SOCIAL INTERACTION EFFECTS IN DISABILITY PENSION PARTICIPATION: EVIDENCE FROM PLANT DOWNSIZING

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Abstract

We estimate the magnitude of social interaction effects in disability pension participation among older workers in Norway. The problem of omitted variable bias is addressed using neighbors' exposure to plant downsizing events as an instrument for the disability entry rate among one's previously employed neighbors. Our IV estimates suggest that a one percentage point increase in the participation rate of previously employed neighbors increased the subsequent 4-year entry rate of older workers by about one-half a percentage point. Numerous robustness and specification tests appear to support the validity of the identifying assumption in our IV strategy.

Keywords: disability, downsizing, layoffs, plant closings, social insurance, social interaction, welfare norms
JEL classification: H55, I12, I38, J63, J65

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1. Introduction

Understanding the determinants of disability program participation is an increasingly important issue for policy makers. Between 1980 and 1999, the share of non-elderly adults receiving disability benefits in the United States increased 60 percent to 4.7 percent.[1] Across the OECD as a whole, disability program participation rates increased 36 percent over the period, to 6.4 percent. The dramatic growth in disability program participation rates has important implications for national productivity and the public financing of disability benefit programs. In 1999, disability benefit payments comprised 1.4 percent of GDP in the U.S. and 2.5 percent of GDP across countries in the European Union.

Notably, the substantial growth in utilization of disability benefits has occurred without any change in the prevalence of self-reported disabilities (e.g. Burkhauser et al. 2001; Cutler and Richardson 1997; Duggan and Imberman 2006). This suggests an important role for non-health factors, and convincing evidence exists that economic conditions affect disability program participation. Black, Daniel and Sanders (2002) demonstrate that the coal boom and subsequent bust had a large impact on disability program participation in U.S. coal-producing states. Autor and Duggan (2003) find that decreasing demand for low-skilled workers and increases in their disability benefit replacement rate have led to large increases in the disability participation of high school dropouts. Autor and Duggan (2006) also cite the increasing real value of Medicaid benefits and liberalization of the screening process as contributing to increased utilization of disability benefits in the U.S.[2]

In this paper we empirically investigate the magnitude of social interaction effects in disability pension (DP) participation in Norway. [3] Specifically, we investigate how a worker's propensity to draw DP is affected by a plausibly exogenous shock to the disability entry rate of similarly-aged workers in the worker's neighborhood. A large and growing empirical literature suggests an important role for social interactions in many behavioral outcomes including teenage childbearing (Crane 1991), educational attainment (Sacerdote 2001; Hoxby 2000; Lalive and Cattaneo 2005), saving decisions (Duflo and Saez 2003), criminal activity (Case and Katz 1991;

[1] Statistics on disability program use and expenditures obtained from OECD (2003).
[2] See also Rupp and Stapleton (1995) and Stapleton et al. (1998) for related studies on the impact of economic climate on the application and receipt of disability benefits.
[3] Throughout this paper, we employ the colloquial expressions "on disability" and "disability participation" to refer to the utilization of disability pension benefits.

Glaeser, Sacerdote and Scheinkman 1996; Katz, Kling and Liebman 2001; Ludwig, Duncan and Hirschfield 2001; Kling, Ludwig and Katz 2005) and welfare participation among ethnic minorities (Bertrand, Luttmer and Mullainathan 2000; Aizer and Currie 2004). If social interaction effects exist in the context of disability insurance, it could help explain the wide variation in disability participation across geographic areas (McCoy et al. 1994) and over time. Moreover, the magnitude of such effects is critical for predicting the impact of policy reforms, demographic changes and economic shocks on disability participation rates.

In the context of disability participation, social interaction effects could potentially operate through a number of mechanisms. For example, social norms against disability participation could reduce the desirability of participating by imposing a utility cost in the form of social stigma (Moffitt 1983; Lindbeck, Nyberg and Weibull 1999).[4] The magnitude of this stigma is expected to decline as disability participation among one's peers increases, thereby reducing one's utility cost of entering disability. In this way social interaction effects give rise to a social multiplier that amplifies the effect of policy changes and economic shocks on aggregate participation rates (see e.g. Brock and Durlauf 2001; Glaeser and Scheinkman 2003). Any change that *directly* affects individuals' rate of disability use will have an additional *indirect* effect through the influence that one's participation has on others.

Generating credible estimates of social interactions effect from observational data is notoriously difficult due to problems of omitted variable bias.[5] Peers are likely similar in ways unobservable in data and are also likely subject to similar unobserved shocks. In this paper, the problem of omitted variable bias is addressed by employing a novel instrumental variable (IV) strategy similar in spirit to the "partial population intervention" approach advocated by Moffitt (2001). Our strategy hinges on the empirical observation that plant downsizing events have a substantial effect on disability entry rates (Rege, Telle and Votruba 2009). We therefore use exposure to such events as an instrument for the disability participation rate among one's previously employed neighbors.[6] The intuition behind this approach is straightforward: if social interaction effects exist, then workers in neighborhoods disproportionately exposed to plant

[4] Social norms are only one possible channel through which social interaction effects might operate in disability participation. Section 2 discusses two other possibilities: leisure complementarities and information exchanges.
[5] Manski (1993, 1995) catalogs the range of estimation problems in observational studies of social interaction effects. Our terminology varies somewhat from his. In particular, what we label "social interaction effects," Manski refers to as "endogenous effects."
[6] Throughout, we use the term "plant" to refer to the establishment at which a worker is employed, which is distinct from the firm of employment (as firms can consist of multiple plants).

downsizing events should exhibit a relative increase in subsequent disability entry rates, independent of one's own exposure to plant downsizing.

Social interaction effects estimated under this IV strategy will not suffer from omitted variable bias provided that downsizing rates in neighbors' plants of employment are uncorrelated with unobservable determinants of DP participation. This identifying assumption is potentially problematic because downsizing events concentrated within a particular neighborhood could reflect or cause a decline in local economic opportunities. Alternatively, plant downsizing may be concentrated in neighborhoods populated by persons with higher propensities to utilize DP. The richness of our data, an 11-year panel dataset containing socio-economic information, employment data, and disability participation records for every person in Norway, allows us to indirectly test the validity of our identifying assumption.

Our analysis indicates that social interaction effects play an important role in DP participation. Our IV estimates suggest that a one percentage point increase in the participation rate of previously employed neighbors increased the subsequent 4-year entry rate of workers employed at the end of 1999 by roughly 0.5 percentage points. This has important policy implications, suggesting the direct effect of demographic shifts, policy changes, health shocks and economic shocks on disability participation understates (by roughly one third) the full response expected in equilibrium.

2. Social Interaction Effects

The logic of social interaction effects rests on notions of utility interdependence. That is, when one's peers engage in a particular behavior, it can potentially affect one's own utility from engaging in that behavior. In the context of disability participation, this interdependence could operate through at least three channels: social norms, information and leisure complementarities.

Disability participation is likely affected by social norms regarding "appropriate" participation behavior.[7] Coleman (1990) defines a social norm as a rule of behavior that is enforced by social sanctions, which can take the form of stigma. Social interaction effects arise if social norms are conditional in nature, that is, when the stigma associated with not adhering to a norm is felt more strongly when one's peers adhere to the norm. For instance, a person with a

[7] See Moffitt (1983), Besley and Coate (1992) and Lindbeck, Nyberg and Weibull (1999) for theoretical models of social norms and economic incentives in the welfare state.

marginal disability would likely feel a higher degree of social stigma from drawing disability benefits if surrounded by peers devoted to their work. Thus, as disability participation increases among one's peers, the incentive to apply for DP among non-recipients is expected to increase.

There exists some empirical evidence that suggests an important role for social norms in welfare utilization. Though not specific to disability programs, Moffitt (1983) finds evidence for a stigma related disutility of welfare participation. Horan and Austin (1974) document negative self-characterization and lack of self respect among welfare recipients. Flaa and Pedersen (1999) document that 20 percent of welfare program recipients in Norway feel a loss of social approval.

In addition to the stigma associated with social norms against drawing disability, navigating the application process may incur a cost in terms of time and frustration. Peers familiar with this process can be a valuable source of information for would-be applicants, reducing the cost of filing an application. This information transfer implies that the cost of applying for disability is lower when more of one's peers draw disability.

Alternatively, a person on disability will have more time available for leisure activities than one engaged in work. Disability participation by one's peers can increase one's value of leisure, making it more attractive to draw disability. Similar to social norms and the information channel, this implies that a person's likelihood of drawing disability increases when participation among his peers increases.

Regardless of the channel through which social interaction effects operate, these effects give rise to a *social multiplier*, and possibly to multiple equilibria, that amplifies the effect of policy changes, demographic shifts and health or economic shocks on aggregate participation rates.[8] Any change that directly affects an individual's likelihood of drawing disability will have an additional indirect effect through the influence that the individual's participation has on others. For example, if an economic shock decreases the opportunity cost of drawing disability for a subset of workers, the subsequent increase in disability participation could reduce the stigma associated with drawing disability, thereby increasing participation rates even among those not directly affected by the shock. This self-reinforcing process continues until a new equilibrium is reached.

[8] For a formal analysis see e.g. Glaeser and Scheinkman (2003) or Brock and Durlauf (2001).

3. Disability Pension Program in Norway

The Norwegian Disability Pension (DP) program[9] serves a similar function as the combined disability programs of Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI) in the U.S. A basic and a supplementary pension provide a benefit that is increasing and concave in prior earnings similar to SSDI, and a special supplement ensures a minimum benefit amount similar to SSI. Even though the Norwegian and U.S. programs have similar benefits formulas, increasing at a decreasing rate in past earnings, the Norwegian disability program is more generous, providing a higher earnings replacement rate particularly for low income workers.

Another important difference between the Norwegian and U.S. programs is that the Norwegian program allows workers to apply for DP while still employed. As a result, it is common for Norwegian workers to receive “sick money” prior to transitioning from employment onto disability without ever being unemployed. Sick money refers to temporary assistance (up to one year) provided to disabled workers, ensuring benefits equal to 100 percent of earnings up to some maximum level. After one year, workers can draw a somewhat smaller rehabilitation pension until returning to work or entering DP. During the first 12 months of sick absenteeism, when the worker is typically covered by sick money, Norwegian law makes it particularly difficult to formally dismiss sick workers. Therefore, unlike the U.S., it is not uncommon for disability entrants to enter directly from employment. Moreover, sick money use at a given time is a strong predictor of future entry onto DP.

It is also worth noting that workers applying for disability benefits can receive a temporary disability pension if it is apparent that the worker will qualify for the permanent benefit. In measuring DP participation we include both temporary and permanent DP recipients, as the vast majority of temporary DP recipients go on to receive permanent DP.

4. Empirical Strategy

Identifying social interaction effects in observational data presents a notoriously difficult challenge. An immediate problem is determining an appropriate definition for “peer groups.” Defining peer groups from existing data sources is always somewhat arbitrary. Ideally, we would like to identify individuals with whom a given worker interacts. Lacking such data, peer groups

[9] See Rege, Telle and Votruba (2009) for a more detailed description of Norway’s disability pension program.

are commonly defined by geographic proximity and/or by characteristics suggestive of "social proximity" (e.g. similar socio-economic or employment characteristics). In this paper, peer groups are defined as workers of similar age residing in the same neighborhood.[10] Norwegian neighborhoods are sufficiently small that it is reasonable to believe workers within a given neighborhood do in fact interact with one another.

A more vexing problem is the econometric challenge of producing plausibly unbiased estimates of peer effects given numerous potential sources of omitted variable bias. To demonstrate, consider a straightforward empirical model intended to estimate the contemporaneous effect of peers' DP participation rate on one's own probability of utilizing DP, illustrated here in the form of a linear probability model:

(1) $$DPy_i = \alpha_0 + \alpha_1 X_i + \alpha_2 P_i + \phi PeerDPy_i + \varepsilon_i$$

where

DPy_i	~ indicator that person i draws DP in year y
$PeerDPy_i$	~ participation rate among i's peers in year y
X_i	~ vector of exogenous characteristics of person i
P_i	~ vector of exogenous characteristics of i's peer group
ε_i	~ error term with mean zero

The parameter of interest in equation (1) is ϕ, intended to capture the effect of peers' DP participation on i's likelihood of drawing DP. Provided the peer participation rate is independent of unobserved (or uncontrolled for) determinants of individual participation, estimation of (1)

[10] Norway is divided into 14,211 geographically-defined neighborhoods (*grunnkrets*) that are small in both geographic area and population. On average, an individual lives in a neighborhood with 614 native citizens. The mean neighborhood size in our analytic sample is 691, the difference resulting from our exclusion of workers in the smallest neighborhoods.

provides an unbiased estimate of ϕ. In the parlance of the literature in social interaction effects, $1+\phi$ represents the social multiplier. That is, for sufficiently large N, the expected peer participation rate approximately equals $(1+\phi)A$ when the expected peer rate in the absence of social interaction effects is A.

The plausibility of the identifying assumption in the contemporaneous model is undermined by several potential problems.[11] First, because individuals self-select into neighborhoods, it is possible that neighbors are similar in terms of their probability of becoming disabled or their distaste for work, yielding higher DP participation rates in some neighborhoods than others. Second, workers within a given neighborhood are similar in terms of the economic environment in which they work and/or search for work. Third, the DP screening process applied to applicants could vary across different locales affecting DP entry rates across neighborhoods. For these reasons, we might expect a positive within-neighborhood correlation of DP entry behavior even in the absence of social interaction effects. Importantly, there are limits in the extent that characteristics of individuals and peers can be controlled for since only characteristics unaffected by DP participation are appropriately included in such a model. Income, work history and even marital status are just some of the characteristics that probably should *not* be controlled for, since each is likely endogenous with DP participation. Notably, the random assignment of persons to neighborhoods

[11] Manski (1993, 1995) provides a more complete and general analysis of the *reflection problem* in identifying social interaction effects. Our discussion of the identification issues is intended to address issues relevant in the context of disability application and participation.

would alleviate only the self-selection bias problem, not the other two sources of bias, which highlights the difficulty in generating plausible estimates of social interaction effects in a contemporaneous model of DP participation.

4.1 Instrumental Variable Approach

Our approach for addressing these omitted variable bias problems is to exploit recent and plausibly exogenous shocks that affect DP participation. Our strategy specifically uses exposure to plant downsizing events to instrument for the DP participation rate of previously employed workers in one's neighborhood. This strategy hinges on two facts about disability participation. First, exposure to plant downsizing is a strong predictor of a worker's likelihood of entering disability in Norway, as previously established in Rege, Telle and Votruba (2009).[12] Second, disability participation is "sticky," in the sense that participants rarely exit the system.[13] As a result, neighbors' exposure to plant downsizing affects their subsequent rate of DP utilization, and this effect persists over time even in the absence of social interaction effects. One drawback of this strategy is that exposure to plant downsizing is confined to persons employed at a given point in time. We therefore restrict our attention to persons working at a certain point in time (the end of 1995), both in our sample and in our construction of peer groups.

The logic underlying our IV strategy is fairly straightforward. Peers' exposure to plant downsizing events affects their DP participation rate at a later date. For workers still employed at that later date, we investigate whether downsizing-induced variation in the peer participation rate contributes to variation in DP entry rates going forward. Provided that the recent exposure of one's peers to plant downsizing events is independent of unobserved determinants of subsequent DP entry, the sources of positive bias discussed above would be alleviated.

Figure 1 provides a visual depiction of the selection criteria we employ, as well as the timeframe of our analysis. Our sample of workers consists of native Norwegian workers, age 45-63 in 1999[14], employed full- or part-time in both 1995 and 1999. A worker's "peers" are defined as similarly aged Norwegians, employed full- or part-time in 1995, and residing in the worker's neighborhood in 1995.

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[12] See also Røed and Fevang (2007) and Huttunen, Møen and Salvanes (2006) for how downsizing and organizational change affects workforce participation more generally.
[13] Less than 1% per year (Annual Statistical Yearbook 2003, Norwegian National Insurance Administration).
[14] We always refer to the employment status and age at the end of a given year (i.e. 12/31/yyyy).

Operationally, we implement a two stage linear probability model (2SLS).[15] The first stage equation predicts the DP participation rate among *i*'s peers at the end of 2000[16]:

(2) $$PeerDP2000_i = \beta_0 + \beta_1 X_i + \beta_2 P_i + \beta_3 N_i + \beta_4 PeerPDR_i + v_i$$

where

$PeerDP2000_i$	~ participation rate among *i*'s peers in year *y*
X_i	~ vector of characteristics of person *i*
P_i	~ vector of characteristics of *i*'s peer group
N_i	~ vector of characteristics of *i*'s neighborhood and municipality
$PeerPDR_i$	~ vector characterizing exposure of *i*'s peers to plant downsizing events between 1995 and 1999
v_i	~ error term with mean zero

The second stage equation determines the likelihood that a worker who is employed in 1999 draws disability in 2003:

(3) $$DP2003_i = \alpha_0 + \alpha_1 X_i + \alpha_2 P_i + \alpha_3 N_i + \alpha_4 Peer\widehat{DP}2000_i + \varepsilon_i$$

where $Peer\widehat{DP}2000_i$ is the predicted peer DP participation rate from estimation of the first-stage equation.

Peers' exposure to plant downsizing (i.e. the vector $PeerPDR_i$) is characterized along two dimensions, based on the magnitude of the downsizing that occurred (fraction of jobs shed at the plant) and the industry of the plant. Specifically, the variables in $PeerPDR_i$ capture the fraction of peers original employed in a particular industry in a plant that downsized a particular amount: 10-30, 30-60, 60-100, and 100 percent (i.e. "full closure"). This decision was made in light of findings reported in Rege, Telle and Votruba (2009) that the direct effect of plant downsizing on individual DP entry varies substantially by industry and often demonstrates substantial nonlinearities. While a less complex specification of instruments would have been preferred (e.g. a simple measure of the

[15] Results for alternative specifications are also presented.
[16] We use peers' DP rate in 2000 as our covariate of interest instead of the rate in 1999, as plant downsizing over 1995-1999 is a stronger predictor of DP use in 2000 than in 1999. We attribute this to the lengthy application approval process as well as the possibility that responses to downsizing events might not be immediate.

mean downsizing rate over all peers' plants), the predictive power of the instruments in the first-stage under more parsimonious specifications was exceedingly small, rendering second-stage estimates too imprecise to be substantively meaningful. The use of so many instruments raises a well-known set of "weak instrument" problems that we address in our empirical analysis.

Under the assumption that peers' exposure to plant downsizing events is independent of unobservable determinants of DP entry, 2SLS will provide consistent estimation of α_4. There are several reasons why the independence assumption may be problematic. First, peers' plant downsizing experiences could be correlated with a worker's own plant downsizing experience, either in the past or going forward. The correlation with a worker's own past plant downsizing experience is particularly likely since workers are sometimes employed in the same plants as their neighbors. We address this concern through robustness tests, investigating whether our estimate is sensitive to inclusion of covariates capturing a worker's past (1995-1999) and future (1999-2003) plant downsizing exposure. Second, local plant downsizing events may be correlated with a decline in economic opportunities or future job prospects even for individuals in non-downsizing plants. Again, we can test whether our estimate is sensitive to inclusion of variables meant to proxy for such things, such as changes in the local unemployment rate. Finally, plant downsizing may be concentrated in neighborhoods populated with persons having higher unobserved propensities to draw sickness-related benefits. If so, we would expect peer downsizing rates to be correlated with rates of sick money and DP use prior to 1995. The richness of our data allows us to test this possibility as well.

4.2 Interpreting the Social Interaction Coefficient

As suggested by the notation, unbiased estimates of α_4 are not precisely analogous to unbiased estimates of ϕ in equation (1). The relationship is complicated by an important distinction between the contemporaneous DP participation model in equation (1) and the entry hazard framework employed in our IV approach. To date, analyses of the empirical challenges in the identification of social interaction effects have focused entirely on the omitted variable bias issues faced in the contemporaneous model (e.g. Manski 1993, 1995). Indentifying social interaction effects in a hazard framework raises issues that have not been established in the econometrics literature. Specifically, we demonstrate here why non-IV estimates of α_4 are not informative of the magnitude of social interaction effects even in the absence of usual sources of omitted variable bias.

To demonstrate, suppose the DP participation rate of a representative peer group evolves over three periods (t=0,1,2) as follows:

(4) $DP_0 = 0$

$DP_1 = \gamma A\,(1+\phi) + s_1 + e_1$

$DP_2 = (A+s_1)\,(1+\phi) + s_2 + e_2$

where

A ~ expected DP participation rate in t=2 in the absence of SI effects

$(1+\phi)$ ~ social multiplier

$\gamma \in (0,1)$, where γA captures the group's expected DP rate in t=1 in the absence of SI effects

s_t ~ permanent "shocks" affecting DP participation rate, with mean zero

e_t ~ transitory variation in DP participation rates, with mean zero

This simple formulation captures three intuitive sources of variation in the evolution of peer group participation rates and, thus, in the corresponding entry rates from period to period. First, peer participation rates vary due to fixed differences across groups, represented as variation in A. In the absence of other variation, $(1+\phi)A$ is the expected peer participation rate t=2, where ϕ is the social interaction parameter in equation (1). Second, and critical to our identification strategy, peer groups might be affected by differential shocks that induce variation in peer entry rates in each period, represented by s_t. For illustrative purposes, we assume that the direct effect of such shocks influences DP entry in the period they occur, while the indirect (social interaction) effect exhibits itself in the successive period. Finally, even in the absence of these sources of variation, we would not expect peer participation rates to evolve in a deterministic fashion. Some "out-of-equilibrium" variation is to be expected due to the randomness in the timing of individual DP entries. In contrast to DP-inducing shocks (s_t), we assume that out-of-equilibrium variation in participation rates (e_t)

has no effect on the long-term equilibrium rate of participation. Thus, we have represented the out-of-equilibrium variation as a transitory phenomenon, such that e_1 has no effect on DP_2.[17]

We now apply the relations in equation (4) to facilitate interpretation of the coefficient α_4 we seek to estimate in equation (3). In the entry hazard framework we employ, the magnitude of social interaction effect (α_4) is inferred from the relationship between DP entry rates in period 1 and DP entry rates in period 2, i.e. $\partial\lambda_2/\partial\lambda_1$, where

(5) $\lambda_1 = DP_1$

and $\lambda_2 = (DP_2\text{-}DP_1)/(1\text{-}DP_1)$

Because these rates vary differentially based on the source of variation, the empirical relationship between λ_2 and λ_1 depends on the relative magnitudes of variance in A, s_1, and e_1.

For instance, suppose that s_1=e_1=0, so that variation in peer entry rates depends entirely on variation in A. Under our formulation, we can see

(6) $\partial\lambda_1/\partial A = \gamma(1+\phi)$

and $\partial\lambda_2/\partial A = (1+\phi)[1\text{-}\gamma(1\text{-} \lambda_2)] / (1\text{-} \lambda_1)$

Thus, if differential group characteristics are the only source of variation

(7) $\partial\lambda_2/\partial\lambda_1= [1\text{-}\gamma(1\text{-} \lambda_2)] / \gamma(1\text{-} \lambda_1)$

and $\partial\lambda_2/\partial\lambda_1 \rightarrow (1\text{-}\gamma)/\gamma$ as λ_1 , $\lambda_2 \rightarrow 0$

This indicates that when entry rates are small and differential group characteristics are the only source of variation in peer group entry rates, the empirical relationship between period 1 entry rates and period 2 entry rates are not informative of the size of the social interaction effect. Under our

[17] An alternative way of incorporating out-of-equilibrium variation would be to allow γ to vary across peer groups, and this produces identical implications as those we discuss. It should also be noted that the presence of social interaction effects could itself be a source of out-of-equilibrium variation, due to the time required for peer groups to equilibrate from past DP-inducing shocks. Our simple formulation does not accommodate this source of out-of-equilibrium variation since we have assumed that initial peer participation rates are zero. Nonetheless, the implications are similar to those we discuss.

formulation and assuming periods of equal length (so that $\gamma \approx 0.5$), we might expect non-IV estimation of equation (3) to produce estimates of α_4 close to 1.

A similar result occurs if the only source of variation in peer group entry rates is differential out-of-equilibrium behavior. In this case,

(8) $\partial\lambda_2/\partial\lambda_1 = \partial\lambda_2/\partial e_1 = -(1-\lambda_2)/(1-\lambda_1) \rightarrow -1$ as $\lambda_1, \lambda_2 \rightarrow 0$

Again, when entry rates are small, the empirical relationship between period 1 entry rates and period 2 entry rates are not informative of the size of the social interaction effect. Instead, we would expect non-IV estimation of equation (3) to produce estimates of α_4 close to -1.

Only in the case where variation in period 1 peer group entry rates are driven entirely by variation in DP-inducing shocks (s_1) is the relationship between λ_2 and λ_1 informative of the magnitude of social interaction effects. In this case,

(9) $\partial\lambda_2/\partial\lambda_1 = (\phi + \lambda_2) / (1-\lambda_1) \rightarrow \phi$ as $\lambda_1, \lambda_2 \rightarrow 0$

Thus, only by specifically identifying α_4 from exogenous shocks to (period 1) entry rates can we hope to uncover a meaningful estimate of the social interaction effect in a hazard model framework. In contrast, non-IV estimates in the hazard model framework are almost certainly useless as estimates of the social interaction effect. We nonetheless produce (non-IV) OLS estimates of equation (3) to assess the expected direction of finite sample bias in our IV estimates. We return to this issue in Section 6.2. For the purpose of producing analogous non-IV estimates of the social interaction effect, we instead estimate a contemporaneous DP participation model analogous to equation 1 and discuss it in Section 6.3.

Two other points should be made about interpreting our IV estimates of α_4. First, since our IV estimate of α_4 represents an estimate of $\partial\lambda_2/\partial\lambda_1$, the fact that entry rates are nontrivial matters for its interpretation. Specifically, as equation (9) indicates, α_4 represents an upward-biased estimate of ϕ, with the degree of bias depending on the magnitude of entry rates. The empirical relevance of this is discussed in Section 6.3. Second, social interaction effects could take longer to fully materialize than our estimation model (and data) allow. It is also conceivable that part of the social interaction effect materializes prior to 1999. In either case, this would lead us to understate the magnitude of social interaction effects.

5. Dataset Description

Our analysis utilizes a database provided by Statistics Norway called *FD-trygd*. *FD-trygd* includes a rich longitudinal dataset containing records for every Norwegian from 1992 to 2003. The variables captured in this dataset include individual demographic information (sex, age, marital status, number of children), socio-economic data (years of education, income, wealth), current employment status (full time, part time, minor part time, self-employed), industry of employment (if employed), indicators of participation in any of Norway's welfare programs, and geographic identifiers for municipality and neighborhood of residence.

In addition, *FD-trygd* contains records for employment "events" since mid-1995. These events, captured by individual and date, include entry and exits into employment, changes in employment status (full time, part time, minor part time), and changes in plant and firm of employment. These employment events are constructed by data analysts at Statistics Norway from raw employment spell records submitted by employers, and verified against employee wage records (not available to us) to ensure the validity of each spell and to eliminate records pertaining to "secondary" employment spells.[18]

From these two data sources, four set of variables were created for use in our analysis, described below. The covariates used in our estimation models are described in greater detail in Appendix A.

5.1 Plant Downsizing Variables

Based on the employment records, we constructed plant-level employment counts at the end of years 1995, 1999 and 2003. The counts were constructed as measures of full-time equivalents (FTEs), with part time and minor part time employment measured as 0.67 and 0.33 FTEs, respectively. Excluded from these counts were any persons identified in *FD-trygd* as self-employed or receiving assistance that should have precluded full time work (those receiving unemployment benefits, a rehabilitation pension or a disability pension). Plant-level FTEs were

[18] If an individual was employed in multiple plants at a given time, primary employment was determined from employment status and recorded income from each source of employment.

then used to construct measures of plant downsizing over two periods of time: from 1995 to 1999 and from 1999 to 2003. The measures, which we refer to as the "plant downsizing rate" (PDR), capture the percent decline in FTEs over the period. For instance, plants that fully closed over a given period were recorded as having a PDR=1 for that period; plants with FTE counts declining by 50 percent were recorded as having PDR=0.5. Plants that grew over a given period were recorded as PDR=0 for that period.

As our empirical strategy relies on the power of plant downsizing events to predict subsequent entry onto disability, we choose to focus on downsizing events in reasonably large plants. Specifically, the PDR variable was set to zero for workers employed in plants with fewer than 5 FTEs in the baseline year. Approximately 11 percent of workers were in plants of this size in 1995.

5.2 Worker Sample and Characteristics

Our analytic sample consists of native Norwegians age 45-63 employed either full time or part time in 1999, and also employed full time or part time in 1995. We chose to focus on older workers since these demonstrate the highest rates of DP entry. The upward age limit was imposed to ensure that none of our sample would be eligible for the normal retirement pension in 2003.[19] Excluded were any workers identified as self-employed or receiving assistance that should have precluded full time work (those receiving unemployment benefits, a rehabilitation pension or a disability pension), as well as any receiving social assistance. We excluded those employed in small plants (<5 FTEs) in 1999, for the purpose of controlling for worker's own exposure to plant downsizing going forward (over 1999-2003). We also limited our sample to those residing in a neighborhood in 1995 that contained at least 10 workers age 41-62 to ensure that each person in our sample had a reasonable number of "peers" under our definition of peer groups. Finally, we omitted 907 workers who had received a disability pension any time between 1995 and 1999, as well as one worker missing income/wealth variables in 1999. The resulting dataset consists of 378,148 workers residing in 10,209 different 1995 neighborhoods.

Variables capturing individual socio-economic characteristics were constructed based on records for 1999. These variables include age, sex, education, marital status, number of children, personal income, other household income, net household wealth and an indicator for receipt of a

[19] The age of eligibility for the normal retirement pension is 67.

widow(er) pension. Employment-related variables include an indicator for part time status, tenure at current firm, plant size in 1999, and fourteen industry indicators.[20] The PDR of the worker's 1999 plant (1999-2003) was captured, as well as the past PDR (1995-1999) for the worker's 1995 plant of employment. For the purposes of controlling for workers' own exposure to plant downsizing events, individual downsizing covariates were constructed as 56 dummy covariates based on the worker's industry and the magnitude of downsizing (10-30, 30-60, 60-100 or 100 percent) occurring at their plant over the specified period. Personal income and household wealth in both 1995 and 1999 were also captured, allowing us to control for changes in the workers' economic standing.

Our outcome of interest is an indicator variable capturing whether the worker received either temporary or permanent DP at the end of 2003, with the one caveat. For workers who died or emigrated prior to 2003 and those drawing an early retirement pension[21] in 2003, the indicator was set to one if the worker received DP prior to the event occurring. In sum, 6.9 percent of our sample received DP in 2003. Summary statistics for the remaining individual-level variables are presented in Table 1 (panel A).

5.3 Peer Groups and Characteristics

As described in our empirical strategy, we define peer groups based on age, neighborhood of residence (in 1995) and employment status. Specifically, neighbors are included in a worker's peer group if they were age 41-62 and employed full or part time in 1995. The upward age limit was imposed to ensure that peers were not eligible for the normal retirement pension in 2000. We defined peer groups based on 1995 neighborhoods of residence in case local downsizing events influenced worker mobility. If so, defining peer groups based on 1999 neighborhood of residence could lead to estimation bias through neighborhood self-selection.

Similar socio-economic and employment variables as those described for the worker sample were constructed at the peer group level, using records for 1995. Summary statistics for these characteristics are presented in Table 1 (panel B). Continuous variables were converted to

[20] Coded based on major categories in the Classification of Economic Activities in the European Community (NACE), with certain categories combined due to small sample sizes (agriculture, hunting and forestry was combined with fishing; activities of households was combined with other community, social and personal service activities; extra-territorial organizations and bodies was combined with public administration and defense).
[21] In some firms, workers satisfying specific work history requirements can qualify for an early retirement pension (AFP) at age 62.

categorical variables to create the peer-level covariates used in our estimation models. For instance, the age and sex composition of a worker's peers was captured as the fraction of peers in 14 age-sex categories (three-year age intervals interacted with sex). Peers' income and wealth were each captured as the fraction of peers in six categories based on the 10^{th}, 25^{th}, 50^{th}, 75^{th} and 90^{th} percentiles for the distribution of the relevant variable over the full sample of peers. Additional program participation variables were created for the fraction of peers on social assistance, receiving sick money at the end of 1995 or having received sick money at any time in 1995. Peers' industry of employment was captured as the fractions in 14 industry categories. As described earlier, the peers' exposure to downsizing events was captured as the fraction of peers in a given industry whose plant downsized a specified amount (10-30, 30-60, 60-100 or 100 percent) over 1995-1999, for a total of 56 peer downsizing variables.

Finally, the DP rate of each worker's peers was constructed as the fraction of peers on permanent or temporary DP in 2000. As in the worker sample, we included in this fraction any peers who received DP prior to dying, emigrating or drawing an early retirement pension in 2000. Over our sample of workers, the mean participation rate of the workers' peer groups was 7.4 percent in 2000.

5.4 Other Municipal and Neighborhood Level Characteristics

We created additional variables to capture characteristics of the 1995 municipality and neighborhood of residence thought to potentially influence DP entry behavior. These include total native population; fraction of immigrants; fraction of natives age <18, 18-41 and ≥62 years old; mean income and wealth; and unemployment rate.[22] Additional variables capture the fraction of neighborhood and municipality residents, age 41-62 in 1995 in nine mutually exclusive "status" categories: receives permanent disability, receives temporary disability, receives rehabilitation pension, receives day money (unemployment), unemployed without

[22] The income, wealth and unemployment rate variables were calculated over natives age 22-67. For calculating the unemployment rate, the "employed" were counted as those working full time or part time, and the "unemployed" were counted as those neither working nor self-employed and having received unemployment benefits or registered as "looking for work" in the past year.

receiving day money, self-employed, employed full time, employed part time, employed minor part time.[23] Summary statistics for these variables are presented in Table 1 (panel C).

6. Empirical Results

6.1 Preliminary First Stage Results

Our IV strategy hinges on the fact that plant downsizing events affect individual DP entry, so that peers' exposure to downsizing (1995-1999) can be used to predict the DP entry rate of one's peers (to 2000). Table 2, Panel A reports linear (OLS) estimates of the effect of peers' exposure to downsizing events on the peer DP rate in 2000 for workers in our sample (i.e. our first stage model, equation 2). Covariates capturing the individual, peer, neighborhood and municipal characteristics in Table 1 are included in this and all subsequent models.[24] Of particular note, a set of 56 covariates captures the fraction of one's peers employed in plants of a particular industry and size.[25]

While the majority of the estimated peer downsizing coefficients in the first stage model are positive (see Table 2), there is substantial variation in these estimates. Twenty-one of the estimates are actually negative in sign, with one of these (marginally) significant.[26] The aggregate predictive power of the peer downsizing covariates is quite low, producing an F-statistic of 2.29. As a result, including the full set of peer downsizing covariates in the instrument set raises a well-known set of "weak instrument" problems.[27] First, IV estimates based on the full set of potential instruments are expected to suffer from "finite sample bias" towards the OLS estimate. Second, the asymptotic assumptions underlying conventional hypothesis testing break down in the face of weak instruments, leading conventional standard errors to exaggerate the precision of IV estimates. Third, if the instruments are not entirely exogenous, the expected bias

[23] A tenth (omitted) status category consists of persons neither employed nor receiving DP, rehabilitation pension or day money. We distinguish between this group and those "unemployed without receiving day money" based on whether the individual had registered as "looking for work" in the past year.
[24] See Appendix A for further details regarding the included covariates.
[25] Doing so addresses potential bias arising from the fact that we do not capture downsizing outcomes for peers originally employed in small plants.
[26] The PDR covariates demonstrating small or negative coefficients in the first stage model are generally those demonstrating smaller effects in similar models estimated at the individual level (see Rege, Telle and Votruba 2009).
[27] These problems are nicely surveyed by Stock, Wright and Yogo (2002). See also Bound, Jaeger and Baker (1995), Staiger and Stock (1997), and Hahn and Hausman (2003) for important contributions to this literature.

is more severe when instruments are weak. Thus, potential violations of the identifying assumption are of greater concern when instruments are weak.

6.2 The Weak Instruments Problem and Alternative 2SLS Estimates

While asymptotic efficiency is obtained from including all valid instruments, the finite sample properties of IV estimates can be improved by selectively excluding valid instruments with weak power (Stock, Wright and Yogo, 2002). A number of "instrument selection" procedures have been suggested in the econometrics literature as means for addressing the weak instruments problem (e.g. Hall and Peixe 2002; Donald and Newey 2001), though a standard method has yet to emerge. For our analysis, we adopted a procedure to select among the set of potential instruments along the lines suggested by Donald and Newey (2001). Specifically, we sought to exclude potential instruments to minimize the mean square error around the IV estimate, the criteria employed by Donald and Newey.

Following Donald and Newey, we constructed a sequence of candidate instrument sets $\{Z_K\}$, where K={1, 2, .., 56} denotes the number of peer downsizing covariates in each set. The set Z_1 consists solely of the covariate with the largest marginal R^2 contribution to the first stage regression (conditional on the other covariates). Each subsequent set, Z_{K+1}, consists of the peer downsizing covariates in Z_K, as well as the additional peer downsizing covariate with the largest marginal R^2 contribution to the first stage regression (conditional on Z_K and the other covariates). Thus, each Z_K set roughly consists of the K potential instruments providing the greatest power in the first stage.[28]

Figure 2 presents 2SLS estimates of α_4 (in equation 3) under alternative instrument sets $\{Z_K\}$ for values of $K \geq 4$. The 2SLS estimates range in magnitude from 0.61 when a restrictive set of instruments (K=6) is employed to roughly 0.45 when fuller sets of peer downsizing covariates are included as instruments ($K \geq 40$). While not monotonic, the 2SLS estimates decline in a fairly linear fashion as progressively weaker instruments are added, consistent with finite sample bias towards a smaller OLS estimate.

[28] Due to the large number of potential instruments, an exact application of Donald and Newey's approach was not attempted and our approach varies in a number of respects. First, due to the large number of candidate instruments, we required that the sequence of candidate instrument sets be nested in one another (i.e. $Z_1 \subset Z_2 \subset \ldots \subset Z_{56}$). Second, alternative sets of instruments were constructed based solely on the power contributed by a candidate instrument in the first stage, rather than grouping potential instruments for *a priori* reasons.

The results from these alternative models were used to calculate the approximate finite-sample bias in the 2SLS estimator and the asymptotic variance around the estimator, from which the approximate mean square error was calculated. The formulas for doing so are presented in Appendix B. As shown in Figure 2, the approximate finite sample bias is negative in sign and growing in magnitude as weaker instruments are included in the instrument set. The 2SLS estimates produced using alternative instrument sets roughly coincide with the approximate bias. Together, these results suggest that, corrected for finite sample bias, our instruments generally produce estimates of α_4 of about 0.6.

Figure 3 plots the approximate mean square error associated with 2SLS estimates under alternative instrument sets. As progressively weaker instruments are added to the model, the bias of the 2SLS estimator increases but the asymptotic variance around the estimator decreases. Thus, in choosing among candidate instrument sets, we are essentially choosing between estimators that are less biased but less precise versus those that are more biased but more precise. Our calculations indicate that the approximate mean squared error around the 2SLS estimator is minimized when the Z_{14} instrument set is employed. For the remainder of our analysis, we therefore concentrate on IV results using as instruments the 14 peer downsizing covariates demonstrating the greatest power in predicting the peer DP rate in 2000.

Panel B of Table 2 presents OLS coefficients from the first stage model using our preferred set of instruments (K=14). The exclusion of weaker instruments from this model, cf. Panel A of Table 2, had only a modest effect on the coefficients for the included instruments. While the F-statistic (7.07) is substantially larger than that produced using the full set of instruments (2.29), it fails to reach levels where the weak instrument problem can be safely ignored (Staiger and Stock 1997). Thus, IV estimates under our preferred instrument set are still expected to suffer (modest) finite sample bias towards the OLS estimate, and conventional standard errors potentially understate the true variance around these estimates.

6.3 Main Results

The main results from our analysis are presented in Table 3. For comparison purposes, the first two columns report non-IV estimates of the social interaction effect. Estimated under a linear probability specification via OLS (column 1), our estimate suggests a one percent increase in the 2000 peer DP rate predicts a modest 0.07 percentage point increase in the subsequent entry rate (to 2003) of workers employed at the end of 1999. A probit specification produced an estimated mean marginal effect about 20 percent smaller. Notably, the non-IV estimates of α_4 are much smaller than the alternative 2SLS estimates in Figure 2. As we discussed in Section 4, non-IV estimates of α_4 are unlikely to be informative of the magnitude of the social interaction effects, therefore the difference in the IV and non-IV estimates should come as no surprise. The fact that IV estimates of α_4 are substantially larger suggests that much of the unexplained variation in peer entry rates to 2000 reflects out-of-equilibrium variation, biasing non-IV estimates of α_4 downwards. The smaller magnitude of non-IV estimates of α_4 is also consistent with our finding that IV estimates tend to be smaller when identified off of progressively larger (and weaker) sets of peer downsizing instruments.

Columns 3-6 provide various IV estimates of α_4 employing our preferred instrument set. Our 2SLS estimate is the same as that depicted visually in Figure 2 (for K=14) and suggests that a one percentage point increase in the 2000 peer DP rate due to recent downsizing events increases the subsequent entry rate (1999-2003) of workers by 0.5 percentage points, a 7.2 percent increase relative to the aggregate rate of entry. Estimating our IV model using limited information maximum likelihood (LIML)[29] had little effect on our estimate (see column 4), as did estimation using two-step feasible generalized method of moments (results not shown). Employing Nagar's (1959) bias-corrected 2SLS model, the estimated α_4 increases about 12 percent. Across each of these specifications, standard tests of overidentifying restrictions passed easily. In an IV-Probit specification, the estimated mean marginal effect is about 13 percent smaller than suggested by the linear models.

For comparison, columns 7-10 report IV results using the full set of peer downsizing covariates as instruments. As anticipated by Figure 2, the 2SLS, LIML and IV-Probit estimates are modestly smaller than before, a result consistent with increasing finite sample bias, while the bias-corrected 2SLS estimate is somewhat larger. Tests of overidentifying restrictions fail in each of these models, another reason we concentrate on the findings using our preferred instrument set.

As mentioned earlier, the conventional standard errors reported for our IV estimates should be interpreted with caution as they potentially overstate the precision of our estimates due

[29] LIML estimators are known to be less biased that 2SLS but suffer from larger small sample variation (Hahn, Hausman and Kuersteiner, 2004).

to the weakness of our instruments. To evaluate the extent of this problem, we compared the conventional confidence interval around our 2SLS estimate to that calculated using the "conditional likelihood ratio" approach developed by Moreira (2003).[30] There was very little difference between the two confidence intervals when evaluated at either the 95 percent or 99 percent levels, suggesting that the conventional standard errors provide a reasonably accurate gauge of estimate precision.

As discussed in Section 4.2, IV estimates of α_4 are not directly interpretable as estimates of ϕ in the contemporaneous model of social interaction effects (equation 1). Instead, IV estimates of α_4 represent upwards biased estimates of ϕ, with the level of bias determined by the DP entry rate of workers in each period. Applying equation (9) to our estimates and assuming rates of entry equal to seven percent per period allows us to calculate rough estimates of ϕ from our IV estimates. Using our baseline IV estimate (Table 3, model 3) implies $\phi \approx 0.40$, while the bias-corrected estimate using our preferred instrument set (Table 3, model 5) implies $\phi \approx 0.46$. These estimates, while large, are nonetheless smaller than non-IV estimates of ϕ produced from a contemporaneous model of social interaction effects (see equation 1). As reported in Appendix C, OLS estimation of a contemporaneous model produces an estimate of $\phi = 0.58$. While the samples and definition of peer groups are not identical across the two types of models,[31] these results are consistent with an upwards bias in the non-IV contemporaneous model of social interaction effects.

6.4 Robustness Tests

The identifying assumption in our IV approach is that the plant downsizing experiences of a worker's peers occur independently of unobserved determinants of DP participation. Table 4 presents the results of robustness checks to test the validity of this assumption. For comparison, results from our 2SLS model (Table 3, column 3) are repeated in column 1.

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[30] For the purposes of this comparison, the model was estimated under the assumption of independent, homoskedastic errors. As currently available in Stata®, the conditional likelihood ratio test statistics can only be calculated under this assumption. Murray (2006) cites Moreira's approach as "state of the art for hypothesis testing with weak instruments" (p126).

[31] In particular, the contemporaneous model employs a less exclusive definition for peer groups since we cannot exclude non-workers without excluding most DP recipients, which might be expected to weaken the measurable social interaction effect.

An important concern for our identifying assumption is that exposure to downsizing is correlated across peers, who are frequently employed in the same plants. As a result, our IV estimate could reflect a delayed reaction to one's own downsizing experience. If so, controlling for the past downsizing events (over 1995-1999) in workers' 1995 plants would be expected to reduce the estimated social interaction coefficient. As indicated in column 2, controlling for the past downsizing events in workers' 1995 plants has negligible impact on the 2SLS estimate, despite adding significant power to the model ($p<.0001$ for F test of joint significance).

Peers' downsizing events could also be indicative of declining local labor market conditions, which could influence disability entry going forward, biasing our estimate upwards. Columns 3-5 include additional covariates expected to capture changes in a worker's labor market opportunities. In column 3, we include covariates capturing downsizing events in workers' 1999 plants going forward (over 1999-2003). In column 4, we include covariates capturing changes in the workers' personal income and household wealth since 1995. In column 5, we add county indicators[32] and covariates capturing the 1999 unemployment rate and mean income in each workers' 1995 neighborhood and municipality. Each additional set of covariates contributes significant power to the model ($p<.0001$), but has negligible effect on the 2SLS estimate with the exception of the last, when the estimate increases modestly.

The remaining robustness checks address the concern that our measure of downsizing is a fairly crude measure of individual workers' exposure to employment shocks. Workers who switched plants over 1995-1999 are a particular concern in this regard, since they may have been exposed to downsizing in their subsequent plants, or may have been laid off from a plant that subsequently increased employment. Column 6 therefore estimates the 2SLS model excluding workers who switched plants. Column 7 reflects an even more conservative approach, restricting the sample to non-switchers in stable or growing plants over 1995-1999. We perceive this last model as a particularly strong test of social interaction effects, since the sample excludes all workers directly exposed to downsizing events.[33] Interestingly, the 2SLS estimate increases somewhat in magnitude when plant switchers are excluded (column 6), perhaps reflecting that social interaction effects are stronger for workers with more stable employment. Indeed, it seems reasonable to think that "stable" workers might have stronger social ties to their neighbors, though we have no way of

[32] Norway is divided into 19 counties.
[33] This specification most closely resembles the "partial population intervention" approach advocated by Moffitt (2001).

confirming this. Further restricting our sample to those in stable or growing plants had only a small effect on our estimate (column 7). Therefore, it seems unlikely that our estimate reflects a bias from unobserved employment shocks that are correlated with the peer downsizing instruments.

6.5 Specification Tests

In this section, we explore the possibility that variation in pre-existing unobservables is potentially correlated with the variation in $PeerDP2000_i$ explained by our instruments. We primarily have two unobservables in mind, which could be labeled broadly as "propensity for work" and "propensity for drawing disability-related benefits." For instance, if workers with lower propensities for work are those with peers disproportionately exposed to downsizing, this would bias IV estimates of the social interaction effect upwards. A number of plausible stories could lead to such a bias. Downsizing events might be concentrated in areas with generally poorer employment opportunities or in areas where attachment to the labor force is generally weaker. Alternatively, since workers sort themselves into neighborhoods for reasons unobserved, workers with weaker attachment to the labor force might self-select neighborhoods where plant-employment patterns are less stable. Similar stories could be told that would lead to correlation between the explained variation in $PeerDP2000_i$ and the unobserved propensity for drawing disability benefits.

In the following specification tests, we use observed outcomes prior to 1995 to proxy for the unobservable propensities for work and for drawing disability-related benefits, and estimate the "effect" of $PeerDP2000_i$ on these outcomes. A significant coefficient represents a potential source of bias.

Table 5 presents two sets of results in this regard. Panel A reports 2SLS estimates for the "effect" of $PeerDP2000_i$ on the probability a worker in our sample is employed full- or part-time at the end of each calendar year. Panel B reports analogous estimates for the probability a worker received sick money at the end of each year. Concentrating on results prior to 1995, downsizing-induced changes in $PeerDP2000_i$ are positively correlated with labor force attachment and (weakly) negatively correlated with sick money use. As a result, we might expect our IV estimates to suffer from a negative bias. Notably, these relationships change signs after 1999. In particular, we find a strong significant effect of $PeerDP2000_i$ on sick money use after 1999, consistent with the emergence of a positive effect of peers' downsizing exposure on workers' willingness to takeup sickness-related benefits. One troubling finding in Table 5 is the marginally significant "effect" of $PeerDP2000_i$ on employment in 1997. This finding could potentially reflect declining labor market opportunities among workers in peer groups disproportionately exposed to downsizing. However, if this were a serious source of bias, we would have expected our estimate to decline under the sample restrictions discussed above (Table 4, columns 6 and 7).

A potential problem with the specification tests presented in Table 5 is that the observed outcomes relate specifically to our sample of workers, who were required to be employed in both 1995 and 1999. As a result, variation in local labor market conditions or in worker tastes might not be captured in the outcomes for this select sample. To address this concern, Table 6 reports the results from similar specifications employing neighborhood-level outcomes. Specifically, we report 2SLS estimates for the "effect" of $PeerDP2000_i$ on DP and labor force participation rates in a worker's neighborhood prior to 1995. These rates are based on the entire population of similarly-aged persons residing in the worker's 1995 neighborhood, not the subset of employed neighbors used to define peer groups. Also, we exclude as covariates from these models the neighborhood-level covariates capturing the fraction of neighbors in different employment and program use categories since these are collinear with the outcomes being modeled. As reported in Table 6, we find no evidence that unexplained pre-existing differences in neighborhood rates of employment or DP use are correlated with the variation in $PeerDP2000_i$ explained by our instruments.

Taken together, these results fail to indicate that pre-existing differences across individuals or neighborhoods present a serious source of bias in our estimation of social interaction effects.

6.6 Disability Pension Entry to Alternative Endpoints

While our social interaction estimate is robust to inclusion of covariates intended to capture changing conditions in the local labor market (Table 4, columns 3-5), these covariates fail to fully capture workers' *perceptions* regarding the local labor market. Workers in neighborhoods disproportionately affected by downsizing events could form poor impressions of their labor market opportunities, triggering an increased rate of DP entry by such workers, biasing our IV estimate upwards. Unfortunately, we have no way of directly testing whether peers' exposure to downsizing events affects workers' perceptions in this way.

As an indirect test, Table 7 presents 2SLS estimates of the social interaction effect altering the "endpoint" at which DP use is measured. If our IV estimate merely reflects workers' response to the psychological shock of observing local downsizing events, we would expect the DP entry responses to be fairly contemporaneous with the occurrence of those downsizing events. That is, we would expect the social interaction coefficient to "level off" rather quickly. Instead, we find no evidence that the DP effect has "leveled off" by 2003. While this result does not preclude a potential "psychological effect" bias, it does undermine the argument that our estimate is merely an artifact of this bias. Moreover, since the social interaction coefficient increases substantially over the last year for which we have data, it suggests that our estimate understates the full magnitude of the effect that would be observed in equilibrium.

7. Conclusion

In this paper we estimate the magnitude of social interaction effects in disability pension participation among older workers in Norway. Specifically, we investigate how workers' propensity to draw DP benefits is affected by the disability participation of their "peers," defined as neighbors of similar age and employment status. To address issues of omitted variable bias, we use peers' exposure to plant downsizing events to instrument for peer rates of DP entry. To our knowledge, this is the first study to examine social interaction effects in disability participation.

Our linear probability estimates suggest that a one percentage point increase in the DP participation rate of previously employed neighbors increased the subsequent 4-year entry rate of employed workers by about 0.5 percentage points. Our non-linear (probit) IV estimate is somewhat smaller (0.44 percentage points), but remains large and highly significant. The presumed direction of finite sample bias suggests these are conservative estimates.

The causal interpretation of our social interaction estimate depends critically on the assumption that peers' exposure to downsizing events is independent of unobservable determinants of disability entry. *Ex ante*, one might reasonably expect downsizing events to be concentrated in neighborhoods with workers having higher pre-existing propensities for entering disability. However, we find no evidence that the variance in peer DP rates induced by peers' exposure to plant downsizing events is associated with the neighborhood rate of DP use prior to the relevant downsizing events. Workers in peer groups disproportionately exposed to downsizing have *higher* rates of employment and *lower* rates of sick money use prior to the downsizing events suggesting, if anything, our estimate is bias downwards.

Alternatively, local downsizing events could adversely affect local labor market conditions, causing a rise in disability entry rates independent of any social interaction effect. Our robustness tests fail to provide support for such a bias. While we cannot entirely rule out the possibility that our estimate is contaminated by the psychological effect of observing local downsizing events, our estimates of the social interaction effect to different points in time shed doubt on this as a major source of bias.

These findings suggest that the social multiplier in disability participation when measured at the level of Norwegian neighborhoods in 1.4. As demonstrated in Glaeser, Sacerdote and Scheinkman (2003), the level of aggregation can greatly affect the size of estimated social multipliers. Norway has a particularly homogeneous population, which could contribute to especially large social interaction effects. Nonetheless, our results suggest a social multiplier similar in magnitude to those estimated by Glaeser, Sacerdote and Scheinkman (2003) in college dorm rates of fraternity membership and county-level crime rates.

A social interaction effect of this magnitude has important implications for research in disability insurance participation. Social interaction effects could conceivably explain the large variation in SSDI participation across U.S. counties (McCoy et al. 1994). They could conceivably contribute to the sizable labor supply response to disability benefit increases observed in Canada (Gruber 2000), as well as the large SSDI response to the coal boom/bust observed in coal-producing states (Black et al. 2002). To the extent that social networks are defined along socio-economic lines, they could help explain the large increase in disability participation among low-skilled U.S. workers, attributed in Autor and Duggan (2003) to the declining demand for low-skilled workers and an unforeseen increase in their disability benefit replacement rates. As a general empirical matter, the existence of large social interaction effects requires careful interpretation of estimates meant to capture the individual-level determinants of disability participation to the extent these determinants are concentrated within particular social networks. Such estimates likely exaggerate the individual-level importance of such determinants while understating the full (aggregate) effect.

For policymakers, our results lend empirical support to concerns about the potential development of welfare cultures arising from poorly designed disability insurance programs. From a social welfare perspective, the existence of large social interactions could dramatically affect the socially optimal replacement rate in social insurance programs, an issue that has received little attention in the program design literature.[34] The existence of social interaction effects would also strengthen arguments made by Autor and Duggan (2006) regarding the importance of developing screening procedures that better identify the individuals meant to be covered by disability insurance programs. Finally, our results indicate that efforts to reduce the impact of economic shocks on disability entry (e.g. retraining programs, job search assistance) would, if effective, also reduce disability participation among persons not directly affected by those shocks.

[34] Kroft (2008) is a notable exception.

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Appendix A: Covariate Details

The table below describes the exact covariates included our estimation models.

Characteristic	Covariates	Analyses	Comments
Panel A: Individual-level characteristics (in 1999 unless otherwise indicated)			
sex/age	indicator for female, plus third-order polynomial of age interested with sex	all	
personal income	third-order polynomial	all	includes all non-capital sources of income, including government transfers
other household income	third-order polynomial	all	total household income net of personal income
household wealth	third-order polynomial	all	
education	indicators for education ≤9, 13-15, and ≥16 years	all	missing assigned to omitted category (10-12 years)
marital status	indicators for married, widowed, and divorced	all	missing assigned to omitted category (single)
number of children	indicators for 1, 2-3, and ≥4	all	number reflects count of persons <18 years old in household
receives widow(er) pension	indicator	all	
employed part-time	indicator	all	
tenure at plant	indicators for 1-3, 3-5, and ≥5 years	all	
1999 plant industry/size	indicators for plant industry (14 categories) and size (3 categories: 5-25, 25-100, and ≥100 FTEs)	all	42 total, one omitted due to colinearity; sample excludes those in (1999) plants employing <5 FTEs
1999 plant industry/PDR	indicators for plant industry (14 categories) and 1999-2003 PDR (4 categories: 10-30%, 30-60%, 60-100%, and 100%)	Table 4, (3)-(7)	56 total
1995 plant industry/size	indicators for plant industry and size (4 categories: <5, 5-25, 25-100, and ≥100 FTEs)	Table 4, (2)-(7)	56 total, one omitted due to colinearity

1995 plant industry/PDR	indicators for plant industry (14 categories) and 1995-99 PDR (4 categories: 10-30%, 30-60%, 60-100%, and 100%)	Table 4, (2)-(7)	56 total, set to zero if workers' 1995 plant employed <5 FTEs
Δincome/wealth, 1995-99	third-order polynomials for changes in personal income, other household income, and household wealth	Table 4, (4)-(7)	
Panel B: Peer group characteristics (in 1995)			
sex/age	fraction of peers in 14 sex-age categories (age categories: 41-44, 44-47, … , 59-62)	all	14 total, one omitted due to colinearity
education	fraction of peers in three categories: ≤9, 13-15, and ≥16 years of education	all	
marital status	fraction of peers in three categories: married, widowed, and divorced	all	
number of children	fraction of peers in three categories: 1, 2-3, and ≥4 children in household	all	
receives widow(er) pension	fraction of peers receiving	all	
receives sick money	fraction of peers receiving	all	
received sick money in year	fraction of peers who received earlier in year (but not at end of year)	all	
receives social assistance	fraction of peers receiving	all	
personal income	fraction of peers in six categories, defined by 10^{th}, 25^{th}, 50^{th}, 75^{th} and 90^{th} percentile in sample distribution	all	one omitted due to colinearity
other household income	fraction of peers in six categories, defined by 10^{th}, 25^{th}, 50^{th}, 75^{th} and 90^{th} percentile in sample distribution	all	one omitted due to colinearity
household wealth	fraction of peers in six categories, defined by 10^{th}, 25^{th}, 50^{th}, 75^{th} and 90^{th} percentile in sample distribution	all	one omitted due to colinearity
employed part-time	fraction of peers	all	

tenure	fraction of peers with ≥1 year of tenure in 1995 plant	all	
1995 plant industry/size	fraction of peers in 56 plant industry/ size categories (defined same as in Panel A)	all	one omitted due to colinearity
1995 plant industry/PDR	fraction of peers in 56 plant industry/ PDR categories (defined same as in Panel A)	--	defines full set of potential instruments
total count of peers	second-order polynomial	all	see text for definition
Panel C: Neighborhood-level characteristics (in 1995 unless otherwise indicated)			
total population	second-order polynomial	all	excludes immigrants
fraction of immigrants	second-order polynomial	all	
age	second-order polynomial for fraction <18, 18-41, and ≥62 years old	all	excludes immigrants
mean personal income	second-order polynomial	all	over natives age 22-67
mean other household inc	second-order polynomial	all	over natives age 22-67
mean household wealth	second-order polynomial	all	over natives age 22-67
unemployment rate	second-order polynomial	all	over natives age 22-67
employment/program status[a]	second-order polynomials for fraction in nine mutually exclusive categories	all, except Table 6	over natives age 41-62
1999 unemployment rate	second-order polynomial	Table 4, (5)-(7)	over natives age 22-67, set to zero if <20 in 1999
1999 mean personal income	second-order polynomial	Table 4, (5)-(7)	over natives age 22-67, set to zero if <20 in 1999
1999 "small" neighborhood	indicator identifying neighborhoods with <20 natives age 22-67 in 1999	Table 4, (5)-(7)	
Panel D: Municipal-level characteristics (in 1995 unless otherwise indicated)			
total population	second-order polynomial	all	excludes immigrants
fraction of immigrants	second-order polynomial	all	
age	second-order polynomial for fraction <18, 18-41, and ≥62 years old	all	excludes immigrants

mean personal income	second-order polynomial	all	over natives age 22-67
mean other household inc	second-order polynomial	all	over natives age 22-67
mean household wealth	second-order polynomial	all	over natives age 22-67
unemployment rate	second-order polynomial	all	over natives age 22-67
employment/program status[a]	second-order polynomials for fraction in nine mutually exclusive categories	all	over natives age 41-62, excluding those in worker's neighborhood
1999 unemployment rate	second-order polynomial	Table 4, (5)-(7)	over natives age 22-67
1999 mean personal income	second-order polynomial	Table 4, (5)-(7)	over natives age 22-67
county	indicators for county	Table 4, (5)-(7)	19 total, one omitted due to colinearity

[a] For the purpose of creating the "employment/program status" covariates, all natives age 41-62 in 1995 were assigned to one of nine mutually exclusive categories, defined as:

- receiving permanent DP
- receiving temporary DP
- receiving rehabilitation pension
- receiving day money (unemployment benefits)
- self-employed
- employed full-time
- employed part-time
- employed minor part-time
- unemployed (without receiving day money)

The "unemployed" category captures persons neither currently employed nor receiving day money, but who were registered with the government as seeking employment in the past year. Thus, it is intended to capture those who are likely still in the workforce. (This group was combined with the "receiving day money" group for the purpose of constructing unemployment rate variables.) To ensure that the status categories were mutually exclusive, statuses were assigned in a stepwise fashion, such that assignment to an "earlier" category precluded assignment to a latter category (with the categories ordered as listed above).

Appendix B: Calculating Approximate Bias and MSE

The following describes the formulas used for calculating the approximate bias and mean squared error (MSE) around the 2SLS estimates presented in Figure 2. Similar representations for these formulas exist in the literature, although these are often expressed for cases where the second stage model includes a single endogenous covariate.[35] These formulas have been modified to accommodate the presence of exogenous covariates and clustering of the error terms within neighborhood. Our notation follows that of Wooldridge (2002).

For a given instrument set (K), the MSE around the 2SLS estimate can be written as:

(B1) $$MSE_K(\hat{\beta}_{2SLS}) = Var_K(\hat{\beta}_{2SLS}) + (Bias_K(\hat{\beta}_{2SLS}))^2$$

or, more succinctly,

$$MSE_K = Var_K + (Bias_K)^2$$

where Var_K denotes the variance around the 2SLS estimator and $Bias_K$ denotes the finite sample bias.

Following Bound, Jaeger and Baker (1995), the finite sample bias in the 2SLS estimator can be approximated as:

(B2) $$Bias_K \approx \frac{\sigma_{\varepsilon v}(K-2)}{\sum_{i=1}^{N} r_{iK}^2}$$

where $\sigma_{\varepsilon v}$ denotes the covariance between the second stage and first stage residuals and r_{iK} denotes the expected change in the predicted value of $PeerDP2000_i$ induced by the included covariates.

(B3) $$r_{iK} = E(PeerDP2000 \mid X_i, Z_{iK}) - E(PeerDP2000 \mid X_i)$$

An estimate of r_{iK} can be derived as the residual from the regression of $Peer\hat{DP}2000_{iK}$, the predicted value employing instrument set K, on the exogeneous covariates:

(B4) $$Peer\hat{DP}2000_{iK} = \hat{\delta}_K X_i + \hat{r}_{iK}$$

[35] E.g. Hahn, Hausman and Kuersteiner (2004).

The denominator in (B2) can therefore be estimated as sum of squared residuals from (B4).

To estimate the numerator, an estimate of $\sigma_{\varepsilon v}$ can be calculated in typical fashion based on the estimated residuals from the first and second stage models. In our calculations, we estimate $\sigma_{\varepsilon v}$ based on the K=56 model (full instrument set), holding this value constant for alternative K, in line with the procedure recommended Donaldson and Newey (2001). In doing so, differences in the approximate bias across different (K) estimators are driven entirely by differences in the number of instruments employed and the explanatory power of those instruments.

The variance around the 2SLS estimator is approximated by an estimate of its asymptotic variance. Adjusting for within-neighborhood clustering,

$$\text{(B5)}\qquad \hat{Var}_K \approx \frac{\sum_{j=1}^{N_j}\left(\sum_{i\in j}\hat{\varepsilon}_i\,\hat{r}_{iK}\right)^2}{\left(\sum_{i=1}^{N}\hat{r}_{iK}^2\right)^2}$$

Again, the estimated second stage residuals ($\hat{\varepsilon}_i$) were derived from the K=56 model and held constant across alternative models, so that differences in the estimated estimator variance across models is primarily the result of differences in the predictive power afforded by different instrument sets.

Appendix C: Contemporaneous Estimates of Social Interaction Effects

The following table presents estimates from contemporaneous models of social interaction effects of the sort described in Section 4.1.

The sample consists of all Norwegians age 49-67 in 2003, consistent with the age range used in our main analysis. Peer groups were defined as *all* neighbors age 49-67, a less restrictive definition than employed in our main analysis. Individuals in peer groups with fewer than 10 members were excluded, as in our main analysis.

The covariate of interest is the rate of DP utilization measured over an individual's peers. A limited set of additional covariates was included to prevent inclusion of covariates potentially endogenous with DP utilization. Individual-level covariates include third-order covariates for age interacted with sex and indicator variables for three educational categories. Peer-level covariates include the means of all individual-level covariates over an individual's peers. For the purposes of measuring DP utilization, persons drawing an early retirement pension (AFP) were identified as "utilizers" if they drew DP prior to drawing retirement. (This had minimal effect on the results, but is consistent with the DP measure used throughout the paper.) Very similar estimates were produced when estimated over alternative years.

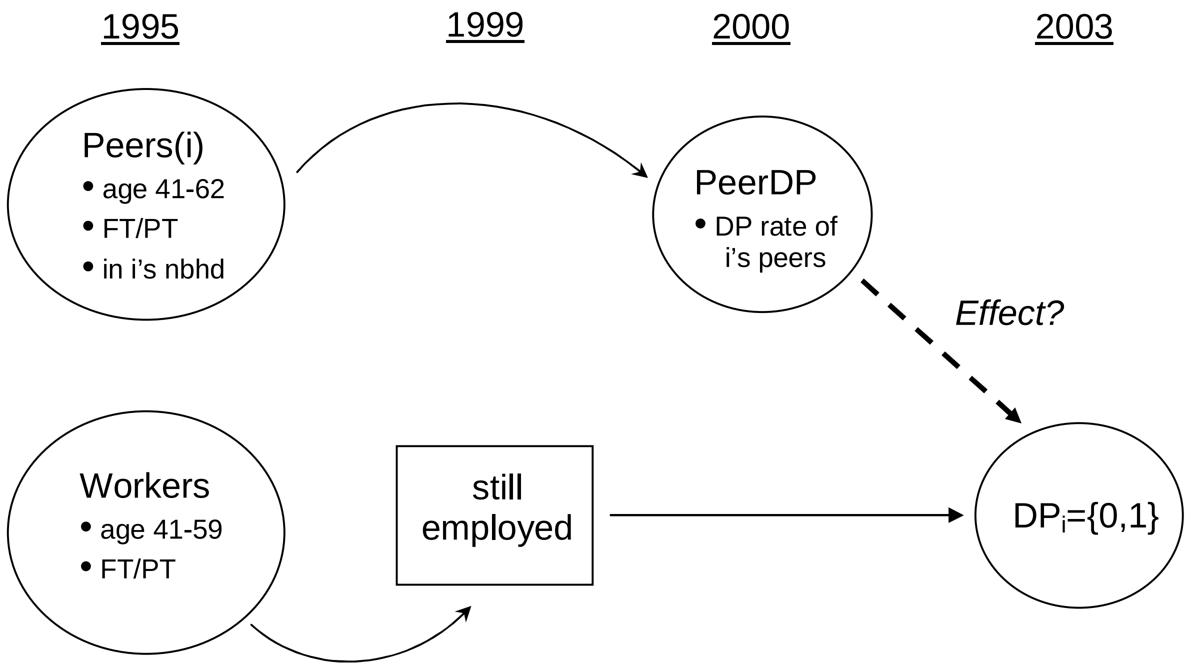
Effect of Social Interaction on Disability Pension Entry: Contemporaneous Estimates

Dependent variable: DP utilization in 2003

	OLS	Probit
Peer 2003 DP rate	.581**	1.991**
	(.007)	(.022)
		[.534]
mean	.234	.234
N	857923	857923

Notes: Robust standard errors in parentheses, corrected for non-independent residuals within neighborhoods. Mean marginal effect estimate presented in brackets for probit model. $^{+}$, * and ** denote significance at the 10, 5 and 1 percent level.

Figure 1: Empirical Strategy Timeline



Linear Probability Model: $DP2003_i = \beta X_i + \phi PeerDP2000_i + u_i$

Figure 2: 2SLS Estimates under Alternative Instrument Sets

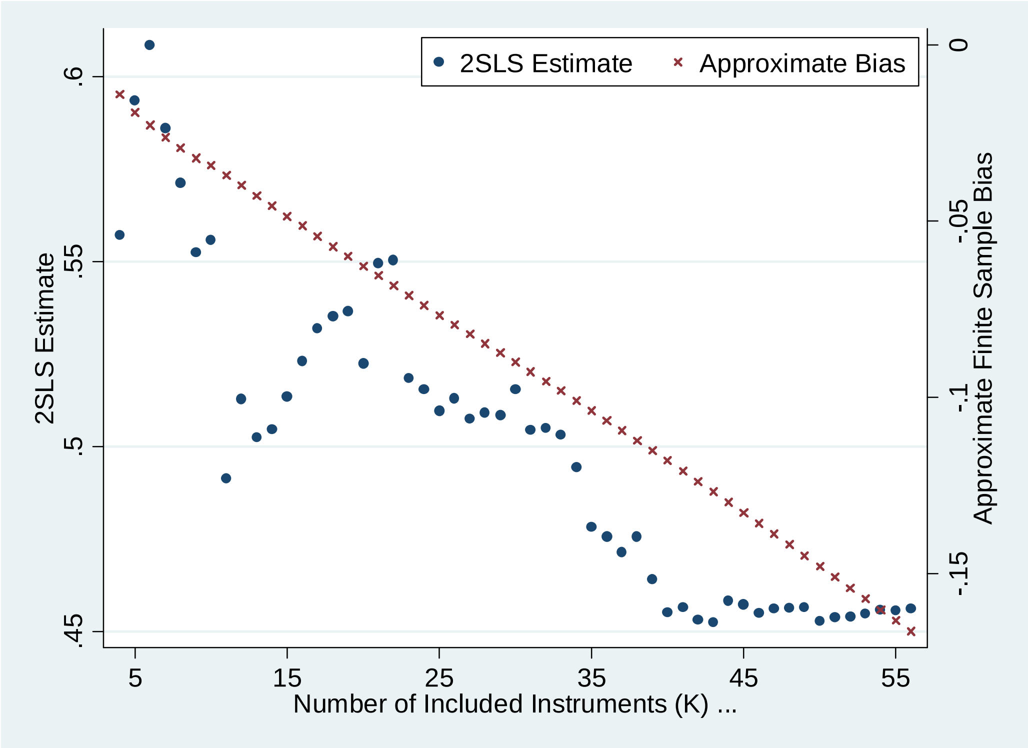


Figure 3: Approximate Mean Square Error of 2SLS Estimates

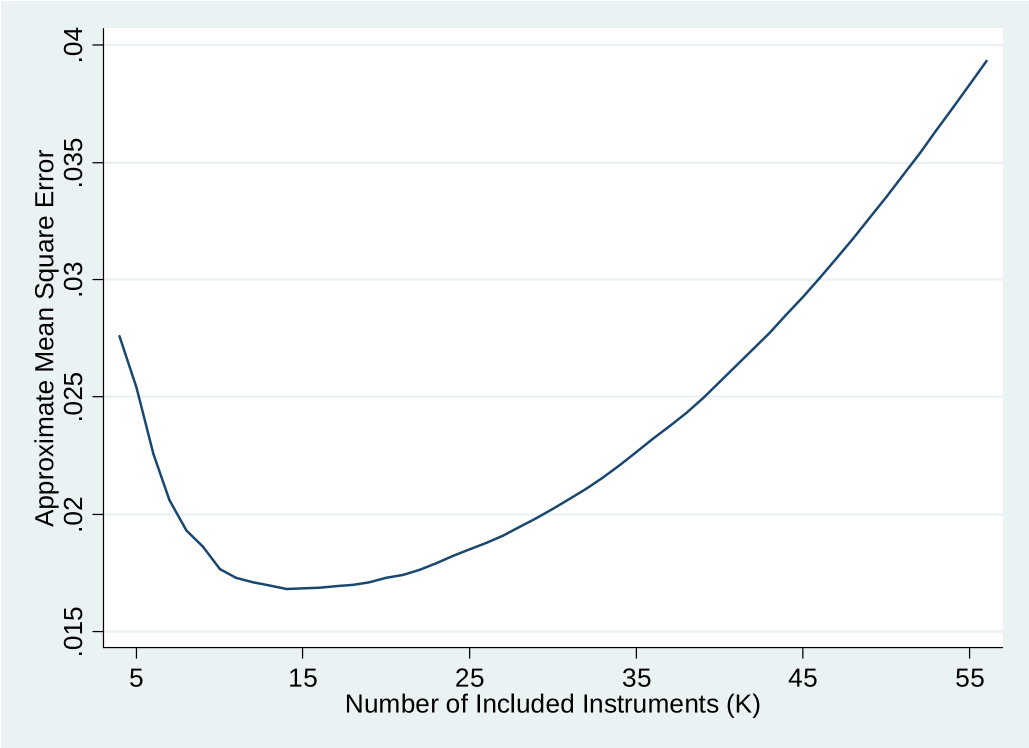


Table 1: Summary Statistics

Panel A: Worker Characteristics (1999)[a]

Characteristic	Fraction/Mean (s.d.)	Characteristic	Fraction/Mean (s.d.)
2003 DP utilization[b]	0.069	Industry	
Age	52.69 (4.715)	agriculture, fishing	0.004
Female	0.423	mining, oil	0.022
Education (years)		manufacturing	0.171
≤9 yrs	0.133	electric, gas, water	0.017
13-15 yrs	0.318	construction	0.055
≥16 yrs	0.195	wholesale/retail trade	0.106
Marital status		hotels, restaurants	0.010
married	0.726	transport, communic.	0.089
widowed	0.024	financial intermed.	0.040
divorced	0.138	real estate, business	0.068
Children <18 y.o.		public admin, defense	0.112
1	0.268	education	0.136
2-3	0.265	health, social work	0.144
≥4	0.012	other services	0.026
On widow(er) pension	0.015	1999-2003 PDR[c]	
Income/wealth		10-30%	0.168
personal income	315969 (169671)	30-60%	0.108
other HH income	279240 (586291)	60-100%	0.134
net HH wealth	320633 (2881228)	100%	0.060
Emp status: PT	0.089	1995-1999 PDR[d]	
Tenure		10-30%	0.185
1-3 yrs	0.208	30-60%	0.076
3-5 yrs	0.160	60-100%	0.127
≥5 yrs	0.579	100%	0.075
Plant size (FTEs)		1995 Income/wealth	
5-25	0.231	personal income	258467 (126669)
25-100	0.267	other HH income	222348 (285368)
≥100	0.318	net HH wealth	141959 (2616800)

[a] Characteristics measured at end of 1999 unless otherwise indicated.
[b] Includes workers entering DP prior to death, emigrating, or drawing early retirement.
[c] Measures decline in employment (FTEs) in worker's 1999 plant of employment.
[d] Measures decline in employment (FTEs) in worker's 1995 plant of employment, set to zero for plants with fewer than 5 FTEs in 1995.

Table 1: Summary Statistics (cont.)

Panel B: Peer Group Characteristics (1995)[a]

Characteristic	Fraction/Mean (s.d.)	Characteristic	Fraction/Mean (s.d.)
2000 DP rate[b]	0.074 (0.045)	Plant size (FTEs)	
Age	49.75 (1.343)	5-25	0.248 (0.094)
Female	0.434 (0.070)	25-100	0.282 (0.087)
Education (years)		≥100	0.359 (0.142)
≤9 yrs	0.163 (0.091)	Industry	
13-15 yrs	0.286 (0.081)	agriculture, fishing	0.006 (0.018)
≥16 yrs	0.171 (0.102)	mining, oil	0.019 (0.035)
Marital status		manufacturing	0.167 (0.108)
married	0.744 (0.145)	electric, gas, water	0.015 (0.026)
widowed	0.021 (0.021)	construction	0.051 (0.041)
divorced	0.124 (0.080)	wholesale/retail trade	0.136 (0.063)
Children <18 y.o.		hotels, restaurants	0.013 (0.020)
1	0.193 (0.076)	transport, communic.	0.085 (0.052)
2-3	0.154 (0.082)	financial intermed.	0.036 (0.029)
≥4	0.007 (0.015)	real estate, business	0.065 (0.048)
On widow(er) pension	0.014 (0.017)	public admin, defense	0.106 (0.067)
On social assistance	0.005 (0.012)	education	0.117 (0.067)
On sick money	0.042 (0.030)	health, social work	0.151 (0.069)
Rec'd SM in year	0.118 (0.050)	other services	0.033 (0.030)
Income/wealth[c]		1995-1999 PDR[c]	
personal income	250928 (37088)	10-30%	0.173 (0.078)
other HH income	226370 (70577)	30-60%	0.081 (0.055)
net HH wealth	177065 (295593)	60-100%	0.129 (0.076)
Emp status: PT	0.124 (0.058)	100%	0.074 (0.050)
Tenure ≥1 yr	0.917 (0.043)	Peer group population	118.9 (115.8)

[a] Characteristics measured at end of 1995 unless otherwise indicated.
[b] Includes workers entering DP prior to death, emigrating, or drawing early retirement.
[c] Measures decline in employment (FTEs) in worker's 1995 plant of employment, set to zero for plants with fewer than 5 FTEs in 1995.

Table 1: Summary Statistics (cont.)

Panel C: Neighborhood and Municipality Characteristics

Characteristic	Fraction/Mean (s.d.)	Characteristic	Fraction/Mean (s.d.)
Neighborhood (1995)		Municipality (1995)	
total population[a]	692.3 (607.7)	total population[a]	75828.0 (116071.9)
fraction immigrant	0.049 (0.058)	fraction immigrant	0.055 (0.045)
fraction <18 y.o.[a]	0.225 (0.065)	fraction <18 y.o.[a]	0.222 (0.031)
fraction 18-41 y.o.[a]	0.518 (0.074)	fraction 18-41 y.o.[a]	0.529 (0.025)
fraction ≥62 y.o.[a]	0.180 (0.090)	fraction ≥62 y.o.[a]	0.192 (0.036)
mean income[b]	174283 (32351)	mean income[b]	170159 (21851)
mean wealth[b]	35685 (128383)	mean wealth[b]	32770 (45732)
unemployment rate[b]	0.136 (0.061)	unemployment rate[b]	0.144 (0.046)
fract. emp'd FT[c]	0.546 (0.099)	fract. emp'd FT[e]	0.524 (0.075)
fract. emp'd PT[c]	0.076 (0.033)	fract. emp'd PT[e]	0.073 (0.017)
fract. emp'd MPT[c]	0.056 (0.029)	fract. emp'd MPT[e]	0.057 (0.017)
fract. self-emp'd[c]	0.071 (0.052)	fract. self-emp'd[e]	0.080 (0.039)
fract. perm. DP[c]	0.115 (0.061)	fract. perm. DP[e]	0.122 (0.032)
fract. temp. DP[c]	0.002 (0.005)	fract. temp. DP[e]	0.002 (0.002)
fract. rehab pension[c]	0.019 (0.016)	fract. rehab pension[e]	0.020 (0.007)
fract. day money[c]	0.043 (0.028)	fract. day money[e]	0.045 (0.016)
fract. unemployed[c]	0.012 (0.013)	fract. unemployed[e]	0.013 (0.004)
Neighborhood (1999)		Municipality (1999)	
mean income[b,d]	220396 (41567)	mean income[b]	216682 (29146)
unemployment rate[b,d]	0.075 (0.045)	unemployment rate[b]	0.079 (0.034)
<20 in neighborhood[b]	0.007 (0.083)		

[a] Calculated over native Norwegians.
[b] Calculated over natives age 22-67.
[c] Calculated over natives age 41-62.
[d] Set to missing if neighborhood no longer exists or contains fewer than 20 natives age 22-67 in 1999.
[e] Calculated over natives age 41-62 excluding those in worker's neighborhood.

Table Notes: N=378148. Sample consists of workers, age 45-63 in 1999, employed FT or PT in 1995 and 1999, excluding those in small 1999 plants (<5 FTEs), on social assistance in 1999, missing income/wealth variables in 1999, or having fewer than 10 persons in defined peer group (see text for definition).

Table 2: Effect of Peer Plant Downsizing on Peer Rate of Disability Pension Utilization

Panel A: Full set of instruments

Dependent variable: Peer 2000 DP rate

PDR	Agriculture, fishing	Mining, oil	Manufacturing	Electric, gas, water	Construction	Wholesale/retail trade	Hotels, restaurants
10-30%	-.065 (.086)	.056 (.046)	.024 (.009)*	.008 (.035)	.014 (.028)	.022 (.021)	.061 (.060)
30-60%	-.044 (.089)	-.003 (.040)	.001 (.012)	-.035 (.053)	.038 (.040)	.056 (.024)*	.011 (.066)
60-100%	.166 (.105)	.178 (.036)**	.025 (.014)+	.120 (.053)*	.055 (.037)	-.002 (.029)	-.071 (.077)
100%	.052 (.093)	.299 (.116)**	.020 (.023)	-.006 (.049)	.026 (.035)	.011 (.028)	.145 (.063)*

PDR	Transport, communic.	Financial intermed.	Real estate, business	Public admin, defense	Education	Health, social work	Other services
10-30%	-.019 (.030)	-.048 (.031)	.031 (.032)	.011 (.019)	.004 (.014)	-.001 (.013)	-.027 (.045)
30-60%	.084 (.032)**	-.016 (.040)	-.040 (.046)	-.036 (.020)+	-.007 (.023)	-.008 (.020)	.009 (.055)
60-100%	.062 (.023)**	.003 (.063)	.071 (.044)	.013 (.015)	.041 (.013)**	.009 (.013)	-.063 (.056)
100%	.022 (.026)	-.002 (.067)	-.008 (.036)	.054 (.033)+	-.021 (.018)	-.019 (.017)	.047 (.056)

Table 2: Effect of Peer Plant Downsizing on Peer Rate of Disability Pension Utilization (continued)

Panel B: Preferred set of instruments

Dependent variable: Peer 2000 DP rate

PDR	Agriculture, fishing	Mining, oil	Manufacturing	Electric, gas, water	Construction	Wholesale/retail trade	Hotels, restaurants
10-30%	--	--	.024 (.009)**	--	--	--	--
30-60%	--	--	--	--	--	.047 (.023)*	--
60-100%	.198 (.092)*	.167 (.033)**	.024 (.013)$^{+}$	.124 (.051)*	--	--	--
100%	--	.295 (.117)*	--	--	--	--	.141 (.060)*

PDR	Transport, communic.	Financial intermed.	Real estate, business	Public admin, defense	Education	Health, social work	Other services
10-30%	--	--	--	--	--	--	--
30-60%	.082 (.031)**	--	--	-.042 (.019)*	--	--	--
60-100%	.059 (.022)**	--	.068 (.043)	--	.045 (.011)**	--	--
100%	--	--	--	.048 (.032)	--	--	--

Notes: N=378148. Mean dependent variable=.074. Estimates in each panel reflect results from single OLS model, adjusted for peer group, neighborhood and municipality characteristics in 1995, and individual characteristics in 1999. Panel B limits the instrument set to minimize the approximate mean squared error around the IV estimate (see text for details). Conventional robust standard errors in parentheses, corrected for non-independent residuals within neighborhoods. $^{+}$, * and ** denote significance at the 10, 5 and 1 percent level.

Table 3: Main Results: Effect of Social Interaction on Disability Pension Entry

Dependent variable: 2003 DP utilization

	No instruments		Preferred instrument set				Full instrument set			
	OLS (1)	Probit (2)	2SLS (3)	LIML (4)	BC-2SLS (5)	IV-Probit (6)	2SLS (7)	LIML (8)	BC-2SLS (9)	IV-Probit (10)
Peer 2000 DP rate	.069**	.461**	.504**	.506**	.568**	3.602**	.456**	.463**	.754**	3.388**
	(.012)	(.089)	(.123)	(.124)	(.144)	(.923)	(.108)	(.110)	(.212)	(.982)
		[.055]				[.441]				[.414]
First-stage results										
F statistic			7.07	7.07	7.07	7.07	2.29	2.29	2.29	2.29
Partial R^2			.0100	.0100	.0100	.0100	.0123	.0123	.0123	.0123
Test of overidentifying restrictions										
J/AR/Sargan statistic[a]			10.35	12.34	12.64		75.20	84.67	92.71	
p-value			.665	.500	.476		.037	.006	.001	
mean	.069	.069	.069	.069	.069	.069	.069	.069	.069	.069
N	378148	378148	378148	378148	378148	378148	378148	378148	378148	378148

Notes: All estimates adjusted for peer group, neighborhood and municipality characteristics in 1995, and individual characteristics in 1999. Conventional robust standard errors in parentheses, corrected for non-independent residuals within neighborhoods. +, * and ** denote significance at the 10, 5 and 1 percent level. Mean marginal effect implied from probit models presented in brackets.

[a] Hansen J statistic reported for 2SLS, Anderson-Rubin statistic for LIML, and Sargan statistic for BC-2SLS.

Table 4: Robustness Checks

Dependent variable: 2003 DP utilization

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Peer 2000 DP rate	.504**	.500**	.504**	.505**	.525**	.677**	.653*
	(.123)	(.125)	(.125)	(.125)	(.138)	(.187)	(.260)
First-stage results							
F-statistic	7.07	7.06	7.06	7.06	6.00	5.71	5.39
Partial R^2	.0100	.0097	.0097	.0097	.0081	.0080	.0083
Added covariates							
PDRs (95-99)[a]		X	X	X	X	X	X
PDRs (99-03)[a]			X	X	X	X	X
Δ income/wealth (95-99)[b]				X	X	X	X
county fixed effects, plus nbhd/munic unemp rate and mean income (1999)[b]					X	X	X
Sub-samples							
employed same plant 95-99						X	X
stable/growing plant 95-99[d]							X
mean	.069	.069	.069	.069	.069	.071	.071
N	378148	378148	378148	378147	378147	229839	115015

Notes: 2SLS estimates using preferred instrument set. All estimates adjusted for peer group, neighborhood and municipality characteristics in 1995, and individual characteristics in 1999. Conventional robust standard errors in parentheses, corrected for non-independent residuals within neighborhoods. $^+$, * and ** denote significance at the 10, 5 and 1 percent level.

[a] Consists of 56 dummy covariates capturing plant industry and downsizing magnitude (10-30, 30-60, 60-100 and 100 percent).

[b] Entered as third-order terms for change in personal income, change in other household income and change in household wealth. One observation missing household wealth in 1995 was omitted.

[c] Entered as second-order terms.

[d] 1999 plant of employment has at least as many FTEs in 1999 as in 1995.

Table 5: Specification Tests: Employment Status and Sick Money Utilization by Year

	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003
Panel A												
Dependent variable: Employed FT/PT at end of year												
Peer 2000 DP rate	.298*	.399**	.148+	--	-.030	-.161+	.046	--	-.360*	-.106	-.125	.010
	(.152)	(.134)	(.087)		(.090)	(.090)	(.070)		(.149)	(.123)	(.147)	(.154)
mean	.910	.928	.963	1.000	.978	.975	.980	1.000	.926	.937	.921	.906
Panel B												
Dependent variable: Received SM at end of year												
Peer 2000 DP rate	-.068	-.228**	.017	.048	.097	-.045	-.033	.201+	.293*	.331**	.044	.278*
	(.067)	(.072)	(.068)	(.075)	(.078)	(.088)	(.084)	(.116)	(.124)	(.129)	(.148)	(.137)
mean	.022	.022	.022	.025	.031	.035	.037	.067	.068	.072	.075	.077
N	377135	377329	377733	378148	377859	377747	377839	378148	366250	354593	340962	326283

Notes: 2SLS estimates using preferred instrument set. All estimates adjusted for peer group, neighborhood and municipality characteristics in 1995, and individual characteristics in 1999. Conventional robust standard errors in parentheses, corrected for non-independent residuals within neighborhoods. +, * and ** denote significance at the 10, 5 and 1 percent level. Results for years 2000-2003 omit workers who died or emigrated, and those drawing early retirement (AFP) or DP. Differences in sample size prior to 2000 reflects persons missing individuals records in *FD-trygd* in a given year.

Table 6: Specification Tests: Neighborhood Disability Participation and Employment Rates by Year

	1992	1993	1994	1995
Panel A				
Dependent variable: Neighborhood DP rate				
Peer 2000 DP rate	.061	.029	.031	.047
	(.086)	(.089)	(.090)	(.091)
mean	.087	.094	.105	.116
Panel B				
Dependent variable: Neighborhood LFP rate				
Peer 2000 DP rate	.181	.204	.128	.033
	(.164)	(.152)	(.144)	(.148)
mean	.622	.619	.625	.622

Notes: N=378148. 2SLS estimates using preferred instrument set. Dependent variables calculated over persons age 41-62 in 1995 neighborhood, defined as fraction receiving DP at end or year (Panel A), or fraction employed FT or PT at end of year (Panel B). All estimates adjusted for peer group, neighborhood and municipality characteristics in 1995, and individual characteristics in 1999, excluding neighborhood-level employment/program status variables (i.e. fraction employed FT, employed PT, employed MPT, self-employed, receiving permanent DP, receiving temporary DP, receiving rehabilitation pension, receiving day money, and unemployed without day money). Conventional robust standard errors in parentheses, corrected for non-independent residuals within neighborhoods. $^{+}$, * and ** denote significance at the 10, 5 and 1 percent level.

Table 7: Effect of Social Interaction on Disability Pension Entry by Year

Dependent variable: DP utilization in year

	2000	2001	2002	2003
Peer 2000 DP rate	.137**	.180*	.313**	.504**
	(.050)	(.074)	(.097)	(.123)
mean	.013	.029	.049	.069

Notes: N=378148. 2SLS estimates using preferred instrument set. All estimates adjusted for peer group, neighborhood and municipality characteristics in 1995, and individual characteristics in 1999. Conventional robust standard errors in parentheses, corrected for non-independent residuals within neighborhoods. +, * and ** denote significance at the 10, 5 and 1 percent level.