 percentage point demand-driven increase in the local employment rate over the next three years is estimated to increases employment propensity by 3.5 percentage points (Column III). Most of this effect comes from reduced unemployment propensity (−2.2 percentage points). However, it is notable that the probability of having moved on to disability insurance is also reduced almost in line with the increase in labor demand (−0.86 percentage points). The much smaller effects estimated for those who al-

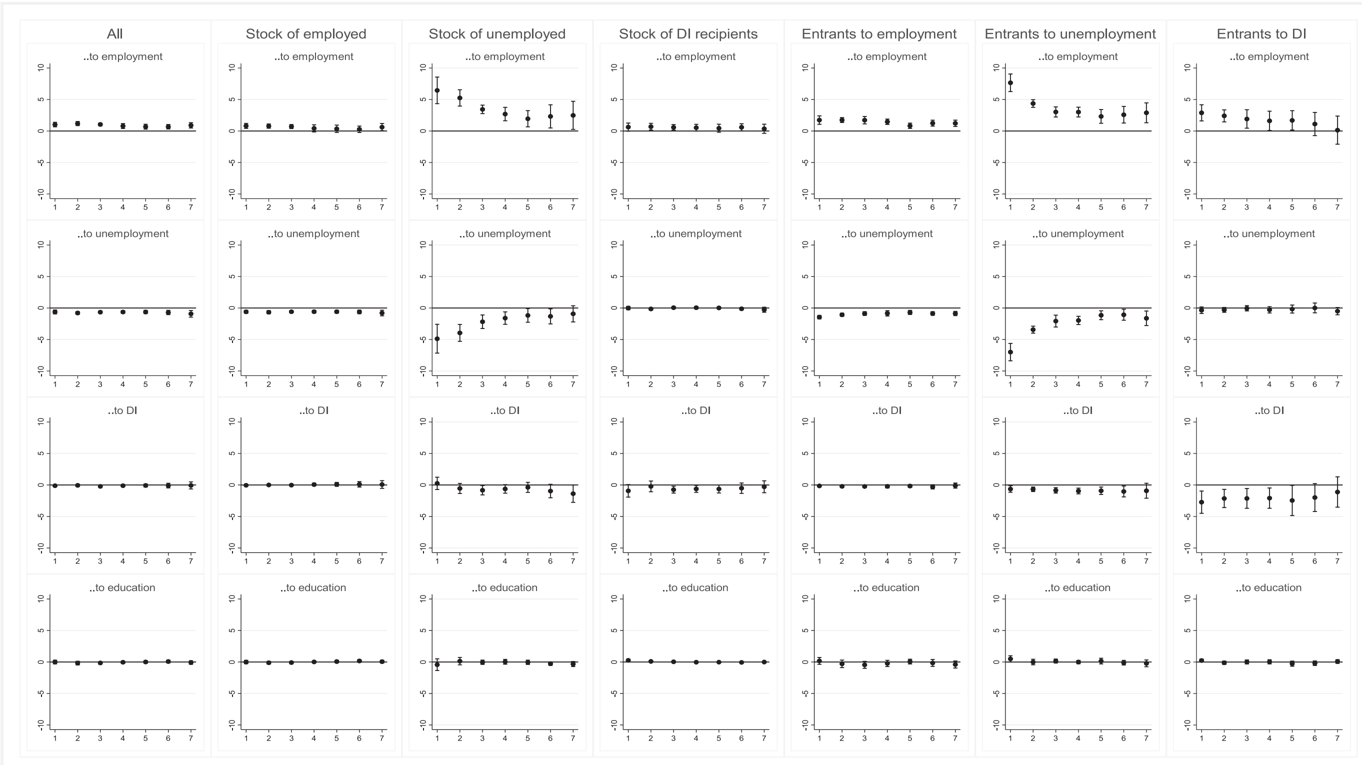

Fig. D1. Estimated effects of local labor demand on the probability of belonging to different states in outcome-year. By initial base-year state and outcome-year (from 1 to 7 years after the base-year)
Note: Each coefficient is a result of a separate weighted 2SLS regression based on Eqs. (3) and (4). Point estimates are shown with 95% confidence intervals.

ready belonged to a disability state in the base-year (Column IV) reflect that the majority of them actually belonged to the state of permanent disability insurance, which tends to be an absorbing state in Norway. Yet, it is notable that the probability of remaining in a disability insurance state after three years fluctuates approximately one for one with demand-driven variations in the employment rate.[5]

Columns V-VII presents the estimation results for the three entrant (flow) samples; i.e. the group of people that had just become either employed, unemployed, or a temporary disability claimant in the base-year. Within all these entrant groups, the probability of employment three years later is highly dependent on local labor demand conditions. It is notable that the labor demand sensitivity of new entrants to unemployment- and disability-related programs is much more similar than it is for the two stocks. While a 1 percentage point demand-driven increase in local labor demand is estimated to raise the probability of being employed three years later by 3 percentage points for the newly unemployed, it raises it by 2 percentage points for the newly disabled.

The choice of a three-year distance between the base-year and the outcome-year is a bit arbitrary. It represents a compromise between ensuring appropriate local industry weights (which requires a relatively short distance) and ensuring sufficient variation in labor demand conditions (which requires a relatively long distance). For the complete stock sample, the choice of distance between base-year and outcome-year should not have any impact on point estimates, as the base-year is non-informative with respect to the initial state. For the samples that are conditioned on a particular state, on the other hand, the choice of distance is potentially substantively important, as longer distance attenuates the influence of the initial state. In Appendix D, we present complete estimation results for alternative choices of the outcome year, from one to seven years after the base-year. As expected, the estimates are quite stable for the complete stock sample, as well as for the samples that are based on initial states that on average tend to be persistent (the stock samples of employed and disability insurance claimants). For the other samples, there is a tendency for the estimated labor demand effects to be largest the closer in time the outcome is measured relative to the (precarious) initial state. This is particularly evident for the two unemployment samples, whose members are known to be looking for jobs in the base-year.

6. Robustness

To examine the robustness of our estimation results, we present, in Appendix E, complete results for outcomes measured three years after the base-year, based on five alternative specifications of the 2SLS model in Eqs. (3) and (4). First, we examine the sensitivity with respect to the inclusion of the control variables contained in $\mathbf{x}_{zb}$ and $\mathbf{y}_{zb}$ (mean individual covariates and the distribution of initial states), by estimating the model without any of these controls. This is of particular interest in relation to the models that are conditioned on an initial state, as the composition of entrants to the various labor market states may depend on labor demand conditions. By excluding/including individual controls, we can assess the results' sensitivity with respect to this potential source of disturbance.

Second, we examine robustness with respect to the inclusion of regional trends in employment that are not driven by demand, but potentially correlated to initial industry weights. We do this by extending the baseline model to include commuting-zone-specific linear time trends.

Third, as we in the baseline model have included each commuting-zone's own employment in the national trends used to construct the

[5] When we estimate models separately for different age- and education groups *conditional on initial state*, the systematic relationship between the effects of labor demand and age/education illustrated in Fig. 5 disappears (not shown).

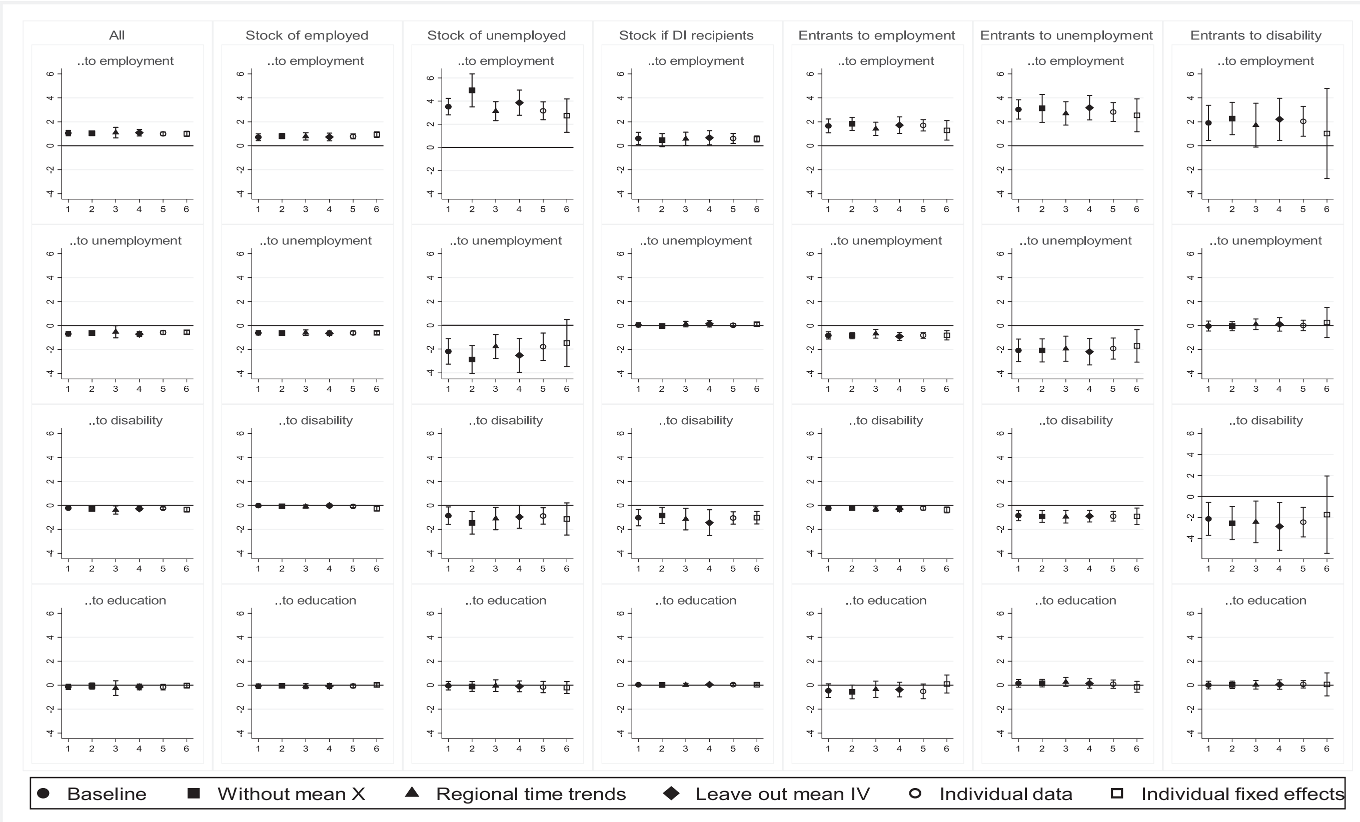

Fig. E1. Robustness analysis. Estimated second stage coefficients with 95% confidence intervals.
Note: The standard errors used to compute confidence intervals are clustered on region (in models with aggregate data) and on region *and* individuals (in the models with individual data.

Bartik instrument, it could be argued that the national trends are not completely exogenous. It is possible to deal with this problem by using of a “leave-out” Bartik instrument; i.e., an instrument where the national industry-specific employment trends are computed without including the focal commuting zone. However, the expansion of employment in one region may be causally related to contraction in another, e.g., because a large production unit has changed location. It is therefore not obvious which strategy provides the best foundation for causal analysis. We thus include a model built on a “leave-out” instrument in the robustness analysis. This leave-out instrument is constructed by substituting Eq. (2) with the following:

$$z^*_{zt} = \frac{\sum_{j=1}^{J} \frac{w_{zbj}}{1-w_{zbj}} \left(L_{tj,-z} - L_{tj,-z}\right)}{N_{zb}}, \tag{5}$$

where the –z subscript indicates that the variable does not include commuting zone z.

Fourth, as we have estimated the model based on aggregate data (commuting-zone-by-year cells), it could be argued that we have not exploited individual data efficiently. In a robustness exercise, we thus use individual observations, allowing for a more flexible use of individual controls and initial states. The outcome variables then take the form of 0–1 (dummy) variables indicating whether or not the person belonged to the state in question in the outcome year, and standard errors are computed with a two-way cluster (individuals and region). The vector $\mathbf{x}_{zb}$ is replaced by $\mathbf{x}_{zbi}$,which contains the individual covariates, and the $\mathbf{y}_{zb}$ is replaced by $\mathbf{y}_{zbi}$, which contains dummy variables indicating the initial state for each person.

Fifth, based on individual data, we estimate a model with person-fixed effects. While this is relatively straightforward in the stock-samples, where most persons are included with 15 observations (one for each base-year), it is a bit more challenging in the entry samples, as many individuals do not experience more than one entry into a particular state. This implies that models with individual-fixed effects are estimated with considerable uncertainty for these samples.

As can be seen from Fig. E1 in Appendix E, the main message coming out of these exercises is that the results are indeed robust with respect to model specification. Although some of the point estimates vary slightly from model to model, none of the main results discussed above would have been substantively changed had we relied on a different version of the model.

A potential concern related to all the models based on Eqs. (3) and (4) is that the time variation in local industry weights within commuting zones may induce a simultaneity problem into the model, as these weights may be correlated to the error term in Eq. (3); confer the discussion in Goldsmith-Pinkham et al. (2019). It is obviously not possible to include commuting-zone-by-base-year dummy variables, as this would exhaust all the identifying information in the data. The stability of the results with respect to the inclusion of local linear time trends is reassuring in this respect. However, it is also possible to deal with this concern more directly; i.e., by keeping local industry weights constant across different base-years, and then include dummy variables for each combination of commuting zone and year of weight construction. The results from such a model are reported in Appendix Table E1, and they confirm robustness of our main findings also with respect to this specification.

As a final check on empirical strategy, we report in Appendix E the results from a placebo version of our baseline model, where we have substituted outcomes observed three years *before* the base-year for the outcomes observed three years after. By construction, labor demand developments in the three-year period after the base year cannot have had

Table C1
Ordinary Least Squares (OLS) estimates: Effects of local labor demand on the fractions belonging to different states in outcome-year. By initial state.

	The complete stock by January each year				Entrants to…		
	All I	Employed II	Unemployed III	Disabled IV	Employment V	Unemployment VI	Temp. disability VII
Employment	0.801*** (0.045)	0.687*** (0.057)	2.116*** (0.204)	0.565** (0.083)	1.199*** (0.111)	2.214*** (0.217)	1.581** (0.201)
Unemployment-related insurance	−0.471*** (0.073)	−0.433*** (0.072)	−1.223*** (0.253)	0.011 (0.041)	−0.633*** (0.080)	−1.409*** (0.261)	−0.001 (0.098)
Disability insurance	−0.277*** (0.057)	−0.177 (0.042)	−0.693** (0.209)	−0.709*** (0.150)	−0.163*** (0.033)	−0.680*** (0.092)	−1.603*** (0.353)
Education	0.002 (0.042)	−0.036 (0.041)	0.040 (0.063)	0.049** (0.020)	−0.244** (0.106)	0.039 (0.066)	0.004 (0.061)
# Observations	690	690	690	690	690	690	690
# Person-years	37 939 858	25 554 728	1 280 145	3 791 759	4 469 054	1 919 349	688 913

Note: Each coefficient in this table is a result of a separate weighted OLS regression based on Equations (3), with the actual employment rate (age 25–60) in the commuting zone in the outcome year substituted for the predicted rate. Standard errors are clustered at commuting zone. */**/*** indicates statistical significance at the 10/5/1 percent levels.

Table E1
Estimated effects of local labor demand on the probability of belonging to different states in outcome-year. By initial state. Alternative model.

	The complete stock by January each year				Entrants to…		
	All I	Employed II	Un-employed III	Disabled IV	Employment V	Unem-ployment VI	Temp. disability VII
Employment	0.856***(0.090)	0.788***(0.098)	3.937***(0.924)	0.821**(0.210)	1.355***(0.475)	3.345***(0.640)	3.103**(0.969)
Unemployment-related insurance	−0.681***(0.082)	−0.736***(0.075)	−1.551**(0.622)	0.277**(0.128)	−0.758***(0.217)	−2.127***(0.431)	0.019(0.224)
Disability insurance	−0.352***(0.095)	−0.047(0.051)	−1.030**(0.521)	−1.523***(0.493)	−0.267***(0.101)	−0.790***(0.243)	−3.466**(1.368)
Education	−0.162(0.102)	0.068(0.079)	−0.432(0.270)	−0.029(0.082)(0.050)	−0.082(0.309)	0.122(0.233)	−0.022(0.236)
# Observations	2990	2990	2990	2990	2990	2990	2990

Note: Each coefficient in this table is a result of a separate 2SLS regression based on Eqs. (3) and (4). Standard errors are clustered on commuting-zone. */**/*** indicates statistical significance at the 10/5/1 percent levels.

Table E2
Placebo analysis: Estimated effects of local labor demand on the probability of belonging to different states three years before the base-year. By state in base-year.

	The complete stock by January each year				Entrants to…		
	All I	Employed II	Un-employed III	Disabled IV	Employment V	Unem-ployment VI	Temp. disability VII
Employment	0.069(0.244)	−0.172(0.183)	−0.204(0.602)	−0.069(0.407)	−0.432(0.328)	0.264(0.620)	0.496(0.980)
Unemployment-related insurance	0.049(0.126)	0.150(0.126)	0.336(0.467)	0.097(0.208)	0.123(0.258)	0.013(0.453)	0.085(0.424)
Disability insurance	−0.065(0.078)	−0.029(0.042)	−0.500**(0.247)	−0.124(0.465)	−0.015(0.128)	−0.283(0.241)	−0.391(0.549)
Education	−0.175(0.144)	−0.184(0.138)	0.280(0.231)	−0.093(0.084)	−0.449**(0.202)	0.042(0.240)	−0.343(0.307)
# Observations	690	690	690	690	690	690	690
# Person-years	37 939 858	25 554 728	1 280 145	3 791 759	4 469 054	1 919 349	688 913

Note: Each coefficient in this table is a result of a separate 2SLS regression based on Eqs. (3) and (4). Standard errors are clustered on commuting-zone. */**/*** indicates statistical significance at the 10/5/1 percent level.

a causal influence on labor market outcomes three years before, so we expect the estimated effects to be zero in this case. As shown in Table E2, the placebo analysis displays no pattern of systematic "effects". Two of the 28 estimated coefficients turn out to be statistically significant at the five percent level, but that is about what we can expect in the case of no systematic relationship.

7. Conclusion

The empirical evidence presented in this paper shows that there is a large gray area between social insurance programs targeted at unemployment-related and disability-related causes for insurance claims. Local labor demand conditions have a large and statistically significant influence on the caseload of temporary disability insurance programs, suggesting that temporary disability insurance in many cases is unemployment in disguise, in the sense that lack of realistic employment opportunities is the major cause behind the insurance claim. The effect of labor demand factors on the probability of entry into disability insurance is significant almost regardless of initial labor market state, and for new temporary disability entrants, the impact of labor demand conditions on the return-to-work probability is quite similar as it is for new unemployment insurance entrants.

Our findings indicate that there is a considerable element of judgement in relation to what kind of program a claimant is assigned to. As the two types of programs also entail different follow-up strategies, the choice of program is likely to have real consequences for future labor market outcomes. While the unemployment-related insurance programs typically contain activation requirements, in terms of monitored job search or active labor market program participation, the disability-related programs focus on pure income insurance and time for recovery.

If a claimant's primary problem is joblessness, the assignment to a disability insurance program may be counterproductive, even when there are severe health problems involved. There is an increasing stock of empirical evidence showing that work is actually a healthy activity for workers with the illnesses and symptoms responsible for the vast majority of disability cases in industrialized countries (musculoskeletal diseases, back pain, and light mental disorders); see, e.g., Waddell (2004), Waddell and Burton (2006), OECD (2008, Chapter 4), van der Noordt et al. (2014), and Joyce et al. (2016). Hence, there is not only a blurred distinction between the two program types in terms of the primary causes of entry, but also in terms of the appropriate treatment and follow-up strategy. It may thus be time for a reconsideration of social insurance institutions that make a sharp distinction between unemployment and disability.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.labeco.2019.101767.

Appendix A. Industry composition in Norway

Fig. A1

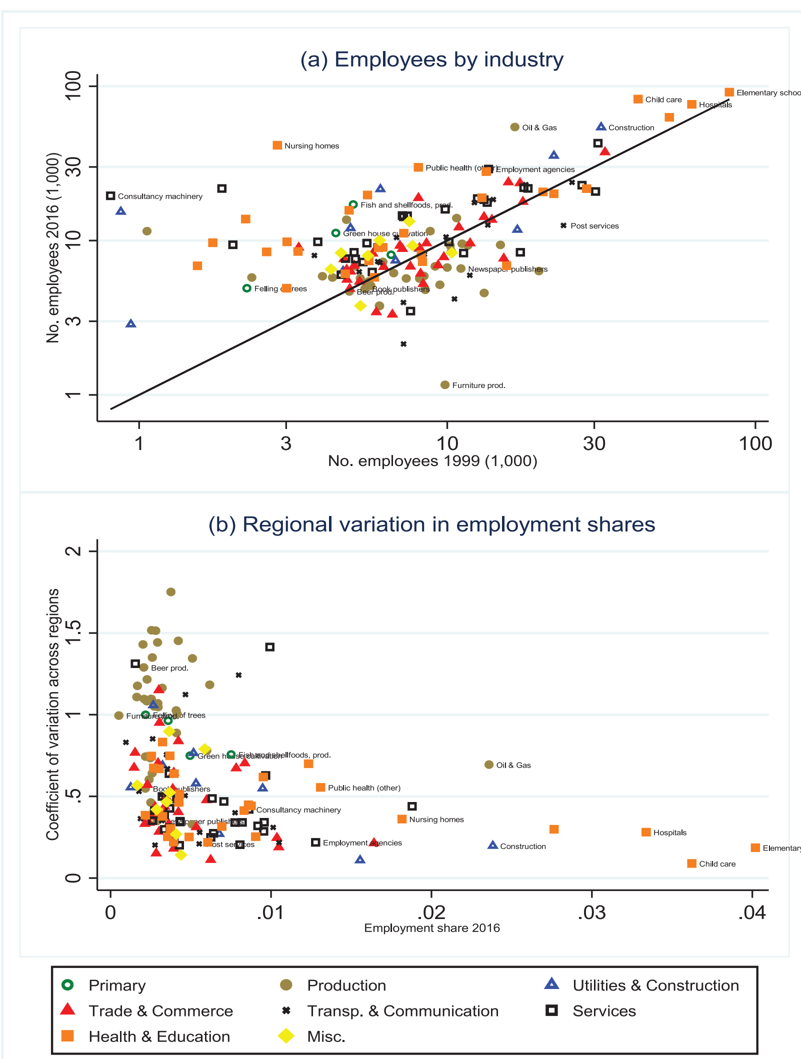

Fig. A1. Longitudinal and cross-sectional variation in industry-composition of employment in Norway

Note: Panel (a) shows a cross-plot of the number of employees in each industry in Norway in 1999 and 2016 on a log-scale. Panel (b) shows the same industries' employment shares in 2016 (on the horizontal axis) and the corresponding coefficients of variation (across the 46 commuting zones). The symbols are used to indicate which main category the different industries belong to. Some data points are equipped with labels to give some flavor of the kind of level/type of aggregation.

Appendix B. Definition of monthly labor market states

To determine monthly labor market states for the whole adult population in Norway, we combine several administrative registers, covering demographics, earnings, business income, social insurance transfers, and education. Since our focus is on labor market outcomes, we restrict the population to individuals aged 18–61.

The four main states applied in this paper are constructed on the basis of a much more detailed state-space, comprising as much as 20 different states. Since it is possible to belong to several states in a given month, we apply a hierarchical ranking. The hierarchy is shown below, with lower numbers always "overwriting" higher numbers:

Hierarchy:

1 Passed away
2 Disability benefits, full-time (*Uførepensjon*)
3 Disability benefits, part-time, employed (*Gradert uførepensjon*)
4 Disability benefits, part-time, unemployed (*Gradert uførepensjon*)
5 Work assessment allowance, temporary disability (*Arbeidsavklaringspenger, rehabiliteringspenger, attføringspenger*)
6 Sickness benefits (*Sykepenger*)
7 Parental benefits (*Fødselspenger*)
8 Unemployment benefits, full-time unemployed (*Dagpenger, heltidsledige*)

9 Unemployment benefits, part-time unemployed (*Dagpenger, deltidssysselsatte*)
10 Employment scheme benefits (*Tiltakspenger/Ventestønad/Individstønad*)
11 Social assistance (*Sosialhjelp*)
12 Qualification benefits (*Kvalifiseringsstønad*)
13 Enrolled in education
14 Outside country
15 Employed, full-time (> 30 h)
16 Employed, part-time 1 (20–29 h)
17 Employed, part-time 2 (4–19 h)
18 Self-employed
19 Transitional benefits for single parents (*Overgangsstønad*)
20 No registered activity

Based on this hierarchy, the four main states used in this paper are defined as follows:

i) Employment: States 6, 7, 15, 16, 17, 18.
ii) Disability-related social insurance: States 2, 3, 4, 5.
iii) Unemployment-related social insurance: States 8, 9, 10, 11, 12.
iv) Education: State 13.

Appendix C: Ordinary Least Squares estimates

Table C1 reports the OLS estimates from a regression of Eq. (3) with the actual employment rate (age 25–60) in the commuting zone in the outcome year substituted for the predicted rate. These coefficients are thus directly comparable with the 2SLS estimates reported in Table 4.

Appendix D: Alternative choices of outcome years

Fig. D1 provides estimated labor demand effects (Eq. (4)) by initial base-year state and outcome-year (from 1 to 7 years after the base-year). Given that our main results (Table 4) consists of 28 different estimates, we have chosen to present the results for alternative choices of outcome years graphically, by plotting the alternative point estimates (with 95% confidence intervals) for each combination of sample and dependent variable in separate panels.

Appendix E: Robustness

Fig. E1 presents estimates based on the five alternative model specifications discussed in Section 6 above. Due to the large number of distinct estimates (see Table 4), we present the results graphically, and compare the alternative results for each of the 28 coefficients in separate panels. For convenience, the baseline results from Table 4 are repeated to the left in each panel.

As explained in Section 6 above, a potential concern related to all the models based on Eqs. (3) and (4) is that the time variation in local industry weights within commuting zones may induce a simultaneity problem into the model, as these weights may be correlated to the error term in Eq. (3). In this appendix, we report estimates from a model where we keep industry weights constant across different base-years, and then include dummy variables for each combination of commuting zone and year of weight construction. This can only be done at the cost of making the instruments weaker, however, as the time distance between the construction of local weights and the predicted employment rates becomes larger. In order to use the data efficiently, it also implies that we have to reuse each observation several times, as the same outcomes can be examined with industry-weights constructed in different years. Let c be the year of local industry weight construction. We can then write the second stage equation as

$$y_{zct} = \alpha_{zc} + \varphi_{tc} + \mathbf{y}'_{\mathbf{zb}}\boldsymbol{\pi} + \lambda e_{zb} + \beta \hat{e}_{zt} + \mathbf{x}'_{\mathbf{zb}}\boldsymbol{\gamma} + \varepsilon_{zt}, \tag{6}$$

where $\{\alpha_{zc}, \varphi_{tc}\}$ are separate commuting zone and year dummy variables for each year of weight-construction. The first stage equation is modified accordingly. The results from this alternative model is presented in Table E1. A comparison with the baseline results provided in Table 4 reveals that our main results are robust also with respect to this alternative model.

Table E2 provides results from a placebo version of our model, where we have substituted outcomes observed three years *before* the base-year for the outcomes observed three years after. Although two of the 28 coefficients are statistically significant at the five percent level, we interpret these results as a confirmation of the absence of a systematic relationship.

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